
Waiting Longer Before Claiming, and Activating Youth. No Point?

Bart Cockx
Eva Van Belle

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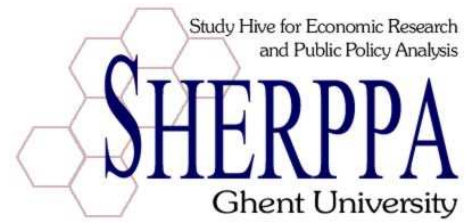
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1 Introduction

The Great Depression of 2008 has had a devastating impact on youth unemployment in Europe. By 2012 the youth unemployment rate in the European Union (EU27) had attained an unprecedented height of 22.8 per cent, which is 7.2 percentage points higher than four years earlier and more than twice as high as the prime-aged adult (25 to 54 years old) rate.¹ In late 2012, the European Commission raised the alarm about the evolution of the youth unemployment and launched in April 2013 the Youth Guarantee to help EU countries get young people into employment, further education or (re)training within four months of leaving school. Individual EU countries also implemented policy reforms to fight youth unemployment. For instance, in 2009 the Flemish² government introduced a Youth Work Plan (YWP) in which young unemployed job seekers below the age of 25 were followed-up more intensively after the first month of unemployment. In 2012 the Belgian government extended for school-leavers aged less than 26 the waiting period before entitlement to unemployment insurance (UI) benefits by three months, from nine to twelve months. This reform aimed at enhancing incentives of unemployed youth to search more intensively for jobs and to accept offers more quickly. This study aims at evaluating the effectiveness of the two aforementioned policies.

UI has usually developed in the *Bismarckian* tradition as an insurance against the risk of *losing* a job after proof of a sufficient contribution record of insurance premiums. The unemployed with an insufficient contribution record are often only eligible to *means-tested* Unemployment Assistance (UA) or Social Assistance (SA). Belgium is one of the few countries in the world in which school-leavers need not have worked to be eligible to (flat rate) *non-means-tested* UI benefits. To the best of our knowledge, Australia and New-Zealand are the only other OECD countries which share these features of UI, even for non-school-leavers.³

A waiting period in UI is usually justified as a means to discourage voluntary quits from jobs as well as temporary lay-offs by firms (Fredriksson and Holmlund 2006, p. 366).⁴ This

¹ Source: Eurostat.

² Belgium is a federal state of three regions. Flanders is the Dutch speaking region in the North.

³ In Belgium laid-off workers with sufficient work experience are entitled to higher UI benefits with, depending on family type and unemployment duration, replacement rates ranging from 40% to 60% of the previous wage (with caps and floors). The three countries also share that no time limit is imposed on benefit receipt (Tatsiramos and van Ours 2014, Table 2).

⁴ It may also reduce the administrative burden by reducing the interest of claiming for short unemployment

argument cannot apply for school-leavers, because they enter the labour market for the first time. Nevertheless, in the spirit of this argument, a waiting period for school-leavers could be a means to discourage early school-leaving.⁵ This was not the main objective of the extension of the waiting period implemented by the Belgian government in 2012, however. Rather, the extension mainly aimed at reinforcing job search incentives. In this research we focus on this latter objective.⁶

We are not aware of any other study that investigates the impact (of an extension) of the waiting period. This is probably because the waiting period, if it exists, mostly lasts only a couple of days. Even in Australia and New-Zealand, the other countries in which no prior working experience is required for eligibility to UI, the waiting period ranges only between one and two weeks (Tatsiramos and van Ours, 2014, Table 2). In these countries job search incentives are rather induced through much stricter job search requirements and follow-up by counsellors than in Belgium, especially in Australia (Langenbucher 2015). Nevertheless, as mentioned, in Flanders, the Dutch speaking region in the North of Belgium youths below the age of 25 are counselled more intensively early on in the unemployment spell than older job seekers. As a consequence of the identification strategy that we use in this research we cannot analyse the impact of the extension of the waiting period without simultaneously analysing the effect of this Youth Work Plan (YWP).

Our research strategy consists in exploiting a discontinuity in the duration of the waiting period at age 26 that was present prior to the reform in 2012: school-leavers younger than 26 were eligible to UI after 9 months, while those older had to wait one year. We investigate whether this discontinuity translates in a discontinuity in a number of labour market outcomes and, hence, provides causal evidence on the effectiveness of the 2012 reform (Imbens and Lemieux 2008; Lee and Lemieux, 2010). First, we consider the effect on unemployment duration and transitions from unemployment to employment. Search theory predicts that the longer waiting period may not only induce youth to search harder for jobs, but also to be less selective in accepting job offers (Mortensen 1977). We therefore also consider the effect on a number of indicators of job quality, such as the daily wage, the time spent in employment, the incidence of part-time work and annual earnings from salaried employment. As mentioned,

spells.

⁵ This complements certain diploma requirements for the eligibility to UI (see Section 2).

⁶ Our data do not permit investigating whether early school-leaving is discouraged, but given the size of the incentive relative to the cost of schooling this is unlikely to have had any important impact.

this identification strategy requires a simultaneous evaluation of the YWP, because participation in it is also conditioned on an age threshold. In the analysis we will therefore allow for two potential age discontinuity points.

The waiting period starts after school-leavers first registration as job seeker at the regional Public Employment Service (PES). Our analysis is based on a follow-up of *all* first registrations in Flanders, the largest of the three Belgian regions, from July to October between 2008 and 2010. In order to obtain information on job quality, these registers are matched to those of diverse social security institutions. The population of interest is restricted to individuals with at least a bachelor's degree. This restriction is imposed to avoid confounding the impact of the extension of the waiting period with that of a hiring subsidy targeted to youth with a lower level of education than a bachelor's degree and which applied at the same age discontinuity at 26.

The remainder of this paper is structured as follows. In Section 2 we describe in more detail the institutional setting and, in particular, the features that may influence the causal regression discontinuity design (RDD). In Section 3 we formulate a number of expectations based on economic theory. Section 4 describes the data and Section 5 the empirical approach. Section 6 reports the results of our analysis, including some sensitivity analysis. Section 7 concludes with a summary of the empirical findings and with a brief discussion of policy implications, the limitations of this study, and suggestions for further research.

2 Institutional Framework

2.1 UI, the Waiting Period and Recent Reforms Regarding Youth

Belgium is a federal state that has decentralised certain policies to regional authorities. The social security system is organised at the federal level. This means that the payment of UI benefits and the issuing of sanctions in the case of non-compliance with the rules are federal competencies. The public employment services (PES) are organised at regional level. They are in charge of counselling, job search assistance, intermediation services and training of unemployed and employed workers.

In Belgium a worker is eligible to UI in two instances: (i) after graduation from school conditional on a waiting period; (ii) after involuntary dismissal from a sufficiently long-lasting job. School-leavers are entitled to flat rate benefits. Dismissed workers earn a gross replacement rate ranging between 40% and 60% of past earnings (bracketed by a floor and a

cap). The benefit level depends on household type (head of household, cohabitant or single) and on unemployment duration for dismissed singles and cohabitants. In contrast to many other countries there is no time limit on the payment of UI benefits.

Before January 2012 the required waiting period for eligibility to UI lasted 9 months if the applicant was younger than 26 at the *end* of this period, while it lasted 12 months for those older than 26. The period starts after school completion from the first registration as job seeker at the regional PES. Since the secondary school year usually runs from 1 September to 30 June, first registrations occur usually in July. However, regulations state that the waiting period cannot start before August 1, unless registration starts after drop-out in the middle of the school year. In order to discourage drop-out, eligibility is conditional on a minimum acquired level of education: for those in the general track (preparation for higher education) at least a diploma of secondary education;⁷ for those in any other track (technical, vocational or arts), completing three of the six secondary school years is sufficient to the extent that they are not younger than 18, the minimum compulsory schooling age in Belgium. The legislator probably introduced this distinction between the general and other tracks with a concern to protect socially disadvantaged groups, who are less likely to complete education in the general track and more likely to repeat grades (Baert *et al.* 2015).

During the waiting period one is supposed to be actively seeking jobs. Before 2012 search effort was not explicitly monitored during the waiting period, but the PES scheduled after predefined periods contacts with caseworkers which were more intensive for youths below the age of 25 (see the description of the “Youth Work Plan” below). At these contacts participation in counselling or training could be proposed. A job seeker refusing participation risks being imposed a sanction by the federal UI agency, but from an international perspective the imposed requirements on job seekers are relatively lenient. Any intervening employment spell or participation in short- to medium-run part-time vocational training counts for the waiting period. By contrast, participation in long-term or full-time training programmes, or resumption of full-time education, resets the waiting time to zero. The waiting period

⁷ In Belgium primary school starts at the age of 6 and lasts 6 years in case of no retention. Secondary school lasts equally long, except for the vocational track for which it lasts 7 years.

is interrupted (without reset) for any other intervening period of inactivity, such as sickness or incarceration.

Since January 1, 2012 the waiting period for those younger than 26 was extended by three months, so that it became as long as that for the older school-leavers. Furthermore, two further restrictions were imposed on UI for school-leavers. First, a time limit of three years was imposed on the entitlement to UI benefits as school-leaver. However, this time limit applies only before the age of 30 for individuals living with other household members with a sufficiently high income. Second, job search effort is evaluated every 6 months since 2012 and school-leavers are only eligible to UI if they satisfy the job search requirements. Before 2012 these evaluations were only implemented after 15 or 21 months, respectively for those younger or older than 25. In 2015, the UI scheme for school-leavers was further reformed. UI benefits can no longer be claimed if older than 25 and school-leavers younger than 21 must at least have successfully completed six years of secondary education.⁸

2.2 Policies Potentially Threatening the RDD

As explained in the Introduction we aim at identifying the effect of an extension of the waiting period on the job search behaviour of school-leavers by exploiting the discontinuity in the duration of this period at age 26. This identification strategy requires that no other policy discontinuities exist at this age. Two types of policies are potential confounders: federally funded hiring subsidies targeted to youth and the “Youth Work Plan” of the Flemish PES.

In the period of analysis (2008-2012) several hiring subsidies were targeted to youth below the age of 26. First, employers who hired youth with at most a secondary school degree were, under certain conditions, granted a flat rate reduction in employer’s social security contributions of €1,000/quarter for two to four years depending on the level of education. After this first period this reduction drops to 400€/quarter until the age of 26. This policy does not impose a threat to the RDD, because the subsidy *gradually* declines to zero as the hired worker approaches the age of 26.

⁸ This corresponds to having a secondary school degree, except for students in the vocational track who obtain this diploma only after successful completion of seven years of secondary education.

Second, from between 2008 and 2009, and from 2012 onwards, employers who hired long-term⁹ unemployed youth before their 25th anniversary were entitled to the so-called “Activa” advantages. These consisted of two subsidy forms: (i) a reduction in employers’ social security contributions of €1,000/quarter during 5 quarters and, in addition, (ii) an *activation* subsidy of €500/month during four years. These Activa subsidies neither impose a threat to our discontinuity design, because the threshold is at 25 and because the job seeker must have been at least 12 months unemployed during the preceding 18 months. The extension of the waiting period the age discontinuity is at 26, measured after 9 months of unemployment.

Finally, between 2010 and 2011 the Activa advantages were temporarily replaced by the so-called “Win-Win”. This temporary measure was introduced to fight the persisting crisis since the Great Recession of 2008. The Win-Win was targeted at youth with at most a secondary school degree and who were less than 26 years old at hiring. Two categories existed: (i) those *without* a secondary school degree and unemployed for at least 3 months; (ii) those with *at most* a secondary school degree unemployed for at least 6 months. Employers hiring workers of the first category in 2010 (2011) were entitled to an *activation* subsidy of €1,100/month during (12) 24 months. For the second category the amount was €1,000/month. Since the age threshold is at 26, this subsidy can in principle threaten the discontinuity design. However, since the subsidy is targeted at youths with at most a secondary school degree, the discontinuity design remains valid if the analysis is restricted to youths with a higher educational degree: bachelors or masters. Not many observations are actually lost by imposing this restriction, since the analysis focuses on youths entering unemployment directly after their studies. Within this target group only a small minority does not have a higher educational degree around the age discontinuity of 26 years.

