

Master thesis

The Employment Effect of a Dutch Social Assistance Reform for the Youth

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July 20, 2016

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**Submitted in fulfillment of the degree Economics and Business
Economics - International Economics**

Erasmus University Rotterdam

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Abstract

We examine the employment effect of a large Dutch social assistance reform targeted at individuals up to 27 years of age. The *Wet Investeren in Jongeren* (WIJ, Work Investment Act for Young Individuals) reform, introduced in 2009, restricted eligibility of social assistance benefits and provided a work-learn offer. We provide an extensive theoretical framework in which we argue that the expected effects on employment are, except from a possible lock-in effect, positive and substantial in competitive labor markets. We expect this positive effect to be smaller in non-competitive labor markets, especially when unemployment is high. We use a difference-in-differences methodology and a large panel data set to study the effect of the reform on the use of social assistance benefits and employment. Our main finding is a large negative treatment effect on income from social assistance of 63 % for individuals 20–22 years of age and 26% for individuals 23–25 years of age, whereas we find no effect on employment. This finding is robust across a large number of dependent variables, specifications and demographic groups. This points at little to no employment effect of stricter eligibility requirements combined with active labor market policies for young individuals during economic recessions. High unemployment rates make labor demand, rather than labor supply, the constraining factor for employment to increase.

JEL codes: C21, H31, J21

Keywords: Youth, welfare reform, NEETs, DD

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1 Introduction

Youth unemployment is considered to be one of the main problems in Europe since the outbreak of the Great Recession (Bell and Blanchflower, 2011). As a reaction to this, policies targeted at individuals not in employment, education or training (NEETs) have been increasingly intensified in the Netherlands, as in many other OECD countries (Carcillo and Königs, 2015). Some of these are ‘Welfare conditionality’ which already exists for a longer period in the UK. Examples of more activating labor market policies (ALMPs) are the New Deal in the UK and the Welfare to Work programmes in the US (Kluve, 2014). In France, the Revenu de solidarit active (RMI, Solidarity Labor Income) was introduced in 2009, which contains an in-work benefit component to restore financial incentives to work and to reduce the inactivity trap for high school dropouts (Bargain and Doorley, 2013).

In this paper we study a social assistance reform targeted at individuals under 27 years old, the *WIJ* (i.e., the Work Investment Act for Young Individuals), introduced in 2009 in the Netherlands. The idea behind the WIJ reform was to prevent so-called NEETs (not in employment, education or training) among the youth. The government wanted everyone to participate in employment, education or training and wanted to reduce welfare dependency at the same time. The right to social assistance benefits was replaced by a right to a work-learn offer with, if necessary, an income supplement. In this paper we study the effects of the WIJ-reform on: participation in social assistance, income from social assistance, employment measured by $\text{income from work} > 0$, income from work, employment measured by $\text{hours worked} > 0$ and hours worked.

We study the effects of the WIJ reform using a difference-in-differences (DiD) design. The treatment groups consist of individuals 20–22 and 23–25 years of age. Our preferred control group consists of individuals 27–29 years of age. We use a large and rich administrative panel data set, the *Arbeidsmarktpanel* (LMP, Labor Market Panel) of Statistics Netherlands. The LMP contains individual and household characteristics and a number of labor market outcomes.

Our main finding is that the reform reduced income from social assistance with 63% for individuals 20–22 years of age and 26% for individuals 23–25 years of age, without having an effect on employment. This finding of a large decrease of income from social assistance without having an effect on employment is robust across a

large number of dependent variables, specifications and demographic groups. Our preferred explanation for this is that labor supply policies are not effective for young individuals when labor demand, rather than labor supply, is the constraining factor for employment to go up. Labor demand constraints can be explained by high unemployment rates caused by the Great Recession. We can draw lessons from this reform for future social assistance reforms targeted at young individuals, of which the most important one is that the effectiveness is expected to be very limited in periods of a shortfall in demand.

We start with a theoretical framework in which we explain the expected effects in competitive and non-competitive labor markets. In perfect labor markets, we expect positive effects of the WIJ reform on employment, both at the extensive and intensive margin. Only a possible lock-in effect could explain a negative effect of our reform on employment in competitive labor markets, as people may search for a job less during a work-learn offer (Van Ours, 2004). However, Van der Ploeg (2003) argues that the European labor market, among which the Dutch labor market, resembles the non-competitive labor market much more with strong trade unions, efficiency wages, costly search and mismatch and high transaction costs. Besides, we deal with minimum wages and sticky wages, which are not in accordance with competitive labor markets either. The expected effect of the reform on employment is expected to be smaller in case of a non-competitive labor market. Also possible crowding out effects of the work-learn offer may decrease the effectiveness of the reform, especially as the reform takes place in a period of high unemployment.

Krueger and Meyer (2002) argue that more research is necessary on the optimal generosity of benefits during economic recessions as well as on the incentive effects caused by interactions among social programs. Landais et al. (2010) already showed in a theoretical paper that unemployment benefits should be countercyclical. We address both research suggestions of Krueger and Meyer (2002) empirically, as we research the effect of a social assistance reform on social assistance benefits combined with active labor market policies (ALMPs) during a recession. Hence, our contribution is both on the effectiveness of ALMPs combined with stricter conditionality for the youth (Kluve, 2014) as on the specific context of a Great Recession contributing to Krueger and Meyer (2002) and Landais et al. (2010). In addition, this study is specifically relevant for Dutch policy makers as it learns us something about the effectiveness of a large Dutch social assistance reform for the youth during

an economic recession.

The outline of the paper is as follows. In Section 2 we provide a theoretical framework in which we explain the expected effects. A discussion of the empirical literature is given in Section 3. Section 4 describes the main features of the reform. In Section 5 we discuss the empirical methodology. A description of the data is given in Section 6. Section 7 gives the results. A discussion of the results and some final remarks are given in Section 8.¹

2 Theoretical framework

In this chapter we discuss the expected effects of the reform on social assistance benefits and employment. We will start with explaining labor supply theory after which we explain the different expected effects in competitive and non-competitive labor markets. We explain the non-competitive case by elaborating on the expected effects in efficiency wage models, labor union models and job search models. Afterwards, we discuss job search models a bit more extensively as these provide us with a better insight in the mechanisms. We continue with discussing the role of the business cycle and wage rigidity as well as productivity and minimum wages. We conclude our theoretical framework with explaining the different effects that are expected for the two main features of the reform, the work-learn offer and stricter conditionality of social assistance benefits.

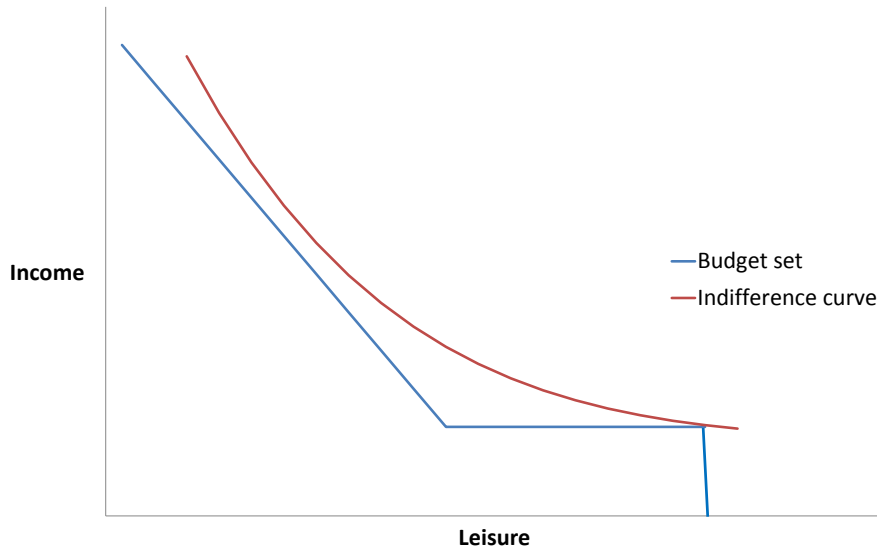
Labor supply theory

Individuals deal with constraints in time and money, therefore a trade-off exists between leisure and income from work. Labor activity itself is assumed to bring a negative utility as most people prefer leisure over labor. The indifference curve between leisure and consumption from labor income is convex, which can be explained by diminishing marginal utility of income. As leisure can be replaced by labor and vice versa, the budget constraint is a line through the two points (income attained if all leisure is sold; zero leisure) and (zero income; leisure if no leisure is sold). We can describe this by $N + W(1 - t)H = Y$, where N is exogenous unearned income, W is hourly wage rate, t is the tax rate on labor, H is total hours worked (calculated by

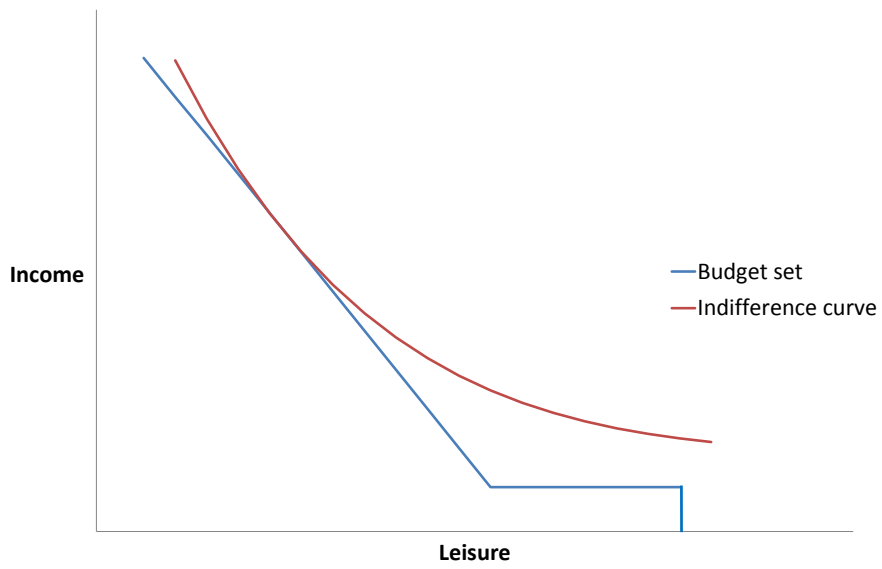
¹The appendix contains some supplementary material

Figure 1: Increased labor supply by stricter conditionality

(a) Labor supply before reform



(b) Labor supply after reform



total hours minus leisure), and Y is total income (Moffitt, 2002). Individuals maximize the utility function $U(H, Y)$, subject to the budget constraint, to obtain the point where the highest possible indifference curve touches the budget constraint. Means-tested benefits create non-convexities in the budget set both under linear and non-linear taxes, but for reasons of simplicity we will limit ourselves in our example to the linear tax. The budget set becomes $N + W(1 - t)H$ if $N + W(1 - t)H > B$ and B if $N + W(1 - t)H < B$, with B for benefits. High social assistance benefits make it more likely that the highest possible indifference curve touches the budget constraint at a point where individuals get their income from social assistance benefits rather than from work, see Figure 1a. More individuals will have their highest attainable indifference curve touching the budget constraint for positive values of labor if social assistance benefits are reduced, see Figure 1b. Hence, lower social assistance benefits are expected to increase labor supply. Furthermore, we can add social assistance participation in the utility function as this carries some disutility, possibly arising from the stigma of being on social assistance, which we denote as S (Moffitt, 1983; Besley and Coate, 1992). This stigma may either increase or decrease by the work-learn offer dependent on how the work-learn offer is perceived by the individual and his environment. At the same time, the work-learn offer, denoted by P , takes effort and is thereby expected to come with negative utility leading to the utility function $U(H, Y, S, P)$. This negative utility for being on social assistance and for the work-learn offer can lead to a decrease of utility for zero units of labor, making it more likely that the highest possible utility curve touches the budget constraint at positive values for labor supply. We expect average social assistance benefits to go down as the reform changed the right to income in the right to work with a temporary income supplement if necessary. Therefore we expect labor supply to go up by the reform according to the canonical labor supply model.

Competitive and non-competitive labor markets

In this part, we start with describing the effects of lower social benefits, stricter conditionality of benefits and ALMPs on wages and employment in both competitive and non-competitive labor markets, see Table 1 and Table 2. We deal with competitive labor markets if individual firms and workers are wage takers. No involuntary unemployment exists in competitive labor markets as no one want to work at a lower wage than the market clearing level wage. When labor markets are non-competitive,

Table 1: Effects on wages in competitive and non-competitive labor markets

	Higher benefits	Stricter conditionality	ALMPs
Competitive labor market	+	-	-
Non-competitive labor market	+	-	-

Table 2: Effects on employment in competitive and non-competitive labor markets

	Higher benefits	Stricter conditionality	ALMPs
Competitive labor market	-	+	+
Non-competitive labor market	-	+	+

individual firms or organised employees do exert some market power to set wages. Labor markets are non-competitive in case of efficiency wages, bargaining power by labor unions or labor market frictions, which all lead to higher wages than the market clearing level and involuntary unemployment.

In competitive labor markets, we expect a negative effect of an increase in social assistance benefits on employment as the outside option of living on benefits becomes more attractive, leading to lower labor supply and higher wages, see Table 1. This works the same as we have explained in the former chapter on labor supply theory. We expect positive effects of both stricter conditionality for benefits and of ALMPs on employment as these stricter conditionality and ALMPs decreases the attractiveness of the outside option of being unemployed. We need to distinguish between the substitution effect and the income effect when we consider tax or benefit changes. The substitution effect is a negative effect of taxes on employment which is explained by leisure becoming cheaper when the net wage decreases by higher taxes. However, there is no effect of the reform on taxes, therefore the substitution effect is zero. The income effect can be defined as the increase in leisure resulting from an increase in real income given that leisure is a normal good. stricter conditionality of benefits reduce the average net income of benefits receivers and people are willing to supply more labor when they are poorer as an increase in income has more value at lower levels of income, due to decreasing marginal utility of income.

