Threat effects of monitoring job search. A discontinuity design

INCOMPLETE AND VERY PRELIMINARY

Bart Cockx* Muriel Dejemeppe†

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Abstract

Since July 2004 the job search effort of long-term unemployed benefit recipients is monitored. We exploit the discontinuity in the treatment assignment at the age of 30 to evaluate the effect of a notification sent at least 8 months beforehand. We find that after 8 months the threat of monitoring significantly increases transitions to employment by 7-8 percentage points. However, the induced employment is of low quality (short and low wage). In addition, in one of the two regions, women substitute sickness for unemployment benefits. In this region specific counseling for the notified group also significantly enhances participation in training.

Keywords[1]: evaluation, monitoring job-search, threat effect, regression-discontinuity, grouped data

JEL-Classification: J64, J65, J68, H43

*Ghent University (SHERPPA and Faculty of Economics and Business Administration), UCLouvain (IRES), IZA (Bonn) and CESIfo (Munich). E-mail: bart.cockx@ugent.be.

†UCLouvain (IRES). E-mail: dejemeppe@uclouvain.be. Corresponding address: SHERPPA, Tweekerkenstraat 2, B-9000 Gent, BELGIUM.

[1]This paper is a thorough revision of an earlier version ((Cockx and Dejemeppe, 2007)). In contrast to the previous paper, we can now identify the labor market states for which unemployment is left. In addition, the previous paper did not contain the methodological contribution relative to the grouped measurement of the running variable. Conclusions also differ in important respects.

1 Introduction

The Luxembourg European Council of November 1997 included active labor market policies as an essential ingredient of the Employment Guidelines set out for the member states of the European Union. By now it has become common knowledge that not all of these policies are equally effective in combating unemployment (see e.g. Martin and Grubb, 2001; Kluve, 2006). A number of studies have identified the monitoring of search behavior of the unemployed coupled with sanctions as a promising tool for enhancing the transition from unemployment to work (Meyer, 1995; Dolton and O'Neill, 1995, 1996; Gorter and Kalb, 1996; Blundell et al., 2004; Graversen and van Ours, 2008). Indeed, for risk averse workers, these policy instruments may deliver the right incentives by imposing less costs than alternatives, such as incomplete coverage or finite entitlement to unemployment benefits (Boone et al., 2001). However, since in these studies monitoring of search behavior was never offered in isolation of counseling, it is difficult to disentangle which policy drives the findings.

A number of recent studies challenge the view that monitoring of job search of unemployed workers is effective.[2] First, Crépon et al. (2006) find that a number of intensive job search assistance programs in France without any threat of sanction accelerate the transition to employment and, in particular, delay the re-entry in unemployment. This proves that monitoring is not a necessary condition for success. Second, it is clear that employment officers can only verify formal proofs of job search behavior. van den Berg and van der Klaauw (2006) show that, as a consequence, monitoring may substitute informal by formal job search and that the total job search intensity among workers predominantly using informal search channels may even decline. Thirdly, Manning (2008) shows that the imposition of stricter job search requirements does not enhance search incentives for all workers, since, if behavior is followed-up too closely, workers may find it too onerous to continue claiming benefits. As a consequence, these workers leave the claimant population and search less intensively. Finally, Klepinger et al. (1997, 2002) report the outcomes of a social experiment especially designed to evaluate the effect of alternative work-search requirements within Unemployment Insurance (UI) in the United States. This study confirms that monitoring may reduce the duration of benefit claim,[3] but that it neither speeds up the transition to employment nor has any impact on the level of subsequent earnings.

[2] This does not imply that the sanction associated to the monitoring isn't effective (see e.g. van den Berg et al., 2004; Abbring et al., 2005; van den Berg and van der Klaauw, 2005).

[3] Ashenfelter et al. (2005) find insignificant effects, but Klepinger et al. (2002, p.19) claim that the discrepancy with their conclusions is essentially caused by the smaller sample size of Ashenfelter et al.'s study, resulting in less precision.