In 2008 the Flemish PES introduced the Youth Work Plan (YWP) as a pilot project targeted at low-educated youth in the largest Flemish cities. From 2009 onwards the YWP was extended to all youth younger than 25 one month after registration. The PES sends e-mails with job vacancies to all job seekers from the moment of their registration at the PES. The YWP consists of a set of specific actions targeted at

⁹ Being unemployed at least 12 months during the last 18 months.

those job seekers who did not yet find a job three months¹⁰ after registration. At that moment a PES counsellor contacts the job seeker by telephone. If impediments to work are detected, the job seeker is invited for a meeting at the PES and counselling or training actions are proposed to the job seeker. If no action has been undertaken after this first contact and the job seeker is still unemployed three months later, he or she is invited directly to a meeting with a PES counsellor who may then propose to undertake particular actions. To the extent that the PES strictly denies these services for those older than 25 one month after registration and the YWP is effective, this could generate an age discontinuity close to the one that determines the length of the waiting period. For the latter the discontinuity occurs at 26, 9 months after registration as job seeker. If the age is measured 9 months after registration, the potential age discontinuity of the YWP would occur at the age of 25 and 8 months.¹¹ In the analysis below we therefore explicitly allow for this second discontinuity.

3. Theoretical Expectations and Existing Evidence

3.1 Lengthening the Waiting Period

A waiting period is the mirror image of a time limit on UI eligibility. With a time limit the job seeker is informed at entry that the UI benefit will expire after a predetermined period. In the case of a waiting period, UI benefits are zero at the onset of unemployment and will become strictly positive after a predetermined period. We expect therefore that the predicted behaviour should be the mirror image of the one predicted for the case of time limit.

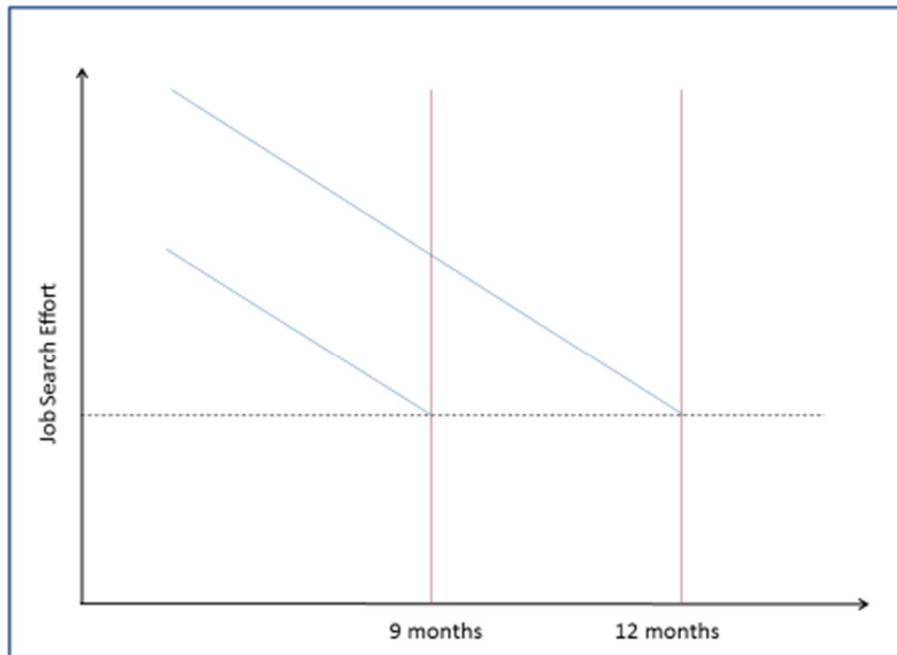
Mortensen (1977) is the first to use non-stationary job search theory to describe the predictions of a time limit in UI scheme on the job search behaviour of rational forward looking individuals. He proves that a job seeker gradually increases job search effort and reduces the reservation wage (or, equivalently, increases the job acceptance probability) as she approaches the moment of benefit exhaustion. At benefit exhaustion, job search effort may discontinuously drop (jump up) if income and leisure are strict substitutes (complements) in household production. By contrast, the reservation wage is always continuous in unemployment duration. Job search theory predicts a gradually increasing job finding rate as

¹⁰ In 2008 this threshold was 6 months for youths with a higher than secondary school degree.

¹¹ For the YWP the age (25 years) is measured one month after registration. Consequently, if age is measured at the end of the waiting period, i.e. 9 months after registration, participants in the YWP should be younger than 25 years and 8 months at that point. Recall that for those registered as job seeker in July the discontinuity is measured slightly later, i.e. 9 months after August 1, starting point of the waiting period for these individuals.

the benefit time limit is approached and a spike (a drop immediately after the time limit) only if income and leisure are strict substitutes. While there is quite some empirical evidence of a spike in the job finding rate at benefit exhaustion,¹² Boone and van Ours (2012) argue that it is unlikely that income and leisure are strict substitutes, because this is at odds with most empirical research that finds that the transition to employment *falls* if the benefit level is raised (Atkinson and Micklewright 1991): if income and leisure are substitutes the job finding rate should *increase*. Standard job search theory seems therefore only partially confirmed by empirical evidence. Boone and van Ours (2012) propose a model that can generate a spike, more so for permanent than for temporary jobs. The model is based on the assumption that UI benefit recipients may have a preference to delay the start of a job they have found until the moment at which benefits expire.

Figure 1: Stylized Impact on the Job Finding Rate of an Extension of the Waiting Period



Based on standard non-stationary job search theory we should therefore expect that a waiting period induces the job finding rate to start at a relatively high level and then to gradually decrease until the moment at which the entitlement to a flat rate benefit is obtained. At that point the job finding rate jumps up (drops) discontinuously if income and leisure are strict substitutes (complements). After that point the job exit rate remains stable. Figure 1 illustrates

¹² See Boone and van Ours (2012) for a brief survey of this evidence. Card et al. (2007) argue that part of the spike is a consequence of measurement error. Nevertheless, as mentioned by Boone and van Ours (p. 415), there are still several studies in which measurement error is not an issue and in which still evidence for a spike is found.

this prediction for the case that the marginal utility of leisure and income are independent of each other, so that no spike is observed. It also illustrates the impact of an extension of the waiting period from 9 to 12 months. Since the benefit level is flat rate without any time limit, the model predicts identical job search behaviour at the start of benefit eligibility, after the waiting period, irrespectively of its length. Similarly, because rational forward looking individuals make identical decisions in case the future profile of UI benefits is the same, job search theory predicts that job search behaviour evolves identically if one moves back in time, irrespectively of whether the waiting period ends after 9 or 12 months. This means that an extension of the waiting period translates in a parallel shift of the search behaviour during the waiting period, as displayed in Figure 1. Job search theory therefore predicts that an extension of the waiting period should unambiguously increase the job finding rate *throughout* the unemployment spell. In Figure 1 the impact of an extension from 9 to 12 months is maximal between the onset of unemployment and 9 months. However, this is a consequence of assuming a linear time profile of job search behaviour. Depending on the form of the utility function, the time profile may, however, be concave or convex as the end of the waiting period is approached, and, hence we cannot predict in general at which unemployment duration the maximal impact should be attained. Nevertheless, because job seekers discount the future, the difference in job search behaviour should diminish the closer one is to the onset of the unemployment spell.

One may question that the aforementioned predictions realise for at least two reasons. First, Mortensen's (1977) prediction of job search behaviour is based on the assumption that other income sources are exogenously fixed. Such an assumption may not be realistic for school-leavers. School-leavers may be financially supported by their parents during the waiting period and only become financially independent from the moment they find a job or are entitled to UI benefits. Whether this is possible may depend on the financial situation of parents. To the extent that the empirical analysis is targeted to youth with at least a bachelor's degree and the intergenerational correlation of educational attainment is high, we may expect that parents are not much credit constrained. In this hypothesis, job search theory would predict no impact of an extension of the waiting period on the job search behaviour of school-leavers. To test for this hypothesis, we will check whether we find a larger effect when parents are expected to be more credit constrained, i.e. when their family income is lower.

A second reason why the aforementioned predictions may not realize is that they are based on the assumption that job seekers form rational and unbiased expectations about the likelihood

of finding jobs. Since the seminal work of Tversky and Kahneman (1974) there is, however, growing evidence that expectations can be severely biased. There is now broad evidence that individuals are overly optimistic regarding positive events and pessimistic with respect to negative events (Moore and Healy 2008). Spinnewijn (2015) refers to ongoing research providing evidence that job seekers strongly underestimate how long they will remain unemployed. In addition, expectations are not only biased they are neither rational. Seminal work of Della Vigna and Paserman (2005) provide evidence that job seekers behave according to time-inconsistent (hyperbolic) time preferences: they are more impatient in the short-run than in the long-run. Both biased beliefs and inconsistent time preferences tend to make job seekers less responsive to future incentives (Spinnewijn 2015; Paserman 2008). Hence, an extension of the waiting period may have much weaker behavioural impact than the one predicted by the standard job search model.

Existing empirical evidence nevertheless shows that financial incentives do play a role, and that the predictions of the standard job search model are not completely washed away by non-rational or biased expectation formation. Both the benefit level and the potential duration of benefits is found to significantly affect the job search effort of unemployed job seekers. The latter factor has a stronger behavioural impact than the former, which means that job seekers do react to future changes in the generosity of UI. By contrast, the evidence with regards the job acceptance behaviour is less clear-cut. For instance, job search theory predicts that extensions of the potential period of benefit receipt should increase the reservation wage and positively affect the job quality. However, the empirical evidence regarding the effect of the design features of UI on the quality of the subsequent job is mixed. Some studies find no effect, others small positive but heterogeneous effects. The latter suggests that there might be only an impact on a smaller set of liquidity constrained unemployed individuals.¹³

3.2 Intensifying Counselling and Training for Youth

The empirical evidence on the effects of an intensification of counselling and training for youth such as in the aforementioned YWP in Flanders is mixed. In their most recent meta-analysis of active labour market programme evaluations Card *et al.* (2015) find that “job search assistance and sanction programs that emphasize “work first” have relatively large short term impacts, on average. Training and private sector employment programs have smaller short term impacts but larger effects in the medium and longer runs.” Since the YWP

¹³ See Tatsiramos and van Ours (2014) for a recent survey of this literature.

comprises the two components, it might be expected to increase the job finding rate of those who are assisted in searching for jobs, while a locking-in effect may result for participants in training programmes. However, active labour market programs are also found to generally work less well for youth than for a prime aged population.

Experimental evidence in Denmark has shown that a combination of meetings, job search courses and early activation could significantly enhance transitions from unemployment to employment. In this case the treatment seemed even particularly effective for youth (Graversen and van Ours 2008). Rosholm (2008) and Vikström *et al.* (2013) show that these large treatment effects derive rather from the intensive early meetings and the threat of participating in active labour market programmes (especially for men) than from the participation in these programmes itself. Follow-up experiments have confirmed that it is especially the intensification of meetings that has played a determining role, while early activation had smaller and even negative effects for women due to lock-in effects (Pedersen *et al.* 2012). In Denmark the unemployed received 6-7 meetings during the first 13 weeks. The intensity of meetings was therefore much higher than in the Flemish YWP where youth were invited to a first meeting only from the third month of unemployment and the actions did not only involve meetings, but could also include participation in training or in other active labour market policies. Moreover, even if participation is in principle mandatory this was, in contrast to Denmark, not very strictly enforced. For these reasons we cannot expect as strong effects of the YWP as the intensified early meetings and activation in Denmark.