In non-competitive labor markets, wages are normally higher than the market-clearing level due to efficiency wages, labor unions or frictions. Efficiency wages which are higher than the market-clearing level are needed to recruit, motivate and retain employees. Shapiro and Stiglitz (1984) argue that the inability of employers to observe workers on the job effort without costs can explain involuntary unemployment. If wages are considerably higher than unemployment benefits, employees have something to lose when they are fired and therefore choose not to shirk. However, it is this higher wages than the market-clearing level that explain involuntary unemployment. In efficiency wage models, the expected effects of social assistance benefits on employment are negative. Higher benefits make the outside option of being unemployed more attractive, leading to lower labor supply. This makes firms to raise wages in order to still be able to recruit, motivate and retain employees. This higher wages lead to lower labor demand and thereby lower employment. Whereas both stricter conditionality of social assistance benefits and ALMPs reduce the out-

side option for employees, increase wages and thereby increase labor demand and employment.

Another explanation for a positive effect of higher social assistance benefits on wages is that these benefits give more bargaining power to labor unions. This increased bargaining power can be explained by the outside option of being unemployed being more attractive when social assistance benefits are higher. This higher wages leads to lower labor demand and thereby lower employment. The opposite holds for stricter conditionality for benefits and ALMPs as these reduce the outside option of being unemployed and thereby decrease the bargaining power of unions, reduce the wages and increase labor demand and thereby employment.

The same mechanism as we explained for efficiency wages and union models also applies to search frictions. Higher benefits increase the outside option of being unemployed, which increases the wages in order to induce people to accept job offers rather than continue with searching. Higher wages reduce labor demand and thereby employment. Whereas stricter conditionality and ALMPs decrease the attractiveness of the outside option, which decreases wages, reduce labor demand and thereby increase employment.

Job search models

job search models focus on frictional unemployment and the job search process (Narendranathan and Nickell, 1985; Flinn and Heckman, 1982). We will discuss job search theory a bit more extensively, like we did with labor supply theory, as it belongs to the core of labor economics. The idea behind the job search model is that unemployed people weigh the discounted value of accepting a job with the discounted value of continued search. Search theory takes uncertainty into account because the job searchers do not know when an appropriate job offer will arrive. The offered wage is compared with the reservation wage of rejecting a job offer. Lower social assistance benefits make the reservation wage lower as it reduces the discounted value of searching. The arrival rate is the rate at which job offers arrive. This arrival rate may be reduced by the reform in the short-run, as the work-learn offer leaves less time for searching, which is called a lock-in effect (Van Ours, 2004). This may explain a negative effect of the reform on labor supply and thereby employment in the short-run. Besides, the work-learn offer itself may increase the arrival rate of job offers in the future, which has a positive effect on the reservation wage.

Nevertheless, we should not forget that we deal with the bottom of the labor market. Here, we expect that the arrival rate of job offers for which the unemployed is accepted is very low. Therefore the discounted value of continued searching is relatively low, making the reservation wage low. If the wage is already higher than the reservation wage, the constraint of matching can be that people are not able to find a job at all, rather than not willing to accept a job. Skills learned at the work-learn offer may increase arrival rates of jobs for which searchers are accepted. This may increase labor participation as these people get higher chances of finding a job. Hence, based on job search theory we expect that, except from a possible negative lock-in effect of the work-learn offer on the short-term, both stricter eligibility of social assistance as the work-learn offer do have a positive effect on labor supply and employment.

Mortensen and Pissarides (1994) find that an aggregate shock induces negative correlation between job creation and job destruction, whereas a dispersion shock induces positive correlation. Hence, also the job search process is highly dependent on the business cycle as an economic downturn may lead to higher job destruction and lower job creation at the same time. Furthermore, they find more volatile dynamics for the job destruction process than for the job creation process. Van den Berg (1990) finds by using a non-stationary structural job search model that the consequences of a downward shift in the level of benefits would increase the elasticity of duration with respect to the level of benefits. This indicates that the effect of a reduction of social benefits on employment is higher for low levels of benefits.

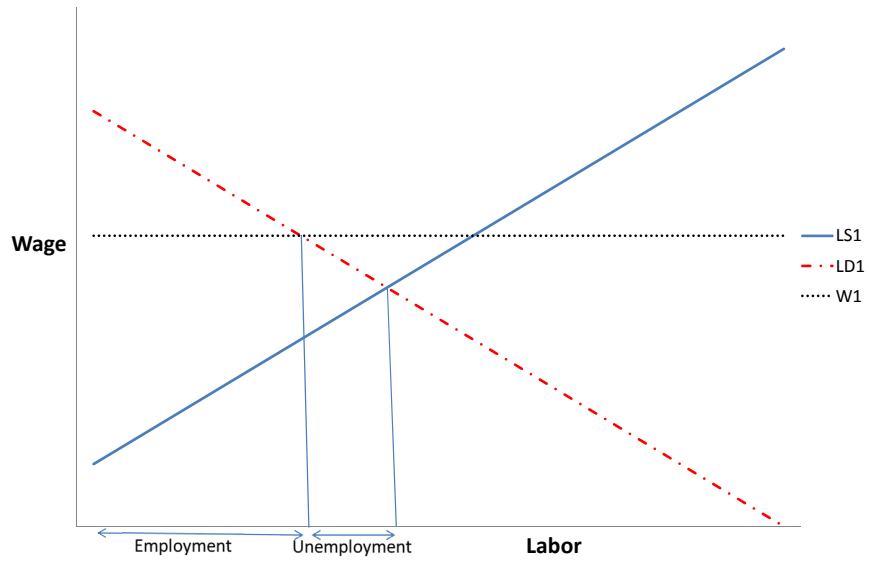
Wage rigidity and the Great Recession

Downward price and wage rigidity have an effect on employment in non-competitive labor markets. This stickiness can be explained by menu cost and employees not willing to accept a decrease in their wage. We have involuntary unemployment if the wage level is higher than the market clearing wage, see Figure 2a. This can be explained by the business cycle which leads to lower labor demand and thereby to higher wages than the market clearing level.

As the eligibility and level of social assistance is reduced by the WIJ reform, we expect labor supply to increase from LS1 to LS2, see Figure 2b. However, to what extent this higher labor supply leads to higher employment depends also on the demand for labor. The demand for labor depends on the labor cost which are

Figure 2: Increased unemployment by increased labor supply

(a) structural unemployment



(b) Increased unemployment

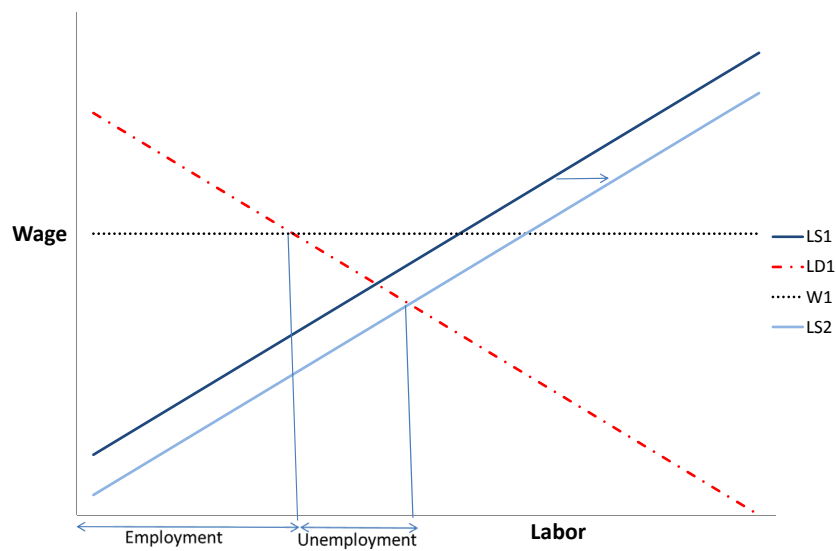
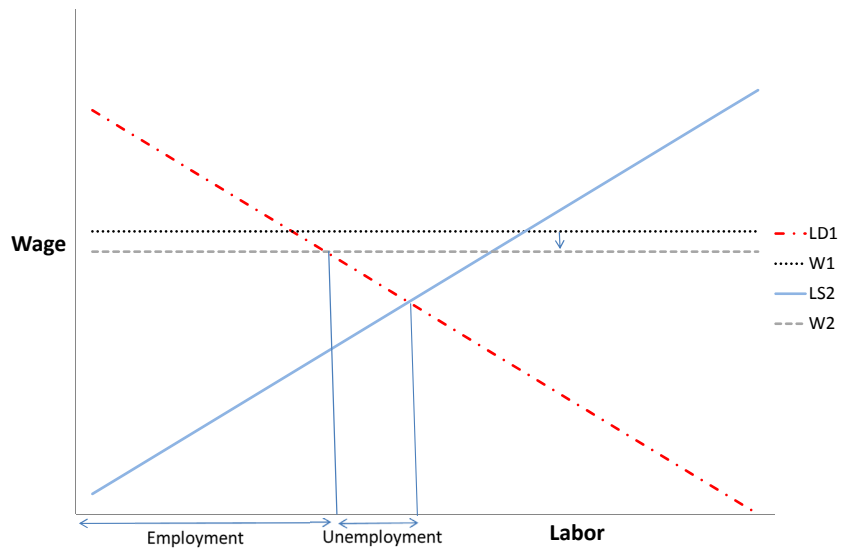
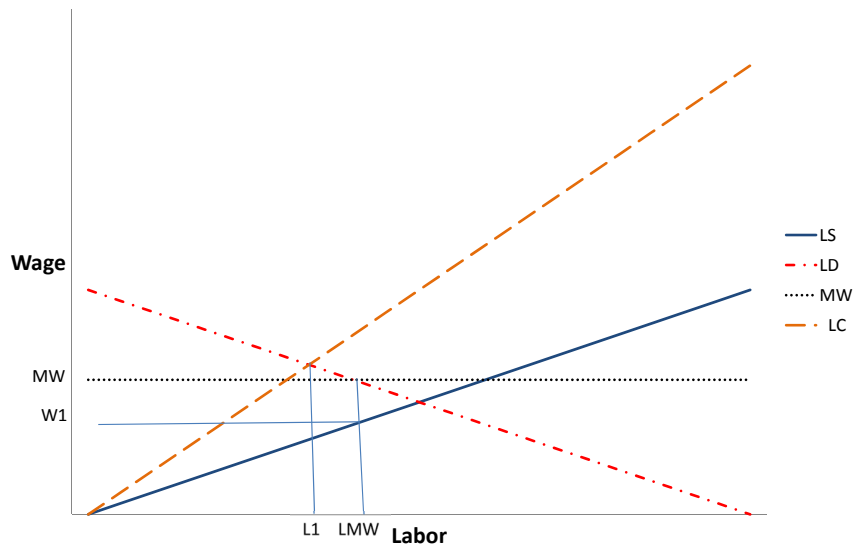


Figure 3: Reduced unemployment by lower or higher wages

(a) Decreased unemployment



(b) Monopsony model



directly related with the real wages. Therefore the effect of higher labor supply on employment is expected to come by a reduction of real wages from W_1 to W_2 , which leads to higher labor demand and thereby employment, see Figure 3a. These people who start supplying more labor will not get employed when there are no jobs available due to a restricted labor demand. This is the case if wages are very sticky and hardly responding to higher labor supply.

Wages were already higher than the market clearing wage during the social assistance reform, because of the Great Recession. It is questionable to what extent higher labor supply caused by the reform helps to reduce the wage even faster. For example, real wage drops are politically much more feasible than nominal wage drops which might be needed to decrease the real wages even faster. Another problem of a potential large wage decrease is that it will increase uncertainty which would have a negative effect on aggregate demand and thereby indirectly on employment.

Krueger and Meyer (2002) argue that there is probably less of an efficiency loss from reduced search effort by the unemployed during an economic recession than during an economic boom. Also Landais et al. (2010) find that welfare losses of high unemployment benefits as well as the effectiveness of ALMPs are expected to be lower during economic recessions. Hence, as our social assistance reform took place during an economic recession, we expect smaller effects of both stricter eligibility requirements for social assistance and of ALMPs on employment.

Low productivity individuals and minimum wage

No effect of the WIJ reform on employment is expected for the individuals for whose productivity is lower than the minimum wage, as long as the minimum wage is unaffected by the reform, and the productivity of the individual is unchanged. These individuals fall under the category of structural unemployment. There is no reason for employers to hire these low productivity individuals, after all they would make a loss on them. However, this people may have wanted to work for a lower wage than the minimum wage for which they would have been hired. The minimum wage makes this impossible, leading to involuntarily unemployment. However, the work-learn offer may increase human capital for these individuals, making some of them more productive than the minimum wage. Besides, increased labor supply by the reform may lead to increased political pressure to reduce, or at least not increase, the minimum wage. Both increased productivity as reduced minimum wages lead to

a decrease in structural unemployment as some of the individuals who are almost as productive as the minimum wage will become more productive than the minimum wage and can thereby become employed. However, as long as the minimum wage and the productivity of individuals remain the same, we expect increased involuntary unemployment and no increased employment as the market clearing wage is lower than the minimum wage and this difference between the minimum wage and the market clearing wage increases by an increase in labor supply induced by the reform. In general, we expect the minimum wage to play no role as social assistance benefits are linked to the minimum wage, which is also the case in the Netherlands. Although, despite stricter conditionality of benefits, there is no indication that our reform led to a minimum wage decrease. Therefore the reform seems to play a role in increasing structural unemployment because the market clearing wage is decreased by an increase in labor supply, whereas the the minimum wage is unchanged.

