Don’t be picky!

The effects of a tightening in the suitable employment policy in the Netherlands

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ABSTRACT

This study estimates the effects of a tightening in the Dutch suitable employment policy implemented on the 1st of July 2015. Unemployed workers are obliged to accept all job offers after being unemployed for at least six months (previously twelve months). The tightening theoretically induces unemployed workers to redeploy earlier and, therefore, may seem an effective way to reduce the unemployment benefit expenditures. Using administrative data from the Dutch unemployment insurance agency (UWV), a regression discontinuity (RD) design and a triple difference-in-differences (DiD) approach are exploited to assess the effectiveness of the policy on the unemployment duration and job quality in terms of earnings and employment stability. The results indicate negative effects on the unemployment duration and employment stability but find no effects for the level of earnings. Consequently, governments face a trade-off between incentivising early redeployment and limiting relapse into unemployment.

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I. Introduction

Unemployment rates in the European Union (EU) have been surging since the economic downturns in 2007 and 2009. On average, 10.9% of the labour force was unemployed in 2013. As of 1998, when the EU started to report unemployment rates, there has never been such a high unemployment rate in the EU. Although the unemployment rate in the Netherlands has been lower than the EU average, it has been rising substantially as well. The unemployment rate doubled in six years; it increased from 3.7% in 2008 to 7.4% in 2014. Inflows into unemployment have been massive as well, amounting to an inflow of 613,230 workers in 2013 alone. This entails that about 7% of the Dutch labour force entered unemployment in a single year. These soaring unemployment rates put pressure on the public budget. Governments do not only grant unemployment benefits to unemployed workers, but also forego tax revenues when workers are unemployed. Lifting unemployed workers from unemployment to employment may therefore have considerable positive effects on the public budget. Redeployment of unemployed workers can be enhanced by increasingly focussing on active labour market policies (ALMPs), such as suitable employment regulations. These suitable employment policies typically require unemployed workers to accept job offers that are regarded suitable to them.

Suitable employment policies may seem attractive at first sight as they, theoretically, reduce the unemployment duration. Unemployed workers may have to accept jobs they would not have accepted if those jobs would not have been defined as appropriate. Moreover, these policies may be implemented at low costs as only the corresponding legislation has to be adjusted. Other sorts of ALMPs, such as job search trainings, come along with additional and recurring costs. Yet, suitable employment policies may also have adverse effects on the quality of the job after redeployment. Unemployed workers might leave unemployment earlier while they have to accept less rewarding and less stable jobs. The suitable employment regulation might induce workers to redeploy in jobs with lower earnings and a higher probability of falling back into unemployment than those workers prefer.

No research has yet been carried out into the effects of these suitable employment policies alone on the unemployment duration and job quality. Research has mainly focussed on estimating the effects of punitive unemployment benefit sanctions as a result of noncompliance with activation programs, to which these suitable employment regulations belong. Overall, the existing literature is small and unambiguous in finding large positive effects of unemployment benefit sanctions. The exit rate out of unemployment increases between 36% and 124%, depending on the size of the (potential) benefit reduction and whether the threat effect (ex-ante) or direct effect (ex-post) is estimated (Abbring, Van den Berg and Van Ours, 2005; Svarer, 2011). Moreover, activation programs' benefit sanctions are estimated to lower earnings between 1.5% and 6.2% and increase the exit rate out of employment by 15% (Arni, Lalive and Van Ours, 2013). The literature is in accordance with the theoretical predictions that activation programs shorten the unemployment duration while lower the job quality. In addition, Svarer (2011) stressed the need for investigating the effects of a tightening in an activation program rather than the effects of a new implementation. The literature did not yet analyse such policy restrictions.

This study aims to fill these two gaps by investigating whether and, if so, to what extent a stricter suitable employment regulation reduces the unemployment duration and the quality of the new job. It exploits an adjustment in the suitable employment policy in the Netherlands. This regulation took effect on the 1st of July 2015 as part of a broader package of labour market reforms, the so-called WWZ (Law Work and Security or *Wet Werk en Zekerheid)*. This policy entails that unemployed workers, if qualified, are obliged to accept any job offer they receive after six months of unemployment, irrespective of the salary, level of work or commuting time. Previously, unemployed workers did not have to accept all job offers until they had been unemployed for more than twelve months. This study exploits a regression discontinuity (RD) design and a difference-in-differences (DiD) approach to quantify the effects of the stricter suitable employment regulation. Both approaches estimate the effects for a one-monthly, quarterly and half-yearly bandwidth. The RD approach compares workers that got unemployed slightly before to those slightly after the implementation of the WWZ. It assumes that those two groups of workers are similar on all background characteristics except for being subject to the WWZ. However, checking the assumptions of the RD design suggests that it may be an invalid approach to quantify the effects of the suitable employment policy. The RD estimations also return widely-varying results. The DiD approach is less restrictive as it does not

require the treatment and control group to be similar. This approach is used to investigate the effects of the policy adjustment on the unemployment duration and job quality. To separate the effects of the stricter suitable employment regulation from the effects of the WWZ, a triple DiD approach, which estimates the effects for two education levels separately, is applied to a subsample of the dataset.

The results of the DiD approach suggest that the stricter suitable employment policy reduces the time spent in unemployment by about 3 days (2.5%). It appears that unemployed workers who got unemployed after the 1st of July 2015 accept job offers that would not have been accepted without the tightening in the suitable employment policy. The results are in accordance with the existing literature and confirm the expected negative effect. The stricter suitable employment policy did not lead to significantly lower earnings. The tightening may either be too small to find significant effects or may not affect the monthly income at all. However, the findings do indicate negative effects on the employment stability. The relapse rate into unemployment one year after the start of the unemployment spell increased by 0.8%-points (11.9%) to 1.4%-points (23.7%). These results suggest that the suitable employment policy induced workers to accept less stable jobs, like jobs with temporary contracts or a probation period. Overall, the long-run budgetary effectiveness and desirability of the adjustment may be questioned as it causes workers to earlier claim another unemployment benefit.

Although the suitable employment regulation negatively affects the unemployment duration, the WWZ seems to prolong the unemployment duration by 6.3 days (4.9%) to 10.8 days (9.2%). Without the tightening in the suitable employment regulation, the unemployment duration would have been extended even more. The lengthening in the unemployment duration caused by the WWZ is counterintuitive. The WWZ consists of five different policy changes of which the majority aims to enhance labour market redeployment. In addition, the WWZ has adverse effects on the average monthly income. Wage reductions amount to approximately €23.42 (1.3%) on a monthly basis. On the other hand, the WWZ reduces the relapse rate into unemployment. The share of workers that got redeployed in the first six months of their unemployment spell and fell back into unemployment slightly after these six months, is reduced by 1.3%-points (14.1%) to 1.9%-points (15.6%). This implies that the WWZ enhances the employment stability but lowers the monthly income.

The remainder of this study is structured as follows. Section II establishes the theoretical framework on which this study is built. The labour market reforms in the Netherlands are described in Section III. The data along with some descriptive statistics are outlined in Section IV. Section V elaborates on the empirical strategies used. Section VI presents the results and Section VII concludes.

II. Theoretical framework

Active labour market programs (ALMPs) mainly aim to improve the functioning of the labour market by improving unemployment benefit (UB) claimants' labour mobility, redeployment and investments in human capital (Boeri and Van Ours, 2013). Four main types of ALMPs can be distinguished, including training programs, subsidized employment, public employment services such as counselling and job search courses, and activation programs like attending interviews with employment counsellors and job creation programs. Participation in activation programs is mandatory in order to remain entitled to unemployment benefits, whereas participation in public employment services is voluntary (Boeri and Van Ours, 2008; OECD, 2005). Since activation programs are typically regarded as a burden by unemployed workers, these programs reduce UB utilization through diminishing the inflow in and enhancing the outflow out of unemployment (Boeri and Van Ours, 2013). Suitable employment regulations can be classified as activation programs.

Suitable employment regulations refer to job offers that match or suit the worker's profile. What is considered as suitable employment typically differs by country and depends on some characteristics of the UB recipient. In general, the classification of suitable employment rests on the UB claimant's previous earnings, level of education and previous job (Abbring et al., 2005). In addition, countries may have supplementary requirements in assessing the suitability of job offers, such as a maximum commuting time and personal circumstances of the UB claimant.[1] The UB recipient is required to accept suitable job offers. This advances the return to the labour market as unemployed workers redeploy into jobs that would not have been accepted without the regulation. Failure to comply with suitable work regulations typically results in benefit sanctions, encompassing disqualification of the UB recipient from all remaining unemployment benefit entitlements, a temporary exclusion of benefit allowances or a reduction in the size of the benefit (International Labour Organization, 1934a; Menard, 1945).

A search of the literature on the effects of suitable employment regulations on unemployment duration yielded no results. This lack of relevant studies leads one to broaden the search base towards the effects of activation programs or ALMPs in general. A large body of literature has emerged investigating the effects of ALMPs. A meta-analysis conducted by Kluve (2010), encompassing 96 evaluation studies for 19 countries, suggests that ALMPs raise the employment probability. The type of program turns out to be a large factor determining the effectiveness of ALMPs. The meta-analysis enables the author to systematically identify any patterns in the effectiveness of European ALMPs. Results found by individual studies may be subject to their research design. A meta-analysis attaches weights to these estimates depending on the validity of the study.[2] Of particular relevance to the current study are the 21 studies that investigate the effects of programs focussed on services and sanctions, i.e. programs aimed at enhancing job search efficiency, such as job search courses, monitoring and sanctions. Yet, the author does not make a distinction between services and sanctions. The results indicate that services and sanctions, compared to training programs, have got an alleviated (reduced) probability of 44.1%-points (19.5%-points) in reflecting a significantly positive (negative) estimate on the worker's employment probability.

Similarly, Card, Kluve and Weber (2015) exploit 207 studies in order to construct a weighted estimate of the probability of employment. They do differentiate between job search assistance and sanctions. Sanctions and threats appear to have substantial positive short-run effects, amounting to an increased employment probability of 10.8%-points compared to training programs. Moreover, in the short run, sanctions and threats seem to be most effective of all ALMPs. Nonetheless, the positive effect of sanctions and threats diminishes over time and is especially lower in the long-run. In contrast

[1] The members of the International Labour Organization (ILO) agreed that work is not suitable to a UB recipient if it involves residence of the benefit claimant in a region in which appropriate accommodation is not available, if the salary offered is lower or other employment conditions are less favourable, if the job is vacant due to a trade dispute or if any personal circumstances make refusal not unreasonable (International Labour Organization, 1934a). In addition, the ILO recommended that 'the length of the claimant's service in the previous occupation, his chances of obtaining work in a similar sector, his vocational training, and his suitability for the work' should be taken into account as well (International Labour Organization, 1934b).

[2] Results of studies are classified as significantly positive, significantly negative or insignificant. Kluve (2010) takes the type of ALMP, the research design, the institutional context and the economic situation in the country into account.

to the findings that effects differ by type of program, stands a meta-analysis carried out by Filges, Smedslund, Knudsen and Jørgenson (2015). After selecting 39 relevant studies, the authors found a small positive effect of ALMPs on the exit rate to employment or on the probability of employment. They did not find any varying effects by type of program. In general, unemployed workers treated with an ALMP have 52% chance of finding work before a non-treated unemployed worker does.

Since the majority of these meta-analyses do not specifically look at activation programs that induce sanctions in case of noncompliance, it may give additional insights when digging deeper into studies examining the effect of UB sanctions in particular. The UB sanctions stipulated by activation programs induce unemployed workers to adhere to these programs and, in turn, increase the transition rate from unemployment to employment. In general, the literature makes no distinction in the cause of the benefit sanction, i.e. which activation program's violation enacts the benefit sanction is not considered.

Noteworthy is the slight distinction the literature makes in terms of the nature of the benefit. Unemployed workers may receive unemployment benefits or other welfare benefits. Although these recipients both aim to enter the labour market, Van den Berg, Van der Klaauw and Van Ours (2004) note that UB and welfare recipients differ substantially with respect to their labour market prospects. UB recipients typically have recent work experience which enhances their redeployment. The meta-analysis by Card et al. (2015) includes studies that examine the effects of general welfare benefit sanctions on the transition rate from unemployment to employment. This may bring along a bias when one aims to analyse the effects of UB sanctions. Therefore, this literature review focusses solely on studies that examine the effects of UB sanctions on UB outflows.

The literature distinguishes two main channels through which benefit sanctions, theoretically, affect compliance with activation programs and, subsequently, reduce the unemployment duration. The ex-ante effect refers to the mere threat of the UB sanction that increases search efforts and lowers reservation wages of unemployed workers. Put differently, before any activation program's violation occurs, search behaviour is altered already. The warning effect corresponds to the effect resulting from the issuing of a warning that a benefit sanction may be imposed shortly, which is the first channel of the ex-post effect (Lalive, Van Ours and Zweimüller, 2005). The second ex-post effect relates to the actual imposition of a benefit sanction which reduces the reservation wages of unemployed workers, i.e. the direct effect.

In general, the body of literature on UB sanctions is small and assents in their findings that sanctions have large positive effects on the UB recipients' probability of returning to the labour market. The large majority of relevant studies uses duration models in estimating the effects of UB sanctions on the transition rate from unemployment to employment (e.g. Abbring et al., 2005; Lalive et al., 2005; Svarer, 2011; Arni et al., 2013; Van den Berg and Vikström, 2014; Van den Berg, Hofmann and Uhlendorff, 2016).[3] As these studies investigate the effects of UB sanctions for a specific sampling period, there may be missing data on some unemployed workers who find work after the sampling period has ended. Therefore, these studies draw on censored regression models, of which the duration model is an application (Wooldridge, 2015).[4]

Abbring et al. (2005) study the ex-post effect of UB sanctions by using data on Dutch workers entering unemployment in 1992. Benefit reductions turn out to be largely temporarily and partially, ranging from 5%-points for 4 weeks to 25%-points or 30%-points for 13 weeks. The authors focus on one manufacturing industry (the metal industry) and one service sector (the banking sector), from which they conclude that individual redeployment rates for males increase by 61% and 36% and for

[3] A study carried out by Hofmann (2008) uses a matching approach to estimate the ex-post effects of UB sanctions. The results indicate positive ex-post effects of UB sanctions on the employment probability in regular employment jobs, which are defined as unsubsidised jobs subject to social contributions. These findings are in accordance with the literature using duration models.

[4] In a duration model the dependent variable is called a duration. This is a variable that measures the time before a certain event occurs, which is the event of leaving unemployment in these studies. If durations are censored, i.e. they start before the sampling period has begun (left-censored) or finish after the sampling period has ended (right-censored), a bias towards zero may result from a simple OLS regression (Bhat and Pinjari, 2007; Wooldridge, 2015). Controlling for this censored data enables the authors to separate the causal and selection effect (Svarer, 2011). The selection effect corresponds to the difference in characteristics between unemployed workers that are sanctioned and those that are not. Unemployed workers facing a reduction in their benefit are likely to have lower labour market prospects beforehand compared to their non-sanctioned counterparts (e.g. Svarer, 2011; Lalive et al., 2005).

females by 98% and 85%, respectively. The effects are caused by a reduction in the UB or by the threat of additional UB sanctions. Even stronger ex-post effects of UB sanctions are found by Svarer (2011), who uses an extensive dataset that comprises all unemployment spells and sanctions in Denmark over the 2003 – 2005 period. Effects amount to increases in the exit rates of around 124%. This result corresponds to a loss of the unemployment benefit for two to three days, for three weeks or until the UB claimant has worked for 300 hours within a period of ten weeks. In addition to the ex-post effect, Lalive et al. (2005) try to quantify the ex-ante effect by using a Swiss dataset. Moreover, the fact that unemployed workers are notified when they do not comply with UB eligibility criteria, enables the authors to study the warning effect independently from the direct effect. The ex-ante effect is estimated by exploiting differences between Swiss unemployment insurance agency's monitoring intensity. This led to large differences in probabilities of receiving punitive benefit sanctions (reductions of 100% for different periods). Lalive et al. (2005) conclude that the warning effect adds up to a 25.2% increase in the exit rate from unemployment and, once the benefit sanction has been imposed, by another 19.8%. The ex-ante effect leads to an increased exit rate of 64% and is therefore larger than the total ex-post effect.

The literature does not only estimate the effects of UB sanctions on the transition rate from unemployment to employment but also incorporates the possibility that effects wear out over time and are heterogeneous. Sanction effects may decrease over time as UB recipients increase search effort directly after issuance of the warning or implementation of the sanction but reduce this effort again as time passes by (Lalive et al., 2005). The literature's findings relating these time-varying effects are ambiguous. Abbring et al. (2005) did not find decreasing effects over time while Svarer (2011) found that the effects decrease after approximately three months. Lalive et al. (2005) found a significant drop in exit rates after thirty days for the warning effect but not for the direct effect. Furthermore, some studies incorporated the heterogeneity of the unemployed population and assessed whether the effects of sanctions differ by group. No unequivocal results emerge from these studies either. On the one hand, Abbring et al. (2005) did not find any different effect when taking the heterogeneity of the population into account. On the other hand, the analysis conducted by Svarer (2011) shows that especially male immigrants and their offspring are more responsive to UB sanctions compared to Danish natives, whereas single men and single women are less responsive. Moreover, Svarer (2011) tried to investigate whether more severe sanctions have larger effects but concludes that the fact that more severe sanctions are only imposed in a limited amount of cases, causes his analysis to be merely explorative and that better data is needed.