4. Data

4.1 Data Sources and Sample Selection Criteria

The empirical analysis is based on Flemish PES register data of the full population registering for the first time as job seeker between July 1 and October 31 for the years 2008 through 2013. The regional PES register data only informs about the potential type of UI entitlement – based on sufficient work experience or educational attainment – not about the effective benefit entitlement, neither about the activity state (education, employment or inactivity) prior to the first registration. They cannot, hence, distinguish between youths who just left school and those who had some intervening spell of employment or inactivity. Since employment spells count for the waiting period and we do not have reliable information on the exact starting date of employment, we exclude the latter group from the analysis as to focus on

youths who are unemployed at the onset of the waiting period.¹⁴ We therefore restrict the population in the following ways. First, since in Belgium the school year ends on June 30 and the academic year in university starts in the last weeks of September, after the exam retakes, restricting first registrations to the July-October period targets the group registering immediately after graduation.¹⁵ Second, we requested the Cross Roads Bank of Social Security (CBSS) to match the register data of the PES to those of the different federal institutions of social security in Belgium. These data contain quarterly information on salaried and self-employment (since 2007) and monthly information on receipt of UI insurance benefits. Based on this information we dropped all individuals who were observed in employment prior to the first registration in the Flemish PES. In addition, we eliminated individuals who were observed to be entitled to UI earlier than they could have been based on their first registration date and their age. The latter inconsistency is likely the consequence of measurement error, since there are only few. Another data inconsistency is that a number of individuals are reported to have left unemployment for a job according to the PES registrations, but were not found to be employed in the social security data. We purged the dataset from these observations.

We mentioned in Section 2.2 that we could only include in the analysis school-leavers with a bachelors or master degree, because otherwise the RDD would be confounded by a wage subsidy targeted at low educated youth. Furthermore, since the focus of the analysis is on the impact of the extension of the waiting period and the identification strategy is based on the discontinuity in the duration of the waiting period at the age of 26, we restrict the sample of analysis to an age window of 1.5 years to the left and to the right of this age. We do not consider a wider window because there are only very few individuals (178) who are older than 27.5 years. We initially included these individuals in the analysis, but graphically the behaviour of these older individuals was as a consequence of small sample size quite erratic and different from the younger group. Finally, we restrict the analysis to the youths entering the labour market between 2008 and 2010. The population entering in 2011 is not considered, because the waiting period of those younger than 26 was extended in the middle of their waiting period, on January 1, 2012. Since it was not clear for this group from which point that they were aware of this change, we exclude this group from the analysis. The entrants in 2012

¹⁴ The onset of the waiting period coincides for this group with the first registration at the regional PES, except if this registration occurs in July. In the latter case the waiting period starts on August 1 (see Section 2.1).

¹⁵ For a few observations the unemployment spell was recorded to start at a different date than the first registration. These observations were dropped from the analysis.

are retained for a placebo analysis. The 2013 inflow is ignored, because the available PES registration data are right censored at the end of November 2013. In conclusion, while the initial population consists of 151,744 individuals, the final sample size retained for the analysis reduces to 5,495 individuals of whom 4,495 are younger than 26 and 1,000 older. Appendix A indicates how the sample size diminishes as particular selection criteria are imposed.

Table 1: Descriptive Statistics of Explanatory Variables

	Total	<26	≥26
Number of Individuals	5,495	4,495	1,000
Mean age at the end of the waiting period	25.36	25.10	26.52
Variable	%	%	%
<i>Gender: Female</i>	47.68	48.59	43.60
Driver license (yes)	99.49	99.51	99.40
<i>Education: master (other = bachelor)</i>	75.18	74.82	76.80
<i>Good knowledge of Dutch</i>	94.54	94.82	92.80
<i>Nationality: Belgian</i>	98.23	98.40	97.50
<i>Household type:</i>			
Single or couple with children	0.42	0.42	0.40
Single	3.73	3.38	5.30
Other (couple w/o child., institution,...)	3.55	3.43	4.10
Child living at parent's house	83.99	84.92	79.80
<i>Year of first registration at PES</i>			
2008	32.67	33.24	34.60
2009	34.43	34.73	33.10
2010	32.90	33.04	32.30
<i>Month of first registration at PES</i>			
July	47.90	48.92	43.30
August	18.89	19.18	17.60
September	27.01	26.14	30.90
October	6.21	5.76	8.20
<i>Equivalent household income*</i>	23'978	24'226	22'845

Note: Descriptive statistics of sample of analysis for the RDD. First registration at Flemish PES in July-October 2008-2010 for those aged between 24.5 and 27.5 years 9 months after registration. All variables except the household type originate from the PES registers and are measured at the first registration. The household type comes from the CBSS and is measured on December 31 of the year preceding the first registration. * Measured in the calendar year prior to first registration as job seeker. This includes labour market earnings and social security allowances of all household members excluding the school-leaver. The income has been scaled by the "OECD-modified scale" assigning a value of 1 to the household head, of 0.5 to each adult household member older than 18 (including the school-leaver) and 0.3 to each child. Reported statistics are calculated after dropping 74 missing observations. In the benchmark analysis these 74 observations are retained, because this analysis does *not* condition on this variable.

4.2 Descriptive Statistics

Table 1 reports the descriptive statistics of the explanatory variables retained in the empirical analysis. All variables except the household type originate from the PES registers and are measured at the first registration in the Flemish PES. The household type originates from the CBSS and is measured on December 31 of the year preceding the first registration at the PES.

Descriptive statistics are reported for the complete sample, for the group younger than 26 (between 24.5 and 26) and for the group aged at least 26 (between 26 and 27.5). The age is calculated (with daily precision) at the (counterfactual) end of the waiting period for those aged less than 26, i.e. 9 months after registration or after August, 1 for those registered in July, as the waiting period cannot start before this moment (see Section 2.2).

There is an asymmetry in the size of the sample around the age discontinuity of 26. The majority in the retained sample is younger than 26. This is because most individuals complete education before this age, i.e. 9 to 10 months earlier before registration as job seeker. Youths ending higher education so late typically have repeated a couple of grades, since 22 or 23 is the typical age at which a master degree without any schooling delay would be attained. It also explains why about three quarters of the sample have a master degree: bachelors must have even more schooling delay to be observed in this age range. For the YWP the age discontinuity is at 25 years and 8 months (9 months after registration) and, hence, a similar reasoning applies. The retained population on which the treatment effects is estimated is therefore rather particular. This is to be kept in mind when interpreting the results.

The vast majority of the retained individuals have the Belgian nationality, a good knowledge of Dutch and a driver's licence. This mirrors the fact that migrants rarely complete higher education in Belgium. More than 80% of the sampled individuals were officially residing at their parent's house at the end of the year preceding the first registration as job seeker at the PES. This comes as no surprise, since, by the imposed selection criteria, these individuals should have been in full-time education at that moment. At the same time this is an indication that the sampled individuals are still financially dependent on their parents and that, hence, the extension of the waiting period might not have any important financial impact (Section 3). Moreover, this interpretation is reinforced by the fact that these parents are unlikely to be credit constrained, since the educational attainment, and hence income, of children is highly correlated to that of parents. As the retained individuals in the sample have at least a bachelor's degree, their parents are likely to be highly educated as well.

As expected, about one third of the sample starts in each of the three retained starting years: 2008, 2009 and 2010. About 50% of the retained sample registers in July. This is because a regular student graduates in June. This share is somewhat smaller for those

older than 26. This may reflect that the older group must have accumulated more schooling delay and is more likely to have to retake exams in September. With respect to the other variables the fractions are relatively balanced over the age groups, except for gender. Women are less likely to be older than 26.

Table 2: Descriptive Statistics of Outcome Variables

Variabile	# observations	average	5%	25%	50%	75%	95%
<i>Unemployment duration</i>	5,495	4.98	2	3	4	6	12
excluding right censored obs.	5,483	4.89	2	3	4	6	12
ending in employment	5,086	4.81	2	3	4	6	11
ending in salaried employment	4,785	4.79	2	3	4	6	11
ending in inactivity	396	5.73	2	3	4	7	14
<i>Working days</i> ¹	4,785	243.3	72	211	270	295	324
<i>Daily wage (€)</i> ²	4,259	106.7	74.7	92.6	104.5	119.2	144.2
<i>Daily wage (€) (corrected)</i> ³	4,540	109.5	77.9	95.8	106.7	123.1	147.1
<i>Earnings</i> ⁵	4,689	26,039	3,867	19,280	27,732	33,466	42,552
<i>Part time work</i> ⁶	4,785	0.098	-	-	-	-	-

All monetary values are expressed in constant 2013 Euros.

¹ Number of working days in salaried employment in quarter of exit and 4 following quarters. Excludes workers transiting first to self-employment.

² Daily wage is the average gross wage (before taxes and personal social security contributions) in the most important salaried job in the quarter of exit to employment. Excludes 526 missing observations.

³ Daily wage in quarter of exit replaced by daily wage in the following quarter if the latter deviates more than 5% from the former. This correction is applied because the daily wage is more sensitive to measurement error (possibly missing) if exit to employment occurs near the end of a quarter and, hence only observed over a short period. Excludes 245 missing observations

⁵ In the quarter of exit and each 4 following quarters the product of the average gross daily wage in the most important job and the number of working days in salaried employment is calculated. The reported figure is the sum of these products over all quarters. Excludes 96 observations for which wage data are missing in all quarters. If the wage is missing in any of the other quarters, earnings was set to zero in the corresponding quarter.

⁶ Indicator variable taking on the value one if a person works part time in the quarter of exit and zero otherwise.

Table 2 displays the descriptive statistics of the outcome variables of interest. We report the number of observations for which we have non-missing values, the mean and percentiles (5, 25, 50, 75 and 95) of its distribution. The first variable of interest is the unemployment duration. This variable comes from the PES registers and is measured at the end of each calendar month. Temporary exits within the month are not recorded. This may lead to a slight measurement error in the determination of the length of the waiting period if the temporary exit is to inactivity (e.g. sickness), because this interruption prolongs the waiting period. In Section 6 below we come back to this point and show that this measurement error does not pose major problems for identification. A second consequence of measuring the unemployment status at the end of the month is that our

data are left truncated: the PES did not select individuals at the actual first registration date, but at the end of the calendar month of this registration. This means that individuals who have left unemployment between registration and the end of the month are not retained and exits from unemployment can only be measured from the second month only. Note, however, even if the selection occurred at the end of the month, we do have exact information at which the registration of these individuals occurred, so that we can exactly determine the potential end date of the waiting period for each individual.

Unemployment duration is right censored at the end of the observation period in November 2013. In the sample of analysis only 12 observations are right censored, which is negligible and a feature that will be exploited in the empirical analysis. The PES identifies at the end of each month whether an individual has left unemployment and, if so, the registers inform whether the exit was to employment or another destination, which we label “inactivity”. In Table 2 the third variable reports the descriptive statistics for the unemployment duration in case of an exit to employment, while the fifth one considers exits to inactivity only. Relatively few individuals (396) leave to inactivity, so that the general unemployment distribution hardly differs from the one that is restricted to spells that end in employment. Median duration is 4 months, implying that only a minority is unemployed throughout the complete waiting period. 95% has left unemployment within one year.