However, higher minimum wages do not always lead to lower employment (Card and Krueger, 1993; Katz and Krueger, 1992). This effect could be explained by a monopsony competition model in which there is one buyer (employer) and many sellers (employees) (Bhaskar et al., 2002), see Figure 3b. In this case a raise in the minimum wage (MW) may increase employment from L1 to LMW as long as the minimum wage is between the market clearing wage under perfect competition and the equilibrium wage when the employer set the wage level, see Figure 3b. The implication of this model is that a reduced minimum wage, which may potentially follow from the reform, can theoretically also lead to lower employment.

Conditionality

The social assistance benefits are conditional on not being able to find a job. The government also refuses to provide social assistance benefits when individuals were laid off because of misconduct or when they resign. By the reform, this requirements are supplemented by the requirement of participation in a work-learn offer. The main argument for work requirements is that they provide optimal incentives. Besley and Coate (1992) analyze the screening argument that work requirements are a way of targeting the poor and the deterrent argument that they may encourage poverty-reducing investments. They conclude that workfare crowds out private sector work, increases the size of the poverty gap by lower wages and thereby increases the cost of poverty alleviation. Therefore it is only an efficient way of screening in

developing countries where the government has limited ability to monitor. Besides, they argue that workfare can only be an effective deterrent if the work requirements are large enough. Furthermore, work requirements are not optimal if the goal of social assistance is higher utility rather than higher income (Besley and Coate, 1992). Nevertheless, these conditionality requirements reduce some of the distortions in the labor market as they boost job search of able individuals. It is a way to deal with asymmetric information as it prevents non-workers who highly value leisure to become social assistance receivers. Conditional benefits can be regarded as an implicit subsidy on work as people who fulfill the work requirements get social assistance benefits. Conditionality aspects may reduce upward wage pressure from high benefits and may even cause benefits to lower the unemployment rate and spur job growth (Van der Ploeg, 2003). This leads to higher labor supply, higher growth and lower inflation. Conditionality of social assistance benefits also reduces the informal economy, as the option of informal sector work combined with social assistance benefits becomes less attractive as individuals need to participate in a work-learn offer as a condition for these social assistance benefits.

ALMPs

The work-learn offer is an active labor market policy (ALMP) as it intervenes in the labor market to help the unemployed to find a job. The goal of ALMPs is to shift the Beveridge curve, the empirical relationship between unemployment rates and vacancy rates, inwards. The Beveridge curve is downward sloping and convex as the number of vacancies is higher and unemployment is lower during an economic boom. Two ways in which ALMPs can reduce unemployment is by an increase in the job-finding rate or through productivity increases due to the training.

A potential problem with ALMPs is that they may crowd out regular employment (Calmfors et al., 2002; Dahlberg and Forslund, 2005). The economic mechanism is that wages and thereby labor demand are sticky. When labor supply increases this may increase involuntary unemployment if the increase of labor supply is not followed by an equal increase in labor demand, see Figure 2. This crowding out effect is expected to be higher when unemployment is high as more individuals end up in ALMPs that would had been employed otherwise. These individuals are able to compete in an unfair way with employed workers when their compensation is lower than the salary that regular employees get, or when compensation is paid by

the government.

The most accurate would be if we could separate the effects of stricter eligibility for social assistance from active labor market policies by the work-learn offer. Unfortunately, this is not possible with our data. Therefore we can only measure the causal effect of the total reform and not of the two different parts.

3 Empirical literature

We start this chapter with discussing the effect of the height and conditionality of social assistance benefits on labor supply. This is the mechanism by which we expect stricter conditionality to have an effect on employment. Afterwards, we discuss the effects of the two different features of the reform, the effect of stricter conditionality of benefits and of ALMPs, on employment. Here we will focus on the effects for young individuals.

The effect of social assistance benefits on labor supply

Krueger and Meyer (2002) claim that most empirical work shows labor supply elasticities close to 1.0 for unemployment insurance and between 0.5 and 1.0 for workers' compensation. These are larger than labor supply elasticities found for wages and taxes (Keane, 2011; Chetty et al., 2012). This could be explained by a low income effect relative to the substitution effect as income goes down less when income from labor is replaced by social benefits. However, they found that disability insurance and social security have much smaller and less conclusive labor supply effects. This can be explained by a relatively short duration of unemployment benefits and workers compensation compared to disability insurance and social security retirement, making intertemporal substitution between leisure and labor more likely. Hence it is impossible to apply the same set of labor supply elasticities to diverse social programs. Our reform is about social assistance with a possible long term character in case the individual is not able to find a job. Therefore, we expect less intertemporal substitution and elasticities closer to what Krueger and Meyer (2002) found for disability insurance or social security. But we will focus in this empirical literature review exclusively on social assistance benefits and ALMPs as the effect of different kinds of benefits on employment are very different according to Krueger and Meyer (2002). Moffitt (2002) provides a very complete overview of the empirical literature

of the effect of social assistance benefits on labor supply. Most studies find that higher social assistance benefits lead to lower labor supply and stricter conditionality leads to higher labor supply. Aid to Families with Dependent Children (AFDC) reduced labor supply by from 10 to 50 percent of non-AFDC levels (Danziger et al., 1981; Moffitt, 1992; Hoyne, 1997). However, the Food Stamp program showed very little labor supply responses. This can probably be explained by very low benefit amounts in the Food Stamp program (Hagstrom, 1996). The Temporary Assistance for Needy Families (TANF) program, which succeeded the AFDC program and put more emphasize on employment, seems to have a positive employment effect, but the separate contributions of the welfare reform, the EITC and the economy are hard to identify (Ellwood, 2000).

Stricter conditionality

We give an empirical literature review of the effects of stricter conditionality of social assistance benefits for young individuals on welfare claims, employment and enrollment in education, see Table 3. Hernæs et al. (2016) exploits a geographically differentiated implementation of conditionality of social assistance benefits for the Norwegian youth to identify the effect of social assistance conditionality on social assistance claims and high school dropout. They find that larger than expected within-municipality changes in outcomes coincide with local timing of conditionality implementation in a way that correlates with ex ante probabilities of becoming a social assistance claimant. They find that stricter conditionality for social assistance benefits for the Norwegian youth reduces social assistance claims and increases high school completion rates. Bargain and Doorley (2013) research a discontinuity in social assistance benefits in France. Childless single individuals under 25 years of age are not eligible for social assistance. Bargain and Doorley (2013) exploit this discontinuity to find the causal effect of social assistance for young people using a regression discontinuity design. They find that eligibility for this program led to a drop of 5 to 9% in the employment rate for young singles who are high school dropouts. They also find that this negative effect was nullified by financial incentives to work to both the working poor and the non-working poor. Lemieux and Milligan (2008) find a similar result for Quebec where social assistance benefits are much lower for recipients under age 30 than over age 30. They find strong evidence that more generous social assistance benefits reduce employment. The employment rate

Table 3: literature review: stricter conditionality

Author	Identification	Data	Findings
<i>Stricter conditionality for young individuals</i>			
Hernaes et al. (2016)	Panel data	administrative data from Norway	stricter conditionality on welfare claims (-) on high school completion rates (+)
Bargain and Doorley (2013)	Regression discontinuity	administrative data from France	eligibility for social assistance on employment (-5-9%)
Limieux and Milligan (2008)	Regression discontinuity	administrative data and Labor Force Survey from Quebec	175% increase of social assistance on employment (-3-5%)
Dahlberg, Johansson and Mörk (2009)	Difference-in Differences	administrative data from Sweden	mandatory activation programs on welfare participation(-) on employment(+) on employment for young people(+) on employment for people born in non-Western countries(+)
<i>Benefit sanctions</i>			
Boone, Sadrieh and Van Ours (2009)	Laboratory experiments		Benefits sanctions on the outflow from unemployment(+)
Lalive, Van Ours and Zweimüller (2005)	Panel data	administrative data from Swiss	Both warning and sanction on exit rate out of unemployment(-)

for less-educated men without dependent children decreased by 3 to 5 percentage points in response to a 175% increase in benefits. Dahlberg et al. (2009) research entry and exit effects by exploiting a Swedish welfare reform in which city districts in Stockholm gradually implemented mandatory activations programs. they find that these mandatory activation programs reduce welfare participation and increase employment. The largest effects are found for young individuals and people born in non-Western countries. In short, all these studies indicate that social assistance benefits for young people have a strong negative effect on employment and stricter conditionality has a strong positive effect on employment.

Boone et al. (2009) find in an experimental study on unemployment benefit sanctions that the effect of the threat of being sanctioned on job search intensity (ex-ante effect) is even larger than the effect of sanctions being imposed (ex-post effect), both do have a positive effect on the outflow from unemployment. Also Lalive et al. (2005) find that benefit sanctions will increase the exit rate out of unemployment for both persons that received a warning as for persons who were sanctioned. By the WIJ-reform, the municipality can reject, reduce or temporarily stop the social assistance benefits if the young person does not cooperate in the work-learn offer. Hence, this sanction is expected to intensify job searching and increase employment.

ALMPs

We provide a literature review on the effect of ALMPs on employment in Table 4. The most recent literature review about active labor market programs (ALMPs) for the youth is given by Caliendo and Schmidl (2016). They find that job search assistance has positive effects on employment. More mixed effects on employment are found for training and wage subsidies and public work programs even have a negative effect on employment. Kluve (2014) argues that most ALMPs have no or only modest effects on employment. The key to a successful ALMP for the youth is comprehensiveness, comprising multiple targeted components, including job search assistance, counseling, training, and placement services. Examples are the New Deal for Young People in the UK and Job Corps in the US (Kluve, 2014). Our reform involves job search assistance, training and seems to be a comprehensive program like The New Deal for Young People in the UK and Job Corps in the US, therefore we expect a positive effect of the work-learn offer at least in the long-run. However, we will not observe the long-term effect in this research as we have only data for the

Table 4: literature review: ALMPs

Author	Identification	Data	Findings
<i>Effects of ALMPs for young individuals</i>			
Caliendo and Schmidl (2016)	Meta-analysis	-	job search assistance on employment (+) public work programs on employment (-) training and wage subsidies on employment (+/-)
Kluge (2014)	Meta-analysis	-	ALM's on employment (+/-) New Deal for the young on employment (+) on employment (+) Job corps on employment (+)
<i>Effects of ALMPs for adults</i>			
Heckman, Lalonde, and Smith (1999)	Meta-analysis	-	ALMPs on employment(+) on unemployment (no effect)
Boone and Van Ours (2009)	Panel data	20 OECD countries	training on employment (+) employment services on unemployment (no effect) subsidized jobs on unemployment (no effect)
Card, Kluge and Weber (2010)	Meta-analysis	-	job search assistance on employment in the short-run (-) in the long-run (+)
Lechner, Miquel and Wunsch (2011)	Panel data	8 years micro data West-Germany	training on employment (+10-20%)
Van der Berg and Van der Klauw (2006)	Panel data	administrative data from The Netherlands	counseling and monitoring on employment (no effect) monetoring causes a shift from informal to formal job search

three years after the reform. Furthermore, Kluve (2014) also finds that the effectiveness of ALMPs is lower for the youth than for adults. Besides, more rigid labor markets make ALMPs less effective.

We will discuss some more general studies on ALMPs in the second part of Table 4, because the literature on ALMPs for young individuals is limited. Heckman et al. (1999) argue that ALMPs only have a modest positive effect on employment but no significant effect on unemployment. Boone and Van Ours (2009) show that training is effective in reducing unemployment, whereas employment services and subsidized jobs are not effective et al. Also Card et al. (2010) find positive effects of job search assistance on employment in their meta-analysis. They find that extensive training to increase human capital has a negative effect in the short-term, but this effect becomes positive in the long-term. This negative effect on employment probability in the short-term is explained by a lock-in effect. Van Ours (2004)) show that almost all training programs reduce employment and earnings in the short term, because of a lock-in effect. However, in the medium and long run some wage subsidies and training programs increase employment and earnings. However, this effect depends on the unemployment level at the start of the program. Negative lock-in effects are smaller and positive long run effects are larger when unemployment is high (Lechner and Wunsch, 2009). This smaller lock-in effect can be explained by lower cost of reduced job search as the change of finding a job is low. The long run effect may be more positive because non-participants are more likely to be unemployed and less likely to build human capital by work experience when unemployment is high. Also Lechner et al. (2011) find negative employment effects in the short term of different government-sponsored training programs in West Germany. They also find that training increases employment rates by 10-20 percentage points in the long run. But it can take as much as three years for this positive effects to appear. Van den Berg and Van der Klauw (2006) find no evidence that counseling and monitoring affect the exit rate to work. Monitoring causes a shift from informal to formal job search.

4 The reform

The WIJ (i.e., the Work Investment Act for Young Individuals) came into force on October 1st 2009. The WIJ reform consists of two components. First of all, munic-

ipalities had to provide a work-learn offer to social assistance recipients. Secondly, the reform restricted the eligibility of social assistance benefits to people who cooperated in a work-learn offer. This law applied to young people under 27 years old who were not enrolled in government paid education and do not have a job that generates the social minimum. The young people already on social assistance in September 2009 had to be transferred from the former social assistance law (WWB) to the WIJ before the first of July 2010.

The objective of the WIJ reform was to improve labor participation among young people. The government wanted to activate the youth and discourage social assistance dependency. This law was introduced in a context of increased policy attention on reducing youth unemployment and early school leaving. The law is based on the idea that young people ideally work or learn and that people increase their chances on the labor market via education (Ministry of Social Affairs and Employment, 2008).

The WIJ replaced the WWB for people between 16 and 27 years old. The WWB remained in place for people with an age from 27 to 65 years old. The WWB gives a right on social assistance independently from cooperation of the recipient in obtaining employment. Only a temporary reduction in the level of social assistance is possible in case of no cooperation. In the WIJ, individuals are not eligible to social assistance benefits when the person rejects the work-learn offer or does unambiguously not cooperate sufficiently. Individuals in the WIJ also need to inform the municipality on all the facts and circumstances that could influence his or her right. The municipality can reject, reduce or temporarily stop the social assistance benefits if the young individual does not cooperate. This is a paradigm shift from a right to income towards a right to a work-learn offer with an associated right on an income supplement when individuals accept the offer and cooperate in the work-learn project (Ministry of Social Affairs and Employment, 2008).