Even if the monitoring activity as such does not promote the transition from unemployment to work, the *threat* of monitoring may do so. Indeed, if unemployed workers dislike being monitored, they will try to avoid it by searching harder for jobs before the employment officers start following-up their behavior. If so, we should regard this anticipatory effect as an integral part of the treatment effect of monitoring. In this paper we examine whether the new follow-up scheme enacted by the Belgian government in July 2004 entails important effects of this kind.

The threat effect of unemployment benefit exhaustion has been extensively studied in the literature (see e.g. van den Berg, 1990; Fredriksson and Holmlund, 2006). This literature provides convincing evidence that incentives to search for and to accept jobs increase significantly as unemployed workers approach the moment at which benefits expire. Only recently researchers have started to investigate whether the prospect of mandatory participation to active labor market policies have a similar impact on the behavior of unemployed workers (Black et al., 2003; Rosholm and Svarer, 2004; Geerdsen, 2006; Geerdsen and Holm, 2007; Graversen and van Ours, 2008; Forslund and Nordström Skans, 2006; Hägglund, 2006). They have shown that these threat effects can be as large as those resulting from a finite entitlement of unemployment benefits and that they may form a major share of the total impact on the return to employment. A major concern remains, however. The threat of participation may accelerate the transition to work at the expense of the quality of the job. van Ours and Vodopivec (2006) report that shortening the potential duration of unemployment benefits in Slovenia did not affect the contract type (temporary versus permanent), neither the employment duration nor the wage of the post-unemployment job. In this study we only have very partial information regarding the quality of the job. Nevertheless, we find some evidence that the threat of monitoring induces workers to accept lower quality jobs, but that this negative effect can be undone if these workers are appropriately counseled.

We estimate the impact of the threat of monitoring on the probability of employment on the basis of a regression-discontinuity (RD) analysis (Campbell, 1969; Hahn et al., 2001). This approach is appealing, since it allows us, as in an experimental setting, to identify the treatment effect under very weak assumptions. We can follow this approach, because the Belgian government phased in the monitoring scheme gradually according to age group. Between June 2004 and June 2005 only unemployed workers younger than 30 years were obliged to participate in the new scheme. In this study we analyze the effect of the new scheme during this initial phase. This means that we exploit the discontinuity in the treatment assignment at the age of 30.

This research also contains two minor methodological contributions. First, we show how the RD analysis can accommodate to a grouped continuous running variable, such as age, which in this research is measured in monthly intervals. The analysis is very much related to related to (Lee and Card, 2008), who treat the case of a discrete running variable. Second, Lee and Card (2008) present a goodness-of-fit statistic to verify whether conventional least squares inference is appropriate. We generalize this statistic for the presence of specification errors.

The outline of the article is as follows. In the next section we describe the institutional setting and the features of the new monitoring scheme. In Section 3 we discuss the effects that can be expected from theory. Subsequently, we describe the data. Section 5 presents the estimation method. Section 6 reports the estimated threat effect of monitoring on various outcomes (transition to and quality of employment, participation in training, transition to sickness benefits and other out-of-the-labor market states). This section includes a discussion of several validity checks. A final section concludes.

2 Institutional Setting

Belgium is a federal state consisting of three language Communities (the French, the Flemish and a small German one) and of three Regions (Flanders, Wallonia and Brussels). In Flanders the official language is Dutch (Flemish), in Wallonia it is French and the large majority of the inhabitants of Brussels speak French, a minority Dutch or other languages.[4] Flanders is the most prosperous region. In 2004, the ILO-unemployment rate was 5.4% on average in Flanders, whereas it was as high as 12.0% in Wallonia and 15.7% in Brussels (Massant, 2005).