Based on the administrative information of the BCSS, we constructed a number of additional outcomes of interest that aim at measuring potential effects of the extension of the waiting period on the quality of employment. We consider the number of working days in salaried employment in the quarter of exit from unemployment and the 4 subsequent quarters, the daily wage in the quarter of exit, the annual earnings in the year of exit, the daily wage multiplied by the number of working days in the quarter of exit from unemployment and the 4 subsequent quarters, and an indicator equal to one if a salaried worker worked part-time in the quarter of exit and zero otherwise. We only observe these additional variables for the individuals who transitioned to salaried employment, not for those who became self-employed, neither for those who did not find a job.

For the daily wage in the quarter of exit, for instance, there is a relatively large number of missing values. This may be a consequence of individuals leaving the unemployment

registers near the end of the quarter while not entering employment immediately afterwards. That is why we also considered a second (corrected) daily wage in which we replace the first wage by the wage measured in the subsequent quarter if this wage deviates more than 5% from the first one and is not zero or missing.

Job seekers who find a salaried job are not all the time employed in the 5 quarters following hiring (including the quarter of hiring): 50% works less than 270 days and one quarter less than 211 days. If we consider that in most sectors the workweek lasts 5 days and that an individual enters on average in the middle of a quarter, then someone who would have worked full-time during these 4.5 quarters would have worked 292.5 days. This corresponds roughly to the number of working days of the individual at the 75th percentile, who worked 295 days. Since only about 10% of the hired individuals worked part-time, there is a substantial share of individuals who have lost their job within 5 quarters. The median daily gross wage is about €105 which means if, as is common for a full-time in Belgium, 7.2 hours per day is worked, that the gross wage per hour (before taxes and Social Security contributions) would be about 14.6€/hour. Considering that some individuals work part-time, this is a lower bound. Finally, we measure the earnings as the sum over the aforementioned 5 quarters of the product of the average gross daily wage and the number of working days in each quarter. The median individual earned €27,732 in this period. This is about €2,054/month.

5. The Empirical Approach

5.1 The Econometric Modelling

The empirical analysis aims at identifying the effect of an extension of the waiting period from 9 to 12 months for school-leavers slightly younger than 26 on the various outcome variables described in the previous section. Identification is based on the discontinuity of the length of the waiting period at the age of 26 prior to 2012. Since the YWP entails another age discontinuity at 25 years and 8 months, the analysis requires to simultaneously take these two age discontinuities into account. The forcing variable A_i is the age of individual ($i = 1, 2, \dots, N$) 9 months after the first registration of school-leavers as job seeker in the Regional PES¹⁶ measured in days and in deviation from the age discontinuity at 26 years. Let $D_{Wi} \equiv 1[A_i \geq 0]$ denote the treatment status (extension of the waiting period from 9 to 12 months) of individual i where $1[.]$ is the indicator function, D_{YWP} is an indicator if the individual is

¹⁶ Nine months after August 1 if registration is in July (see Section 2.1).

younger than 25 years and 8 months (and hence eligible to the YWP) and zero otherwise,¹⁷ \mathbf{X}_i the vector of explanatory variables listed in Table 1,¹⁸ and $F(A_i)$ and $F_1(A_i)$ two polynomial functions, assumed to be linear in the benchmark models. The following log-linear regression equation then identifies the proportional treatment effect β_W of the extension of the waiting period and β_{YWP} for the YWP:

$$\log(Y_i) = \alpha + \beta_W D_{Wi} + \beta_{YWP} D_{YWPi} + F(A_i) + D_i F_1(A_i) + \mathbf{X}_i \boldsymbol{\gamma} + U_i \quad (1)$$

where in the benchmark models Y_i is one of the outcome variables listed in Table 2¹⁹ and U_i the error term. In regression specification (1) we implicitly impose the same (linear) polynomial to the left and to the right of the age cut-off for the YWP. This assumption is made, because this cut-off at 25 years and 8 months is very close to 26 years, so that a different polynomial between these two cut-offs would be identified on very few data points only. Moreover, based on the graphical analysis reported below, this assumption does not seem to be violated. Nevertheless, in a sensitivity analysis we will test this assumption, although only for the outcome for which we find a statistically significant treatment effect. We will also check for this outcome whether the results are sensitive to the age window and the choice of the polynomial function (Section 6.4).

In case the outcome variable is a duration, then for some individuals this duration is bound to terminate after the end of the observation period, or exits to a particular destination (e.g. employment) are not observed, because an exit to another destination (e.g. to inactivity) precedes exit to the destination of interest. These are instances of right censoring. Because the number of right censoring is very limited (see Table 2), we first ignore the right censored observations and run regression equation (1). Subsequently, in a sensitivity analysis, we treat right censored observations correctly by estimating the discrete duration model as a sequence of monthly binary choices (Kiefer 1988; Jenkins 1995).²⁰

Let $t \in \{2, 3, \dots\}$ and ϵ_i denote the elapsed unemployment duration and the unobserved determinants of the exit rate from unemployment for individual i , respectively.²¹ We assume that these unobserved determinants are independently distributed from the observed

¹⁷ Since the YWP was not yet implemented for the high-educated in 2008, this indicator also zero for individuals of any age in 2008.

¹⁸ In the benchmark analyses the equivalent household income is not included as explanatory variable.

¹⁹ Since the data are left truncated at the end of the first month, we normalize the duration by subtracting one.

²⁰ See Lammers *et al.* (2013) for a similar treatment of RDD within a hazard modeling framework.

²¹ Recall that because of left truncation the first exit can be observed from the second month only.

covariates. This allows to take the dynamic selective sorting of the pool of unemployed individuals over the course of the unemployment spell into account (Salant 1977). The conditional discrete-time hazards $h(t; A_i, \mathbf{X}_i, \epsilon_i) \equiv P(T_i = t | T_i \geq t; A_i, \mathbf{X}_i, \epsilon_i)$ associated to these binary choices take on the complementary log-log specification if they are derived from a continuous time hazard model:

$$h(t; A_i, \mathbf{X}_i, \epsilon_i) = 1 - \exp[-\exp(\alpha_t + \beta_{Wt}D_{Wi} + \beta_{YWP}D_{YWPi} + F(A_i) + D_iF_1(A_i) + \mathbf{X}_i\boldsymbol{\gamma} + \epsilon_i)] \quad (2)$$

where $\exp(\alpha_t)$ is the baseline hazard, $\exp(\beta_{Wt})$ and $\exp(\beta_{YWP})$ are the proportional treatment effect on the hazard of, respectively, the extension of the waiting period and the YWP. We consider only linear polynomials in the empirical analysis. The discrete baseline is assumed to be constant within the following sets of discrete duration months: $\{3,4\}, \{5,6\}, \{7,8,9\}, \{10, 11,12\}, \{13, 14, \dots\}$. The treatment effect of the extension of the waiting period is assumed to be either fixed over the complete unemployment spell ($\beta_{Wt} = \beta_W$) or piecewise-constant over the following sets of months: $\{2, 3, 4, 5, 6, 7, 8, 9\}, \{10, 11, 12\}$ and $\{13, 14, \dots\}$. The latter specification aims at testing whether there are differential effects of the extension of the waiting period over the course of the unemployment spell, as predicted by standard non-stationary job search theory (Section 3). We maintain the assumption of a constant treatment effect of the YWP, because the sample size is too small to allow for time-varying effects.

This model is estimated by maximum likelihood. To form the likelihood function, note that the discrete survival rate after an elapsed duration of t months is simply the product of one minus the discrete-time hazards in all preceding periods: $\prod_{s=2}^t (1 - h(s; A_i, \mathbf{X}_i, \epsilon_i))$. Consequently, if c_i denotes an indicator that is equal to zero in case of right censoring and one otherwise, then the log-likelihood function, from which the unobserved determinants are integrated out, can be written as follows:

$$\log \mathcal{L} = \sum_{i=1}^N \log \left[\int_{-\infty}^{+\infty} [h(t_i; A_i, \mathbf{X}_i, \epsilon_i)]^{c_i} \prod_{s=2}^{t_i-1} (1 - h(s; A_i, \mathbf{X}_i, \epsilon_i)) dG(\epsilon_i) \right] \quad (3)$$

where $G(\epsilon_i)$ is the distribution of unobserved heterogeneity. We perform estimations in which we either assume that there is no unobserved heterogeneity or that it is Normally distributed with mean zero and variance σ^2 .

5.2 Validity Tests

We executed a number of validity tests for the RDD. First, the RDD approach is only valid to the extent that individuals cannot alter their behavior – “manipulate the forcing variable” – to avoid (or to benefit from) the treatment, i.e. “to precisely “sort” around the discontinuity threshold” (Lee, 2008; Lee and Lemieux, 2010). A first way in which school-leavers could manipulate the age at the end of the waiting period, i.e. 9 months after the onset of unemployment, is by registering *earlier* as job seeker at the PES. By doing so the school-leaver could aim at avoiding to be older than 26 at the end of the 9 months waiting period and, hence, escape an additional waiting period of 3 months before becoming eligible to UI benefits. However, any school-leaver has already an incentive to register as early as possible after graduation, since any delay postpones UI eligibility. Manipulation could therefore only occur if a student deliberately stops her studies in the middle of the school year and forgo obtaining a diploma as to avoid an extension of the waiting period. This is therefore very unlikely.

A second potential manipulation would be to interrupt the unemployment spell temporarily by inactivity, since this postpones the end of the waiting period. However, such a behavior is very unlikely to be intentional, because this can only make the individual older at the end of the waiting period and therefore can only delay the entitlement to UI. Note that in the analysis we in principle do not consider individuals that exit to inactivity beyond the moment of their exit, so that this should not be an issue for our analysis. However, as mentioned in Section 4.2, in the data temporary exits within a month are not recorded. To the extent that there are such temporary exits to inactivity this could slightly postpone the end of the waiting period for these individuals. Since we measure the age for all individuals after 9 months, this means that we measure the age for these individuals too early. Some individuals below the age threshold of 26 may therefore not be eligible to UI after the waiting period of 9 months and a few individuals may even have to wait 12 months before they become eligible, because these interruptions induce them to be older than 26 at the actual end of the waiting period. In the next section we will show that we indeed observe such “non-compliance” in the data. However, we will show that it is not very important and therefore does not threaten the validity of our analysis.

Even if manipulation of the forcing variable is very unlikely in our setting, we nevertheless include some graphical tests that aim at detecting manipulation in Appendix B. A first test is

based on the suggestion of McCrary (2008) is to check whether the number of individuals observed per age bin does exhibit a discontinuity at the age threshold of 26 (and neither at 25 years and 8 months). In addition, the relation of the explanatory variables to age should be smooth at the age cut-offs. We could not detect any discontinuities in these graphs.

Finally, we report in Appendix C placebo tests in which we repeat the analysis for the school-leavers who registered for the first time as job seeker at the Flemish PES in 2012, year in which the waiting period is 12 months irrespectively of age. The treatment effects are never found to be statistically significant from zero.

6. The Empirical Findings

6.1 Discontinuities in the Timing of Benefit Receipt and in the Participation in the YWP

Figure 2 displays how UI benefit receipt varies over age at various unemployment durations. By construction nobody is entitled to UI before 9 months, because the waiting period is ongoing. Let us first consider panel B. From 9 months the benefit receipt rate jumps up for those younger than 26 to 40-60%, depending on the specific age. Not everyone is entitled, because of slight measurement errors, in particular potentially due to brief interruptions of inactivity (see previous section). Compatible with this interpretation, the receipt rate increases further to more than 80% after 10 months and to 90-100% in month 11. For those older than 26 a similar pattern is observed from the moment their waiting period has come to an end, i.e. after 12 months (Panel B). The benefit receipt rates are more unstable for this group, but this is a consequence of the small numbers involved: only 5% on average are unemployed for 12 months or more (Table 2) and the sample size of the older group is much smaller.