Two more recent papers extended the literature by investigating the effects of UB sanctions on the quality of the job, namely the post-unemployment income, the employment stability and the number of hours worked after redeployment (Arni et al., 2013; Van den Berg and Vikström, 2014). Unemployed workers may redeploy in less rewarding and less stable jobs to circumvent UB sanctions (Arni et al., 2013). These sanctions may induce unemployed workers to intensify their search effort but may also reduce workers' reservation wages (Arni et al., 2013). A reduction in the reservation wage may be undesirable from a governmental perspective as lower wages reduce (in)direct tax revenues.

By exploiting data on Swiss unemployed workers Arni et al. (2013) are able to distinguish the ex-ante and ex-post effects (warning and direct effect) of UB sanctions. The authors find that the combined ex-post (ex-ante) effect reduces the income after redeployment by 6.2% (1.5%). The authors claim that the positive effects of leaving unemployment earlier do not outweigh the losses in earnings to the worker. No ex-ante effect on the employment stability could be observed in their data. However, the direct ex-post effect increases the exit rate out of employment by 15%. Arni et al. (2013) argue that unemployed workers will start searching for a temporary contract only after the imposition of a sanction. These findings are in line with the results of Van den Berg and Vikström (2014) who examined the effects of UB sanctions on the earnings of and the number of hours worked by Swedish unemployed workers. On average, imposed UB sanctions lower the earnings of workers by 4%. Van den Berg and Vikström (2014) also found that sanctioned unemployed workers are 15% less likely to enter a full-time job. Overall, both studies point towards negative effects of sanctions on the job quality in terms of earnings, employment duration and workhours.

This study appends the existing literature in three ways. First, while most studies lump all activation programs together, this study investigates the effectiveness of the suitable employment

program specifically. Secondly, this study aims to reduce the gap put forward by Svarer (2011). He stated that better data is needed to examine the effects of a stricter benefit sanctions regime. The Dutch suitable employment adjustment corresponds to a tightening sanction regime. Lastly, the limited amount of studies investigating the effects of ALMPs on the job quality after redeployment is extended by analysing the earning and employment effects of the tighter suitable employment regulation.

III. Labour market reforms in the Netherlands

In February 2014, Dutch parliament adopted a new law known as WWZ (Law Work and Security, *Wet Werk en Zekerheid*) encompassing changes in Dutch dismissal laws and the unemployment law called WW (Unemployment Law, *Werkloosheidswet*).[5] The WWZ introduced five changes in the Dutch labour market regulations, including (1) a shortening of the period after which all work is defined as suitable, (2) the introduction of income-based calculations of benefits, (3) a change in the severance payment, (4) the calculation of the daily wage and (5) a limitation of the number of temporary contracts. All changes took effect on the 1st of July 2015.

Suitable employment

In general, job offers are considered suitable when they match the level of work out of which the UB recipient turned unemployed, pay an income that is at least 70% of the income retrieved in the recipient's last job and have a commuting time by public transport of maximally two hours a day. The Dutch unemployment law (WW) states that the UB claimant is exempted from accepting job offers that are classified as suitable in case of being unable to completely execute the specified tasks in terms of physical, mental or social well-being.[6] Regardless of the WWZ, UB claimants are required to accept suitable job offers for the duration of six consecutive months. Before the WWZ entered into force, the UB claimant had to accept suitable work at a single lower education level as well after being unemployed for six months. In case the claimant has not been able to redeploy within twelve months, all jobs are defined suitable, irrespective of the level of work, salary and commuting time (upper part of Table 1). From the 1st of July 2015 onwards, all job offers are considered suitable for workers that were unable to redeploy within six months. This implies that workers possessing an academic or higher vocational education degree must accept job offers at the basic education level after six months of unemployment. Workers with vocational or basic educational backgrounds are not affected by this policy change as outlined in the lower part of Table 1.

The foremost aim of this policy is to reduce the unemployment duration by broadening the search base of the unemployed. A tightening of the suitable employment regulation, i.e. the faster enactment of all job offers being defined as suitable, theoretically, reduces the unemployment duration in two ways. First, unemployed workers accept a suitable job offer after six months of unemployment that might have been refused before the WWZ entered into force. Secondly, some recipients may enter into jobs that are not defined as suitable during the first six months of their unemployment spell, such as jobs that comprise a longer commuting time than allowed by the suitable employment definition. Accepting 'unsuitable' jobs may be rational in case recipients expect to face worse job offers – which they would have to accept – after six months of unemployment. In case the recipient mistakenly refuses a suitable job offer, the Dutch unemployment insurance agency will reduce one's unemployment benefit for the number of hours and length of the job offer. Benefit sanctions should ensure compliance with the suitable employment regulation.

Income-based calculation of benefit

On the 1st of July 2015, an adjustment took place in the way additional labour earnings are calculated and subtracted from the unemployment benefit. Previously, additional labour income reduced the unemployment benefit in a proportionate way to the number of hours worked, the so-called hour-based calculation. This implies that the unemployment benefit only covers the number of hours that the recipient is still unemployed after starting to work. This hour-based calculation of the benefit reduces the incentive to work if the claimant receives a lower wage than the unemployment benefit pays. The income-based calculation of additional earnings, which refers to the idea that additional income, rather than hours, reduces the unemployment benefit, should solve this deficiency. If the individual starts working for an income not exceeding 87.5% of the worker's previous income, 70% of any additional

[5] More information on the WWZ can be retrieved from Schulinck (2007).

[6] Dutch UB claimants are obliged to apply for jobs four times within four weeks, to register at an employment agency and to provide information to and adhere to requests of the Dutch unemployment insurance agency (UWV).

Table 1. The Dutch suitable employment policy

2008 – June 2015				
Level / *Qualification*	Academic and higher vocational education (WO + HBO)	Intermediate vocational education (MBO)	Vocational education (VMBO)	Basic education
Academic and higher vocational education (WO + HBO)	0 – 6 months	6 – 12 months	After 12 months	After 12 months
Intermediate vocational education (MBO)		0 – 6 months	6 – 12 months	After 12 months
Vocational education (VMBO)			0 – 6 months	After 6 months
Basic education				0 – 6 months
As of July 2015				
Academic and higher vocational education (WO + HBO)	0 – 6 months	After 6 months	*After 6 months*	*After 6 months*
Intermediate vocational education (MBO)		0 – 6 months	After 6 months	*After 6 months*
Vocational education (VMBO)			0 – 6 months	After 6 months
Basic education				0 – 6 months

Note: months refer to the unemployment period in which UB recipient with a certain educational qualification is obliged to accept suitable job at a specific level of education.

earnings is withdrawn from the claimant's unemployment benefit.[7] To redeploy will always be beneficial when using the income-based calculation of the benefit. As of the 1st of July 2015, the income-based calculation takes immediate effect on the first day of the unemployment spell while previously starting after one year of UB hour-based calculations.

Transition payment

Workers getting fired are entitled to some form of severance payment by the employer. The WWZ replaces the old severance payment's scheme by a transition payment that aims to compensate the worker for losing a job and intends to facilitate the transition to a new job. The transition payment aims to make dismissal fairer and thereby makes dismissal less expensive to employers. Consequently, in most cases, the transition payment will be lower than the former severance payment. Workers who had been employed for at least two years at their previous employer and who had been fired by their employer are entitled to the transition payment. The size of the transition payment depends on the worker's tenure at the previous employer. For every year worked at the previous employer up till 10 years, one-third of a monthly salary is added to the transition payment. Every additional year after 10 years of employment raises the transition payment by one-half of a monthly wage.

Daily wage

The size of the unemployment benefit is calculated as a percentage of the daily wage of the unemployed worker, amounting to 75% for the first two months and to 70% from the third month

[7] Assume that a UB recipient used to earn €2,000 a month and receives an unemployment benefit of €1,400 (70% of his last-earned income). In case the recipient starts working at a wage of €1,200, the income based calculation reduces the benefit to €560, which is 70% of €800 (€2,000 - €1,200). The recipient's total monthly income will be €1,760 (€1,200 + €560), which is €360 more than without working (Schulinck, 2017).

onwards. As of the 1st of July 2015, the calculation of the daily wage has been adjusted. Previously, the daily wage was calculated as the total amount of earnings in the last twelve months before unemployment divided by the total number of days worked during this period. The new daily wage makes use of a slightly adjusted calculation, namely dividing by the number of workable days (261) instead of the number of days worked. Unemployed workers who had been working for fewer days than the total number of workable days in the previous twelve months will receive a lower unemployment benefit when they get unemployed on or after the 1st of July 2015. Workers with temporary contracts might be affected by this adjustment. Those workers may have been unemployed in between two temporary contracts and have therefore not worked all workable days.

Temporary contracts

An increasing part of the Dutch labour force is in the possession of a temporary contract with the accompanying restricted social security. By implementing the WWZ the Dutch government aims to reduce the gap that has emerged between workers with permanent and temporary contracts. Before the reform took effect, employers were allowed to hire workers up to three temporary contracts within three years. When the period in between any two temporary contracts exceeded three months, the counting was restarted. This regulation makes it possible for employers to wait until the 'cooling-down' period has passed and hire the same worker with a temporary contract again. Consequently, some workers may be trapped in temporary contracts. The WWZ tries to address this deficiency by reducing the period in which three consecutive temporary contracts are allowed to maximally two years. In addition, a 'cooling-down' period of six months is now required before any new count can be restarted. When the worker fulfilled three temporary contracts without any intermediate periods exceeding six months, the employer is obliged to offer the worker a permanent contract.

IV. Data

This study uses data from three different datasets made available by the Dutch unemployment insurance agency (UWV). The WW-database contains information on the unemployment spell, such as the registered duration, the maximum duration and reasons why the spell has started and ended, and encompasses some background characteristics of the unemployed worker, such as gender, year of birth and household type. Yet, the registered duration of an unemployment spell might not be the actual duration. Unemployed workers may end their unemployment benefit for various reasons which are in many cases not reported or not (accurately) registered, such as claiming a different benefit, withdrawing from the labour market all together, getting sanctioned or starting to work. Moreover, even if unemployed workers start working according to the WW-database, they may have returned to the labour market earlier than registered, for instance by starting to work without telling the unemployment office or by earning less than 87.5% of their last income. In the latter case, the worker is still defined as unemployed while possibly working full-time, implying that the unemployment insurance agency will be too late in registering actual redeployment.

The POLIS-database solves this deficiency by providing the date of first registration of earnings by the previously unemployed worker. In addition, the database reports the amount of income earned and the number of hours worked after returning to the labour market. This information is provided by the employer to the unemployment insurance agency. Thirdly, the education level of the unemployed worker is retrieved from the SONAR-database. The unemployment insurance agency also constructed an indicator denoting whether an unemployment spell is subject to the WWZ.

The sample covers a period of almost three consecutive years, ranging from January 2014 till October 2016 and consists of 1,034,243 unemployment spells of 846,337 individual unemployed workers. This implies that some workers had multiple unemployment spells (up to 11) over the sample period. In total, 23% of all unemployment spells corresponded to a restarted spell. The sample only considers unemployment spells that started in 2014 or 2015 and follows these until they end or until the sample period ends in October 2016. The number of workers getting unemployed differs substantially by month and by year as can be observed from Figure 1. One can distinguish a slight W-shape in the monthly number of workers turning unemployed over a yearly basis. Furthermore, the number of workers that enter unemployment even fluctuates largely within months. The inflow is high at the beginning of a month, on Mondays and at the end of a month.

Observations that have been excluded from the sample are denied applications, spells that had a maximum duration of zero days, spells with a negative benefit amount, unemployed workers with maximum spell durations exceeding 38 months, unemployed workers for which no daily wage has been reported and unemployment spells that were larger in size than employment in the last job (in terms of hours).[8] The outcome variable of interest is the unemployment duration measured in days according to the beginning date of the unemployment spell and the day of first reported income. Table 2 displays some descriptive statistics of the unemployed worker, the unemployment spell and the previous job.

Figure 2 displays the outflow out of unemployment for a one-monthly, quarterly and half-yearly comparison over a 52-weeks period. It illustrates the outflow for a treated group (July, 3rd quarter or 2nd half year) and non-treated group (June, 2nd quarter or 1st half year) in 2014 and 2015. It follows that the treated groups have a slightly higher outflow out of unemployment and that the difference in outflow between treated and non-treated groups diminishes as the size of the studied sample increases. Unemployed workers' characteristics may differ per month because of some workers getting fired out of temporary (half-yearly) contracts or seasonal work, such as teachers. Moreover, as of accustomed and historical reasons, more workers are dismissed halfway or at the end

[8] Unemployment spells having a negative total unemployment benefit have been excluded since these spells may function as a correction to earlier unemployment benefits or might be incorrectly registered. Spells with a maximum unemployment benefit duration exceeding 38 months (up to more than 19 years) have been removed as well. These 38 months correspond to the existing maximum duration before January 2016, after which it started to gradually decrease. Furthermore, workers for whom no wage has been reported are exempted from the database as this prevents a correct calculation of the unemployment benefit. Lastly, some workers entered unemployment for a larger number of hours than they actually worked. This is not possible and likely stems from incorrect registration.

Figure 1. Number of starting unemployment spells per month

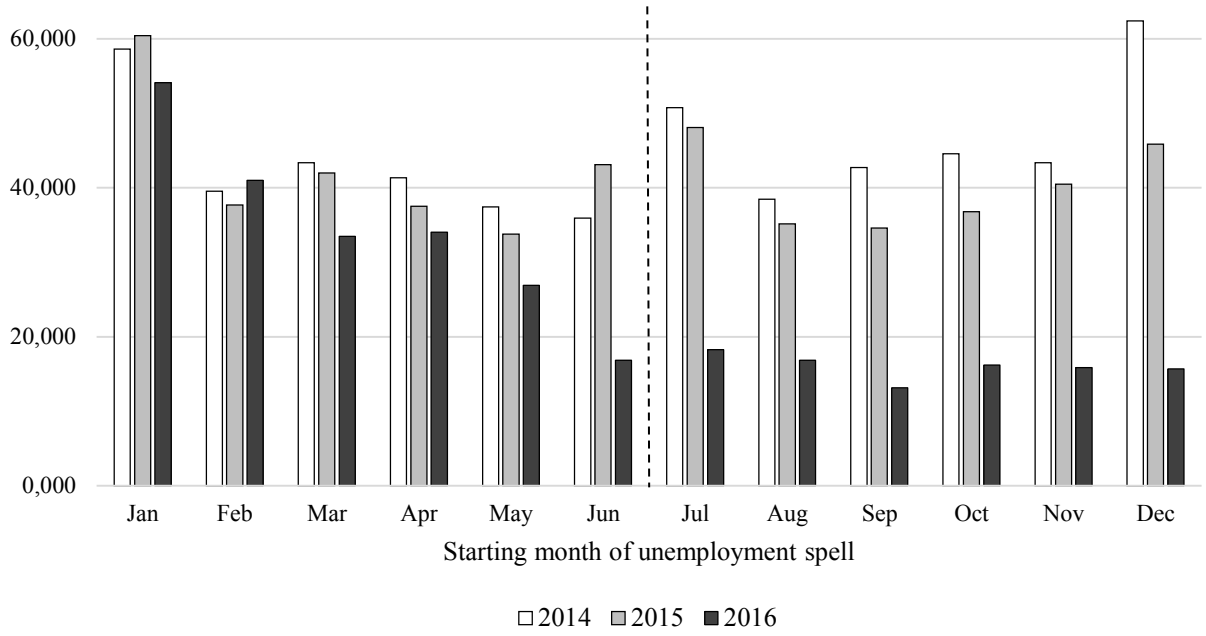


Table 2. Descriptive statistics (January 2014 – December 2015)

	Mean	*Std. dev.*	*Min*	*Max*	*Sample*
Unemployed worker					
Female	0.47	0.50	0	1	1,033,058
Age	39.97	12.17	15	66	1,033,060
Years of education	11.84	2.90	6	16	1.023.480
Married	0.38	0.49	0	1	1,032,200
Unemployment spell					
Size of unemployment (hours)	30.25	10.47	1	80	834,827
Duration of spell (weeks)	17.70	22.08	0	147.29	823,010
Total amount of benefit (in euro)	10,489.61	14,722.09	0	113,590	1,034,243
Previous job					
Weekly number of hours worked	32.26	9.30	1	80	834,827
Monthly salary (in euro, 8 weeks before)	1,880.26	2,918.46	0	1,227,210	1,034,243
Permanent contract (8 weeks before)	0.28	0.45	0	1	1,034,243
Years in last job	3.33	5.40	0	49.99	896,224