The WIJ requires the municipalities to invest in the labor participation of all young people who are not able to find a job or education, also in those with unfavourable employment prospects. The work-learn offer can lead to work, learning or a combination of both. It has to be an offer that suits the situation of the young person, his capacities and his preferences. The municipality needs to offer multiple series of education and training if necessary, continuing until the person finds a job with sufficient income. Municipalities have some freedom in determining the content of the work-learn offers, but are obliged to make municipal regulation on it.

They must make a regulation on the content of the work-learn offer, the sanction of a reduction of social assistance benefits, combating fraud, client participation and on how the norms differ for certain categories of young people (like young mothers or people with very unfavourable employment prospects) and which norms apply under which conditions. The right on a work-learn offer in the future is not affected when a person rejects a work-learn offer. This gives young people always the chance to change their minds. Exemption for the work-learn offer is only given for exhaustive reasons like serving in the army, no cooperation or serious misconduct from the side of the young person. Further, it is an individual right which means that the situation of the partner does not infringe on this right (Ministry of Social Affairs and Employment, 2008). Moreover, young individuals are not required to report themselves because this would be a form of forced labor which is forbidden by law.

Social assistance

The height of social assistance benefits depends on the age and household composition and follows from both the WIJ and the municipal regulation and can thereby differ per municipality. No income supplement is given when an income of at least social assistance level is generated with the work-learn offer. Another requirement to claim an income supplement is owning no or very little assets. Also the income and property of the people with whom the young person is living together or is married with counts for this restriction (Ministry of Social Affairs and Employment, 2008).²

Heterogeneous groups

Individuals from 16 and 17 years old who are not enrolled in school, work less than 16 hours and have a basic qualification, or an exemption for this qualification, have a right on a work-learn offer, but not on an income supplement. Single parents with children under 6 years old are exempted for the labor requirement but do have an education requirement to increase the chance of finding a job after the exemption. Disabled can apply for the WAJONG (disability benefits), but in some cases they do not fulfill the requirements, among others because of the age criteria.³ The pos-

²allowed property without losing the right to social assistance benefits was 5.480 euro for singles and 10.960 euro for couples.

³Became disabled before the age of 17 or during a study before the age of 30.

sibilities and limitations of the disabled are taken into account in the customized work-learn offer. When working or learning is not possible for the young person, for example because of disability, this person can get an income supplement without accepting a work-learn offer (Ministry of Social Affairs and Employment, 2008).

Evaluation

CentERdata came with an evaluation report of the WIJ in 2011. They find that 69.8 % of the applicants received a work-learn offer, 11.1 % received only an income supplement and for another 15 % the status was unknown. According to the municipal data, 63 % of the young people with a work-learn offer received an income supplement. Besides, 74 % of the applicants were satisfied with their work-learn offer. These numbers are based on a sample of municipalities and a limited time period and are therefore not as accurate as the data we will describe later in our own analysis. They concluded that the municipalities were reaching the youth target group to a large extent and that many of them were activated by work-learn programs. The amount of inactive youth seems to decrease, many young people were activated to regular work or education in an early stadium, often even before a WIJ-request. Cooperation between municipalities and the education and healthcare sector improved. At the same time the number of young people with an income supplement increased. This could possibly be explained by the economic crisis.

The effectiveness of the WIJ should be seen in the context of some other active labor market policies like the integrated approach to truancy and the youth action plan. These could have affected the effectivity of the WIJ or had positive effects on the position of the young people on their own. It is hard to disentangle the effect of the WIJ reform from this other policies, because this policies were introduced in the same period. However, the scale of this other programs is very small compared to the WIJ reform, which makes them less of a problem for finding the effects of the reform.

Household income test and other reforms

Since the first of January 2012, the WIJ was formally replaced by the WWB. However, at the same time the WWB was tightened for young people till 27 years old and the work-learn offers were continued.⁴ In the new WWB, young people till 27

⁴This is what we found out when we did some interviews with both policy makers and caseworkers from the municipality of Amsterdam and read some policy documents from the administration

years old could only apply for social assistance after they could show that they had been searching for a job or education for at least a month. Besides, the WWB was tightened by a household income test in which first degree relatives lost their right on social assistance benefits when the income of the household was higher than a certain level. We treat this as a continuation of the reform as the work learn offers where still in place and the conditionality of social assistance benefits became even stricter.

Moreover, there were some smaller reforms with respect to social assistance benefits in 2012. The law work and income artists (WWIK), which provided an income supplement to artists if their artistic work was not enough to provide for living expenses, was abolished. municipalities also received the possibility to ask some volunteer work in return from people who live on social assistance benefits. Also the exemption of the obligation to search for a job for single parents with a young child up to 5 years of age was abolished. Furthermore, the earnings release for working part time for single parents with children till 12 years old was expanded. Further, the length someone on social assistance was allowed to go on holiday abroad was shortened to 4 weeks. Besides A statutory maximum for municipal income supplements is introduced of 110 percent of the social assistance norm. At last, municipalities need to make municipal regulation on special social assistance benefits for families with children that go to school. All this policies are expected to have a positive effect on labor participation. However, this smaller reforms applied to both treatment and control groups and are therefore expected not to interfere with the effect we will find for our reform.

5 Empirical methodology

We use difference-in-differences (DiD) to estimate the effect of the WIJ reform on a number of outcome variables.⁵ In the DiD approach we estimate the impact of a policy reform by taking a double difference between the treatment group and a control group. First, we take the difference in the outcome variable between the treatment group and the control group after the reform. Second, we subtract the

of the city of Rotterdam

⁵For a general introduction to the differences-in-differences methodology see e.g. Blundell and Dias (2009) and Angrist and Pischke (2009).

difference in the outcome variable between the treatment group and the control group before the reform. In this way we control for the time-invariant difference between the treatment and control group and for common time effects in the outcome variable. The WIJ reform targets individuals up to 27 years of age. Our first treatment group consists of individuals 20–22 years of age and our second treatment group consists of individuals 23–25 years of age.⁶ Our base control group consists of individuals 27–29 years of age. This is the best possible control group for which the WIJ reform does not apply. We prefer this control group above a possible control group of individuals of 15–17 years of age as the latter is less comparable with the treatment groups. Besides, individuals of 17–18 years of age were eligible for a work-learn offer but not for an income supplement. We do not look at a treatment group of individuals younger than 20 years of age because of the larger age differences between them and the preferred control group. The first assumption that needs to hold for the difference-in-differences methodology is that the treatment groups need to be similar to the control group as we are comparing this treatment groups with the control group to find the effect of the reform. If the treatment groups and control group are very different, it may be other things rather than the treatment that explains the effects that we will find. We will show in the next section that the treatment groups are indeed similar to the control group. The second assumption for difference-in-differences methodology is the parallel trends assumption, meaning that the time trend for the treatment groups is similar to the time trend of the control group. This makes it possible to correct for common time effects. We will show by the descriptive graphs in the results that the parallel trends assumption holds between our treatment groups and control group.

Although the common trend assumption seems to hold in the figures for the period 1999–2012, we need to be careful as the Great Recession may have affected the treatment and control group differently. Bell and Blanchflower (2011) find that young individuals 16–25 years of age have suffered disproportionately during the recession. It could be the case that the effects of the WIJ reform are (partly) masked by the Great Recession. However, the common trends assumption seems to hold in the figures from 1999–2012, including the early 2000s recession. Also, placebo

⁶Age is measured on the 1st of January, and the outcome variables are measured on an annual basis. Therefore, we exclude observations from individuals 26 years of age, because they move from the treatment group to the control group during the year.

treatment dummies in the years prior to the reform are typically not significant. Therefore, a priori we expect that the common trend assumption also holds during the Great Recession. Besides, we control for age specific unemployment effects. Nevertheless, the Great Recession was much deeper than the recession during the early 2000s, and therefore we need to be careful in drawing too strong conclusions.

As outcome variables we consider the participation rate in social assistance, the participation rate in employment, the level of social assistance benefits, the number of hours worked and income from work. We have considered to take the enrollment rate in education as an extra outcome variable, but almost no one of 27-29 years of age, our control group, is still enrolled in education, see Figure A.1. This makes it hard to find the effect of the reform on enrollment in education by comparing our treatment groups with our control group, therefore we have decided to focus exclusively on the effect on employment.

For the participation rates, we estimate a linear probability model (Angrist and Pischke, 2009) which is a special case of a binomial regression model where the outcome variable is 1 if the individual is participating and 0 if the individual is not participating. In this way the treatment effect will give us probabilities that an individual is participating in social assistance or employment. Let y_{igt} be a dummy variable that is 1 if individual i in age group g is participating (in e.g. social assistance) in period t . In our most extensive specification, we regress the outcome variables on a set of year fixed effects (α_t), group fixed effects (β_g), group specific trends for the control and treatment groups with coefficients (ν_g), a treatment effect (δDD_{igt}) for individuals in the treatment group g in a given year t in the post-reform period, time-varying individuals and household characteristics (X'_{it}), age specific unemployment fixed effects (ϕ_g), control year dummies (μ_{it}) and an error term (ϵ_{igt}):

$$Y_{igt} = \alpha_t + \beta_g + \nu_g t + \delta DD_{igt} + X'_{it} \gamma + \phi_g t u + \mu_{it} + \epsilon_{igt}. \quad (1)$$

The primary interest is in the treatment coefficient δDD_{igt} . We also estimate a linear model for the level of social assistance benefits, income from work and hours worked (including the zeros ⁷). We choose to control extensively for year fixed effects, group fixed effects and time varying individuals and household characteristics to find a more accurate treatment effect. We do this because some of the different changes after the reform between the treatment groups and the control group may actually

⁷Again see Angrist and Pischke (2009).

be explained by these controls rather than by the treatment itself. Besides, we consider it to be important to control for differential trends between treatment and control group as we expect that the treatment groups are affected more severely by the Great Recession than the older control group (Bell and Blanchflower, 2011). In an extension we consider the coefficients of placebo treatment dummies in years prior to the reform. These placebo reform dummies are informative about potential differential time effects (because of e.g. differential trends or differences in business cycle responses) between the treatment and control groups, and also about potential anticipation effects of the reform.

To allow for correlation in the error terms at a higher level of aggregation than the individual, we use clustered standard errors (Bertrand and Mullainathan, 2004; Donald and Lang, 2007). By clustering our error terms we allow for intragroup correlation, relaxing the usual requirement that the observations need to be independent. The observations are independent across clusters but do not need to be independent within clusters. This makes our standard errors robust. Hence, we do not need to worry about possible autocorrelation or heteroskedasticity problems as observations are independent across clusters now. Since the identifying variation is in age, we cluster standard errors by year of birth. To have enough clusters, we interact this with ethnicity (classified in three groups: i) native, ii) Western immigrant and iii) non-Western immigrant). In the base specification, this results in 69 clusters (23 years of birth interacted with 3 categories of ethnicity), which is deemed sufficient by Angrist and Pischke (2009) for accurate inference based on the large-sample properties of the estimator.

We research the effect of the WIJ reform for different household types: singles, single parents, children living with their parents, couples and couples with children. We control for this differences between household types in our base regression specification, but we also include a separate heterogeneity analysis to find the effect of the reform for the different household types. We expect the strongest treatment effect for children living at home because they are less often entitled to an income supplement as they can rely in most cases on there parents for a living. Furthermore, we expect a stronger effect for singles and single parents as they are overrepresented in social assistance. Besides, we control for gender, because we expect higher effects for women than for men as labor elasticities both at the extensive and intensive margin are higher for women than for men (Kimmel and Kniesner, 1998).

Finally, we control for different ethnic groups: natives, western immigrants and non-western immigrants. We may expect a lower effect for non-western immigrants as they face multiple cultural and linguistic barriers and also discrimination on the labor market which makes it harder to get employed (Bertrand and Mullainathan, 2004). On the other hand, the work-learn offer may be of more importance for immigrants as they are more in need of human capital, for example in language skills, than natives. Large positive effects of active labor market policies are found for immigrants in the literature (Heinesen et al., 2013; Åslund and Johansson, 2011; Joonas and Nekby, 2012).

6 Data set

We use data from the *Arbeidsmarktpanel* 1999–2013 (LMP, Labour Market Panel) of Statistics Netherlands (2015). The LMP is a large and rich household panel data set, tracking 1.2 million individuals over the period 1999 to 2012.⁸

We use the years 1999–2009 as the pre-reform years, and 2010–2012 as the post-reform (treatment) years. For hours worked, we need to restrict the analysis to the period 2006–2012, because hours worked are not available in the data set before 2006.

To ensure that the treatment and control groups are sufficiently similar in their characteristics, we limit the sample to individuals 20–29 years of age. We define two treatment groups. The first treatment group consists of individuals 20–22 years of age. The second treatment group consists of individuals 23–25 years of age. The control group consists of individuals 27–29 years of age. We remove observations for individuals 26 years of age, since these are partly in the treatment group and partly in the control group in the year that they turn 27.

As outcome variables we consider the participation rate in social assistance, income from social assistance benefits, the participation rate in employment, the number of hours worked and income from work. An individual is counted as enrolled in social assistance when income from social assistance benefits is greater than zero. An individual is counted as working when income from work is greater than zero or profit income is unequal to zero (hence we count self-employed that make a loss

⁸For a limited number of variables, the data set also contains data for 2013. Unfortunately, the financial variables are not available for 2013.

in a given year also as working individuals). We include profit income to take the self-employed into account. We do this because we consider self-employment as a form of employment and we do not want our results to be biased by a shift from being employed towards self-employment. As an alternative employment indicator we use hours worked, and count all individuals having positive working hours as working.