From Figure 2 we can conclude that there is a clear discontinuity in the waiting period before entitlement to UI at the age of 26. Even if, as a consequence of measurement error, the RDD is not completely sharp and the treatment effects must be, hence, interpreted as “intention-to-treat” effects, we therefore expect that if the differential waiting period has an impact on job search behaviour, this should show up in a discontinuity in the unemployment duration and, possibly, in the selected indicators of employment quality.

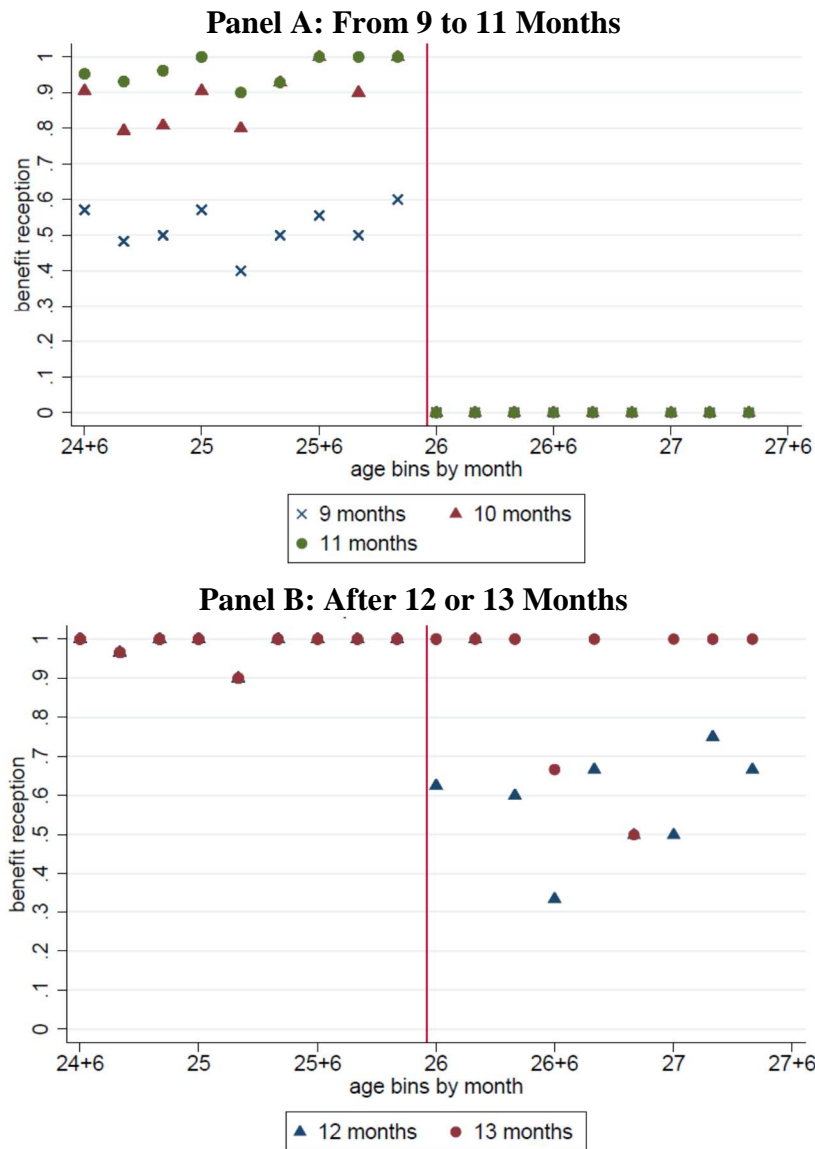
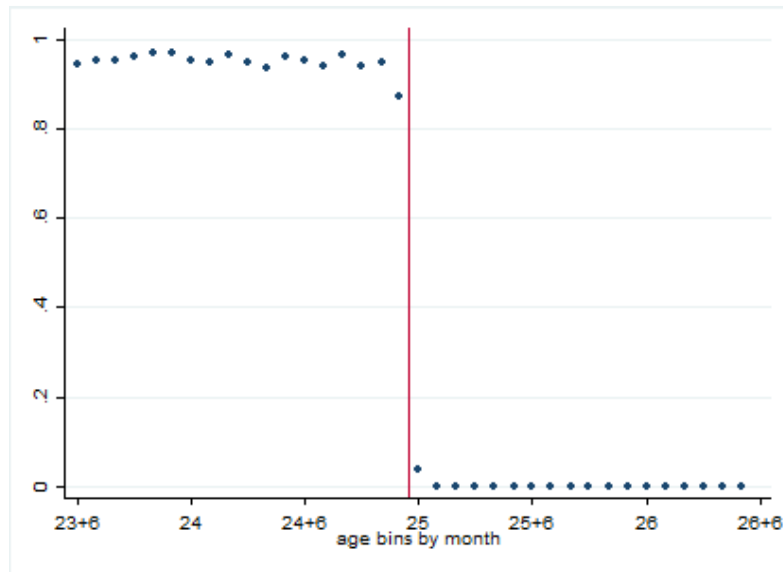
Figure 2: UI Benefit Receipt at Various Unemployment Durations by Age

Figure 3 displays the evolution of fraction of individuals that are labelled in the YWP by age, as measured one month after the registration as job seeker.²² The fraction labelled drops clearly sharply at the age of 25. This justifies the inclusion of a second discontinuity point in the analysis at 25 years and 8 months, if age is measured at the potential start of benefit receipt, i.e. 9 months after registration as job seeker.

²² Note that this analysis is based on a larger dataset, since the participation indicator to the YWP is not available in the dataset that was matched to the BCSS. Hence, we could not exclude individuals who experienced employment prior to registration as job seeker, neither could we exclude individuals who were entitled to UI prior to the end of the waiting period and, hence, could not be school-leavers. Individuals who registered in 2008 are excluded from this analysis, because the YWP was then not yet implemented for the high-educated.

Figure 3: Evolution of the fraction labelled in the YWP by age



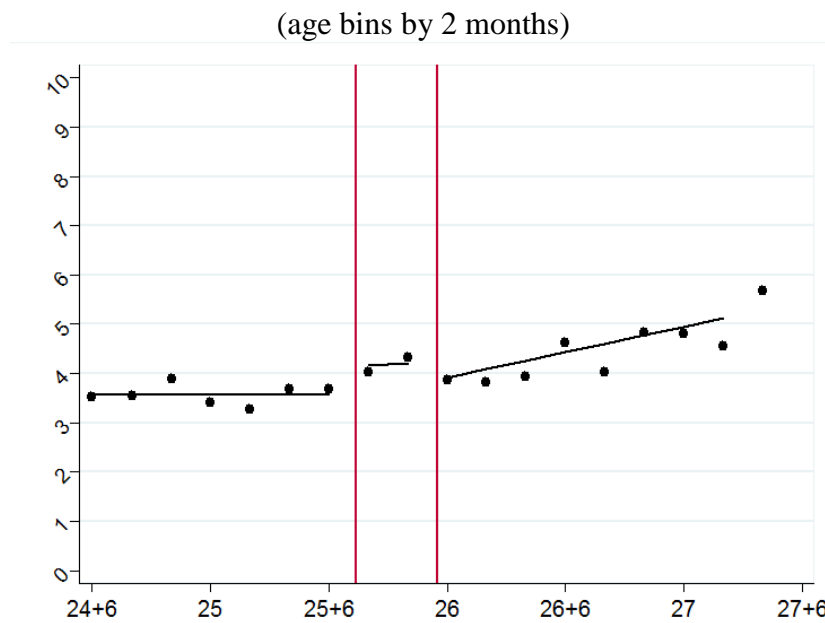
6.2 The Effects on Unemployment Duration

Figure 4 displays the unemployment duration as a function of age where dots represent the averages by age bins of 2 months. As mentioned in the section describing the data, older school-leavers are more likely to have repeated grades and are, hence, less attractive for potential employers. This is reflected in an increasing relationship of unemployment duration with age. However, despite the clear discontinuity in benefit receipt at age 26 and of participation in the YWP at 25 years and 8 months, the unemployment duration only drops slightly at 26 and evolves very smoothly at 25 years and 8 months. This means that benefit extension seems to have only a slight impact on job search behaviour, if any, while the YWP not at all.

The graphical evidence is quite salient. The formal econometric analysis just confirms this evidence. Table 3 summarize the findings of the econometric estimations of the linear regression model (1) and the discrete hazard model (2). In all of them the polynomial in age is specified as a linear spline. The first four columns of the first row report the estimated benchmark treatment effects (and associated standard errors) of the extension of the waiting period ($\hat{\beta}_W$) and of the YWP ($\hat{\beta}_{YWP}$) obtained by estimating the linear regression model (1). The first two columns consider the (log) unemployment duration to any exit destination, while in the two subsequent columns the analysis is restricted to (log) durations ending in employment. In the first analysis 12 individuals are right censored at the end of the observation period and in the second, in addition, 396 observations with exits to inactivity are

excluded from this analysis. The last two columns of Table 3 presents the results of interest for the discrete hazard model. The coefficients are reported in exponential form, so that they can be interpreted as multipliers of the hazard. In column (5) we report the findings of the model that assumes a constant proportional treatment effect throughout the unemployment spell. Column (6) shows the results for the model that allows a different treatment effect of the extension of the waiting period in months 2 to 8, 9 to 12 and beyond 12 months.

Figure 4: Unemployment Duration by Age



Note: 0.24% of the full sample are right censored observations and, hence, dropped.

In line with expectations of job search theory, the extension of the waiting period is found to decrease unemployment duration. However, the extension by 3 months reduces the unemployment duration of a 26 year old job seeker by 0.13 months (column (1)) or 1.1% (column (2)) only, and is not statistically different from zero. The effect of the YWP is positive for both the linear and the log-linear model and even closer to zero than the effects of an extension of the waiting period. In Appendix D we present the complete estimation results of these benchmark models.

The third (and fourth) column presents the results of the same model in which we replace the outcome of interest by the (log-) unemployment duration ending in a transition to employment. The findings hardly alter, because only very few job seekers leave unemployment to inactivity (just 396 out of 5,483). The discrete hazard model in column (5) that allows for a correct treatment of right censoring and exits to inactivity displays slightly larger proportional effect of the extension on the hazard (+4.4%) and a small negative effect

of the YWP ($-4.7\% = (1-0.953)*100$), but these effects are again not statistically significant. Finally, the findings reported in column (6) shows some non-monotonic variation of the extension of the waiting period in the considered sub-periods, but the treatment effects are never significantly different from one. This insignificance could be the consequence of lack of precision induced by the double age discontinuity, one at 25 years and 8 months and one at 26 years. Before providing any interpretation of these potential treatment effects we consider their effects on some indicators of quality of employment.