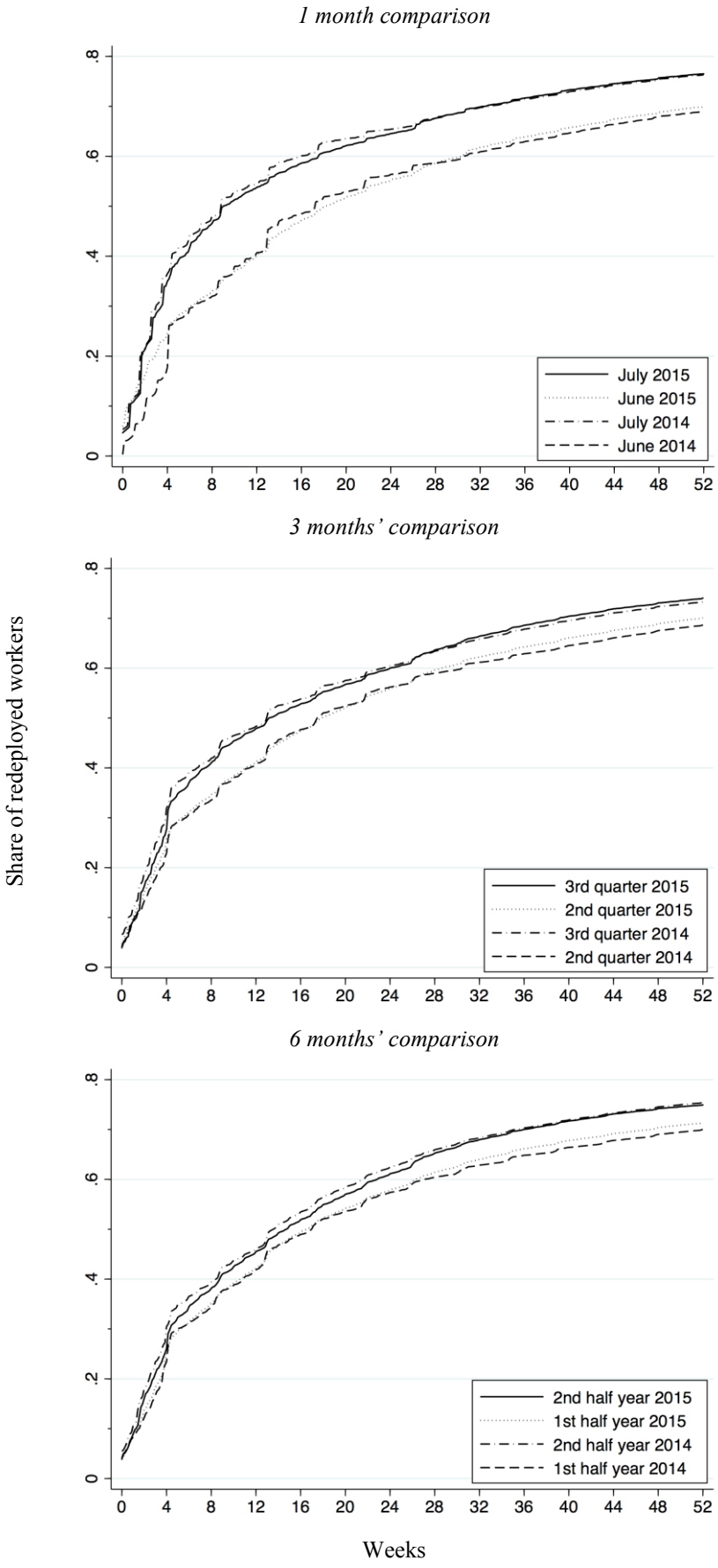
Note: Years of education assume six years of elementary school. The variable 'years in last job' showed some unrealistically high numbers. These observations are excluded in the maximum value of 'years in last job'.

of the year when those workers' contracts end. Workers getting fired out of temporary contracts may have a higher probability of redeployment than workers who previously had a permanent contract. Workers with temporary contracts will be willing to accept temporary jobs immediately after getting unemployed compared to solely searching for jobs with a more permanent nature. Employers, in turn, will be more willing to hire workers with a temporary contract as those workers come along with fewer obligations, such as lower wages, lower contributions to social security and a reduced risk of hiring an unqualified worker.

Overall, the figure shows that about 50% of all unemployed workers found a job after approximately 16 weeks of unemployment. When solely considering the treated groups, this period is reduced to approximately 12 weeks. The figure also reports that on average about 5% of the

unemployed workers have some earnings on the first day of their unemployment spell (and thereby signal to be redeployed). Some workers get partially unemployed and, therefore, report some income and receive an unemployment benefit at the same time. Moreover, some workers may receive some additional income from their previous employer after starting their unemployment benefit, think of end-of-year benefits, holiday allowances or compensation payments for dismissal.

Figure 2. Outflow out of unemployment (2014 and 2015)



V. Identification strategy

The tightening of the Dutch suitable employment regulation on the 1st of July 2015 creates a natural experiment in estimating its effects. This study exploits two strategies to quantify the effects of the adjustment in the suitable employment regulation on the unemployment duration: a regression discontinuity (RD) design and a difference-in-differences (DiD) approach. Irrespective of the identification strategy used, effects are estimated based on a monthly, quarterly and half-yearly comparison, i.e. comparing June and July groups, 2nd and 3rd quarters and the first and second half year where the former and later refer to the control and treatment group respectively. In addition, this research quantifies the effects of the stricter suitable employment program on the job quality in terms of earnings and job stability after redeployment. The difference-in-differences approach is exploited again to estimate these effects.

Separating the suitable employment effect

The adjustment in the Dutch suitable employment program is accompanied by the income-based calculations of the benefit, the transition payment, the new calculation of the daily wage and the tightening of the temporary contracts regulation. These adjustments potentially influence the probability of redeployment and should, therefore, be controlled for.

The income-based calculation of the benefit makes working alongside receiving an unemployment benefit more beneficial and therefore shortens the unemployment duration. Moreover, the income-based calculation may lead unemployed workers to enter lower-paid jobs. As these eventually only receive 30% of their additional earnings, the weight of the salary itself as a determinant in whether to work is lower. The new calculation may also induce workers to enter less stable jobs. Workers may start working on a temporary or part-time basis. To exclude the effects of the UB income-based calculation, separate estimations are carried out for an upper and lower educational part of the sample. Workers with a vocational or basic education degree, who together compose the lower educational part, are not subject to the adjustment in the suitable employment policy. Assuming that the effects of the income-based calculation of benefits are similar for the upper and lower educational part and subtracting the effects of the lower education group from the upper one, enables one to credibly exclude the effects of the UB income-based calculation.

The transition payment aims to enhance the employability of the unemployed worker and is usually less generous to the worker. The transition payment might cause the unemployment duration to decline and might induce workers to intensify their search effort or lower their reservation wages which will make them more likely to enter into lower-paid and less stable jobs. The effects are likely to be small in magnitude. To control for the effect of the transition payment unemployed workers that worked for more than two years at their last employer are excluded. As only those workers are subject to the transition payment, it enables one to disentangle the effect of the transition payment from the suitable employment regulation. Not controlling for both the income-based calculation of the benefit and the transition payment would lead to an upward bias in the results.

The new calculation of the daily wage and the adjustment in the temporary contracts regulation are not controlled for in this study as of a lack of required data. The adjusted daily wage computation has adverse effects on the benefit for workers that worked less than the total workable days in the year prior to their unemployment. These workers might increase search effort or reduce their reservation wage, which may lead to an upward bias in the effect of the stricter suitable employment regulation. The number of workers that started their last job within one year before their current unemployment spell started, had been unemployed before they started their last job and turned unemployed on or after the 1st of July 2015 amounts to 13,006 workers, which corresponds to 5.5%. Yet, one cannot state with certainty that those workers have been subject to the new daily wage calculation and, moreover, such information is not available for workers getting unemployed before the 1st of January 2015. Therefore, this study does not control for the effect of the new daily wage. Overall, the effects are likely to be small because the number of workers subject to the new daily wage is likely to be low and the reduction in the benefit is likely to be small as most workers probably had a (temporary) contract for six months or more. Moreover, as those workers are probably used to having temporary contracts, they likely respond less strongly to reductions in their unemployment benefit.

In addition, the tightening of the temporary contracts regulation will probably have no effect or a slight upward effect on the unemployment duration. The redeployment probability of workers close to the maximum number of temporary contract might be reduced. These unemployed workers may have to find a temporary contract at another employer, which may take longer to find. Yet, other unemployed workers willing to work on a temporary basis might fill this gap. Therefore, no or slightly positive effects will occur on average on the unemployment duration. The temporary contracts regulation will probably not affect the quality of the job.

Regression discontinuity design

The regression discontinuity design uses the assignment to the treatment to construct a treatment and control group. This assignment is determined by whether the assignment variable (date of dismissal) exceeds the cut-off (the day of implementation), as first explained by Thistlethwaite and Campbell (1960). Workers getting unemployed before the cut-off (the 1st of July 2015) are not subject to the policy and should be a good comparison to workers turning unemployed after it (Lee and Lemieux, 2010). The only difference between both groups should be their assignment to the policy. Principally, all workers turning unemployed after or on the 1st of July are contingent on the stricter regulation. However, workers that got entitled to a UB before the 1st of July, worked for less than 26 weeks and felt back into unemployment after or on the 1st of July do not have to adhere to the new regime. These workers' old UB entitlements and their corresponding rules are simply restarted. Therefore, the 1st of July does not serve as a perfect cut-off of WWZ-treatment, implying that a fuzzy RD should be applied. The probability before and after the implementation of the WWZ can be written as

$$\lim_{days \downarrow 0} Pr(WWZ = 1 \mid dd = C_{July\ 1st} + days) = 0$$
$$\lim_{days \uparrow 0} \Pr(WWZ = 1 \mid dd = C_{July\ 1st} + days) > 0$$

where WWZ corresponds to a dummy equalizing 1 when an unemployed worker is subject to the WWZ, dd refers to the dismissal date of the worker, $C_{July\ 1st}$ describes the cut-off, and $days$ displays the number of days deviating from the 1st of July 2015. Figure 3 displays the probability of WWZ eligibility by month in 2015. On average, 86.4% of workers turning unemployed after or on the 1st of July is subject to the WWZ.

A fuzzy RD design works analogously to an instrumental variable (IV) estimation as both approaches deal with imperfect compliance.[9] The discontinuity at the cut-off is only caused by unemployed workers that are subject to the WWZ. Put differently, the fuzzy RD measures the local average treatment effect (LATE) for the group of compliers (e.g. Lee and Lemieux, 2010; Jacob, Zhu, Somers and Bloom, 2012).[10] To estimate an unbiased local average treatment effect with a fuzzy RD approach, a two-stage least squares (2SLS) estimation should be applied (Hahn, Todd and Van der Klaauw, 2001).[11] The 2SLS estimation can be written as

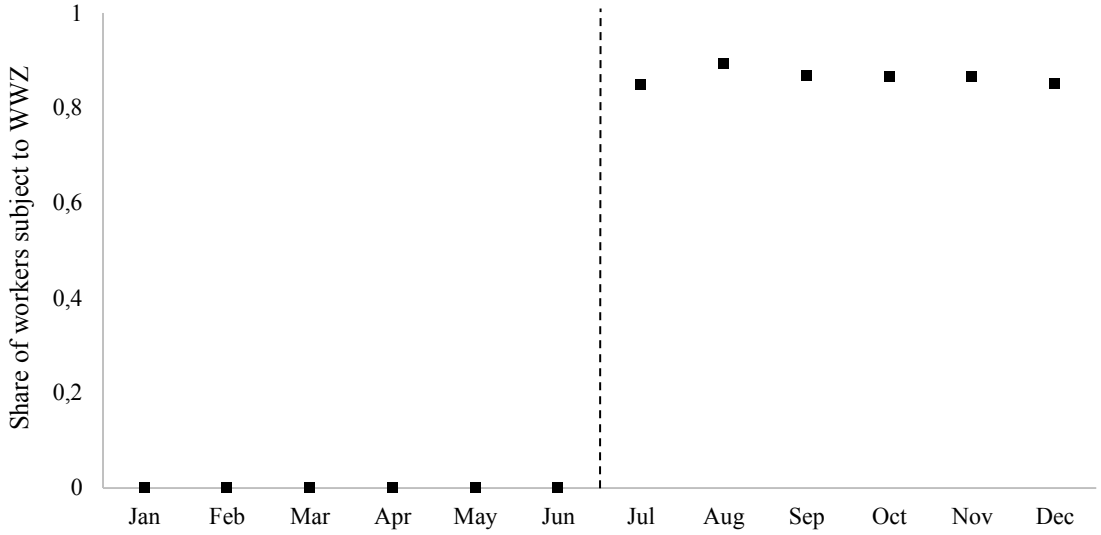
$$WWZ_i = a + bZ_i + cf(Day - C) + d(f * Z_i) + gX_i + e_i \quad (1) \quad \textit{first stage}$$
$$UD_i = \alpha + \beta\widehat{WWZ}_i + \gamma f(Day - C) + \delta(f * \widehat{WWZ}_i) + \zeta X_i + \varepsilon_i \quad (2) \quad \textit{second stage}$$

[9] Workers getting unemployed on or after the 1st of July but do not have to adhere to the stricter suitable employment regulation are defined as never-takers. Always takers are those that got unemployed before the 1st of July 2015 and are subject to the new policy regulation. This group does not exist. The RD design faces one-sided noncompliance. Jacob, Zhu, Somers and Bloom (2012) define this type of RD as Type I fuzzy designs where there are no-shows but no crossovers.

[10] By simply drawing on the assignment variable – the date of dismissal – one is estimating the intention to treat effect instead of the local average treatment effect. Since some of these unemployed workers are not subject to the WWZ and therefore are not treated, a downward bias will result.

[11] Lee and Lemieux (2010) state that the fuzzy RD design should meet the four criteria of the IV estimation to return valid estimates. First, the treatment should be randomly assigned. This refers to the independence assumption and is elaborated on in the following part of this section. Secondly, the exclusion assumption requires that the assignment variable does not influence the unemployment duration via other ways than through treatment. It seems implausible that whether an unemployment spell started after or on the 1st of July has an effect, via an (un)observable variable, on the unemployment duration. In line with the need of having a strong first stage in an IV approach, one needs to verify the existence of a discontinuity between the date of dismissal and the eligibility to the WWZ. This discontinuity can be observed in Figure 3. At last, the monotonicity assumption states that it should be impossible for unemployed workers that are subject to the WWZ to switch to WWZ non-eligibility. Switching is impossible, implying that this assumption is satisfied as well.

Figure 3. Fuzzy RD design with imperfect compliance (2015)



where WWZ refers to a dummy with a value of 1 in case an unemployed worker has to adhere to the WWZ, a (α) corresponds to a constant, Z_i functions as an instrument and refers to a dummy that is equal to 1 after the 1st of July 2015, $f(Day - C)$ relates to a function of the assignment variable, $(f * Z_i)$ $((f * \widehat{WWZ}_i))$ is an interaction between the assignment variable and the dummy allowing for different trends before and after the cut-off, X_i denotes a vector of background characteristics, e_{im} (ε_{im}) is the error term and UD_i refers to the number of days a worker was unemployed. The β parameter is of most importance to this analysis as it displays the effect of the WWZ on the unemployment duration.

To credibly use an RD approach, one should ensure that the policy assignment has been done randomly. This implies that the treatment and control group should not be able to influence their assignment to the policy and should be alike (Lee and Lemieux, 2010). First, workers are unlikely to be able to manipulate their assignment to the WWZ. The starting date of the unemployment spell simply determines which workers are subject to the policy and, thereby, does not rely on any characteristics of the unemployed worker. To manipulate their assignment to the WWZ, unemployed workers would have to change their date of dismissal. Yet, employers usually expel workers without taking the considerations of the employee into account, implying that workers are unable to affect their dismissal date. This makes valid randomisation plausible. One can also test this assumption by studying the density of unemployment spells around the cut-off. Figure 1 in Section IV displayed the density by month.[12] If workers would have been able to advance their dismissal, the number of starting unemployment spells should be higher in June. Comparing the number of unemployment spells in June and July 2015 to those in 2014 might indicate some control over the assignment variable. The June 2015 group may be inflated. However, an analogous comparison to 2016 contradicts this statement since the June and July groups relate similarly as those in 2015 did. The large fluctuations in the monthly unemployment spells depicted in the figure refrain one from making an unambiguous assessment of the level of control over the assignment variable. Yet, intuitively it seems highly unlikely that workers have precise control over their date of dismissal.