As control variables we include year, gender, household type (adult child living at the home of the parents/single parent/single/couple with children/couple without children) and ethnicity (native/Western immigrant/non-Western immigrant).

Table 5 and 6 give descriptive statistics for treatment group 20–22 and 23–25 respectively, along with the differences and normalized differences with the control group in the pre- and the post-reform period. Differences in age are substantial by definition, given our treatment and control groups, but we do not expect this to be a problem as the different age groups are very comparable. Besides, we control for age differences in our regression analysis. Differences in gender and ethnicity are small between the treatment groups and the control group, and the same is true for the so-called normalized differences (the mean differences divided by the square root of the sum of variances). Imbens and Wooldridge (2009) argue that this is an informative way to check if the treatment and control group have sufficient overlap in the covariates. As a rule of thumb they suggest that when the normalized difference exceeds a value of .25, linear regression becomes sensitive to the specification. The normalized differences for gender and ethnicity stay well below .25. Differences are more pronounced for household type. This suggests that the results should be interpreted with the appropriate care when we consider all individuals. However, we focus in our analysis also on the results by household type. Tables 5 and 6 show that the change in the differences in the control variables from the pre- to the post-reform period is small. This is in line with what we observe in Figures A.2 and A.3, where we show the different control variables graphically over time. Hence, there is no indication of differential compositional changes between the treatment and control groups.

We also present descriptive statistics for the outcome variables in Table 5 and 6. The differences for participation in social assistance and income from social assistance are slightly lower after the reform, suggesting that the reform made social assistance eligibility stricter for the treatment groups relative to the control group.

Table 5: Descriptive statistics treatment group 20–22 and control group 27–29

	Treatment Group (1999–2009)		Control Group (1999–2009)		Differences (treatment–control)		Norm. differences (treatment–control)	
	Mean	SD	Mean	SD	1999–09	2010–12	1999–09	2010–12
Explanatory variables								
Age	20.994	0.817	28.030	0.816	−7.036	−7.030	−6.092	−6.088
Male	0.507	0.500	0.485	0.500	0.023	0.011	0.032	0.015
Female	0.493	0.500	0.515	0.500	−0.023	−0.011	−0.032	−0.015
Native	0.838	0.369	0.827	0.378	0.011	0.004	0.021	0.008
Non-Western immigrant	0.095	0.294	0.098	0.297	−0.003	0.003	−0.007	0.007
Western immigrant	0.067	0.250	0.075	0.264	−0.008	−0.007	−0.022	−0.020
Couple without children	0.141	0.348	0.404	0.491	−0.263	−0.242	−0.437	−0.421
Couple with children ^b	0.025	0.155	0.286	0.452	−0.262	−0.248	−0.548	−0.541
Single	0.186	0.389	0.190	0.392	−0.003	−0.004	−0.006	−0.007
Single parent	0.007	0.083	0.022	0.146	−0.015	−0.018	−0.088	−0.104
Adult child ^c	0.641	0.480	0.098	0.298	0.543	0.511	0.961	0.879
Dependent variables								
Participation in SA	0.029	0.168	0.040	0.196	−0.011	−0.012	−0.042	−0.051
Income from SA	175.4	1297.3	332.7	1905.1	−157.3	−210.3	−0.068	−0.086
Employment: income>0	0.917	0.276	0.907	0.291	0.010	−0.034	0.026	−0.083
Income from work	11405	10389	22797	19140	−11393	−16991	−0.523	−0.786
Employment: hours>0 ^d	0.899	0.302	0.878	0.327	0.020	0.006	0.046	0.013
Hours worked ^d	1051	750	1452	740	−401.1	−479.2	−0.381	−0.459
Observations 1999–2012: treatment group 20–22 = 555,339, control group 604,242.								

Source: Own calculations using the Labour Market Panel (Statistics Netherlands). Treatment group: individuals 20–22 years of age. Control group: individuals 27–29 years of age. Normalized differences are mean differences divided by the square root of the sum of the variances (see Imbens and Wooldridge, 2009). ^bYoungest child living at home is less than 18 years of age. ^cAdult child living at the home of the parents. ^dHours worked are not observed for the period 1999–2005. Observations hours worked and participation based on hours 2006–2012: treatment group 271,672, control group 255,386.

Table 6: Descriptive statistics treatment group 23–25 and control group 27–29

	Treatment Group (1999–2009)		Control Group (1999–2009)		Differences (treatment–control)		Norm. differences (treatment–control)	
	Mean	SD	Mean	SD	1999–09	2010–12	1999–09	2010–12
Explanatory variables								
Age	24.011	0.817	28.030	0.816	−4.019	−4.021	−3.481	−3.480
Male	0.496	0.500	0.485	0.500	0.011	0.007	0.016	0.010
Female	0.504	0.500	0.515	0.500	−0.011	−0.007	−0.016	−0.010
Native	0.832	0.374	0.827	0.378	0.005	0.009	0.010	0.016
Non-Western immigrant	0.098	0.297	0.098	0.297	0.000	−0.004	−0.001	−0.009
Western immigrant	0.070	0.256	0.075	0.264	−0.005	−0.005	−0.013	−0.013
Couple without children	0.334	0.472	0.404	0.491	−0.070	−0.070	−0.102	−0.106
Couple with children ^b	0.089	0.285	0.286	0.452	−0.197	−0.194	−0.369	−0.381
Single	0.218	0.413	0.190	0.392	0.029	0.020	0.050	0.033
Single parent	0.014	0.116	0.022	0.146	−0.008	−0.010	−0.043	−0.054
Adult child ^c	0.345	0.475	0.098	0.298	0.246	0.253	0.439	0.434
Dependent variables								
Participation in SA	0.038	0.192	0.040	0.196	−0.002	−0.003	−0.006	−0.013
Income from SA	292.5	1768.6	332.7	1905.1	−40.2	−83.7	−0.015	−0.030
Employment: income>0	0.921	0.270	0.907	0.291	0.014	−0.012	0.035	−0.032
Income from work	18231	12098	22797	19140	−4567	−8157	−0.202	−0.338
Employment: hours>0 ^d	0.898	0.302	0.878	0.327	0.020	0.009	0.044	0.020
Hours worked ^d	1363	753	1452	740	−89	−163	−0.085	−0.152
Observations 1999–2012: treatment group 23–25 = 547,408, control group 604,242.								

Source: Own calculations using the Labour Market Panel (Statistics Netherlands). Treatment group: individuals 23–25 years of age. Control group: individuals 27–29 years of age. Normalized differences are mean differences divided by the square root of the sum of the variances (see Imbens and Wooldridge, 2009). ^bYoungest child living at home is less than 18 years of age. ^cAdult child living at the home of the parents. ^dHours worked are not observed for the period 1999–2005. Observations hours worked and participation based on hours 2006–2012: treatment group 271,672, control group 255,386.

The differences between the treatment groups and the control group for all four variables for labor participation are smaller after the reform, which means that labor force participation has decreased for the treatment groups relative to the control group after the reform. Indeed, the descriptive statistics suggest quite the opposite as we would expect as employment seems to be decreased rather than increased by the reform, which would be the opposite from the intention of the reform. However, below we will see that differential trends between the treatment and control groups play a role in the effect on the employment, income from work and hours worked variables. This means that the parallel trends assumption does not hold completely, for example because the control group may be affected less severe by the economic crisis than the treatment group. We correct for this differences in trends by adding different group specific trends and control year dummies in our most extensive regression specification.

7 Results

First we present graphical evidence on the treatment effects of the reform. We plot the outcome variables for the treatment and control groups, for the period 1999–2012. 1999–2009 is the pre-reform period and 2010–2012 is the post-reform period.

Figure 4 gives the outcome variables over all individuals. Participation in social assistance and income from social assistance for the treatment and control groups move more or less in tandem in the pre-reform period. After the reform, participation in social assistance and income from social assistance move up for the control group (presumably due to the Great Recession), but move down in 2011 and 2012 for the treatment groups. This suggests a negative treatment effect for participation in social assistance and income from social assistance. The employment rate of the control group appears to be trending upward before the reform, whereas there is no clear trend for the treatment groups. After the reform, the employment rate of the treatment groups go down relative to the control group, suggesting a negative treatment effect for employment as well, but here we should be wary of the pre-reform differential trend between the treatment and control groups. This trend may be explained by higher participation in education for the treatment groups relative to the control groups, see Figure A.1. For hours worked, the treatment groups seem to have a negative trend relative to the control group, again making it hard to

separate the treatment effect from trend differentials using an eye-ball test.

Table 7 shows the regression results for all individuals. The regressions allow us to control for differences in observed and unobserved (but assumed fixed) characteristics and for differential trends between the treatment groups and the control group. Column (1) shows the results of the basic DD setup, where we only include year dummies and a group dummy for each individual age group. We find a negative but insignificant treatment effect on participation in social assistance and on the level of social assistance benefits. The treatment coefficients for employment measured by using income, and for income from work are negative for both treatment groups and significantly different from zero. The treatment effect on employment measured by using hours worked, is also negative, as is hours worked, but only the treatment effect on hours worked for the treatment group 20–22 is significant. Note that income is available for the period 1999–2012, and hours worked is only available for the shorter period 2006–2012. Adding demographic controls, in column (2), makes our negative treatment coefficients for employment measured by hours, and hours worked significant. This means that some of the variation can be explained by the demographics. By controlling for this we are able to find more accurate effects of the reform by reducing the standard error. Including employment age dummies, column (3), does not change much. However, when we control for different age trends in column (4), all coefficients turn insignificant except from hours worked. Besides, the coefficients for employment measured by hours worked become positive, but are still insignificant. This indicates that differences in trends do play a role in our results. Hence the parallel trends assumption does not hold completely. However, the parallel trends assumption still holds to a large extent as we showed in Figure 4. Besides we control for this differential trends. Adding trends for our control variables, column (5), makes the negative treatment coefficient on income from social assistance significant. It also makes the treatment effect on employment measured by income significant with a negative coefficient. When we replace the control trends with control year dummies, in column (6), the negative treatment effects on income from social assistance become more negative and more significant for both treatment groups, whereas all other coefficients turn insignificant. Column (6) is our preferred specification as we control for confounding factors, among which differential time effects, in the most extensive way. Also the effects on the 4 different employment variables become less negative, which is in line with the theory and

Figure 4: DD plots: all individuals



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Table 7: Regression results: all individuals

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Participation in SA</i>						
Treatment 20–22	−0.0022 (0.0103)	−0.0014 (0.0039)	−0.0027 (0.0039)	−0.0014 (0.0053)	−0.0015 (0.0034)	−0.0029 (0.0042)
Treatment 23–25	−0.0024 (0.0104)	−0.0010 (0.0025)	−0.0021 (0.0027)	−0.0001 (0.0044)	−0.0007 (0.0021)	−0.0017 (0.0026)
<i>Income from SA</i>						
Treatment 20–22	−54.175 (101.14)	−40.095 (30.347)	−43.908 (33.811)	−58.952 (42.046)	−60.204* (35.732)	−77.715** (37.347)
Treatment 23–25	−44.439 (105.51)	−28.779* (15.583)	−39.708* (23.012)	−40.685 (25.339)	−44.806* (23.062)	−57.179** (26.386)
<i>Employment: income > 0</i>						
Treatment 20–22	−0.0438*** (0.0153)	−0.0424*** (0.0035)	−0.0347*** (0.0038)	−0.0056 (0.0039)	−0.0062** (0.0030)	−0.0041 (0.0097)
Treatment 23–25	−0.0259 (0.0145)	−0.0268*** (0.0031)	−0.0223*** (0.0035)	−0.0065 (0.0041)	−0.0063** (0.0025)	−0.0051 (0.0056)
<i>Income from work</i>						
Treatment 20–22	−5,329.2*** (866.57)	−5,259.6*** (367.95)	−4,461.2*** (418.99)	−214.43 (280.84)	−245.59 (283.19)	284.61 (530.11)
Treatment 23–25	−3,383.2*** (946.01)	−3,402.9*** (414.22)	−2,817.5*** (393.87)	−421.92 (333.16)	−430.24 (273.01)	−112.39 (287.41)
<i>Employment: hours > 0^a</i>						
Treatment 20–22	−0.0189 (0.0155)	−0.0181*** (0.0025)	−0.0164*** (0.0037)	0.0022 (0.0027)	0.0024 (0.0028)	0.0066 (0.0063)
Treatment 23–25	−0.0126 (0.0186)	−0.0135*** (0.0022)	−0.0139*** (0.0038)	0.0001 (0.0029)	0.0002 (0.0030)	0.0032 (0.0040)
<i>Hours worked^a</i>						
Treatment 20–22	−104.35** (47.561)	−98.404*** (7.5498)	−92.816*** (10.975)	−18.188 (6.2110)	−17.91*** (5.733)	−1.1414 (20.339)
Treatment 23–25	−76.909 (58.555)	−77.566*** (8.9211)	−72.454*** (13.469)	−4.7375 (7.1068)	−4.539 (6.198)	4.4249 (9.7738)
Demographic controls	NO	YES	YES	YES	YES	YES
Unemployment-age dummies	NO	NO	YES	YES	YES	YES
Trends age	NO	NO	NO	YES	YES	YES
Trends controls	NO	NO	NO	NO	YES	NO
Control–year dummies	NO	NO	NO	NO	NO	YES
Observations	1,706,989	1,706,989	1,706,989	1,706,989	1,706,989	1,706,989
Individuals	343,408	343,408	343,408	343,408	343,408	343,408
Clusters	69	69	69	69	69	69

Cluster-robust standard errors in parentheses, clustered at year of birth (23 years) interacted with ethnicity (3 categories), * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. ^aHours worked are not observed for the period 1999–2005. Observations *Hours worked*: 830,745. Clusters *Hours worked*: 48.