Table 3: Estimation Results for Unemployment Duration and Transitions to Employment as Outcomes of Interest

	Any Exit		Exit to employment				
Coef.	(1) Linear	(2) Log-linear	(3) Linear	(4) Log-linear	Exp. (Coef.)	(5) Hazard	(6) Hazard
β_W	-0.126 (0.283)	-0.011 (0.049)	-0.156 (0.293)	-0.012 (0.051)	$\exp(\beta_W)$	1.044 (0.088)	-
	-	-	-	-	$\exp(\beta_{W_{2-9}})$	-	1.047 (0.088)
	-	-	-	-	$\exp(\beta_{W_{10-12}})$	-	0.921 (0.183)
	-	-	-	-	$\exp(\beta_{W_{13-\infty}})$	-	1.180 (0.232)
β_{YWP}	0.122 (0.243)	0.003 (0.041)	0.049 (0.246)	-0.001 (0.042)	$\exp(\beta_{YWP})$	0.953 (0.067)	0.953 (0.066)
					Variance heterogeneity	0.635*** (0.082)	0.631*** (0.082)
N	5,483	5,483	5,087	5,087	N	5,495	5,495
R^2	0.043	0.047	0.041	0.047	log-likelihood	-11338.2	-11337.6

Notes: Heteroskedastic robust standard errors between parentheses. All models include the control variables mentioned in Table 1 (except for the equivalent household income) and a linear spline in age. In the (log-) linear models right-censored observations are dropped: 12 observations in case of exits to any destination reported in the first two columns; an additional 396 individuals who leave from unemployment to inactivity in case duration until exit to employment is considered. In the hazard models the aforementioned dropped observations are right censored. "Variance heterogeneity" is the variance of the Normal mixing distribution of the unobserved heterogeneity. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

6.3 The Effects on the Quality of Employment

Figure 5 displays the evolution over age of various indicators of the quality of employment found after the first unemployment spell: the log number of working days in the quarter of exit and the 4 subsequent quarters, the fraction of individuals working part-time, the (possibly corrected) log daily wage at the end of the quarter of exit, the log of annual earnings in the quarter of exit and the 4 subsequent quarters. The daily wage (corrected or not) and the indicator of part-time work evolve very smoothly at the two age discontinuities. This provides convincing evidence that these outcomes are not influenced by the extension of the waiting period or by the YWP. For the number of working days and the earnings the evidence is less

clear. In both variables²³ the age bin at 25 years and 10/11 months displays an upward shift, while at 25 and 8/9 months there is no discontinuity at all to the left, i.e. age discontinuity point of the YWP. This suggests, if any, a negative effect on days worked induced by the benefit extension.

Figure 5: Indicators of Quality of Employment by Age
(age bins by 2 months)

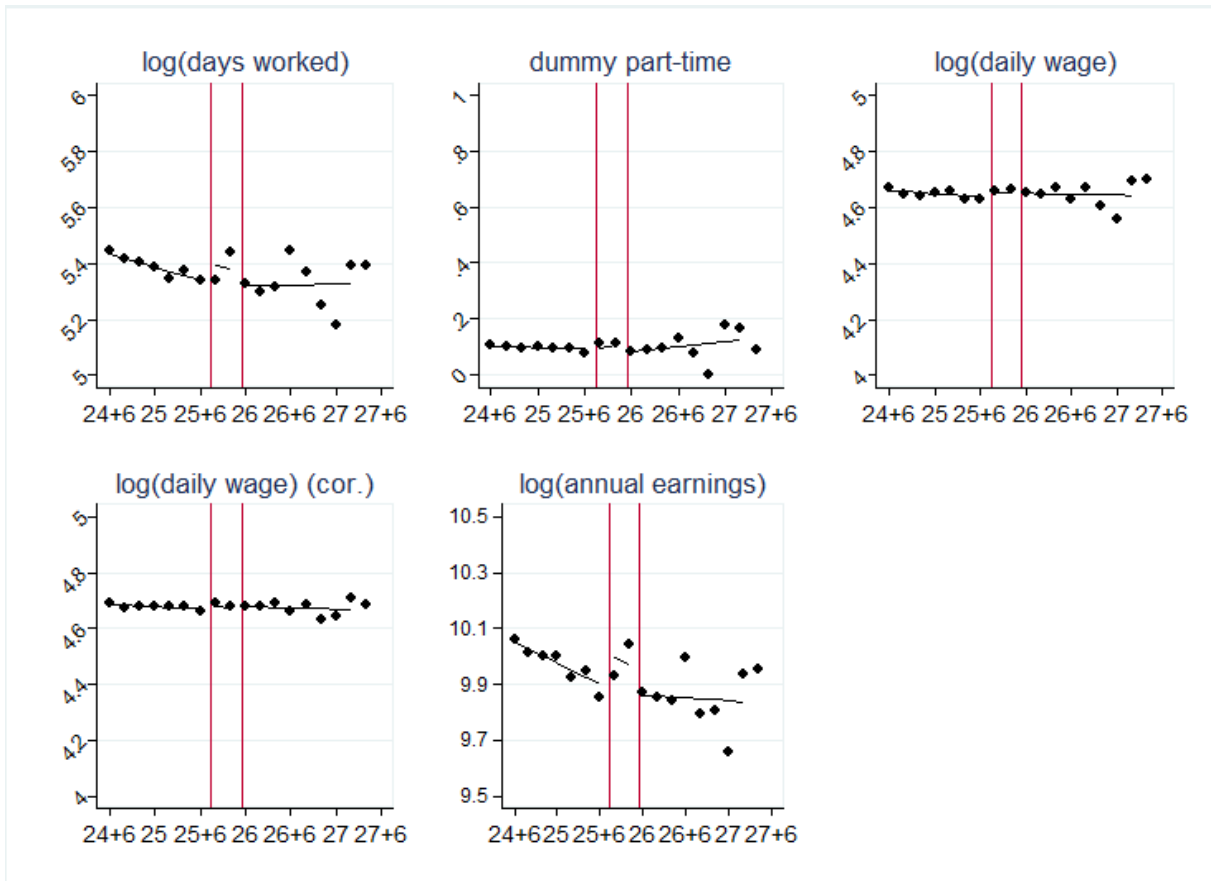


Table 4 presents, based on the linear regression model (1), the formal econometric estimates of the parameters of interest for the aforementioned outcomes. Since these outcomes are only measured for individuals in *salaried* employment for whom there is no missing value in the registers, we first check whether we should not be concerned by a sample selectivity at the age discontinuity points of interest (Heckman 1974). We therefore first ran a linear probability model specified as in equation (1), where the dependent variable is equal to one if the considered outcome has a non-missing value and is equal to zero otherwise. From these regressions we conclude that there is only a major concern for the non-corrected daily wage

²³ Since the daily wage does not display this shift and earnings is obtained as a product of wages and days worked, the shift in earnings just reflects the shift in hours worked.

for which we find very statistically significant negative effects at both age discontinuities. For the corrected wage variable, there seems to be much less of an issue. Furthermore, we plotted for each of the retained samples of each of the considered outcomes, the evolution of each of the explanatory variables against age and did not find evidence of any clear discontinuities.²⁴ Nevertheless, even if the outcomes of these tests are comforting, they are no formal proof of the absence of sample selectivity at the age discontinuities.

Table 4: Estimation Results on Employment Quality and Associated Selection Indicators

	Days worked		Dummy part time	Daily wage	
Coef.	Select D	Log-lin.	Lin.	Select D	Log-lin.
$\hat{\beta}_W$	-0.027 (0.018)	-0.035 (0.047)	-0.023 (0.019)	-0.071** (0.027)	0.003 (0.015)
$\hat{\beta}_{YWP}$	-0.006 (0.014)	-0.068* (0.039)	-0.008 (0.016)	-0.044* (0.022)	-0.010 (0.012)
N	5,495	4,785	4,785	5,495	4,259
R^2	0.017	0.024	0.053	0.018	0.075
	Daily wage (corrected)		Earnings		
Coef.	Select D	Log-lin.	Select D	Log-lin.	
$\hat{\beta}_W$	-0.037 (0.024)	0.004 (0.014)	-0.032 (0.020)	-0.055 (0.063)	
$\hat{\beta}_{YWP}$	-0.026 (0.019)	-0.005 (0.011)	-0.015 (0.016)	-0.054 (0.051)	
N	5,495	4,540	5,495	4,689	
R^2	0.020	0.072	0.022	0.036	

Notes: Heteroskedastic robust standard errors between parentheses. All models include control variables mentioned in Table 1 (apart from the equivalent household income) and a linear spline in age. *Select D* refers to an indicator that is equal to one if the associated outcome is observed and zero if it is missing. It therefore refers to a linear probability regression based on Equation (1) that aims at identifying whether there is any sample selectivity at the age discontinuity of 26. *Log-lin.* Refers to a log-linear regression of the associated indicator of employment quality based on equation (1). * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

In line with the graphical evidence, both policies have a small and statistically insignificant effect on the gross wage and on part-time employment. Also in line with the graphical evidence, the extension of the waiting period by three months is found to have a negative impact on hours worked of 3.5% and on earnings of 5.5%, but neither effect is statistically significant. Less in line with the graphical evidence, the YWP reduces the working time by 6.8% (p-value of 8.3%) and earnings by 5.4% (statistically insignificantly). The upward shift observed at the age bin at 25 years and 10/11 months seems to drive this negative effect.

In order to see whether the effects on working days and earnings are robust, we performed a

²⁴ These graphs can be obtained from the authors upon request.

couple of sensitivity analyses on these variables. These are reported in Table 5. We considered a more narrow window around the age discontinuity of 26 years (columns (1) and (5)); a spline at 25 years and 8 months, i.e. the age discontinuity of the YWP, capturing the effect of the waiting period by a dummy variable similar to how we captured the YWP (columns (2) and (6)); the linear in age specification as in the benchmark, but no spline (columns (3) and (7)); the specification as in the benchmark, but a quadratic function of age instead of spline. Even if the effects are now never statistically significant at the 10% level, the findings are nevertheless remarkably robust, especially for the YWP. Only if we consider the spline at the age discontinuity of the YWP or we do not consider any spline at all the effects of the extension of the waiting period is much smaller or even positive.

Table 5: Sensitivity Analysis for Days Worked and Annual Earnings

	Days worked				Annual earnings			
Coef.	(1) Age window	(2) Spline YWP	(3) No spline	(4) Quad. spline	(5) Age window	(6) Spline YWP	(7) No spline	(8) Quad. spline
β_w	-0.054 (0.054)	0.016 (0.061)	-0.015 (0.043)	-0.045 (0.047)	-0.069 (0.072)	-0.050 (0.085)	-0.035 (0.057)	-0.070 (0.064)
β_{YWP}	-0.069 (0.048)	-0.057 (0.054)	-0.058 (0.038)	-0.063 (0.038)	-0.048 (0.064)	-0.080 (0.069)	-0.044 (0.050)	-0.049 (0.049)
N	2,832	3,213	4,785	4,785	2,762	3,156	4,689	4,689
R^2	0.023	0.021	0.024	0.024	0.035	0.041	0.036	0.037

Notes: Heteroskedastic robust standard errors between parentheses. All models include control variables mentioned in Table 1 (apart from the equivalent household income) and a linear spline in age. All reported results are based on log-linear regressions of the associated indicator of employment quality as specified in equation (1). In columns (1) and (5) the specification is as in the benchmark, but the age window is narrowed down to 1 year to the left and right of the discontinuity at 26 years; in columns (2) and (6) the spline is set at the 25 years and 8 months (i.e. the discontinuity of the YWP) and the effect of the waiting period is captured by a dummy variable at 26 years; in columns (3) and (7) the specification is linear in age without any spline; in columns (4) and (8) the specification is quadratic in age without any spline. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

For the extension of the waiting period the sign of the effect is in line with standard job search theory in that job seekers respond by lowering the job acceptance requirements, although only with respect to the expected employment duration and with respect to the reservation wage. According to the point estimates of the various models this decreases earnings between 3.5% and 7%. Moreover, we can see in Table 3 that the job finding rate increases (statistically insignificantly) by 4.5%. This is of the same order of magnitude as the effect on earnings and, hence, suggests that this effect (if any) is not so much generated by more job offers, but rather by inducing unemployed youths to accept more lower quality jobs that, despite paying the same wage, last less long.