Secondly, workers turning unemployed before and after the policy change should have similar characteristics. The validity of the RD design might be questioned as workers getting dismissed in the first half year might differ from those in the second half year, e.g. workers with seasonal work or with

[12] A McCrary test checks whether workers can manipulate their assignment to the policy. It checks whether there exists a discontinuity around the cut-off. A discontinuity may signal sorting on the favourable side of the cut-off. However, a jump occurs in the number of unemployment spells starting in June compared to July, irrespective of the year observed. Therefore, the McCrary test will show an inaccurate discontinuity that naturally occurs. Larger daily and weekly fluctuations will invalidate the McCrary test even further. Moreover, the McCrary test does not allow for varying bin sizes, such as months lasting for 28, 30 or 31 days (Lee and Lemieux, 2010). Therefore, the McCrary test was not conducted.

temporary contracts. This validity can be tested by analysing whether the characteristics of unemployed workers evolve smoothly over time. The similarity of these characteristics depends to some extent on the size of the control and treatment group. Decreasing the bandwidth, which infers a smaller sample group, reduces any bias in the estimates but at the same time increases the variance as the number of observations is being reduced.

This study considers a ± 1 month, ± 3 months' and ± 6 months' bandwidth, as displayed in Table 3.[13] The table shows that all reported characteristics are significantly different for all bandwidths. When comparing the means of the treated groups to the means of the control groups, one may conclude that the former group has slightly favourable characteristics. Overall, these workers were more often male, were younger, had a higher level of education, worked for more hours and had a higher salary. Table 3 also shows that groups subject to the WWZ were more likely to have a temporary contract and worked for a shorter period at their last employer. These differences imply that the treated groups already have a higher probability of redeployment without taking the policy into account. This might bias the results upwards. This prediction is confirmed by Figure 2 in Section IV, which shows a higher outflow out of unemployment for workers in the second half year. Even when including polynomials or when checking the statistical difference between the means for males and females separately, the majority of the characteristics remain significantly different, as can be observed from Table A1, Table A2 and Table A3 in the Appendix. Although differences between means are statistically different, those differences seem to be small in size. The substantial sample size causes small differences to be easily statistically different. A similar analysis for 2014 shows an analogous result to 2015, i.e. almost all background characteristics are significantly different and those getting unemployed in the second half of the year have slightly favourable characteristics.[14] This suggests that any differences unlikely originate from the WWZ.

Although including a polynomial when estimating the statistical difference only marginally improved the comparison, it may partially take account of some differences and reduce the bias. Nevertheless, the RD design seems an inferior approach in estimating the effects of the tightening in the suitable employment program. The DiD approach will bring a more robust and unbiased estimate of the causal effect of the adjusted program.

[13] Calculating the optimal bandwidth shows that a bandwidth of about 45 days should be preferred.

[14] Only 'married', 'weekly number of hours worked' and 'education' are insignificantly different at the 5% level for the ± 1 month, ± 3 months' and ± 6 months' bandwidth respectively.

Table 3. Comparison of background characteristics for multiple bandwidths

	± 1 month			*± 3 months*			*± 6 months*		
	June (1)	July (2)	p-value (3)	2nd quarter (4)	3rd quarter (5)	p-value (6)	1st half year (7)	2nd half year (8)	p-value (9)
Female	0.51	0.47	0.00	0.51	0.49	0.00	0.51	0.46	0.00
Age	40.89	40.26	0.00	40.94	40.11	0.00	40.59	40.08	0.00
Education (in years)	11.81	12.02	0.00	11.93	12.05	0.00	11.98	11.83	0.00
Married	0.38	0.40	0.00	0.39	0.38	0.00	0.38	0.38	0.00
Weekly number of hours worked	31.67	34.01	0.00	31.77	33.53	0.00	31.74	34.42	0.00
Monthly salary (in euro, 8 weeks before)	1,968.84	2,645.23	0.00	1,876.25	2,171.04	0.00	1,886.08	1,972.75	0.00
Permanent contract (8 weeks before)	0.29	0.26	0.00	0.30	0.28	0.00	0.31	0.26	0.00
Years in last job	3.73	3.32	0.00	3.75	3.49	0.00	3.68	3.30	0.00
Observations	43,045	48,015		114,290	117,596		254,371	239,491	

Note: p-values correspond to statistical difference between means reported in columns to the left of the p-value. Number of observations refers to observations reported for 'female'. The number of observations for 'weekly number of hours worked' is much lower for the treatment groups, ranging from 8,117 for a one-monthly comparison to 43,079 for a half-yearly comparison.

Difference-in-differences approach

A second approach in estimating the effects of the tightening in the suitable employment regulation is a difference-in-differences approach (DiD). This approach calculates the difference between the outflow from unemployment of the treatment group before and after the policy implementation and the difference between the outflow of the control group before and after the WWZ entered into force. The difference of these two differences results in the causal effect of the policy. This approach enables one to circumvent the assumption that the treatment and control group should be similar on all background characteristics.

Only a small fraction of workers entering unemployment after the 1st of July 2015 is not subject to the new regulation. This implies that data on the control group after the policy implementation is limited and that this control group likely differs from any earlier control group. The closest available group of unemployed workers subject to the old regulation consists of those workers getting unemployed in June 2015. These workers' outflows from unemployment are related to the outflows of workers entering unemployment in June 2014. The same strategy applies to the treatment group, which consists of July 2014 before the policy adjustment and of July 2015 after the adjustment. When denoting UD^x as the average unemployment duration of unemployed workers in group x, UD_i as the number of days a worker was unemployed, $July_i$ as a dummy with a value of 1 (0) for July (June) groups, Y_i^{2015} as a dummy equalizing 1 for the year of the policy adjustment, X_i as a vector of background characteristics and ε_i as the individual error term, the DiD estimator can be displayed in two ways:

$$(UD^{July\ 2015} - UD^{June\ 2015}) - (UD^{July\ 2014} - UD^{June\ 2014}) \quad (3)$$

$$UD_i = \alpha + \beta July_i + \gamma Y_i^{2015} + \delta(July_i * Y_i^{2015}) + \vartheta X_i + \varepsilon_i \quad (4)$$

The DiD estimator corresponds to the result of equation (3) and is depicted by the δ parameter in equation (4). This study also estimates the effects for larger treatment and control groups, consisting of the second and third quarter and the first and second half year respectively.

To exclude the effects of the income-based calculation of the benefit, one must take the difference of the DiD estimator for the upper and lower educational part of the sample, i.e. a triple DiD estimator. The effects of the suitable employment regulation can then be displayed by

$$UD_i = \alpha + \beta July_i + \gamma Y_i^{2015} + \delta Educ_i + \zeta\left(July_i * Y_i^{2015}\right) + \eta(July_i * Educ_i) + \theta\left(Y_i^{2015} * Educ_i\right) + \lambda\left(July_i * Y_i^{2015} * Educ_i\right) + \mu X_i + \varepsilon_i \quad (5)$$

where $Educ_i$ displays a dummy with a value of 1 for the upper educational part of the sample, interaction effects between $July_i$, Y_i^{2015} and $Educ_i$ are included and the remaining variables are defined as in equation (4). The effect of the tightening of the suitable employment regulation itself is given by the μ parameter, which describes the effect of the interaction between the treatment group, the treatment period and the level of education, as displayed by $(July_i * Y_i^{2015} * Educ_i)$. The same approach is used for a subsample of the dataset in order to exclude the transition payment. The effect of the stricter suitable employment program can also be displayed by an adjustment of equation (3) where $high$ (low) refers to the upper (lower) levels of education:

$$\left[\left(UD^{July\ 2015}{}_{high} - UD^{June\ 2015}{}_{high}\right) - \left(UD^{July\ 2014}{}_{high} - UD^{June\ 2014}{}_{high}\right)\right] - \left[\left(UD^{July\ 2015}{}_{low} - UD^{June\ 2015}{}_{low}\right) - \left(UD^{July\ 2014}{}_{low} - UD^{June\ 2014}{}_{low}\right)\right] \quad (6)$$

The DiD approach assumes the existence of a common trend among both groups, i.e. the outflow from unemployment for the treatment and control group would have developed similarly in case there would have been no policy change. This assumption ensures that any difference between both groups results from the policy itself. It can be tested by comparing the outflow out of unemployment for workers that turned unemployed in 2014. From Figure 2 in Section IV it follows that the unemployed workers in treatment groups in 2014, those workers turning unemployed in the second half of the year, have a slightly higher outflow out of unemployment, as in accordance with the favourable background characteristics depicted in Table 3. These workers perform especially better in

the first few weeks of their unemployment spell. After these first weeks, both treatment and control groups develop similarly. Put differently, the difference in outflow is largely caused by differences in the outflow at the beginning of the unemployment spell and exit rates do not diverge as time passes by. Overall, as treatment and control groups evolve similarly over time, the common trend assumption is likely to hold.

Job quality effects

The DiD approach is used to identify the effects of the stricter suitable employment regulation on the quality of the new job in terms of monthly earnings and employment stability. The tightening shortens the period in which an unemployed worker is allowed to search for equally qualifying jobs. This may induce unemployed workers to accept less rewarding and less stable jobs. The identification strategy remains the same for both the income and employment stability effects. The income effects are quantified by estimating the effects of the policy adjustment on the average monthly earnings over a range of 60 weeks after the beginning of the unemployment spell. Average incomes are retrieved by averaging 4-weekly non-zero income registries reported by the employer.

The employment stability effects are estimated by analysing the relapse rate into unemployment. This relapse rate is defined as the share of workers that got redeployed in the first six months of their unemployment spell and did not have an income in the 28th week of their unemployment spell.[15] These estimations assess the relapse rate into unemployment and do not consider whether workers got redeployed in jobs with a lower number of hours. When estimating the employment stability effects of the suitable employment policy, a slightly adjusted outcome variable is used. The relapse rate into unemployment is compared between unemployed workers who had been unemployed for at least six months, found a job within the preceding six months and did not report an income in the 56th week of their unemployment spell. This adjusted definition is used because the tightening in the suitable employment policy may only have effect after six months of unemployment. Reporting a lower income and falling back into unemployment may signal that the new job was not a good match and that the worker may have been pushed by the new regulation to accept a less suitable job.

[15] Data on the starting date of new unemployment spells (WW-database) was limited to 2014 and 2015. This implies that for workers getting unemployed in the second half of 2015 no data was available to assess whether they relapsed into unemployment in the first six months. However, the POLIS-database contains income registries up to and including October 2016. These registries are available for every 4 weeks up to a maximum of 60 weeks. If no income is reported in the 28th week, it is regarded as unemployment.

VI. Results

Unemployment duration effects (RD)

The regression discontinuity design estimates the effects of the tighter suitable employment regulation by comparing the spell duration of workers getting unemployed before and after the 1st of July 2015. This approach controls for day-specific effects by including polynomials of the date of dismissal into the regression. These polynomials can be of different orders, allowing for different functional forms. The order of polynomials with the best fit to the data is preferred. Figure 4 illustrates the functional form by displaying the unemployment duration for each date of dismissal. Alongside, the figure displays the fit of a first order polynomial and allows for different linear specifications on either side of the cut-off.[16] It appears that the unemployment duration differs substantially by starting day, which might cause the linear specification to be a suboptimal fit for the one-monthly specification. However, the first-order polynomial seems to describe the unemployment durations for the half-yearly bandwidth well.

The Akaike information criterion (AIC) official tests which order of polynomials best describes the variance in the data, as described by Jacob, Zhu and Somers (2012). By comparing different polynomial orders the AIC examines the trade-off between bias and variance in the model. Nevertheless, the AIC is not able to conclude whether any functional form fits the data better than no functional form at all. The test suggests using higher order polynomials exceeding the second order polynomial. Yet, Gelman and Imbens (2014) state that one should refrain from using these higher order polynomials in an RD design. Polynomials of orders above the quadratic form lead to inconsistent weights for certain observations creating noisy estimates, to results that are sensitive to the order of polynomials used and to poor confidence intervals (Gelman and Imbens, 2014). Therefore, this study's RD design only exploits linear and quadratic functional forms.

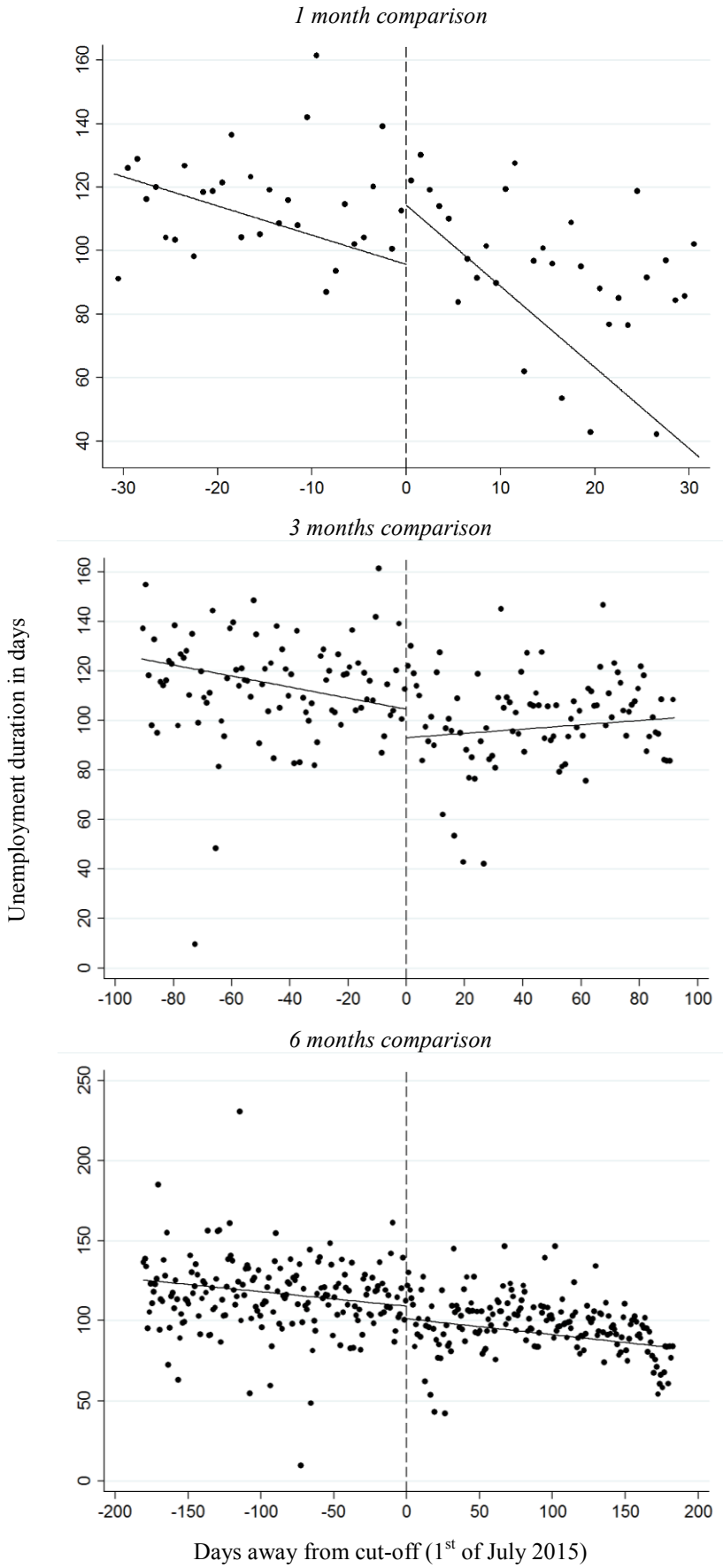
The first stage, where the date of dismissal functions as an instrument for WWZ eligibility, returns highly significant estimates ranging from 0.848 to 0.915 depending on whether and which order of polynomials is used, as can be observed from Table A4 in the Appendix. Overall, the polynomials affect the coefficient of the instrument only to a slight extent. Moreover, these estimates are in accordance with the WWZ eligibility of workers that turned unemployed after the 1st of July 2015, which is 86.4%. Table 4 displays the effects of the WWZ on the unemployment duration for the three different bandwidths and for specifications with and without polynomials. These findings correspond to the second stage of the IV approach. The second stage returns highly significant but ambiguous results that strongly depend on the studied bandwidth and the order of polynomials included.

The specifications excluding polynomials suggest that the WWZ substantially decreases the unemployment duration. However, incorporating polynomials greatly influences the results. Estimates range from highly negative effects of 32.1 days to equally sized positive effects of 30.7 days. Since a large amount of variation exists in the daily number of workers getting fired and since workers may be different per day of dismissal, including polynomials of the assignment variable and interaction terms is preferred. Still, results vary widely between the three bandwidths studied. Moreover, the effects for the one-monthly and quarterly specifications differ only marginally between a linear and quadratic specification. This implies that although the linear fit seems suboptimal, estimates do not change extensively when including a possibly better fit polynomial.

One may expect that a one-monthly bandwidth is preferred over the quarterly and half-yearly bandwidth. Treatment and control groups are closer to each other and should, therefore, show more similarity. Yet, as can be observed from Table 3 in Section V and from Table A1, A2 and A3 in the Appendix, the background characteristics of unemployed workers differ to a similar degree for all three bandwidths. Another possible, but valid, reason to prefer the one-monthly comparison is that the quarterly and half-yearly comparison overestimate the effects of the WWZ. Those groups are more exposed to business cycle effects which alter the employment probability of the unemployed worker. In 2015 the Dutch economy was still reviving from the downturns of 2007 and 2009, implying that the

[16] The first order polynomial fits the data into a linear relationship while an interaction term between the WWZ and the polynomial allows for different coefficients on both sides of the cut-off.