Table 8: Regression results: all individuals with placebo's

	Placebo 2007-2009		Placebo 2002-2004	
	(1)	(2)	(3)	(4)
<i>Participation in SA</i>				
Treatment 20-22	0.0008 (0.0046)	-0.0020 (0.0066)	-0.0015 (0.0034)	-0.0028 (0.0043)
Treatment 23-25	0.0008 (0.0043)	-0.0015 (0.0047)	-0.0010 (0.0022)	-0.0019 (0.0025)
Placebo 20-22	0.0022 (0.0041)	0.0009 (0.0049)	0.0006 (0.0014)	0.0025 (0.0027)
Placebo 23-25	0.0015 (0.0031)	0.0002 (0.0035)	-0.0035*** (0.0012)	-0.0025 (0.0018)
<i>Income from SA</i>				
Treatment 20-22	-66.262* (33.772)	-124.69** (52.711)	-59.681 (36.095)	-74.368* (37.826)
Treatment 23-25	-77.616* (39.128)	-116.89*** (40.487)	-45.668* (23.896)	-56.079** (26.089)
Placebo 20-22	-6.0706 (28.220)	-45.570 (39.437)	89.758 (16.751)	42.005* (23.167)
Placebo 23-25	-31.751 (22.744)	-57.901* (29.377)	-10.894 (15.365)	78.757 (14.075)
<i>Employment: income > 0</i>				
Treatment 20-22	-0.0084* (0.0043)	-0.0030 (0.0153)	-0.0057* (0.0030)	-0.0036 (0.0095)
Treatment 23-25	-0.0051 (0.0057)	-0.0022 (0.0078)	-0.0062** (0.0024)	-0.0050 (0.0057)
Placebo 20-22	-0.0021 (0.0036)	0.0012 (0.0104)	0.0072*** (0.0026)	0.0075 (0.0061)
Placebo 23-25	0.0011 (0.0047)	0.0029 (0.0058)	0.0000 (0.0024)	0.0002 (0.0033)
<i>Income from work</i>				
Treatment 20-22	-859.10** (368.21)	-268.21 (737.56)	-213.39 (285.36)	307.52 (525.39)
Treatment 23-25	-658.45 (421.32)	-278.51 (340.21)	-414.30 (273.59)	-104.93 (289.37)
Placebo 20-22	-590.70* (300.45)	-531.47 (474.66)	377.08* (205.12)	287.76 (315.39)
Placebo 23-25	-223.44 (313.44)	-162.33 (246.69)	123.28 (147.56)	53.056 (141.02)
<i>Employment: hours > 0^a</i>				
Treatment 20-22	0.0033 (0.0044)	0.0047 (0.0097)		
Treatment 23-25	-0.0003 (0.0045)	0.0012 (0.0064)		
Placebo 20-22	0.0009 (0.0037)	-0.0018 (0.0076)		
Placebo 23-25	-0.0005 (0.0037)	-0.0019 (0.0047)		
<i>Hours worked^a</i>				
Treatment 20-22	-9.4873 (11.429)	-1.7621 (31.004)		
Treatment 23-25	0.8521 (11.035)	4.7059 (16.349)		
Placebo 20-22	8.0300 (9.1180)	-0.5893 (19.574)		
Placebo 23-25	5.1519 (9.0798)	0.2676 (12.625)		
Trends controls	YES	NO	YES	NO
Control-year dummies	NO	YES	NO	YES
Observations	1,706,989	1,706,989	1,706,989	1,706,989
Individuals	343,408	343,408	343,408	343,408
Clusters	69	69	69	69

Cluster-robust standard errors in parentheses, clustered at year of birth (23 years) interacted with ethnicity (3 categories), * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. ^aHours worked are not observed for the period 1999-2005. Observations Hours worked: 830,745. Clusters Hours worked: 48. We control for demographics, unemployment-age dummies and age specific trends.

Table 9: Regression results: different household types

	Adult child. living at home	Singles	Single parents	Couples without children	Couples with children
	(1)	(2)	(3)	(4)	(5)
<i>Participation in SA</i>					
Treatment 20–22	–0.0089* (0.0050)	–0.0105** (0.0050)	–0.0136 (0.0360)	0.0060*** (0.0021)	0.0337** (0.0141)
Treatment 23–25	–0.0076 (0.0057)	–0.0057 (0.0044)	–0.0708** (0.0330)	0.0026* (0.0015)	0.0104** (0.0047)
<i>Income from SA</i>					
Treatment 20–22	–101.92** (45.173)	–107.16** (49.879)	–528.71 (571.15)	5.6945 (11.323)	100.17 (88.460)
Treatment 23–25	–73.419 (50.174)	–69.222 (47.660)	–1,413.1*** (516.26)	8.0549 (8.4311)	22.457 (34.100)
<i>Employment: income > 0</i>					
Treatment 20–22	–0.0004 (0.0037)	–0.0062 (0.0045)	0.0039 (0.0439)	–0.0060 (0.0043)	–0.0020 (0.0178)
Treatment 23–25	–0.0029 (0.0040)	–0.0048 (0.0045)	–0.0194 (0.0293)	–0.0024 (0.0022)	–0.0185* (0.0106)
<i>Income from work</i>					
Treatment 20–22	987.24* (572.93)	708.05 (467.75)	129.62 (745.32)	–36.684 (320.81)	–678.68 (635.06)
Treatment 23–25	721.68 (601.78)	199.32 (468.60)	391.03 (537.13)	–369.45* (195.49)	–616.25 (392.73)
<i>Employment: hours > 0^a</i>					
Treatment 20–22	0.0156** (0.0062)	0.0011 (0.0073)	0.0074 (0.0321)	0.0105 (0.0067)	0.0257 (0.0252)
Treatment 23–25	0.0119* (0.0064)	0.0022 (0.0069)	0.0660* (0.0371)	0.0037 (0.0026)	–0.0136 (0.0136)
<i>Hours worked^a</i>					
Treatment 20–22	10.685 (20.107)	–16.555 (26.782)	98.534 (63.486)	45.427*** (15.352)	8.9028 (23.157)
Treatment 23–25	24.304 (21.653)	–7.4150 (26.196)	85.708 (59.355)	6.8863 (9.3816)	–10.150 (22.564)
Observations	609,213	353,576	24,592	490,334	229,274
Individuals	186,369	130,793	9,142	181,794	88,691
Clusters	69	69	69	69	69

Cluster-robust standard errors in parentheses, clustered at year of birth (23 years) interacted with ethnicity (3 categories), * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. ^aHours worked are not observed for the period 1999–2005. Observations *Hours worked*: 315,468 for adult children living at home, 189,510 for singles, 11,724 for single parents, 215,649 for couples without children, 98,394 for couples with children. Clusters *Hours worked*: 48. We control for demographics, unemployment-age dummies, age specific trends and control year dummies.

empirical literature. Furthermore, Table 8 shows that the placebo treatments turn insignificant in specification 6, which is a good sign that specification 6 is indeed the best model to pick up the effect of the reform.

We will interpret the size of the coefficients to get a better understanding of potential effect sizes, because we care about economically significant next to statistically significant. However, we can not stress enough that we must be careful in drawing conclusions as the results, except from the treatment effect on income from social assistance, are insignificant. Also the exact size of the coefficients is of course dependent on the chosen specification and should therefore not be confused with accurate effect sizes.

We can divide the coefficients of our preferred specification 6 by the values of the outcome variables for 2009, which is the year before the treatment started, to find the relative treatment effect sizes. For participation in social assistance, we find coefficients of 0.0029 for individuals 20–22 years of age and 0.0017 for individuals 23–25 years of age, whereas participation rates in social assistance in 2009 were 0.0203 and 0.0274 for the youngest and oldest treatment group, respectively. This means a treatment effect for participation in social assistance of around -14% for the youngest treatment group and -6% for the older treatment group compared to 2009. For income from social assistance, we find that average income from social assistance decreased by the reform with 77.72 euro a year for individuals 20–22 years of age and with 57.18 euro a year for individuals 23–25 years of age, whereas the average incomes from social assistance for these treatment groups were 122.9 and 218.2 euro a year in 2009. This means a treatment effect of around -63% for individuals of 20–22 years of age and of around -26% for individuals 23–25 years of age. The treatment effects on labor participation both measured by income (-0.0041 and -0.0051) and by hours (0.0066 and 0.0032), are negligible, as the treatment coefficients are very small compared to the employment rate in 2009 (0.903 and 0.923 if measured by income and 0.889 and 0.890 if measured by hours worked). The treatment effects of income from work are a bit larger but still small as they are 284.6 euro for individuals of 20–22 years of age and -112.4 euro for individuals of 23–25 years of age, whereas the pre-reform means are 11,779 and 20,386 euro, respectively. The treatment effects on Hours worked are -1.141 hours for individuals 20–22 years of age and 4.425 for individuals 23–25 years of age, whereas the pre-reform means for these treatment groups are 1006 and 1326.

Hence, the most pronounced effect is on income from social assistance which is not only statistically significant but also has a large effect size of -63% for the youngest treatment group and -26% for the older treatment group. Whereas the effect on employment is negligible both with regard to the effect size as with regard to the significance level.

In Table 8 we include a placebo treatment for the years 2007-2009 and a placebo treatment for the years 2002-2004, for specification 5 and 6 of Table 7. Most placebo treatment dummies turn insignificant in our preferred specification, see column 2 and column 4. However, for the placebo treatment 2007-2009 we do find a significant coefficient of -57.90 at a 10% level for income from social assistance for individuals 23-25 years of age. But this effect is smaller than the treatment effect, which is -116.9 in this specification, and is hardly significant. However, this shows some evidence for an already existing trend of decreasing income from social assistance, which we have already noticed in Figure 4. We use the second placebo treatment to research how the outcome variables for our treatment groups change relative to the control group during an economic recession. We want to know this as our reform took place during an economic downturn which might have influenced the results. During the years 2002-2004 there was a small recession caused by the bursting of the Internet bubble and therefore we take this years as a second placebo treatment. The only significant effect that we find for this placebo treatment is on income from social assistance for individuals of 20-22 years of age. This coefficient is 42.00 , but only significant at a 10% level. This indicates, that income from social assistance normally goes up for the treatment group relative to the control group during an economic recession, even when we control for unemployment. Whereas our treatment effect was negative and significant, making the case stronger that it was the reform that caused income from social assistance to decrease in the treatment group relative to the control group.

The treatment effects of the reform may vary over different household types as these are differently affected by the reform. Figures A.4 to A.8 give the outcome variables for adult children living at home, singles, single parents, couples without children and couples with children. For adult children living at home and singles, The treatment and control groups for participation in social assistance and income from social assistance move more or less in tandem up to the pre-reform period. Whereas, we notice a decrease in these treatment groups relative to the control group

in the post reform period, suggesting a negative treatment effect for participation in social assistance and income from social assistance for both adult children living at home and singles. For the other outcome variables, as well as for the other household types, it is hard to find a treatment effect based on these figures as the treatment groups and control group move parallel to each other both before and after the treatment, suggesting no treatment effect. Or move to volatile in the pre-reform period, making it hard to identify a treatment effect in the post-reform period.

Table 9 shows our preferred specification for the different household types. For adult children living at home, we find that the treatment effects for participation in social assistance and for income from social assistance for individuals 20–22 years of age are negative and significant (-0.0089 and -101.92). Besides, the treatment effect for income from work for the youngest treatment group is positive and significant (987.24). The treatment coefficients on employment measured by hours worked larger than zero are positive and significant (0.0156 and 0.0119). For singles, the treatment coefficients for individuals 20–22 years of age on participation in social assistance (-0.0105) and on income from social assistance (-107.16) are negative and significant, whereas the other treatment effects are insignificant. For single parents we find negative significant treatment coefficients for individuals of 23–25 years of age on participation in social assistance (-0.0708), income from social assistance ($-1,413.1$) and a positive significant treatment effect on employment measured by hours worked (0.0660). For couples without children we find a positive significant treatment effect on participation in social assistance for both treatment groups (0.0060 and 0.0026), a negative significant treatment effect on income from work (-369.45) for individuals 23–25 years of age and a positive significant treatment effect on hours worked (45.427) for the youngest treatment group. For couples with children we find a positive significant treatment effect on participation in social assistance (0.0337 and 0.0104) and a negative significant treatment effect on employment measured by income (-0.0185) for individuals 23–25 years of age.

Besides household heterogeneity, we also want to look into heterogeneity between different cities, see Figure A.9 to A.12 and Table A.3. The largest effects are found for Rotterdam, The Hague, Utrecht and Maastricht. We find that the effects between different cities are significantly different from each other. At the same time, we do not observe a clear pattern between right or leftwing oriented cities, nor between big and small cities.

We continue with researching the effect of individual year treatments in Table A.1. We find that the effect increases over the years, which indicates that it took some years before the full effect of the reform occurred. Nevertheless, the effect on income from social assistance is negative and significant for all different year treatments.

Finally, we show individual age treatments in Table A.2. We find the largest treatment effects on income from social assistance for the youngest age groups. Hence, it seems to be the case that municipalities were most severe for the youngest individuals.

Summarizing, for all individuals, we only find a significant treatment effect on the outcome variable income from social assistance. This effect is large and significant for both treatment groups. Besides, we find that the largest negative treatment effects on income from social assistance are found for adult children living at home, singles and single parents. Furthermore, The effect of the reform seems to increase over the post-treatment period and has the largest effect for the youngest age groups.