The significant negative impact of the YWP on days worked suggests that PES caseworkers

induced unemployed school-leavers to accept more temp jobs and/or fixed-term contracts than they would have done in the absence of the YWP. Caseworkers could have justified this strategy based on the argument that short-term jobs could be stepping stones to a more permanent job. However, the empirical evidence on the effectiveness of this strategy is mixed. For instance, stepping-stone effects have been found by Booth *et al.* (2002) in the UK, Ichino *et al.* (2008), de Graaf-Zijl *et al.* (2011) in the Netherlands and Cockx and Picchio (2012) in Belgium, while other researchers have found adverse effects, such as Güel and Petrongolo (2007) in Spain, and Autor and Houseman (2010) in the US. Givord and Wilner (2015) in a recent paper argue that these mixed findings may be a consequence of lumping together temp jobs and fixed term contracts when analyzing their effect. These authors find on French data that “although fixed-term contracts may provide a ‘stepping-stone’ to permanent positions, temporary agency work is hardly better than unemployment in this regard.”²⁵ We do not have any evidence on which type of advice caseworkers effectively transmitted to youths, but this succinct literature review reveals that our findings could be consistent with the existing empirical evidence.

The finding that the YWP reduced selectivity in job acceptance seems to contradict the finding reported in Table 3 that the YWP also, if anything, reduced the exit from unemployment to employment. An explanation is that the YWP also induced some school-leavers to participate in training and that this training temporarily “locked” these individuals into unemployment. This locking-in effect could have counterbalanced the positive effect of the reduced selectivity on the job finding rate.

Even if the aforementioned interpretations make sense, more firm conclusions require further research, because the impacts are so imprecisely estimated that they are consistent with the finding that the two considered policies did not generate any behavioral impact at all.

6.3 Treatment Heterogeneity

In Section 3 we argued that the extension of the waiting period might not induce that important effects on job search behaviour, because the parents might financially compensate

²⁵ This might not be the only explanation for the contradicting evidence. For instance, Garcia Perez *et al.* (2014) have recently found that the more intensive use of fixed-term contracts after the liberalization in 1984 in Spain has led, as we find here, to significantly less accumulated days of work and earnings. This suggests that the institutional environment may also play an important role. In line with this interpretation, Givord and Winter (2015) also argue that temporary work agencies are more used as a flexibility device, while fixed-term contracts are screening device. The role played by these different type of contracts may, however, depend on the institutional environment.

for the income loss. The fact that more than 80% of the school-leavers lived at their parent's house at the end of the calendar year preceding their first registration as job seeker at the PES (Table 1) suggests that this might be relevant. To investigate this issue further we split the sample in two groups. These groups consider unemployed school-leavers for whom the equivalent income from labour income and social security allowances of other household members was either below or above the median.

Table 6: Heterogeneous Effects by Equivalent Household Income

	Log duration in unemployment		Days worked	
Coef.	Income \leq median	Income $>$ median	Income \leq median	Income $>$ median
$\hat{\beta}_W$	0.018 (0.071)	-0.074 (0.069)	-0.065 (0.069)	-0.032 (0.064)
$\hat{\beta}_{YWP}$	0.003 (0.061)	0.002 (0.057)	-0.123** (0.055)	-0.032 (0.057)
<i>N</i>	2,705	2,704	2,361	2,358
<i>R</i> ²	0.059	0.045	0.036	0.023
	Dummy part-time		Daily wage	
Coef.	Income \leq median	Income $>$ median	Income \leq median	Income $>$ median
$\hat{\beta}_W$	-0.027 (0.027)	-0.026 (0.028)	0.023 (0.021)	-0.015 (0.022)
$\hat{\beta}_{YWP}$	-0.014 (0.023)	0.004 (0.024)	-0.003 (0.017)	-0.016 (0.017)
<i>N</i>	2,361	2,358	2,104	2,099
<i>R</i> ²	0.045	0.070	0.074	0.081
	Daily wage (cor.)		Earnings	
Coef.	Income \leq median	Income $>$ median	Income \leq median	Income $>$ median
$\hat{\beta}_W$	0.008 (0.020)	-0.006 (0.020)	-0.055 (0.089)	-0.079 (0.091)
$\hat{\beta}_{YWP}$	0.003 (0.016)	-0.018 (0.016)	-0.077 (0.071)	-0.051 (0.076)
<i>N</i>	2,238	2,239	2,307	2,318
<i>R</i> ²	0.067	0.081	0.049	0.031

In the first two columns of the first panel of Table 6, we report the findings of this analysis for the benchmark outcome, i.e. log unemployment duration with exits to all destinations. For this outcome variable the effect of an extension of the waiting period is qualitatively the reverse of what we would have expected. The effect on unemployment duration is more negative for youths living in households with high equivalent income. Standard errors are, however, again

very large, so that no firm conclusion can be drawn. The two first columns of the subsequent panel report the effects on days worked. In this case the effect goes in the expected direction. A longer waiting period induces low-income-youths to accept jobs that reduce working time by 6.5%, while this reduction is only 3.2% for high-income-youths. While these effects are not significantly different from zero, the impact of the YWP for the low-income group is -12.3% and significant at the 5% level, in contrast to the -3.2% statistically insignificant effect for the high-income group. Unfortunately, this ordering of the effects according to income does not remain as clear if effect on earnings (the last panel of Table 6) is considered. As in the benchmark case, the effects on daily wage and on part-time work are small and no clear heterogeneity in the effect can be detected. To conclude, the evidence partially confirms that low-income youths are more likely than high-income youths to accept short-term jobs in response to the considered policy changes. The evidence does not confirm, however, that this translates in shorter unemployment spells for this group.

7. Conclusion

In this research we exploited two policy discontinuities at two age cut-offs (at 26 years and 25 years and 8 months) to evaluate by a Regression Discontinuity Design (RDD) the effects of two active labour market policies targeted to youth on the transition rate from unemployment to employment and on the quality of this employment. The first policy consisted in an extension of the waiting period from 9 to 12 months that was imposed on Belgian school-leavers before they were entitled to UI. The second was the Youth Work Plan (YWP) which aimed at providing more intensive counselling and training to young job seekers earlier on in their unemployment spell than to older job seekers. In order to avoid that the estimated treatment effect would be confounded by a wage subsidy programme targeted to low educated youth, the analysis was restricted to youths who recently graduated from a bachelor's or master's degree.

The study finds that such an extension of the waiting period slightly, but statistically insignificantly, increased the transition rate to employment. We argued that a potential explanation of this small impact could be that these youths were not much financially constrained by this extension, because most of them would still be financially dependent on their parents' income and, hence, not experience an effective drop in income. However, we did not find supporting evidence for this hypothesis, since, if the analysis was conducted separately for the group with equivalent household income above and below the median, it

was found that the positive impact on the transition rate to employment was more driven by the group with high income, i.e. for the group that was the *least* financially constrained. The precision of these estimated effects was low, suggesting that there was not sufficient power to test this hypothesis.

Another potential explanation of this weak impact could be that these youths form biased or non-rational expectations that could make them less responsive to future incentives (Spinnewijn 2015; Della Vigna and Paserman 2008). Even if these elements could play a role, the analysis nevertheless finds that future incentives do affect job *acceptance* behaviour. While the extension of the waiting period did not affect the level of the accepted wage, there we did find some suggestive, but robust evidence that it did reduce the number of working days and, hence, earnings in the five quarters following exit from unemployment. This means that the extension of the waiting period induces young job seekers to accept more easily short-term job offers. In line with expectations these effects were also found to be larger for youths living in poorer households.

The YWP did not have any significant positive impact on the exit rate from unemployment to employment. However, as for the extension of the waiting period, it did robustly reduce the number of working days by about 6-7%, while leaving the wage unaffected. For youths living in households with below median equivalent income this working time fell even by about 12% and was significant at the 5% level. The effect on earnings was also negative, although slightly smaller and never statistically significant. An explanation for these findings is that PES caseworkers advised young unemployed graduates to accept more temp jobs and fixed-term contracts potentially arguing that these could be stepping stones to a permanent job. However, our findings point that the stepping stone hypothesis should be refuted as the number of working days within the first 5 quarters in the labour market were actually reduced. The fact that the YWP induced some school-leavers to participate in training may explain why these lower aspirations in the job acceptance behaviour did not enhance the job finding rate. For, it is well known that training temporarily “locks-in” participants into unemployment and that the effects of training may only realize in the longer run. This locking-in effect could have counterbalanced the positive effect of the reduced selectivity on the job finding rate.

Overall, even if the findings did not deliver many statistically significant estimates of the treatment effects, we nevertheless can formulate some policy conclusions. First, our analysis revealed that an extension of the waiting period either did not enhance much the transition

rate to employment or, if it did, it did so at the cost of reduced working time and, hence, earnings. This suggests that threatening with a sanction is not the right method to activate youth and that supportive measures might work better. However, the Youth Work Plan (YWP) is precisely offering this kind of support and our analysis revealed that this approach produced very similar, if any, effects as the one that involves a financial sanction. Part of the explanation is that caseworkers might have given misleading advice that short-term or temp jobs are stepping stones to long-term employment, while we have seen that there is no univocal empirical evidence to sustain this claim. However, currently we do not have firm evidence that caseworkers provided such advice, so that this conclusion is speculative. Another reason is that the Flemish YWP was not sufficiently intensive. As mentioned, experimental evidence in Denmark suggests that very intensive (fortnightly) meetings with caseworkers can generate significantly positive effects on the job-finding rate.

Finally, even if the RDD approach followed in this research is generally a very convincing and powerful method to identify causal effects, we faced a number of limitations in the implementation of this method. First, we were confronted with two policies the participation in which was delineated by two sharp age cut-offs which were only 4 months apart. This sizeably reduced the width of the age window to detect a corresponding discontinuity in behaviour. Second, even if we had access to population data, the size of the population close to the age thresholds was very much reduced and hereby also the statistical power of the estimator. Third, related to the previous point in order to exploit the RDD, we were forced to restrict our analysis to a quite specific sub-population of unemployed school-leavers. These were required to have a bachelor's or master's degree and have accumulated substantial (at least two years) schooling delay to be observed at the aforementioned age thresholds. Consequently, this questions the external validity of the estimated treatment effects and calls for further research to investigate whether the reported findings are representative for the whole population of unemployed school-leavers.