Figure 4. Functional form around cut-off



Note: The bin size is equal to one day. Lines correspond to first order polynomials.

Table 4. Effects of WWZ on unemployment duration

Dependent variable: unemployment duration in days						
	± 1 month					
	(1)	(2)	(3)	(4)	(5)	(6)
WWZ	-31.750***	-32.100***	22.829***	30.683***	23.954***	27.457***
	(1.041)	(1.095)	(1.700)	(1.639)	(2.133)	(1.668)
WWZ * (days from cut-off)			-2.608***		-8.558***	
			(0.115)		(0.472)	
WWZ * (days from cut-off)^2					0.129***	
					(0.013)	
Controls	No	Yes	No	No	No	No
Polynomials	No	No	1st	1st	2nd	2nd
Observations	70,411	63,220	70,411	70,411	70,411	70,411
	± 3 months					
	(7)	(8)	(9)	(10)	(11)	(12)
WWZ	-23.599***	-22.147***	-11.542***	-13.125***	-3.649**	-12.545***
	(0.646)	(0.682)	(1.186)	(1.164)	(1.699)	(1.170)
WWZ * (days from cut-off)			0.278***		-0.584***	
			(0.024)		(0.082)	
WWZ * (days from cut-off)^2					0.010***	
					(0.001)	
Controls	No	Yes	No	No	No	No
Polynomials	No	No	1st	1st	2nd	2nd
Observations	176,638	157,433	176,638	176,638	176,638	176,638
	± 6 months					
	(13)	(14)	(15)	(16)	(17)	(18)
WWZ	-30.179***	-26.019***	-8.931***	-8.874***	-19.923***	-8.940***
	(0.425)	(0.441)	(0.855)	(0.843)	(1.282)	(0.845)
WWZ * (days from cut-off)			-0.009		0.564***	
			(0.008)		(0.003)	
WWZ * (days from cut-off)^2					-0.003***	
					(0.000)	
Controls	No	Yes	No	No	No	No
Polynomials	No	No	1st	1st	2nd	2nd
Observations	381,008	341,672	381,008	381,008	381,008	381,008

Note: Heteroscedasticity robust standard errors are presented in parentheses. Control variables included are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations.
*** significant at the 1% level
** significant at the 5% level

unemployment duration likely reduces as time passes by. This leads to an upward bias in the effects of the WWZ that is especially apparent in the quarterly and half-yearly comparison.

Table 5 displays the effects of the suitable employment regulation on the unemployment duration.[17] These effects appear to differ by specification used, though to a lesser degree than the

[17] Table A5 in the Appendix extends Table 5 by first displaying the effects of the suitable employment policy along with the effects of the transition payment. It continues by excluding the effects of the transition payment, such that the effectiveness of the stricter suitable employment policy can be assessed.

effects of the WWZ. The specifications using a first or second order polynomial suggest that the stricter suitable employment regulation significantly increased the unemployment spell duration, with effects ranging from 10.1 days to 24.0 days. The varying magnitudes of the results may be explained by the dissimilarity between workers getting fired at the end of June and those at the beginning of July. This possibly leads to a bias in the results. For instance, if workers turning unemployed on the 1st of July are especially those that had a temporary contract, then their unemployment duration will be artificially lower. A 'donut-hole' RD may provide a solution to this problem. It estimates the effects of the new suitable employment policy by excluding observations close to the cut-off. Workers getting unemployed two days before, on or one day after the 1st of July are not considered in this 'donut-hole' RD. Table 6 displays the effects of the suitable employment policy for the adjusted RD design. The 'donut-hole' RD does not solve the issue of nonconformity of the estimates but may have even raised the disparity. Results range from reductions in the unemployment duration of 37.4 days to increases of 19.7 days. The ambiguity in the results refrains one from making a definite conclusion about the effectiveness of the WWZ and the stricter suitable employment policy.

These inconclusive results may be explained by the nature of the workers getting unemployed. Especially the first day of a month, the last day of a month and Mondays show high inflows into unemployment. Figure 5 displays the size of the inflow in unemployment for 14 days before and after the 1st of July. It displays large fluctuations in the number of unemployed workers per day, with values ranging from 18 workers to 17,679 workers per day. These fluctuations create within-group and between-group variations that may lead to biases. The within-group variation implies that workers getting fired in the beginning or at the end of a month may differ from those turning unemployed during the month. For instance, workers in the former group may have had more permanent contracts, while workers in the latter group may have been fired out of temporary contracts or had other (personal) reasons to leave employment, such as being fired on the spot or resigning. In addition, the between-group variation refers to differences in characteristics between treatment and control group. Workers getting unemployed during the treatment period are different from those turning unemployed in the control period. Both effects infer that the background characteristics of the unemployed workers may not evolve smoothly over time. Workers' possibilities of returning to the labour market will then be different beforehand and will, therefore, be soaked up by the WWZ estimator in the RD design. The RD design's estimates would then turn biased and inconsistent.

Polynomials of the appropriate order may take account of some of these differences. Yet, the large variety that exists in unemployment duration per day cannot easily be captured by any functional form. The fit of the linear and quadratic functional form to the data is likely to be low, implying that the trade-off between bias and variance is imbalanced towards the bias. Higher order polynomials may do a better job in fitting the data. Nevertheless, the question arises which order of polynomials describes the data best and keeps the interpretation of the WWZ estimates intuitive. Moreover, any of those higher order specifications will be subject to the flaws outlined by Gelman and Imbens (2014).

To estimate the effectiveness of the stricter suitable employment policy by means of an RD design may come along with two biases. The dissimilarity within and between treatment and control groups together with the bias that emerges because of not controlling for business cycle effects may invalidate the results. It leads one to focus on the difference-in-differences approach. This approach does not build upon the assumption that treatment and control groups should be similar and cancels out business cycle effects by controlling for the development in the control groups over time.

Unemployment duration effects (DiD)

Figure 6 displays the difference-in-differences estimator of the effect of the WWZ on the outflow out of unemployment. This estimator is calculated by subtracting the difference in the outflow of the control groups from the difference in the outflow of the treatment groups, as equation (2) displayed. The graphs in the figure suggest that the effect of the WWZ on the outflow out of unemployment is a negative one, implying that the WWZ prolongs the unemployment duration. According to the figure, the effect of the WWZ roughly corresponds to an increase in the exit rate of about 1%-point on average.[18]

[18] The graphs show a sharp downward spike of up to approximately 9.5%-points in the beginning of the unemployment spell. These large differences in redeployment seem to disappear after workers have been unemployed for 4 weeks. Moreover, the

Table 5. Effects of WWZ and suitable employment policy on unemployment duration

Dependent variable: unemployment duration in days

	± 1 month					
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
WWZ (excluding transition pay)	-32.624*** (1.570)	-47.376*** (2.571)	24.704*** (2.654)	12.338*** (4.400)	24.761*** (3.397)	24.751*** (5.398)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	29,351	10,922	29,351	10,922	29,351	10,922
Suitable employment		14.752		12.366		0.010
Statistically different: Prob. > χ^2		0.000		0.016		0.999

	± 3 months					
	High (7)	Low (8)	High (9)	Low (10)	High (11)	Low (12)
WWZ (excluding transition pay)	-21.614*** (0.962)	-26.001*** (1.610)	-11.735*** (1.805)	-35.476*** (3.076)	-5.232** (2.659)	-27.759*** (4.282)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	72,972	25,850	72,972	25,850	72,972	25,850
Suitable employment		4.387		23.741		22.527
Statistically different: Prob. > χ^2		0.019		0.000		0.000

	± 6 months					
	High (13)	Low (14)	High (15)	Low (16)	High (17)	Low (18)
WWZ (excluding transition pay)	-25.214*** (0.642)	-24.196*** (1.034)	-7.553*** (1.289)	-17.621*** (2.145)	-23.799*** (1.945)	-47.785*** (3.321)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	157,361	58,904	157,361	58,904	157,361	58,904
Suitable employment		-1.018		10.068		23.986
Statistically different: Prob. > χ^2		0.403		0.000		0.000

Note: Heteroscedasticity robust standard errors are presented in parentheses. 'High' corresponds to upper two education levels (academic and higher vocational education together with intermediate vocational education). 'Low' refers to two lower education levels (vocational and basic education). Prob. > χ^2 indicates the statistical difference between the effect of the WWZ on unemployment duration for higher and lower educated workers. None of the specifications include control variables.

*** significant at the 1% level

** significant at the 5% level

spike is approximately halved in size for the quarterly and half-yearly bandwidth. This suggests that especially workers getting unemployed shortly after the implementation of the WWZ experienced difficulties in finding jobs in the first weeks of their unemployment spell. These differences may be caused by the income requirement in the WWZ. Workers turning unemployed after the 1st of July 2015 should not only get unemployed for a minimum number of hours but should also earn less than 87.5% of their previous income to get entitled to an unemployment benefit. This implies that some workers will not be regarded unemployed during the WWZ while they would have been before the WWZ took effect. As these workers are often only unemployed for a very short period (around 4 weeks), the outflow out of unemployment is higher in the control groups and in the treatment group before treatment. This may explain the sudden drop in the outflow out of unemployment.

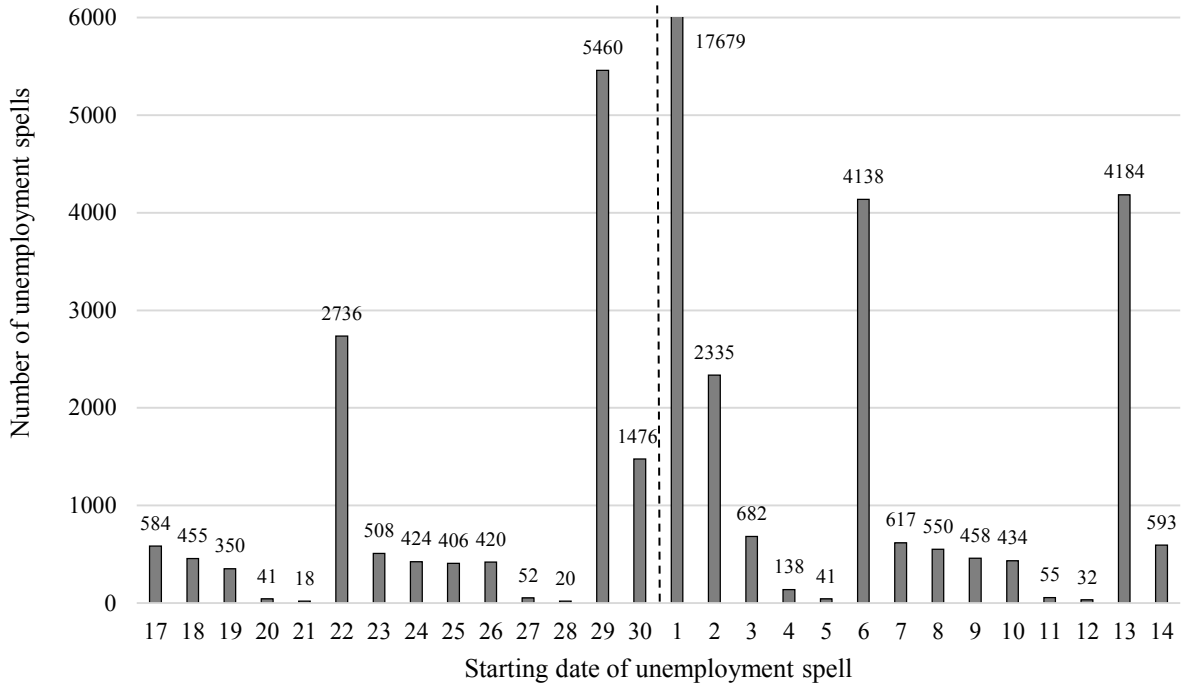
Table 6. Effects of suitable employment policy on unemployment duration ('donut hole' RD)

Dependent variable: unemployment duration in days

	± 1 month			*± 3 months*			*± 6 months*		
	(1)	(2)	(3)	(5)	(6)	(7)	(9)	(10)	(11)
Suitable employment	6.184	7.481	-37.361	1.904	19.691	1.023	-2.552	6.727	17.668
Statistically different: Prob. > χ^2	0.069	0.373	0.006	0.332	0.000	0.902	0.040	0.014	0.000
Polynomials	No	1st	2nd	No	1st	2nd	No	1st	2nd
Observations	30,716	30,716	30,716	89,265	89,265	89,265	206,708	206,708	206,708

Note: 'Donut hole' RD refers to fuzzy RD design where observations two days below and above the cut-off are excluded. Prob. > χ^2 indicates the statistical difference between the effect of the WWZ on unemployment duration for higher and lower educated workers (not displayed). Number of observations corresponds to the total number of observations for the upper and lower education levels together. None of the specifications include control variables. Number of interaction effects are in accordance with the number of polynomials used in the specification.

Figure 5. Inflow in unemployment around cut-off (1st of July 2015)



Note: Numbers above bars correspond to number of starting unemployment spells at specific date. X-axis values to the left (right) of the dotted line refer to dates in June (July). June 22nd, June 29th, July 6th and July 13th are Mondays.

The effects of the WWZ are quantified in Table 7. The results seem to confirm the prolonging effect of the WWZ as resulted from the graphical DiD estimator. Overall, the WWZ appears to increase the unemployment duration between 6.3 days (4.9%) and 12.4 days (10.6%).[19] The preferred specifications are those including control variables, which limits the results to an increase of 6.3 days (4.9%) to 10.8 days (9.2%).[20] The results in the table also show that simply comparing the June and July 2015 groups, as the RD design does, would result in strongly biased estimates. A comparison returns a significantly negative effect of 26.6 days. However, this comparison does not take into account that some differences between the June and July groups would have occurred anyways, as quantified by the June and July 2014 comparison. It turns out that the unemployment duration decreased to a stronger degree in 2014 than it did in 2015, implying that the WWZ prolonged the time spent in unemployment.

Nevertheless, a bias might be present in the DiD estimates of the WWZ. Without the policy adjustment, business cycle effects should be of equal magnitude in the control and treatment groups. A downward (upward) bias will result if the business cycle effects are stronger (smaller) for the control group. Since no policy adjustments took place over the June 2014 to June 2015 period, the business cycle effects reduced the unemployment duration by 41.9 days (26.9%) over a single year. The cyclical effects together with the effect of the WWZ amount to a smaller reduction in the unemployment spell of 29.5 days (25.3%). In the absence of any bias, this leads to the lengthening effect of the WWZ.

[19] Percentages relate the DiD-estimator to the average unemployment duration of workers in the treatment group before treatment took place (July 2014, 3rd quarter 2014 and 2nd half year 2014). The same is true for other effects that are displayed in percentages.

[20] Table A6 in the Appendix measures the effects of the WWZ including first and second order polynomials. The specifications including first order polynomials return estimates similar to those presented in Table 7. Yet, when including second order polynomials, estimates for the one-monthly and quarterly comparison switch sign and turn significantly negative. The half-yearly bandwidth presents slightly positive effects, though smaller in size than without polynomial. Since DiD approaches are typically used without polynomials, this study focusses on those specifications.