Within this institutional setting, Social Insurance, of which Unemployment Insurance (UI) is one division, is organized at the federal level. It is this federal institution which pays out unemployment benefits (UB) and issues sanctions if the unemployed worker does not comply to the rules. Assistance (counseling, various types of training and other activation measures) and intermediation services to unemployed workers is provided by Public Employment Services (PES) at the Regional level.[5]

The new monitoring and counseling scheme introduced by the Belgian government in July 2004 induced a major reform within (i) UI and (ii) the assistance for unemployed persons offered by the Regional PES. We will review these two changes in turn.

[4]Due to the presence of the institutions of the European Union and other international organizations, there are many foreigners living in Brussels.

[5]VDAB in Flanders, FOREM in Wallonia and ACTIRIS in Brussels

2.1 Unemployment Insurance

In Belgium a worker is entitled to unemployment benefits in two instances:[6] (i) after graduation from school conditional on a waiting period of 9 months.[7]; (ii) after involuntary dismissal conditional on having contributed sufficiently long[8] to the UI scheme during the employment spell. In contrast to many other countries, in principle in Belgium the entitlement to UI does not exhaust. School-leavers are entitled to flat rate benefits ranging between 340€ /month and 890€ /month[9], depending on the household type (cohabitant, single or head of household). Dismissed workers earn a replacement rate of 60% or 55%, depending on whether one is head of a houshold or not. However, the benefit level is bracketed between 575€ /month and 1,070€ /month, so that the replacement rate can be higher or lower. In addition, after a year the replacement rate for singles and cohabitants drops to respectively 50% and 40% after a year and three months later the benefit level of cohabitants decreases further to a flat rate of 405€ /month. In 2005, roughly one quarter of the claimants were entitled to benefits as school-leaver. Among these persons 35% were head of household and 44% cohabiting (RVA, 2006b).

Before July 2004, the benefits could be withdrawn (temporarily or permanently) for two main reasons. First, sanctions could be imposed for not complying to administrative rules: (i) making a false declaration (e.g. with regards the household type or an undeclared employment relationship) or (ii) being unavailable for the labor market (not registered as a job searcher at the Regional PES, not turning up at a meeting summoned by a caseworker, refusing a "suitable" job offer or refusing participation in an activation or training program, etc.). Before the reform in 2004, roughly 80% of the monitoring reports regarding availability concerned not turning up at a meeting to which one is summoned (RVA, 2006a, pp. 72). Moreover, job search was not monitored.

Second, Article 80 of the UI legislation imposed a finite entitlement (between 24 and 99 months) to UI for cohabitants whose household income was not too low (Cockx and Ries, 2004). Since the large majority of cohabitants are women, the scheme has been criticized for being implicitly discriminatory. This was the main reason why the Belgian government decided to abolish Article 80 at the same pace as the new monitoring scheme was phased in, i.e. gradually according to age-group.

In July 2004, the federal government chose to replace Article 80 with a new, “fairer system

[6]See www.onem.be for more information.

[7]This waiting period lasts only 6 months for those aged less than 18 years and 12 months for youth between 26 and 30.

[8]The length of the contribution period depends on age.

[9]These figures and those below refer to October 2006

which could, at the same time, guarantee the viability of an UI system in which benefits do not expire". Two main features characterize the new system.

First, the new cooperation agreement reached on April 30, 2004 between the Federal State, the Communities and the Regions resulted in a much *more systematic* and *electronic* exchange of data in regard to the availability for the labor market between the Regional PES and the federal UA: Before 2004, this information transmission did not occur very systematically and was based on paper files. Moreover, the Walloon PES were not very collaborative and transmitted hardly any information, this in contrast to Flanders and to Brussels. This improved transmission of information was essential for the Federal unemployment agency's capacity to issue effective sanctions, since only the Regional PES can verify whether a worker is available for the labor market.

Second and more importantly, the reform assigns to the federal UA the competence to monitor, within a number of meetings, the *effort* that claimants devote to job search. Consequently, the capacity of the agency to sanction workers no longer depends exclusively on the information transmitted by the Regional PES.