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Appendix

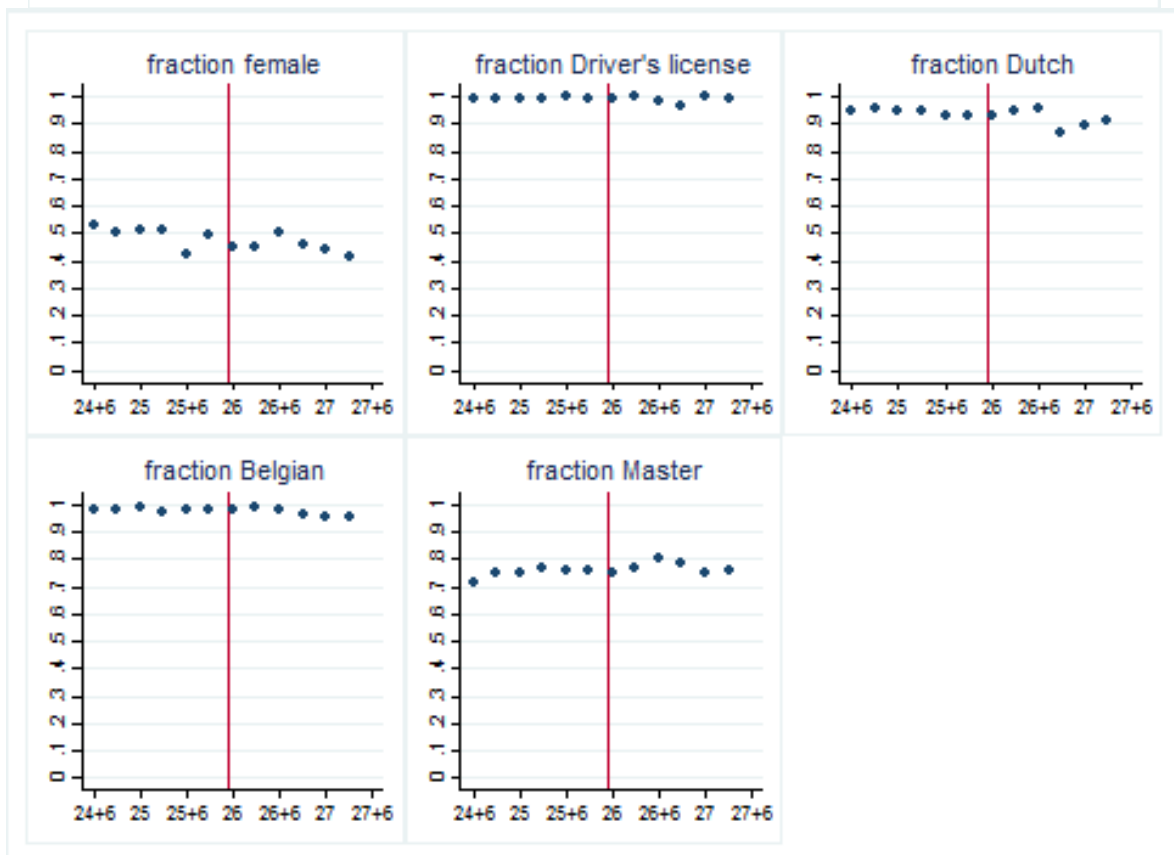
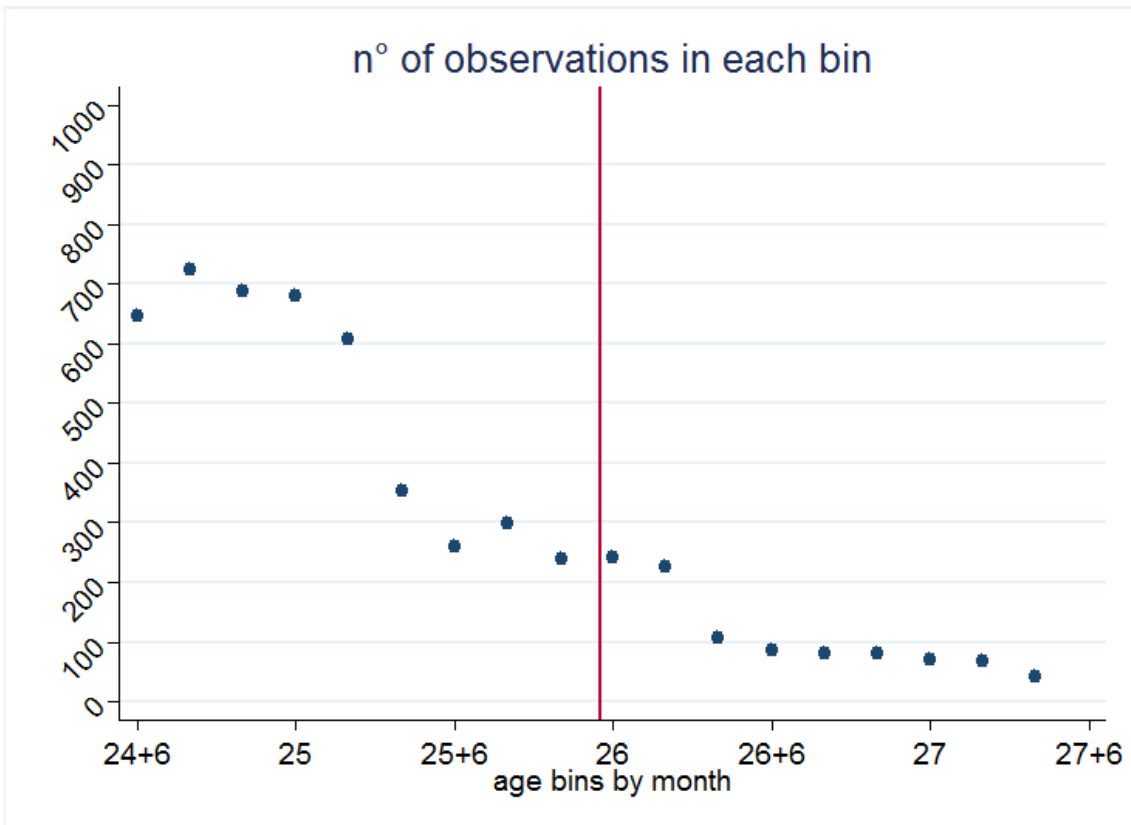
Appendix A: Reduction of Sample Size after Imposition of Selection Criteria

The initial population consists of 151,744 individuals. We make consecutively the following selections:

- Delete individuals with the onset of unemployment at a different date than the first registration: 197 individuals (0.13%); observations left: 151,547;
- Delete individuals entering unemployment in 2011, 2012 and 2013: 76,716 individuals (50.62%); observations left: 74,831;
- Delete individuals with at most a secondary education degree: 43,150 individuals (57.66%); observations left: 31,681;
- Delete individuals who have worked prior to the onset of the waiting period: 9,003 individuals (28.42%); observations left: 22,678;
- Delete individuals finding a job according to the PES, but not found to be employed in the corresponding quarter in the social security files: 432 individuals (1.90%); observations left: 22,246;
- Delete individuals who receive UI benefit before the end of the waiting period: 80 individuals (0.36%); observations left: 22,166;
- Retain individuals within an age window of 3 years around the age discontinuity at 26 years (1.5 years to the left and to the right): 16,479 individuals deleted to the left of the discontinuity (74.34%) and 192 individuals to the right of the discontinuity (3.38%); observations left: 5,495.

The final sample for analysis consists of 5,495 individuals.

Appendix B: Graphical Tests to Detect Manipulation of the Forcing Variable



Appendix C: Placebo Test on First Registrations at the PES in 2012

Duration in unemployment							
Any Exit			Exit to employment				
Coef.	(1) Linear	(2) Log-linear	(3) Linear	(4) Log-linear	Exp. Coef.	(5) Hazard	(6) Hazard
β_W	-0.058 (0.424)	0.013 (0.097)	0.220 (0.426)	0.070 (0.100)	$\exp(\beta_W)$	1.060 (0.191)	-
	-	-	-	-	$\exp(\beta_{W2-9})$	-	1.040 (0.188)
	-	-	-	-	$\exp(\beta_{W10-12})$	-	1.076 (0.324)
	-	-	-	-	$\exp(\beta_{W13-\infty})$	-	1.679 (0.569)
β_{YWP}	-0.589 (0.389)	-0.182 (0.091)	-0.366 (0.394)	-0.130 (0.095)			
					Variance heterogeneity	0.807*** (0.172)	0.798** (0.174)
<i>N</i>	1826	1826	1676	1676	<i>N</i>	1950	1950
<i>R</i> ²	0.055	0.069	0.052	0.068	log-likelihood	-4208.10	-4206.90
Coef.	(7) Days worked		(8) Dummy parttime		(9) Daily wage	(10) Daily wage (cor.)	(6) Annual earnings
Exit to employment							
β_W	0.163 (0.106)		0.028 (0.041)		0.018 (0.027)	0.035 (0.027)	0.074 (0.134)
β_{YWP}	0.064 (0.098)		-0.039 (0.038)		0.034 (0.025)	0.031 (0.024)	0.052 (0.126)
<i>N</i>	1585		1585		1451	1515	1546
<i>R</i> ²	0.059		0.065		0.091	0.081	0.077

Notes: Heteroskedastic robust standard errors between parentheses. All models include the control variables mentioned in Table 1 (except for the equivalent household income) and a linear spline in age. In the (log-) linear models right-censored observations are dropped: 12 observations in case of exits to any destination reported in the first two columns; an additional 396 individuals who leave from unemployment to inactivity in case duration until exit to employment is considered. In the hazard models the aforementioned dropped observations are right censored. “Variance heterogeneity” is the variance of the Normal mixing distribution of the unobserved heterogeneity. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

Appendix D: Complete Estimation Results for the Benchmark Outcome

Unemployment duration is the benchmark outcome for our analysis. In this appendix we report the full estimation results (except for the region dummies and the year and monthly entry dummies) for both the linear regression model defined by Equation (1) and the hazard model defined by Equation (2). The full estimation results for the other outcomes can be obtained from the authors upon request.

Coef.	(1) Linear	(2) Log-linear	Exp. Coef.	(3) Hazard	(4) Hazard
β_W	-0.126 (0.283)	-0.108 (0.049)	$\exp(\beta_W)$	1.040 (0.083)	-
	-	-	$\exp(\beta_{W2-9})$	-	1.044 (0.084)
	-	-	$\exp(\beta_{W10-12})$	-	0.829 (0.154)
	-	-	$\exp(\beta_{W13-\infty})$	-	1.267 (0.234)
β_{YWP}	0.122 (0.243)	0.003 (0.041)	$\exp(\beta_{YWP})$	0.960 (0.064)	0.959 (0.063)
<i>Age</i>	0.001** (0.001)	0.001 (0.001)		1.000** (0.001)	1.000** (0.001)
<i>Age*</i> β_W	0.002 (0.001)	0.001 (0.001)		1.000 (0.001)	1.000 (0.001)
<i>Female</i>	-0.737*** (0.106)	-0.152*** (0.020)		1.323*** (0.047)	1.322*** (0.047)
<i>Dutch</i>	-0.789** (0.289)	-0.184*** (0.052)		1.365*** (0.116)	1.366*** (0.116)
<i>Bel</i>	-1.126* (0.580)	-0.176* (0.091)		1.344** (0.181)	1.343** (0.180)
<i>Driver's license Master</i>	-1.751 (1.708)	-0.035 (0.190)		1.163 (0.286)	1.166 (0.286)
	-0.097 (0.132)	0.029 (0.024)		0.977 (0.039)	0.977 (0.039)
<i>Family status</i>					
- <i>family</i>	1.966* (1.066)	0.291 (0.198)		0.509** (0.140)	0.510** (0.140)
- <i>single</i>	0.406 (0.285)	0.066 (0.057)		0.878 (0.093)	0.879 (0.093)
- <i>children</i>	1.041*** (0.128)	0.237*** (0.032)		0.634** (0.040)	0.635*** (0.040)
- <i>other</i>	0.437 (0.324)	0.072 (0.061)		0.820* (0.090)	0.822* (0.090)
<i>Region dummies</i>	YES	YES		YES	YES
<i>Entry dummies</i>	YES	YES		YES	YES
			λ_{2-3}	2.133*** (0.119)	2.124*** (0.118)
			λ_{4-5}	2.166*** (0.214)	2.148*** (0.212)
			λ_{6-8}	2.482*** (0.328)	2.454*** (0.324)
			λ_{9-11}	2.327***	2.419***

				(0.393)	(0.422)
			λ_{12+}	1.933**	1.791**
				(0.419)	(0.406)
<i>cst</i>	7.027***	1.287***		0.120***	0.120***
	(1.847)	(0.219)		(0.036)	(0.036)
			Variance heterogeneity	0.588***	0.581***
				(0.076)	(0.076)
<i>N</i>	5,483	5,483	<i>N</i>	5,495	5,495
<i>R</i> ²	0.043	0.047	log-likelihood	-11884.9	-11883.2

Notes: Heteroskedastic robust standard errors between parentheses. All models include the control variables mentioned in Table 1 (except for the equivalent household income) and a linear spline in age. In the (log-) linear models right-censored observations are dropped: 12 observations in case of exits to any destination reported in the first two columns; an additional 396 individuals who leave from unemployment to inactivity in case duration until exit to employment is considered. In the hazard models the aforementioned dropped observations are right censored. “Variance heterogeneity” is the variance of the Normal mixing distribution of the unobserved heterogeneity. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

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