Not finding an effect on employment can potentially be explained by an implementation problem as municipalities were responsible for the implementation of this national law and this implementation may not have been a priority for them. Another interpretation is that there was already a downward trend in participation in social assistance, making it hard to identify the treatment effect. In line with this, it could be the case that the WIJ-reform was the codification, rather than the cause of implementation, of stricter conditionality combined with ALMPs, as municipalities might have already started with this trend before the reform. We find some evidence for this in Figure 4 as we indeed notice a downward trend of both participation in social assistance as in income from social assistance in the years before the reform, which is slightly stronger for young individuals. We also find some evidence for this in interviews which we have done with policy makers in the municipality of Amsterdam. However, we still expect to observe a treatment effect as the parallel trends assumption seems to hold in general. Besides, the WIJ-reform seems to be a large scale reform based on a first evaluation of the reform (Leenheer et al., 2011). Furthermore, we do find a large negative significant treatment effect on income from social assistance when we correct for differential trends. Therefore, we assume that the reform took place and was sizable.

Another possible explanation for not finding an effect on employment is a lock-

in effect in the short-run. But it is unlikely that this is the main explanation as a possible lock-in effect is expected to be smaller during an economic recession due to lower losses from reduced search effort (Lechner and Wunsch, 2009). Besides, the large negative effect on income from social assistance remains unexplained by the explanation of a lock-in effect. However, it would be less problematic if not finding an effect of the reform on employment is caused by a lock-in effect, as this possible lock-in effect may explain not finding an effect on employment in the short-run, but there may still be positive effects of the work-learn offer in the long run, which we are not able to measure yet. Nevertheless, a potential lock-in effect may have contributed to not finding an effect on employment.

Besides, the work-learn offer may have reduced employment by a crowding out effect, which is expected to be higher when unemployment is high. We expect that young individuals are overrepresented in certain sectors as in bars, restaurants and supermarkets and therefore young individuals may crowd out relatively more other young individuals rather than crowding out older individuals. This crowding-out effect is expected to be larger during an economic recession.

Based on the theoretical part, we expect both stricter conditionality as ALMPs to have a positive effect on employment. We explained that the mechanism goes by increased labor supply, lower wages, an increase in labor demand and thereby higher employment. The reason that we do not find an effect on employment may be explained by no effect of the reform on wages, labor demand and thereby employment. This works the same for stricter conditionality as for ALMPs because both have a positive effect on labor supply, but not necessarily on employment. The reason that labor demand and employment are not affected by an increase in labor supply can be explained by the great recession.

8 Discussion and concluding remarks

In this paper we consider the labor market effects of a Dutch reform towards more activating social assistance for the younger. The WIJ reform, introduced in 2009, made the eligibility of social assistance for the young stricter and tried to prevent NEETs (not in employment, education or training) via a work-learn offer. We use a difference-in-differences methodology and a long and rich panel data set to find the effect of the WIJ reform on participation in social assistance, income from social

assistance, employment, income from work and hours worked. We find that the reform reduced income from social assistance with 63% for individuals 20–22 years of age and with 26% for individuals 23–25 years of age compared to 2009. Despite of the large negative effects of the reform on income from social assistance, we find no effect of the reform on employment for individuals of 20–22 years of age neither for individuals 23–25 years of age.

We find that the reform had a negative significant effect on participation in social assistance and on income from social assistance for adult children living at home 20–22 years of age, singles 20–22 years of age and for single parents 23–25 years of age. Whereas, we find a positive significant effect of the reform on participation in social assistance for couples without children for both treatment groups. Besides, we find a positive significant effect on income from work for adult children living at home for individuals 20–22 years of age and a negative significant treatment effect for couples without children for individuals 23–25 years of age. We find a positive significant effect on employment measured by hours worked for both treatment groups for adult children living at home and for individuals 23–25 years of age for single parents. Finally, we find a positive significant effect on hours worked for couples without children for individuals 20–22 years of age.

We also find that the effect size increased over time, which indicates that it took some time before the full effect of the reform had taken place. Besides, the largest effect is found for young individuals of 20, 21, 22 and 23 years old, indicating that the reform was most severe for them.

The most plausible interpretation for not finding an effect of the reform on employment can be found in the Great Recession. The Great Recession caused high unemployment rates which followed from a combination of a shortfall in demand and sticky wages. The WIJ-reform may have increased labor supply, but there will be no positive effect on employment without a decrease in wages and thereby an increase in labor demand. Our results lead to the conclusion that labor supply policies for young low productivity individuals have no effect on wages, labor demand and employment in periods of a shortfall in aggregate demand. Hence, our results point at labor demand, rather than labor supply, being the constraining factor for employment to go up, which is not expected to be affected much by the reform.

Hence, we contribute to Kluge (2014) by researching the combination of stricter conditionality and ALMPs for young individuals. We find in our theoretical and

literature parts that both components increase labor supply, but we find in our empirical part that even together they were not able to have an effect on employment during the Great Recession. Besides, we contribute to Krueger and Meyer (2002) and Landais et al. (2010) by researching the employment effect of stricter conditionality and ALMPs during an economic recession. We conclude that the phase of the business cycle is very important for the effectiveness of both stricter conditionality and ALMPs on employment, which is in line with Landais et al. (2010).

We learn from this paper that the WIJ-reform had a large negative effect on income from social assistance but did not have a positive effect on employment. This indicates that the aim of the reform, to increase employment, is not achieved. If any effect on employment, it would be the opposite form the intended effect as employment went actually down for the treatment groups relative to the control group in the post-reform period. Hence, people dropped out of social assistance without having found a job. This must have had negative welfare consequences as some of these individuals are expected to fall into poverty, be a financial burden for their family and friends, build up debts, or even earn money illegally with criminal activities or informal work. Therefore the main policy implication of this research is that the context of different age groups and different labor market conditions is very important for policy making. Policy makers should be more careful and take this demand constraints into account when they make policies to increase employment.

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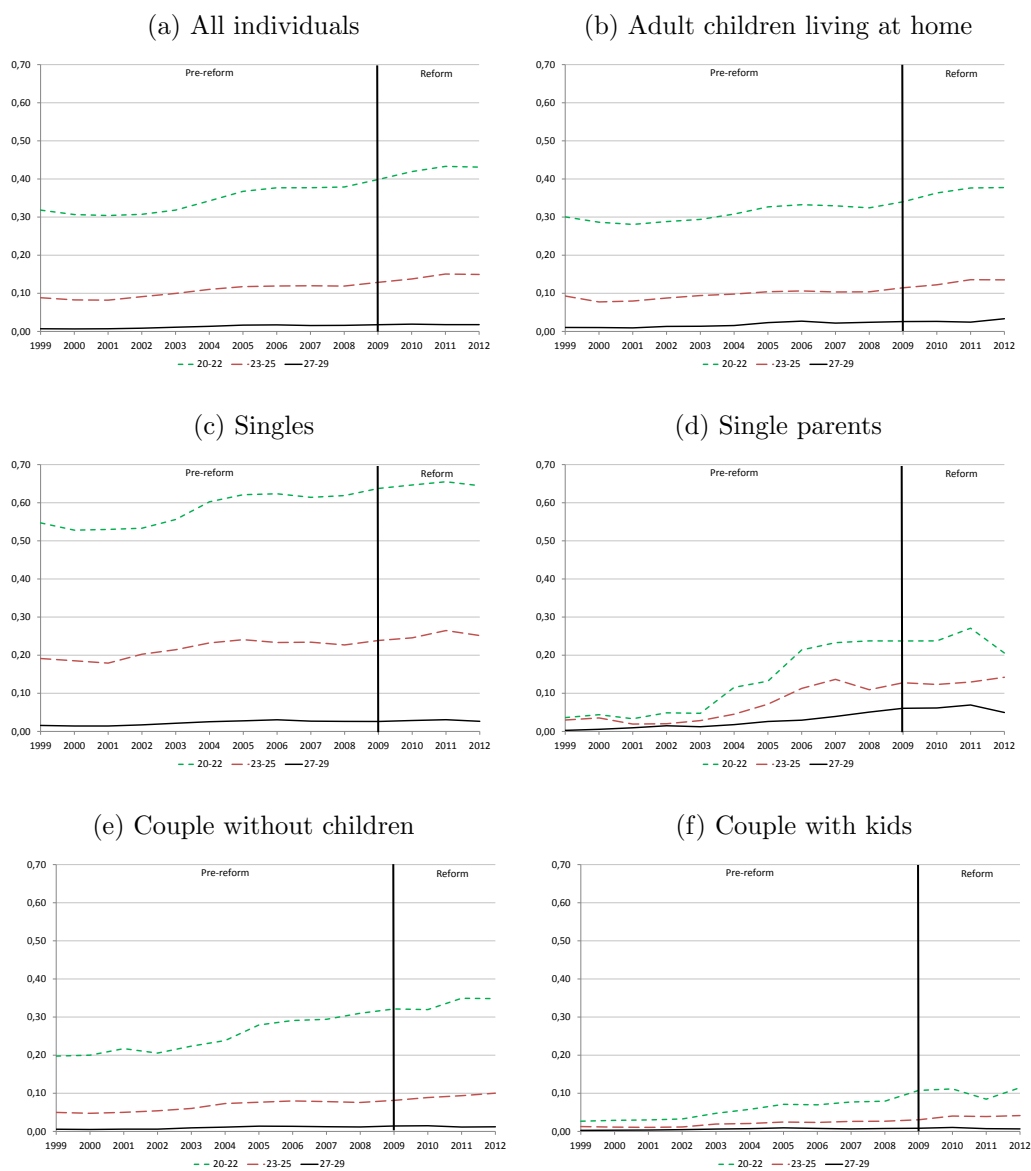
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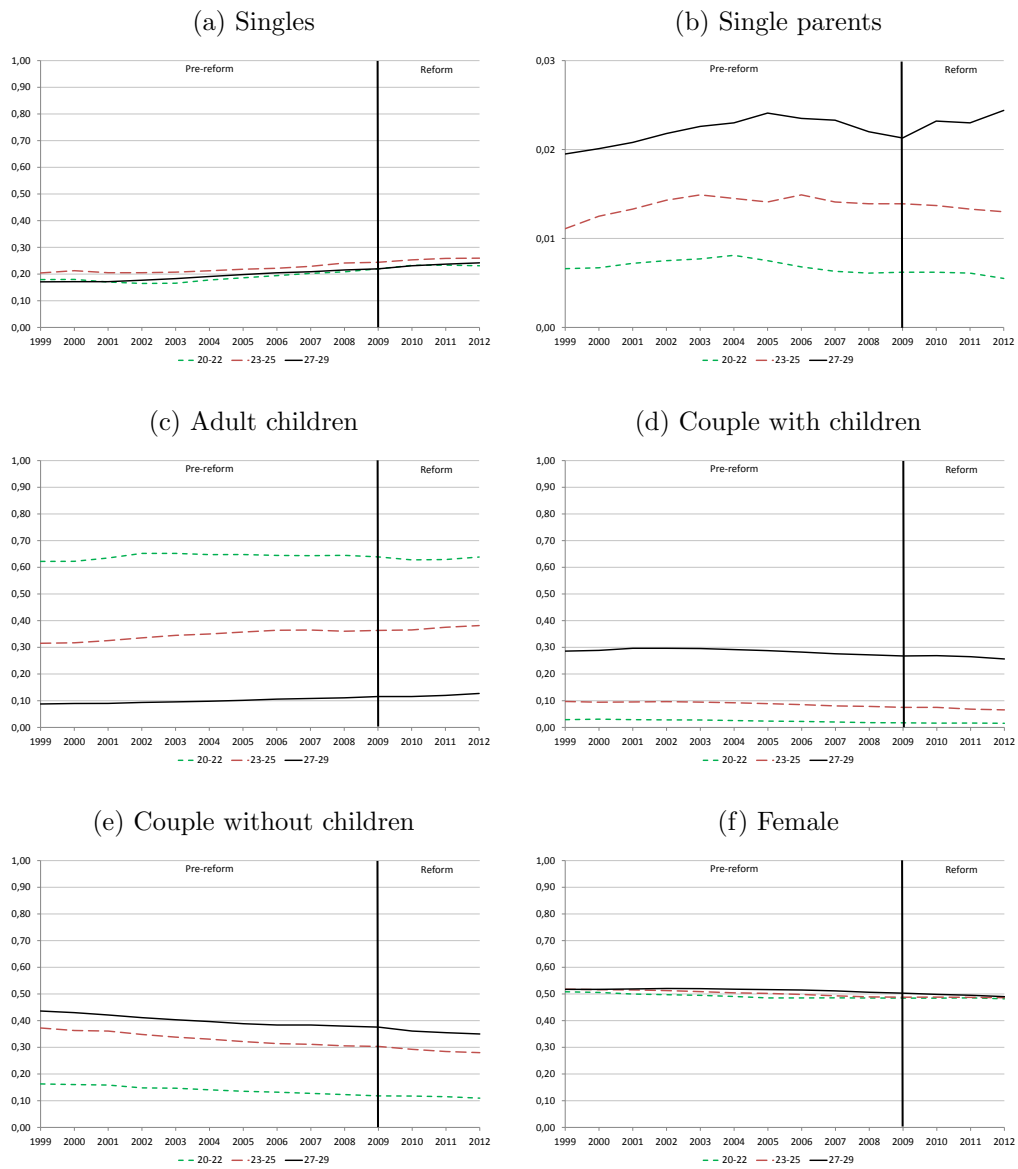
A Appendix