Figure 6. Effects of WWZ on outflow out of unemployment (DiD graphically)

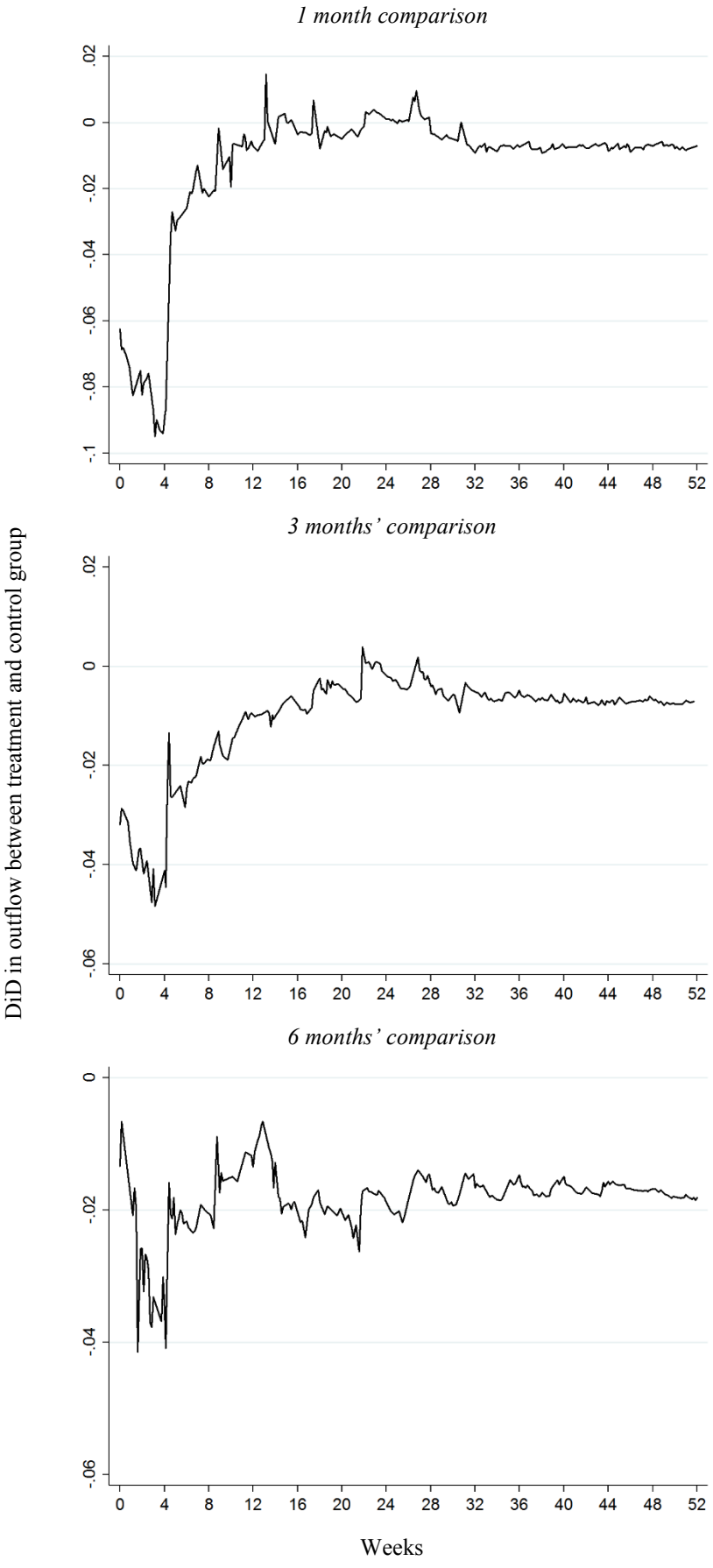


Table 7. Effects of WWZ on unemployment duration

	Dependent variable: unemployment duration in days								
	± 1 month			*± 3 months*			*± 6 months*		
	June	July	Difference	2nd quarter	3rd quarter	Difference	1st half year	2nd half year	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Before WWZ	155.390	116.390	-39.001***	156.805	128.687	-28.118***	157.099	123.612	-33.487***
			(1.378)			(0.829)			(0.545)
After WWZ	113.525	86.918	-26.607***	115.667	95.287	-20.381***	118.485	92.502	-25.983***
			(0.873)			(0.558)			(0.374)
Difference	-41.865	-29.472	12.394***	-41.138	-33.400	7.737***	-38.614	-31.110	7.505***
			(1.631)			(1.000)			(0.661)
Controls			No			No			No
Observations			141,954			376,518			823,010
Difference			10.748***			6.340***			7.229***
			(1.611)			(1.032)			(0.676)
Controls			Yes			Yes			Yes
Observations			126,381			331,814			730,510

Note: Heteroscedasticity robust standard errors are presented in parentheses. Control variables included are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations.

*** significant at the 1% level

Testing the similarity of the cyclical effects for the treatment and control group can be done by analysing the developments in the labour market prospects of unemployed workers. The labour market prospects, measured as the number of open and filled vacancies, should develop similarly for both groups. Figure 8 illustrates the number of vacancies by quarter for 2014 and 2015. At first sight, it seems that the number of vacancies develops at a constant pace over time. However, when quantifying the development in the number of vacancies available for the control and treatment group separately, it follows that the number of open (filled) vacancies was higher by 3,300 (1,500) for the control group in the quarterly comparison. This number entails that the strength of economic recovery was slightly higher for the second quarter, thereby suggesting the presence of a slight downward bias in the estimates. The half-yearly bandwidth returns different results. It suggests that the number of open (filled) vacancies is higher (lower) by 200 (3,400) for the control group in the half-yearly analysis. This corresponds to the absence of any bias or the presence of a slight upward bias in the half-yearly WWZ estimator. Overall, effects are small in size and therefore unlikely to bias the effect of the WWZ on the unemployment duration strongly. The estimated coefficients of the WWZ depicted in Table 7 also confirm this intuition as they do not differ widely between the quarterly and half-yearly bandwidth.

The positive effect of the WWZ on the unemployment duration is counterintuitive. One would expect that the WWZ, which consist of multiple incentive programs that aim to enhance redeployment, reduces the time spent in unemployment. Exactly the opposite effect results. A simple and full explanation of this finding is difficult to provide. Possibly, multiple interpretations that are not mutually exclusive may account for these counterintuitive results.[21] First, before the WWZ took effect workers were officially regarded unemployed if they had some minimum loss in hours worked. However, after the implementation, entitlements to unemployment benefits also depended on the loss in earnings. Unemployed workers still earning more than 87.5% of their previous income were not entitled to a benefit. As of this additional income requirement some workers with small reductions in their number of hours worked and income do not flow into unemployment after the WWZ while they would have been unemployed before the implementation. These workers are likely to be unemployed shortly because of high labour market prospects. Therefore, these workers' absence in unemployment increases the average unemployment duration in the treatment group and thereby partially explains the positive effect of the WWZ.

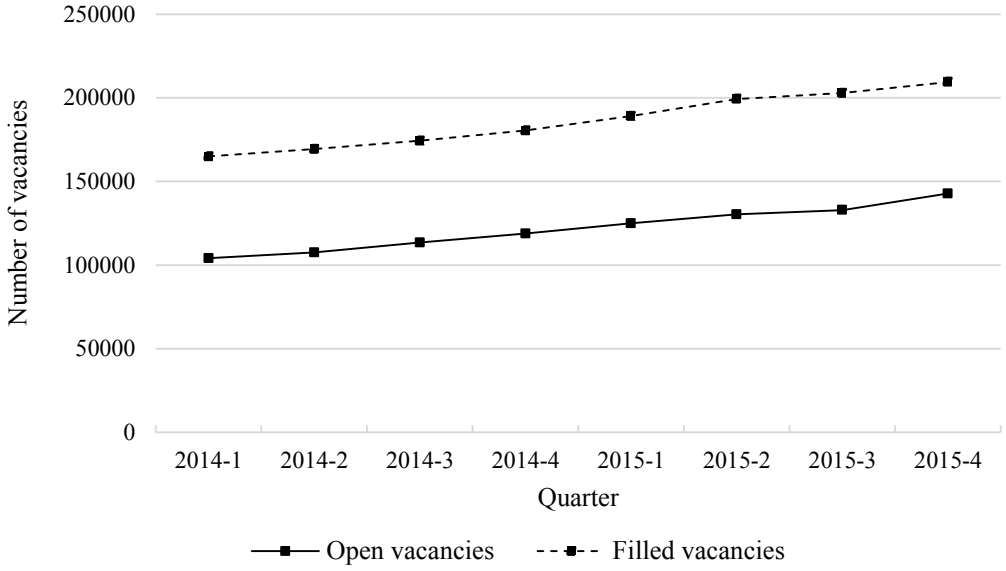
Secondly, the new calculation of the daily wage may also clarify some part of the prolonging effect of the WWZ. As the new daily wage is based on the total number of workable days instead of the number of days worked in the previous years, unemployed workers' incentives to redeploy into a temporary contract are decreased. Unemployed workers accepting a temporary contract are likely to fall back into unemployment after the contract has ended. Yet, as they did not work all workable days in the previous year, their unemployment benefit will be lower. This implies that some unemployed workers will not apply for jobs with temporary contracts and thereby lengthen the unemployment duration. Noteworthy is that the new daily wage calculation has been partially reversed on the 1st of December 2016.[22] Even workers who got unemployed before this date and after the 1st of July 2015 got compensated for this adjusted daily wage calculation. Thirdly, the adjusted temporary contracts regulation may make redeployment more difficult. While workers could previously return to the same employer, they will be less quickly accepted at this employer after the WWZ took effect. Employers will have to offer permanent contracts to these workers at an earlier stage. Finding a temporary job at another employer may be slightly more demanding and may subsequently raise the time spent in unemployment.

Furthermore, a fourth potential interpretation is a technicality of the WWZ. It entails that unemployed workers with an irregular working pattern, like those working during the weekends, got an earlier official starting date of their unemployment spell. The starting date of the unemployment

[21] The author thanks five employees at the strategy, policy and research department (SBK) of the Dutch unemployment insurance agency (UWV) for their contributions to the interpretation of the effect of the WWZ.

[22] As of the 1st of July 2015 the daily wage was calculated as the total earnings in the last twelve months before unemployment divided by the total number of workable days in this period (261). After the 1st of December 2016, the total earnings are divided by the total number of workable days in the months in which the unemployed worker was employed.

Figure 8. Number of open and filled vacancies (2014 and 2015)

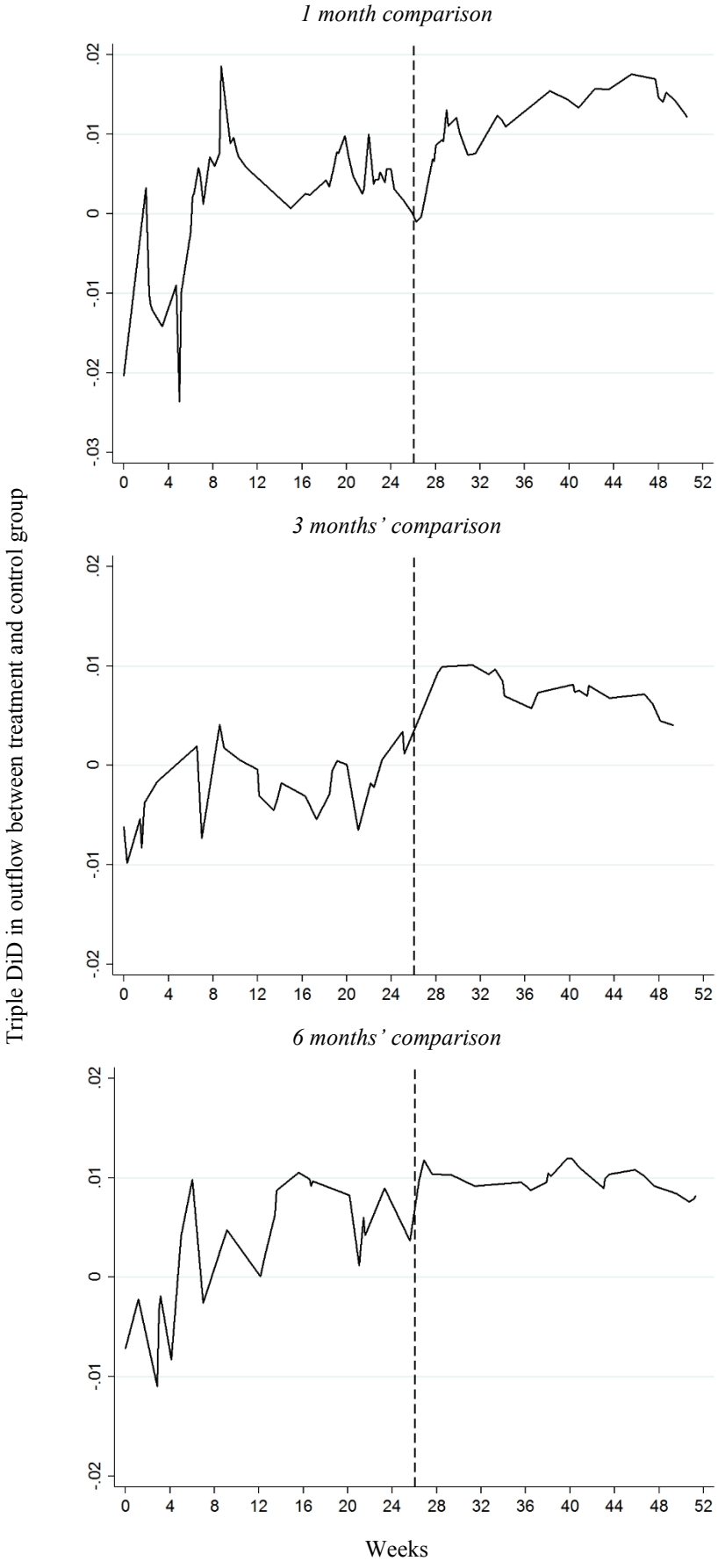


Note: Numbers are seasonally adjusted. Number of open vacancies corresponds to the number of vacancies at the end of the quarter. Number of filled vacancies refers to total number of vacancies filled in the relevant quarter. Data is not available at the monthly level.
Source: CBS (https://www.cbs.nl/nl-nl/maatwerk/2017/20/vacatures-stroomcijfers-seizoensgecorrigeerd)

spell is forwarded to the first of the month in case workers with irregular working patterns are dismissed within the first seven days of a month. This adjustment is implemented to circumvent unemployment benefits that need to be paid for some part of a month instead of a full month, which would come along with computational difficulties. The unemployment spell is lengthened with maximally seven days for workers with an irregular working pattern. These workers are entitled to unemployment benefits over this additional period and therefore increase the number of unemployed days. Lastly, employers might have dismissed their (older) workers just before the WWZ took effect. Employers would have to pay the transition payment to them. Although this transition payment is usually lower than the previous severance payment, employers could go to court to oppose the severance payment. This is not possible anymore after the WWZ was implemented. Yet, these effects should diminish when the bandwidth is broadened. From the analysis, it follows that significant positive effects can still be observed for the quarterly and half-yearly bandwidth, implying that this effect only slightly contributes to the prolonging effect of the WWZ. Overall, each of these five potential interpretations may explain parts of the positive effect of the WWZ. Uncertain is whether these explanations fully counteract effects that reduce the unemployment duration, such as possibly parts of the UB income-based calculation and suitable employment regulation. Although the WWZ seems to raise the unemployment duration, it does not necessarily imply that the new suitable employment regulation did so as well. Figure 7 displays the effect of the suitable employment regulation alone. The effects of the income-based calculation of the benefit and the transition payment are excluded. Theoretically, no or small positive effects should occur in the first 26 weeks, while a jump should arise at or after the 26th week. The graphs seem to point to some extent towards a negative effect of the stricter suitable employment policy on the unemployment duration. On average, the exit rate is reduced by roughly 1%-point.

The effects of the tightening in the suitable employment policy are quantified in Table 8A and Table 8B. The effects are first presented excluding the income-based calculation of the benefit but including the transition payment. The final estimation returns the effects of the suitable employment

Figure 7. Effects of stricter suitable employment policy (triple DiD graphically)



regulation alone.[23] It follows that the suitable employment regulation significantly reduced the time spent in unemployment by 3.0 days (2.5%) when control variables are included and by 3.4 days (2.7%) without control variables for the half-yearly comparison.[24]

These results confirm the prediction that the tightening in the suitable employment regulation causes unemployed workers to accept suitable jobs at an earlier stage in their unemployment spell. It appears that workers subject to the adjustment are induced to accept job offers they would have refused in the old regime. The size of the effects is difficult to relate to the existing literature. First, the literature measures the effects of all activation programs' unemployment benefit sanctions on the exit rate out of unemployment. Secondly, this study examines the effects of a tightening in the existing suitable employment regulation. A tightening, rather than the implementation of an entirely new policy, most likely leads to smaller reductions in the unemployment duration. As Svarer (2011) stated, so far no research has been done in estimating the effects of stricter activation programs. Therefore, these study's findings cannot be related easily to the existing literature.

Table 8A and Table 8B also report the effects of the suitable employment regulation together with the transition payment. Comparing these estimates to those from the suitable employment policy alone enables one to assess the effectiveness of the transition payment. Estimates seem to barely differ, implying that the transition payment's effect on the unemployment duration is likely to be minor. Although the transition payment aimed to enhance redeployment, this result is not surprising. Unemployed workers were not obliged to use the transition payment to invest for instance in human capital or training. Ultimately, the transition payment differed hardly from the previous severance payment and this might explain the negligible effect.

Theoretically, the stricter suitable employment policy should only affect the unemployment duration after six months of unemployment. Only those workers who are unemployed for more than six months have to accept suitable job offers. Table 9 presents the effects separately for workers who had an unemployment duration of six months or less and for workers who had been unemployed for more than six months. Note that selecting on the dependent variable comes along with a sample selection bias. Estimates are biased and inconsistent when the sample selection is correlated to the error term (Wooldridge, 2015). The expected unemployment duration of the restricted sample is unequal to the expected value of the population, which causes the bias (Wooldridge, 2015). Therefore, the estimations presented in Table 9 have an explorative nature. Interestingly, the effects are insignificant and close to zero for workers with unemployment spells that lasted for six months or less. In addition, the restricted sample of unemployed workers with spells exceeding the duration of six months returned highly significant and positive effects for the quarterly and half-yearly bandwidth. The reductions in the unemployment duration range from 14.2 days to 17.3 days. Yet, these numbers are misleading as workers with unemployment spells longer than six months are also unemployed for a longer period than the average duration. These reductions amount to 5.8% and 4.7% respectively. Still, one needs to take into account that these effects are likely biased because of selecting on the dependent variable.