Figure A.1: Participation in education



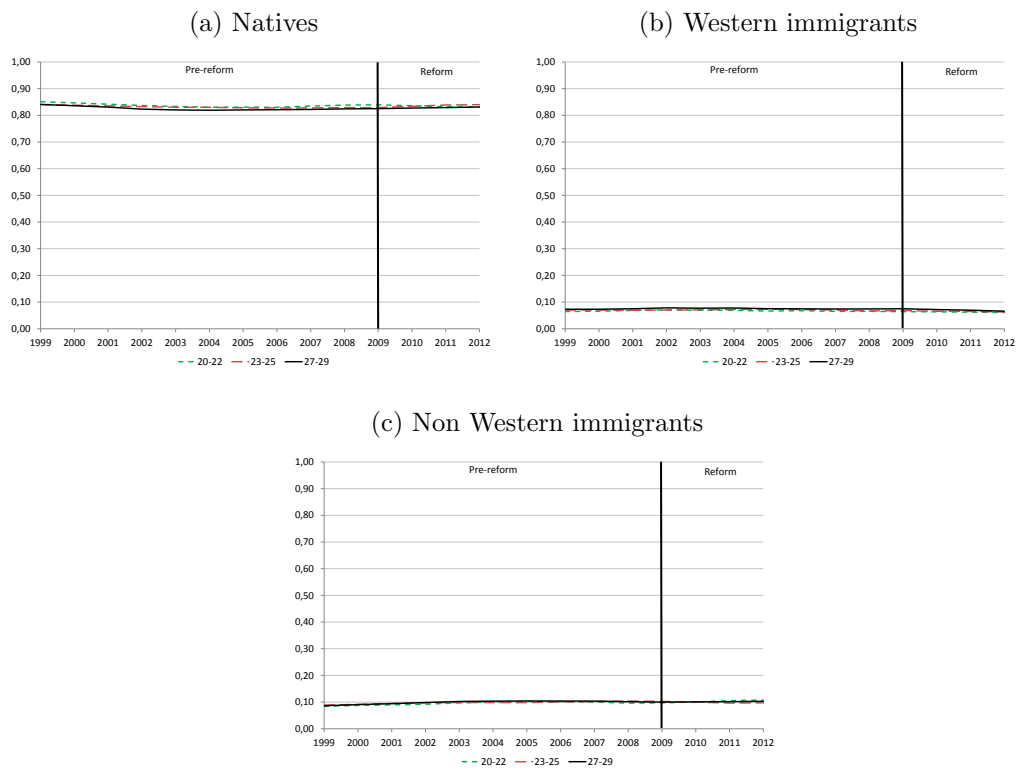
Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.2: DD plots: control variables: Householdtype and gender



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.3: DD plots: control variables: Ethnicity



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.4: DD plots: adult children living at home



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.5: DD plots: singles



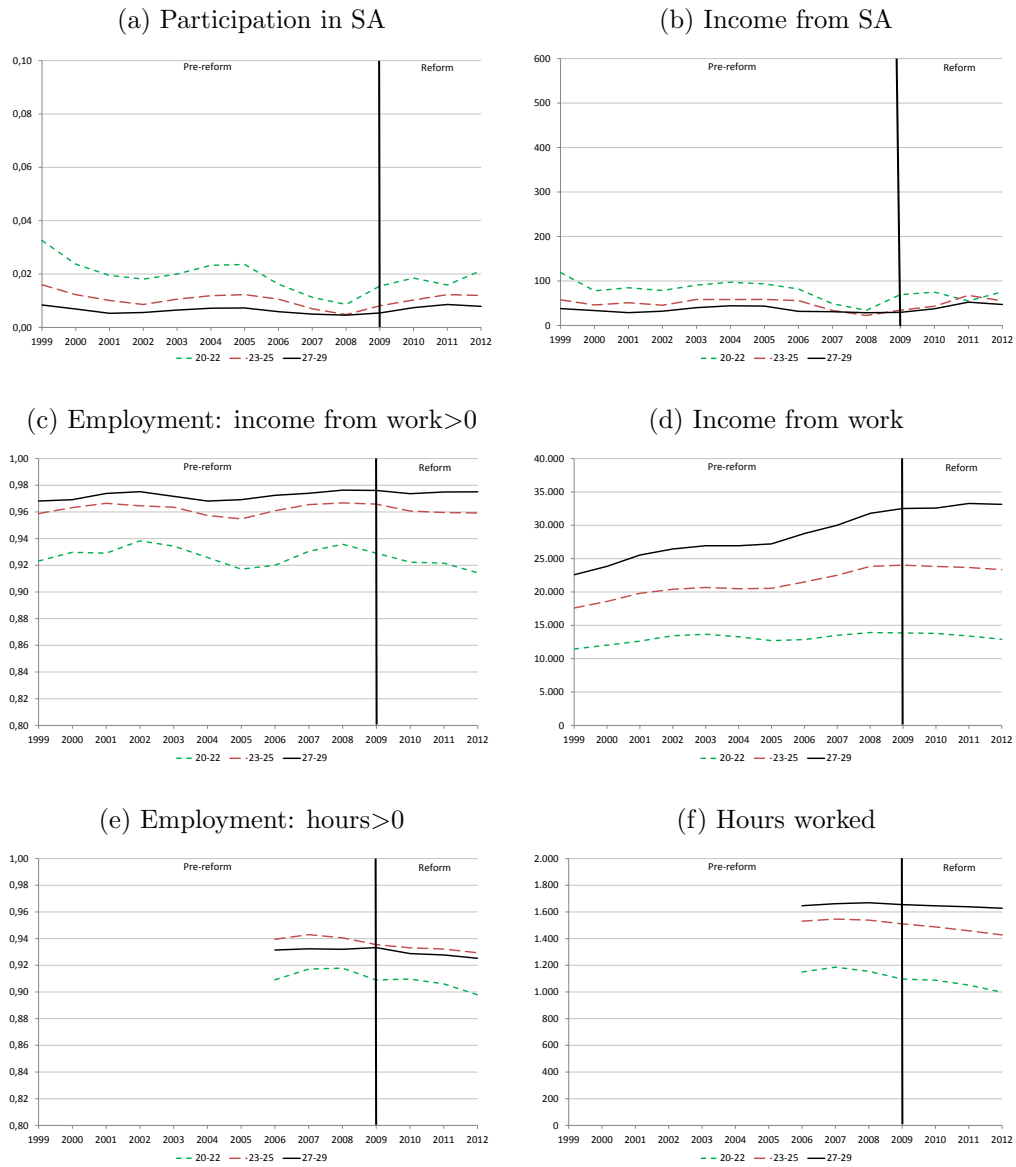
Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.6: DD plots: single parents



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.7: DD plots: couples without children



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.8: DD plots: couples with children



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.9: DD plots: Amsterdam



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.10: DD plots: Rotterdam and The Hague



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.11: DD plots: Utrecht



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.12: DD plots: Groningen



Source: Own calculations using the Labour Market Panel (Statistics Netherlands).

Table A.1: Regression results treatment dummies per year

	(1)	(2)	(3)	(4)	(5)	(6)
	Participation in SA	Income from SA	Employment: income>0	Income from work	Employment: hours>0	Hours worked
Treatment 20–22 - 2010	–0.0004 (0.0037)	–66.599* (35.313)	–0.0031 (0.0080)	111.63 (456.59)	0.0071 (0.0086)	2.2305 (24.410)
Treatment 20–22 - 2011	–0.0034 (0.0046)	–80.459* (43.835)	–0.0024 (0.0104)	431.73 (605.00)	0.0064 (0.0054)	7.9589 (13.147)
Treatment 20–22 - 2012	–0.0059 (0.0059)	–89.858* (49.714)	–0.0077 (0.0136)	352.37 (745.81)	0.0094 (0.0130)	20.473 (36.453)
Treatment 23–25 - 2011	0.0005 (0.0023)	–41.015* (24.449)	–0.0035 (0.0047)	36.849 (287.82)	0.0109 (0.0072)	11.459 (21.466)
Treatment 23–25 - 2011	–0.0018 (0.0028)	–57.396* (30.364)	–0.0043 (0.0059)	–111.95 (318.94)	0.0099 (0.0219)	23.294 (55.784)
Treatment 23–25 - 2012	–0.0046 (0.0036)	–79.201** (38.133)	–0.0083 (0.0078)	–317.06 (320.25)	0.0154 (0.0112)	16.601 (33.680)
Observations	1,706,989	1,706,989	1,706,989	1,706,989	1,706,989	1,706,989
Individuals	343,408	343,408	343,408	343,408	343,408	343,408
Clusters	69	69	69	69	69	69

Cluster-robust standard errors in parentheses, clustered at year of birth (23 years) interacted with ethnicity (3 categories), * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. We have controlled for demographics, unemployment-age dummies, age specific trends and control year dummies.

Table A.2: Regression results treatment dummies per age

	(1)	(2)	(3)	(4)	(5)	(6)
	Participation in SA	Income from SA	Employment: income>0	Income from work	Employment: hours>0	Hours worked
Treatment20	–0.0044 (0.0057)	–90.569* (52.444)	–0.0015 (0.0110)	603.62 (734.71)	0.0150 (0.0092)	–4.4872 (27.873)
Treatment21	–0.0032 (0.0047)	–80.293* (42.267)	–0.0048 (0.0102)	359.61 (608.45)	0.0006 (0.0088)	8.4554 (21.715)
Treatment22	–0.0014 (0.0039)	–65.631* (35.058)	–0.0061 (0.0092)	–76.542 (433.34)	0.0057 (0.0065)	–9.6809 (16.325)
Treatment23	–0.0021 (0.0031)	–65.492** (28.336)	–0.0060 (0.0071)	–155.70 (344.62)	0.0101 (0.0078)	–5.6346 (18.939)
Treatment24	–0.0016 (0.0028)	–56.773* (29.384)	–0.0061 (0.0058)	–122.36 (305.09)	–0.0002 (0.0043)	0.3979 (15.126)
Treatment25	–0.0016 (0.0024)	–51.215* (26.368)	–0.0032 (0.0048)	–34.740 (284.42)	0.0005 (0.0042)	17.559 (13.156)
Observations	1,706,989	1,706,989	1,706,989	1,706,989	1,706,989	1,706,989
Individuals	343,408	343,408	343,408	343,408	343,408	343,408
Clusters	69	69	69	69	69	69

Cluster-robust standard errors in parentheses, clustered at year of birth (23 years) interacted with ethnicity (3 categories), * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. We have controlled for demographics, unemployment-age dummies, age specific trends and control year dummies..

Table A.3: Regression results: cities

	Amsterdam	Rotterdam/ The Hague	Utrecht	Groningen	Maastricht	Enschede	Den Bosch	Overige
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Participation in SA								
treatment 20-22	-0.0108 (0.0085)	-0.0136 (0.0082)	-0.0148** (0.0074)	-0.0232*** (0.0083)	-0.0179 (0.0141)	0.0079 (0.0126)	-0.0283* (0.0155)	-0.0009 (0.0044)
treatment 23-25	-0.0063 (0.0076)	-0.0057 (0.0064)	-0.0113 (0.0075)	-0.0013 (0.0084)	-0.0262** (0.0124)	0.0185 (0.0119)	-0.0209 (0.0165)	-0.0007 (0.0026)
Income from SA								
treatment 20-22	-95.595 (81.310)	-230.96** (102.33)	-149.30** (64.122)	-81.562 (68.175)	-259.00** (118.25)	53.023 (123.29)	-340.25** (138.27)	-64.764* (33.754)
treatment 23-25	-81.633 (66.460)	-162.97** (77.404)	-116.16* (64.476)	9.1039 (68.100)	-312.72*** (112.89)	126.43 (119.54)	-180.54 (153.25)	-44.670* (23.870)
Employment: income>0								
treatment 20-22	-0.0089 (0.0163)	0.0038 (0.0126)	0.0090 (0.0133)	-0.0112 (0.0140)	-0.0052 (0.0231)	-0.0245 (0.0240)	-0.0179 (0.0205)	-0.0089 (0.0103)
treatment 23-25	-0.0077 (0.0127)	-0.0142 (0.0100)	-0.0122 (0.0095)	0.0040 (0.0116)	-0.0195 (0.0161)	-0.0321** (0.0134)	-0.0083 (0.0136)	-0.0051 (0.0061)
Income from work								
treatment 20-22	3,876.2 (2,547.1)	1,734.2* (998.06)	2,047.5** (807.57)	2,048.8*** (628.31)	9,956.6 (6,244.1)	172.84 (1,031.0)	1,059.4 (1,422.7)	-815.24 (583.43)
treatment 23-25	1,872.0 (2,250.1)	34.292 (792.27)	-915.84 (878.43)	1,384.1** (573.25)	6,563.5 (5,277.9)	-365.26 (948.32)	198.85 (1,366.9)	-657.02* (276.80)
Employment: hours>0								
treatment 20-22	0.0046 (0.0200)	0.0203 (0.0170)	0.0013 (0.0176)	-0.0086 (0.0208)	-0.0198 (0.0317)	-0.0284 (0.0237)	-0.0167 (0.0303)	0.0069 (0.0063)
treatment 23-25	-0.0257 (0.0179)	0.0061 (0.0120)	-0.0063 (0.0130)	-0.0091 (0.0151)	-0.0050 (0.0219)	-0.0224 (0.0159)	-0.0649** (0.0246)	0.0065 (0.0043)
Hours worked								
treatment 20-22	-23.248 (36.553)	6.3016 (27.429)	-1.0727 (40.428)	-6.8751 (33.479)	-79.520 (63.605)	-93.492* (53.854)	0.9351 (69.480)	-8.5937 (24.774)
treatment 23-25	-28.523 (46.014)	8.6250 (24.087)	0.6496 (43.109)	-3.1463 (29.662)	-50.317 (63.154)	-83.606* (48.101)	-98.538 (64.118)	6.8179 (13.862)
Observations	83,814	153,382	66,533	62,239	26,653	30,733	23,674	1,297,070
Individuals	16,165	29,295	12,154	10,917	4,945	5,564	4,443	265,928
Clusters	69	69	69	69	69	69	69	69

Cluster-robust standard errors in parentheses, clustered at year of birth (23 years) interacted with ethnicity (3 categories), * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. ^aHours worked are not observed for the period 1999-2005. Observations *Hours worked*:830,745. Clusters *Hours worked*: 48. We have controlled for demographics, unemployment-age dummies, age specific trends and control year dummies.