Although the suitable employment policy reduced the time spent in unemployment, the WWZ in general appears to prolong the unemployment duration. It would have increased even to a stronger degree in case the tightening in the suitable employment policy was not part of the WWZ. A back-of-the-envelope calculation shows that the tighter suitable employment regulation reduced the government expenditures on unemployment benefits between €101 and €113 per worker getting

[23] The coefficients of the effect of the suitable employment policy in the specification with control variables are manually calculated by subtracting the coefficient of the WWZ for the lower education levels from the coefficient for the upper education levels. Therefore, no standard errors have been retrieved for these estimates. Including control variables in a triple DiD estimator returns different estimates than by simply subtracting the two DiD effects. It should be theoretically possible to include control variables into a triple DiD estimation. Yet, Pischke (2005) states that in some cases it is not feasible to calculate the triple DiD estimator including all control variables. Some second level interactions and control variables may have to be excluded to find correct estimates.

[24] Table A7 in the Appendix quantifies the effect of the stricter suitable employment regulation when first and second order polynomials are included. The one-monthly bandwidth and half-yearly bandwidth including second order polynomials return significant negative estimates of 2.5 days (2.1%) and 5.7 days (4.6%) respectively.

Table 8A. Effects of WWZ and suitable employment policy on unemployment duration

Dependent variable: unemployment duration in days						
	± 1 month		*± 3 months*		*± 6 months*	
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
WWZ (DiD)	11.650***	14.688***	7.220***	9.527***	6.567***	10.270***
	(1.887)	(3.241)	(1.152)	(2.008)	(0.774)	(1.272)
Observations	106,300	35,654	284,193	92,325	607,949	215,061
Suitable employment (incl. transition pay)		-3.038		-2.306		-3.703**
		(3.750)		(2.316)		(1.489)
WWZ (excl. transition pay, DiD)	9.174***	12.492***	4.255***	6.944***	5.642***	9.021***
	(2.295)	(3.710)	(1.417)	(2.332)	(0.939)	(1.476)
Observations	59,481	22,002	153,322	55,311	336,205	132,318
Suitable employment		-3.318		-2.689		-3.379*
		(4.363)		(2.729)		(1.749)

Note: Heteroscedasticity robust standard errors are presented in parentheses. Reported regressions are without control variables.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Table 8B. Effects of WWZ and suitable employment policy on unemployment duration (control variables included)

Dependent variable: unemployment duration in days						
	± 1 month		*± 3 months*		*± 6 months*	
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
WWZ (DiD)	10.657***	13.045***	5.776***	9.081***	6.178***	9.666***
	(1.925)	(3.227)	(1.186)	(2.019)	(0.795)	(1.279)
Observations	94,227	32,154	249,584	82,230	536,651	193,859
Suitable employment (incl. transition pay)		-2.388		-3.305		-3.488
Statistically different: Prob. > χ^2		0.525		0.158		0.021
WWZ (excl. transition pay, DiD)	10.024***	13.281***	4.901***	8.084***	6.239***	9.282***
	(2.297)	(3.676)	(1.422)	(2.316)	(0.943)	(1.475)
Observations	58,500	21,931	150,865	55,094	331,173	131,835
Suitable employment		-3.257		-3.183		-3.043
Statistically different: Prob. > χ^2		0.452		0.242		0.082

Note: Heteroscedasticity robust standard errors are presented in parentheses. Control variables included are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations. Prob. > χ^2 indicates the statistical difference between the effect of the WWZ on unemployment duration for higher and lower educated workers.
*** significant at the 1% level

Table 9. Effects of WWZ and suitable employment policy on unemployment duration (less than or more than six months of unemployment)

Dependent variable: unemployment duration in days						
	≤ 6 months					
	± 1 month		*± 3 months*		*± 6 months*	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
WWZ (DiD)	7.170***	7.247***	4.018***	3.087***	1.351***	1.242**
	(0.843)	(1.410)	(0.544)	(0.928)	(0.378)	(0.623)
Observations	49,147	18,193	123,768	44,563	274,694	108,902
Suitable employment		-0.077		0.932		0.109
		(1.643)		(1.076)		(0.729)
	> 6 months					
	± 1 month		*± 3 months*		*± 6 months*	
	High	Low	High	Low	High	Low
	(7)	(8)	(9)	(10)	(11)	(12)
WWZ (DiD)	12.338**	25.682***	3.130	20.382***	-1.100	13.067***
	(5.294)	(8.940)	(3.091)	(5.279)	(2.176)	(3.601)
Observations	10,334	3,809	29,554	10,748	61,511	23,416
Suitable employment		-13.344		-17.252***		-14.167***
		(10.388)		(6.117)		(4.207)

Note: Heteroscedasticity robust standard errors are presented in parentheses. Reported regressions are without control variables. Regressions including control variables return similar results. Regressions in upper (lower) part of the table only include unemployment spells that lasted for six months or less (longer than six months).
*** significant at the 1% level
** significant at the 5% level

unemployed in the second half of 2015 and being subject to the tightening.[25] In total, this saved the government about €6.48 million to €7.20 million over the six months following the implementation of the policy. If the tighter suitable employment regulation would have been implemented one year earlier (1st of July 2014), government expenditures would have been reduced between €15.53 million and €17.25 million.[26] These estimations exclude the (in)direct taxes these redeployed workers pay, which will have additional favourable effects on the government budget. Moreover, the costs of implementing the tightening in the suitable employment regulation mainly concern legislative costs and are therefore minor to the analysis. While short-term effects will likely be favourable to the government budget, medium-term effects may adjust these estimates downwards or yield detrimental results to the budget. Redeployed workers may earn less than what they would have earned without the policy tightening. This subsequently reduces tax revenues. Earlier redeployment may therefore have additional costs that need to be taken into account. Moreover, over the medium-term unemployed workers may fall back more easily into unemployment, thereby raising benefit payments again. These job quality effects should be incorporated to assess the desirability of the tightening in the suitable employment policy.

[25] In the second half of 2015, 63,916 unemployed workers have been subject to the tighter suitable employment regulation. These workers got unemployed in the second half of 2015, had an unemployment duration longer or equal to six months and had a high education level (upper part).
[26] In total, 147,820 additional unemployed workers would have been subject to the new suitable employment policy if the tightening was implemented one year earlier.

Job quality effects

The WWZ aims to foster the return to the labour market. It does so by raising the benefits of working (UB income-based calculation) and by reducing the benefits of unemployment (stricter suitable employment regulation and transition payment). Both effects theoretically induce unemployed workers to leave unemployment earlier. Consequently, unemployed workers that aim to return earlier to the labour market may have to accept lower quality jobs in terms of earnings and employment stability.

Table 10 displays the effects of the WWZ on the average monthly income after the unemployed worker returned to the labour market. The specifications in the lower part of the table include all major control variables supplemented by the average number of working hours after redeployment. The results indicate that the WWZ lowered the average monthly earnings in the new job. The estimates range from a reduction of €23.42 (1.3%) in the monthly income when control variables are included to €48.28 (2.7%) without control variables. These results are in accordance with the expectation that unemployed workers are willing to or obliged to accept less rewarding jobs. As discussed in Section V, three components of the WWZ may explain the occurrence of lower earnings. First, the income-based calculation enables unemployed workers to raise their monthly expenditures by 30% of any additional labour income. As those workers only receive part of their labour income, the income itself is not of great importance in whether to accept work. Therefore, unemployed workers will redeploy earlier. In addition, the transition payment is typically lower than the previous severance payment and may therefore incentivize unemployed workers to return earlier to the labour market. Finally, the suitable employment regulation obliges these workers to accept suitable job offers. Overall, the WWZ slightly reduces the average monthly income.

The effects of the new suitable employment regulation on the average monthly income are reported in Table 11. No significant effect of the tightening can be observed. Apparently, the stricter suitable employment regulation does not induce unemployed workers to enter into less rewarding jobs. Yet, the reduction in the unemployment duration may also by too small to find significant effects. Most specifications do return negative estimates but remain insignificant. In case this intuition is correct, the results confirm the consensus in the existing literature. Two studies examining the effects of activation programs' benefit sanctions on the average income in the new job found slightly negative results, ranging from reductions of 1.5% to 6.2% (Arni et al., 2013; Van den Berg and Vikström, 2014). Both studies consider the effect of a new policy implementation, instead of a tightening in the prevailing regulation. This might be a reason why no negative effects could be observed.

The job quality of the previously unemployed worker may also be affected in terms of employment stability. The stability of the job is estimated by whether an unemployed worker falls back in unemployment. This relapse rate into unemployment is defined as the share of workers that got redeployed in the first six months of their spell and did not have an income in the 28th week of their unemployment spell. Table 12 displays these effects of the WWZ and suggests that the WWZ increases the employment stability. The share of unemployed workers that had been employed in the first six months of their unemployment spell and were unemployed slightly after it is reduced by 1.3%-points (14.1%) to 1.9%-points (15.6%). These favourable though counterintuitive effects may have two possible explanations. First, the tightening in the temporary contracts regulation may reduce the available number of temporary contracts. Employers might be reluctant to provide temporary contracts to workers that are close to the maximum number of temporary contracts at this employer. Some of these workers may get redeployed in a permanent contract at a different employer and, therefore, fall less quickly back into unemployment. This might partially explain the increase in the employment stability. Secondly, the estimations do not take into account the number of hours for which the workers got redeployed. Unemployed workers subject to the UB income-based calculation are more inclined to work in addition to their benefit, which, theoretically, may induce unemployed workers to redeploy at a lower number of hours. Moreover, the tightening in the suitable employment policy may also push unemployed workers to accept jobs with less hours than those workers would have accepted. Should one aim to investigate the employment stability as well in terms of the number of hours, the effects presented in Table 12 may then correspond to an overestimation.

To estimate the effects of the suitable employment policy on the employment stability the relapse rate into unemployment is compared between unemployed workers who had been unemployed

Table 10. Effects of WWZ on average income after redeployment

Dependent variable: average monthly income in euro									
	± 1 month			*± 3 months*			*± 6 months*		
	June (1)	July (2)	Difference (3)	2nd quarter (4)	3rd quarter (5)	Difference (6)	1st half year (7)	2nd half year (8)	Difference (9)
Before WWZ	1,730.63	1,788.43	57.80***	1,775.58	1,747.27	-28.31***	1,737.65	1,775.00	37.35***
			(9.455)			(6.023)			(3.838)
After WWZ	1,809.86	1,848.33	38.47***	1,845.75	1,818.01	-27.74***	1,819.07	1,808.14	-10.93***
			(12.667)			(7.188)			(4.555)
Difference	79.23	59.90	-19.33	70.17	70.74	0.57	81.42	33.14	-48.28***
			(15.806)			(9.378)			(5.956)
Controls			No			No			No
Observations			136,999			360,845			788,860
Difference			-23.42*			0.46			-1.27
			(12.495)			(7.363)			(4.637)
Controls			Yes			Yes			Yes
Observations			122,824			320,471			705,511

Note: Heteroscedasticity robust standard errors are presented in parentheses. Control variables included are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations, supplemented by the average number of hours worked after redeployment

*** significant at the 1% level

* significant at the 10% level

Table 11. Effects of WWZ and suitable employment policy on average income after redeployment

Dependent variable: average monthly income in euro

	± 1 month		*± 3 months*		*± 6 months*	
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
WWZ (DiD)	-3.25	-25.95	-14.32	-3.03	-68.60***	-61.65
	(28.327)	(22.11)	(14.27)	(13.84)	(8.56)	(8.99)
Controls	No	No	No	No	No	No
Observations	58,356	22,058	149,914	55,312	328,924	131,992
Suitable employment		22.70		-11.30		-6.95
		(35.94)		(19.88)		(12.41)

	± 1 month		*± 3 months*		*± 6 months*	
	High (7)	Low (8)	High (9)	Low (10)	High (11)	Low (12)
WWZ (excl. transition pay, DiD)	-9.55	14.11	-0.70	13.79*	-9.42	0.89
	(23.67)	(11.62)	(11.24)	(7.78)	(7.61)	(5.40)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	57,294	21,954	147,199	54,984	323,335	131,281
Suitable employment		-23.66		-14.49		-10.31
Statistically different: Prob. > χ^2		0.370		0.242		0.270

Note: Heteroscedasticity robust standard errors are presented in parentheses. Control variables included are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations, supplemented by the average number of hours worked after redeployment. Prob. > χ^2 indicates the statistical difference between the effect of the WWZ on unemployment duration for higher and lower educated workers.

*** significant at the 1% level

* significant at the 10% level

for at least six months, found a job within the preceding six months and did not report an income in the 56th week of their unemployment spell. Table 13 suggests that the tightening in the suitable employment regulation had a positive effect on the relapse rate. The results suggest that the tightening increased the relapse rate by 0.8%-points (11.9%) to 1.4%-points (23.7%). As expected, the stricter suitable employment regulation has prompted some unemployed workers to accept jobs that are less stable, such as temporary jobs, which eventually cause them to fall back into unemployment. These estimates correspond to the share of workers that relapsed into unemployment and do not include any effects in terms of the number of hours in these contracts. Theoretically, the employment stability would have decreased even further when also assessing possible reductions in hours. Although the literature uses different variables when estimating the effects on the employment duration, the sign of the results is analogous. Arni et al. (2013) found an increased exit rate out of employment of 15%, while Van den Berg and Vikström (2014) concluded that unemployed workers are 15% less likely to enter full-time employment.

Overall, the tightening in the suitable employment policy did not affect the average monthly income but increased the number of workers falling back in unemployment. Therefore, the tightening may come along with additional costs to the government budget. Public expenditures on unemployment benefits are reduced for the first unemployment spell but are at the same time extended as a larger share of workers turns unemployed again. Consequently, the favourable effects of the adjusted suitable employment regulation are reduced by the lower stability of the new job.

Table 12. Effects of WWZ on employment stability

Dependent variable: relapse rate at 28th week									
	± 1 month			*± 3 months*			*± 6 months*		
	June (1)	July (2)	Difference (3)	2nd quarter (4)	3rd quarter (5)	Difference (6)	1st half year (7)	2nd half year (8)	Difference (9)
Before WWZ	0.093	0.122	0.029***	0.086	0.104	0.018***	0.080	0.092	0.012***
			(0.002)			(0.001)			(0.001)
After WWZ	0.092	0.103	0.012***	0.088	0.095	0.008***	0.081	0.085	0.004***
			(0.002)			(0.001)			(0.001)
Difference	-0.001	-0.019	-0.017***	0.002	-0.009	-0.010***	0.001	-0.007	-0.008***
			(0.003)			(0.002)			(0.001)
Controls			No			No			No
Observations			177,909			479,060			1,034,243
Difference			-0.019***			-0.014***			-0.013***
			(0.004)			(0.002)			(0.001)
Controls			Yes			Yes			Yes
Observations			122,824			320,471			705,511

Note: Heteroscedasticity robust standard errors are presented in parentheses. Numbers correspond to share of unemployed workers that did not have an income in the 28th week after the start of their first unemployment spell. Control variables included are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations, supplemented by the average number of hours worked after redeployment.

*** significant at the 1% level

Table 13. Effects of WWZ and suitable employment policy on employment stability

Dependent variable: relapse rate at 56th week

	± *1 month*		± *3 months*		± *6 months*	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
WWZ (DiD)	-0.004*	-0.017***	-0.002	-0.008***	-0.010***	-0.021***
	(0.002)	(0.004)	(0.001)	(0.003)	(0.001)	(0.002)
Controls	No	No	No	No	No	No
Observations	66,730	25,455	173,626	64,770	378,828	153,434
Suitable employment		0.013***		0.007**		0.012***
		(0.004)		(0.003)		(0.002)
		± *1 month*		± *3 months*		± *6 months*
	High	Low	High	Low	High	Low
	(7)	(8)	(9)	(10)	(11)	(12)
WWZ (excl. transition pay, DiD)	-0.005**	-0.019***	-0.002***	-0.010***	-0.011***	-0.024***
	(0.002)	(0.005)	(0.001)	(0.003)	(0.001)	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	57,294	21,954	147,199	54,984	323,335	131,281
Suitable employment		0.014		0.008		0.013
Statistically different: Prob. > χ^2		0.007		0.024		0.000

Note: Heteroscedasticity robust standard errors are presented in parentheses. Control variables included are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations, supplemented by the average number of hours worked after redeployment. Prob. > χ^2 indicates the statistical difference between the effect of the WWZ on unemployment duration for higher and lower educated workers.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level

VI. Conclusion

The suitable employment policy in the Netherlands was restricted on the 1st of July 2015 as part of a broader package of labour market reforms. The salary of, the level of and the commuting distance to the new job may not refrain an unemployed worker from accepting a job offer after being unemployed for six months (previously after twelve months). The tightening in the suitable employment regulation in the Netherlands may be an effective way to reduce the unemployment duration. It induces unemployed workers to broaden their search base and to accept job offers that may not seem appropriate at first sight. Yet, obliging unemployed workers to accept any job offer may come at a cost. Unemployed workers may have to accept lower-quality jobs such that their earnings and employment duration are reduced. Eventually, unemployed workers may pay less taxes and fall back into unemployment earlier.

This study confirms the expected negative effect of the adjusted regulation on the unemployment duration by exploiting a triple difference-in-differences approach on a subsample of the dataset. This approach enables one to exclude the effects of the income-based calculation of the benefit and the transition payment from the effects of the stricter suitable employment regime. The time spent in unemployment is reduced by about 3 days (2.5%) for workers subject to the stricter regulation. A back-of-the-envelope calculation shows that this saved the Dutch government about €6.48 million to €7.20 million for the second half year of 2015. These benefits are likely to be underestimated as redeployed workers will pay (in)direct taxes in turn. The current study also exploits an RD design to investigate the effects of the tightening on the unemployment duration. Yet, testing the assumptions showed that the RD design is likely to return biased results. The treatment and control group are statistically different and the RD design is subject to business cycle effects. Moreover, the results were widely varying per specification used. Therefore, this study focuses on the DiD approach.

Furthermore, the DiD analysis suggests that workers earnings after redeployment are unaffected by the tightening in the suitable employment regulation. The policy adjustment may either be too small to find significant effects or may not affect the average monthly income at all. However, sizable negative effects could be observed for the employment stability. The share of workers falling back into unemployment one year after the start of their unemployment spell increases significantly by 0.8%-points (11.9%) to 1.4%-points (23.7%). So, although the stricter suitable employment policy reduces the unemployment duration in the first spell, it induces workers to enter into less stable jobs that may eventually cause them to relapse into unemployment. Overall, the favourable effects on the government budget may diminish or disappear when taking the medium-term effects into account.

Even though the suitable employment policy alone reduced the unemployment duration, the broader package of labour market reforms, to which the suitable employment policy belongs, increased the time spent in unemployment to a greater extent. Overall, the WWZ prolonged the unemployment duration between 6.3 days (4.9%) to 10.7 days (9.2%). Would the suitable employment regulation not have been restricted, the unemployment duration would have increased even more. The positive effect of the WWZ on the unemployment duration is counterintuitive. This study presented five potential explanations for this positive effect, as described in Section VI. Opposing the effects of the suitable employment regulation on the job quality, the WWZ reduced the average income in the new job and increased the employment duration. Overall, workers subject to the WWZ face a reduction in their income of €23.42 (1.3%), but fall less quickly back into unemployment. The share of workers relapsing into unemployment six months after the start of their unemployment spell is reduced by 1.3%-points (14.1%) to 1.9%-points (15.6%).

The suitable employment policy may seem an effective policy to shorten the unemployment duration and to reduce the financial burden of unemployment to the government budget. Therefore, governments will be inclined to implement these suitable employment policies. Nevertheless, governments should take into account that these policies come along with additional costs. Although these regulations shorten the unemployment duration in the first job, they also increase the relapse rate into unemployment. The suitable employment policy may require unemployed workers to accept inferior jobs, such as jobs with temporary contracts. Governments face a trade-off when implementing such a suitable employment policy. On the one hand, the policy should incentivize unemployed workers to redeploy while, on the other hand, should not push them back too quickly as those workers are more likely to fall back into unemployment earlier.

This study is unable to provide a single framework when or to what extent the trade-off is most favourable to the government budget. A first extension to the current study would be to further quantify the adverse effects on the employment stability. This study found that some workers fall back into unemployment quicker because of the suitable employment regime. Yet, it did not estimate whether those workers become unemployed for a longer period. Further research should also quantify the budgetary costs of these recidivists to the government budget. This continuation would require extensive data over a wider range than the current study uses. Secondly, although the expected effect of and the number of unemployed workers subject to the new daily wage calculation on the unemployment duration is small, excluding it would get rid of any possible bias still present in the estimates. So far, the unemployment insurance agency did not collect data on the number of days worked in the previous year, which would be needed to correctly control for the effect of the new daily wage. A third extension to the present study would be to separate the threat effect (ex-ante) and direct effect (ex-post) of benefit sanctions. Many existing studies separately quantified the relevance of both effects. This study was not able to do so. The unemployment insurance agency only registers the reason of outflow out of unemployment and does not report whether or why unemployed workers have been warned or sanctioned during their unemployment spell. These three extensions to the current study require additional data which is in most cases not reported by the unemployment insurance agency. Therefore, carrying out further research on one of these extensions for the Dutch labour market may be challenging though desired.

Nevertheless, additional research may be directed towards extending the existing literature. Overall, the existing literature consents in their findings, but is limited in size. Further research into the effects of activation programs' benefit sanctions on the unemployment duration may enhance the robustness of the findings or may provide new insights. Secondly, the literature focusses on the effects of activation programs in general. Yet, activation programs may differ in their effectiveness of reducing the unemployment duration, thereby creating a need for further research into the effects of specific activation programs. Finally, the number of studies investigating the effects of activation programs on the quality of the new job is limited as well. These effects correspond to the medium-term and long-term effects of activation programs and are important when assessing the overall effectiveness. Overall, the current study appends the existing literature by confirming the negative effect of a particular activation program on the unemployment duration. Encouraging unemployed workers to be less picky results in a shorter unemployment duration.

Table A1. Comparison of background characteristics for multiple bandwidths (first order polynomials included)

	± 1 month			*± 3 months*			*± 6 months*		
	June (1)	July (2)	p-value (3)	2nd quarter (4)	3rd quarter (5)	p-value (6)	1st half year (7)	2nd half year (8)	p-value (9)
Female	0.51	0.47	0.00	0.51	0.49	0.57	0.51	0.46	0.00
Age	40.89	40.26	0.00	40.94	40.11	0.00	40.59	40.08	0.02
Education (in years)	11.81	12.02	0.00	11.93	12.05	0.00	11.98	11.83	0.00
Married	0.38	0.40	0.00	0.39	0.38	0.00	0.38	0.38	0.00
Weekly number of hours worked	31.67	34.01	0.00	31.77	33.53	0.00	31.74	34.42	0.00
Monthly salary (in euro, 8 weeks before)	1,968.84	2,645.23	0.00	1,876.25	2,171.04	0.00	1,886.08	1,972.75	0.00
Permanent contract (8 weeks before)	0.29	0.26	0.00	0.30	0.28	0.00	0.31	0.26	0.00
Years in last job	3.73	3.32	0.00	3.75	3.49	0.00	3.68	3.30	0.00
Observations	43,045	48,015		114,290	117,596		254,371	239,491	

Note: p-values correspond to statistical difference between means reported in columns to the left of the p-value. Number of observations refers to observations reported for ‘female’. First order polynomials are included.

Table A2. Comparison of background characteristics for multiple bandwidths (males)

	± 1 month			*± 3 months*			*± 6 months*		
	June (1)	July (2)	p-value (3)	2nd quarter (4)	3rd quarter (5)	p-value (6)	1st half year (7)	2nd half year (8)	p-value (9)
Age	40.40	41,03	0.00	41.58	40.81	0.00	41.21	40.72	0.00
Education (in years)	11.60	12.61	0.72	11.72	11.71	0.77	11.74	11.52	0.00
Married	0.38	0.41	0.00	0.39	0.38	0.69	0.38	0.38	0.44
Weekly number of hours worked	35,89	37,23	0.00	35.93	37.15	0.00	35.95	37.71	0.00
Monthly salary (in euro, 8 weeks before)	2,393.97	3,003.72	0.00	2,289.81	2,562.58	0.00	2,260.83	2,305.31	0.00
Permanent contract (8 weeks before)	0.27	0.22	0.00	0.28	0.25	0.00	0.29	0.23	0.00
Years in last job	3.59	3.02	0.00	3.65	3.20	0.00	3.61	3.01	0.00
Observations	21,104	25,488		56,197	59,895		125,715	129,978	

Note: p-values correspond to statistical difference between means reported in columns to the left of the p-value. Number of observations refers to observations reported for 'age'.

Table A3. Comparison of background characteristics for multiple bandwidths (females)

	± 1 month			*± 3 months*			*± 6 months*		
	June (1)	July (2)	p-value (3)	2nd quarter (4)	3rd quarter (5)	p-value (6)	1st half year (7)	2nd half year (8)	p-value (9)
Age	40.41	39.40	0.00	40.31	39.38	0.00	40.00	39.32	0.00
Education (in years)	12.03	12.50	0.00	12.14	12.42	0.00	12.22	12.22	0.78
Married	0.39	0.39	0.21	0.39	0.38	0.00	0.39	0.38	0.00
Weekly number of hours worked	27.62	27.91	0.13	27.75	28.12	0.00	27.63	38.51	0.00
Monthly salary (in euro, 8 weeks before)	1,562.37	2,243.65	0.00	1,477.65	1,768.47	0.00	1,520.73	1,581.61	0.00
Permanent contract (8 weeks before)	0.31	0.31	0.23	0.32	0.31	0.00	0.33	0.30	0.00
Years in last job	3.90	3.69	0.00	3.86	3.83	0.35	3.75	3.69	0.03
Observations	21,941	22,526		58,093	57,700		128,655	109,509	

Note: p-values correspond to statistical difference between means reported in columns to the left of the p-value. Number of observations refers to observations reported for ‘age’.

Table A4. First stage of instrument on WWZ eligibility

	± 1 month			*± 3 months*			*± 6 months*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Instrument	0.848***	0.889***	0.915***	0.880***	0.874***	0.867***	0.881***	0.884***	0.869***
	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)
Polynomials	0	1st	2nd	0	1st	2nd	0	1st	2nd
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interaction effect	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	63,218	63,218	63,218	157,429	157,429	157,429	341,663	341,663	341,663

Note: Heteroscedasticity robust standard errors are presented in parentheses. Instrument corresponds to dummy with value 1 after the cut-off (1st of July 2015). Controls and interaction effects included. Interaction effect is equal to the cut-off multiplied by the days away from the cut-off. Control variables are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations.

*** significant at the 1% level

Table A5. Effects of WWZ and suitable employment policy on unemployment duration (extended version of Table 5, RD)

Dependent variable: unemployment duration in days						
	± 1 month					
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
WWZ	-29.186***	-39.744***	26.492***	16.889***	24.458***	26.737***
	(1.204)	(2.082)	(2.004)	(3.350)	(2.549)	(4.040)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	52,503	17,908	52,503	17,908	52,503	17,908
Suitable employment (incl. transition pay)		10.558		9.603		-2.279
Statistically different: Prob. > χ^2		0.000		0.014		0.633
WWZ (excl. transition pay)	-32.624***	-47.376***	24.704***	12.338***	24.761***	24.751***
	(1.570)	(2.571)	(2.654)	(4.400)	(3.397)	(5.398)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	29,351	10,922	29,351	10,922	29,351	10,922
Suitable employment		14.752		12.366		0.010
Statistically different: Prob. > χ^2		0.000		0.016		0.999
	± 3 months					
	High (7)	Low (8)	High (9)	Low (10)	High (11)	Low (12)
WWZ	-23.197***	-24.905***	-7.941***	-22.828***	1.639	-16.309***
	(0.742)	(1.318)	(1.367)	(2.420)	(2.004)	(3.285)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	133,545	43,003	133,545	43,003	133,545	43,003
Suitable employment (incl. transition pay)		1.708		14.887		17.948
Statistically different: Prob. > χ^2		0.259		0.000		0.000
WWZ (excl. transition pay)	-21.614***	-26.001***	-11.735***	-35.476***	-5.232***	-27.759***
	(0.962)	(1.610)	(1.805)	(3.076)	(2.659)	(4.282)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	72,972	25,850	72,972	25,850	72,972	25,850
Suitable employment		4.387		23.741		22.527
Statistically different: Prob. > χ^2		0.019		0.000		0.000

Table A5 (continued)

	± 6 months					
	High (13)	Low (14)	High (15)	Low (16)	High (17)	Low (18)
WWZ	-30.615***	-28.744***	-7.656***	-12.849***	-15.912***	-32.062***
	(0.504)	(0.859)	(0.984)	(1.731)	(1.484)	(2.600)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	284,537	96,471	284,537	96,471	284,537	96,471
Suitable employment (incl. transition pay)		-1.871		5.193		16.150
Statistically different: Prob. > χ^2		0.060		0.009		0.000
WWZ (excl. transition pay)	-25.214***	-24.196***	-7.553***	-17.621***	-23.799***	-47.785***
	(0.642)	(1.034)	(1.289)	(2.145)	(1.945)	(3.321)
Polynomials	No	No	1st	1st	2nd	2nd
Observations	157,361	58,904	157,361	58,904	157,361	58,904
Suitable employment		-1.018		10.068		23.986
Statistically different: Prob. > χ^2		0.403		0.000		0.000

Note: Heteroscedasticity robust standard errors are presented in parentheses. 'High' corresponds to upper two education levels, i.e. academic and higher vocational education together with intermediate vocational education. 'Low' refers to two lower education levels, i.e. vocational and basic education. Prob. > χ^2 indicates the statistical difference between the effect of the WWZ on unemployment duration for higher and lower educated workers. None of the specifications include control variables.

*** significant at the 1% level

Table A6. Effects of WWZ on unemployment duration including polynomials

Dependent variable: unemployment duration in days

	± 1 month			
	(1)	(2)	(3)	(4)
WWZ (DiD)	7.038***	8.818***	-40.704***	-31.371***
	(1.640)	(1.613)	(2.971)	(2.934)
Controls	Yes	No	Yes	No
Polynomials	1st	1st	2nd	2nd
Observations	126,381	141,954	126,381	141,954
	± 3 months			
	(5)	(6)	(7)	(8)
WWZ (DiD)	5.918***	7.904***	-11.621***	-6.996***
	(1.024)	(1.00)	(1.906)	(1.869)
Controls	Yes	No	Yes	No
Polynomials	1st	1st	2nd	2nd
Observations	331,814	376,518	331,814	376,518
	± 6 months			
	(12)	(13)	(11)	(12)
WWZ (DiD)	6.791***	6.977***	3.164**	11.842***
	(0.676)	(0.661)	(1.277)	(1.254)
Controls	Yes	No	Yes	No
Polynomials	1st	1st	2nd	2nd
Observations	730,510	823,010	730,510	823,010

Note: Heteroscedasticity robust standard errors are presented in parentheses. Control variables included are those specified in Table 2, except for 'weekly number of hours worked' as of a very low number of observations.

*** significant at the 1% level

** significant at the 5% level

Table A7. Effects of WWZ and suitable employment policy on unemployment duration including polynomials

Dependent variable: unemployment duration in days

	± 1 month				*± 3 months*	
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
WWZ (DiD)	8.024***	6.436**	-39.903***	-39.527***	5.336***	8.812***
	(1.905)	(3.205)	(3.441)	(6.008)	(1.186)	(2.020)
Polynomials	1st	1st	2nd	2nd	1st	1st
Observations	94,227	32,154	94,227	32,154	249,584	82,230
Suitable employment (incl. transition pay)		1.588		-0.376		-3.476
Stat. different: Prob. > χ^2		0.670		0.957		0.138
WWZ (DiD, excl. transition pay)	7.893***	8.474***	-31.018***	-28.483***	4.777***	8.286***
	(2.275)	(3.656)	(4.160)	(7.128)	(1.422)	(2.315)
Polynomials	1st	1st	2nd	2nd	1st	1st
Observations	58,500	21,931	58,500	21,931	150,865	55,094
Suitable employment		-0.581		-2.535		-3.509
Stat. different: Prob. > χ^2		0.893		0.090		0.196

	± 3 months		*± 6 months*			
	High (7)	Low (8)	High (9)	Low (10)	High (11)	Low (12)
WWZ (DiD)	-10.061***	-11.215***	5.899***	8.986***	0.978	11.261***
	(2.206)	(3.781)	(0.795)	(1.282)	(1.485)	(2.498)
Polynomials	2nd	2nd	1st	1st	2nd	2nd
Observations	249,584	82,230	536,651	193,859	536,651	193,859
Suitable employment (incl. transition pay)		1.154		-3.087		-10.283
Stat. different: Prob. > χ^2		0.792		0.041		0.000
WWZ (DiD, excl. transition pay)	-6.091***	-8.520***	5.922***	8.796***	1.610	7.301**
	(2.627)	(4.307)	(0.943)	(1.477)	(1.765)	(2.850)
Polynomials	2nd	2nd	1st	1st	2nd	2nd
Observations	150,865	55,094	331,173	131,835	331,173	131,835
Suitable employment		2.429		-2.874		-5.691
Stat. different: Prob. > χ^2		0.630		0.101		0.090

Note: Heteroscedasticity robust standard errors are presented in parentheses. All specifications include control variables, which are those specified in Table 2, except for 'weekly number of hours worked' because of a very low number of observations. Prob. > χ^2 indicates the statistical difference between the effect of the WWZ on unemployment duration for higher and lower educated workers.

*** significant at the 1% level

** significant at the 5% level

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