The monitoring procedure consists in several steps[10]: a notification and up to three meetings with a caseworker in which search effort is verified. If search effort at the first meeting is deemed insufficient an action plan is imposed. If the worker does not fulfill the plan, a second, stricter action plan is enforced and benefits are temporarily (possibly partially) withdrawn. If at the third meeting the worker does not comply, benefits are completely withdrawn and the worker can regain entitlement only after being full-time employed during at least one year.

The timing of these steps is as follows (see Figure 1). A notification letter stating the job search requirements and explaining the different steps in the monitoring procedure is sent after 7 months of unemployment for those under the age of 25 and after 13 months for those older than 25. At least 8 months later the worker is summoned to the first meeting. If search effort evaluated negatively two additional meeting may take place with intervening periods of at least four months. By contrast, if search effort is deemed sufficient, the next monitoring of search effort will only take place 12 to 16 months later. This contrasts quite starkly with the frequency of monitoring in many other countries: half of OECD countries require reporting of job search (in most cases) every two weeks or at least monthly (OECD, 2007).

Job search effort is evaluated on the basis of proofs delivered by the unemployed worker

[10] See Cockx et al. (2007) for more details

(copies of letters of application, registration in temporary help agencies, proofs of participation to selection procedures, etc.). These requirements are not, however, precisely defined in the rules, so that the outcome of evaluation partly depends on the discretion of the caseworker. This discretion together with the fact that in the beginning phase of the reform no information was available on which proofs would be regarded sufficient could partly explain why such a relatively loose monitoring scheme from an international perspective could cause the important threat effects reported in the empirical analysis.

To cope with capacity problems, the new program was gradually phased in according to age. In the first year, only workers younger than 30 years were contacted. In the second year, starting in July 2005, the target group was enlarged to those younger than 40 and, in the third year, those between 40 and 50 years old were included. Claimants older than 50 are not targeted.

The gradual phasing-in resulted during the first years in discontinuous relationships between the age and the program participation. In the empirical analysis we exploit the discontinuity at the age of 30 years to identify the threat effect induced by the the above-mentioned notification letter. (see Section 4).

2.2 Assistance

The reform in July 2004 did not only reinforce the "stick", it also enhanced the "carrot". Indeed, the Regional PES significantly increased their assistance to unemployed workers. In all three regions, the supply of counseling and training programs has risen importantly (Cockx et al., 2007, pp. 26-52). In addition, the reform was seized upon as an opportunity to move closer to the recommendations described in the first European guideline for employment. This recommends, on the one hand, a preventive approach aimed at activating all unemployed persons at an early stage in their period of unemployment and, on the other hand, a curative approach aimed at systematically directing the long-term unemployed towards appropriate actions that promote their re-employment. Before 2004, the Walloon and Brussels' regional PES offered a preventive approach to low-skilled *youth* only. In contrast, the Flemish PES introduced the preventive approach already in 1999 and this for all unemployed job-seekers. Since 2004, Wallonia and Brussels expanded the preventive approach to adults and all three regional PES introduced a curative approach, previously non-existent.

Since 2004, the assistance provided to unemployed workers is structured in a similar way in the three regions. It starts with an individual intake meeting with a caseworker in which a mandatory action plan is proposed. The action plan may (but need not) consist in the participation in (a sequence of) counseling and/or training programs. Participation in the plan is mandatory and refusal is notified to the federal UA that may impose a sanction on this basis. The curative approach differs from the preventive one in that a collective information meeting precedes the individual intake.

Regional differences concern the target group and timing of the intervention. We ignore Brussels in this comparison, since due to the limited sample size we could not include this region in the empirical evaluation. Moreover, we focus on the curative actions, since only these relate to the the estimated treatment effects.

In Wallonia, the unemployed are summoned to the first collective information meeting within two months after dispatch of the notification letter by the federal UA, i.e. after 7 and 13 months of unemployment respectively for individuals younger and older than 25 years. The new assistance scheme was phased-in according to age-groups at exactly the same pace as the new monitoring scheme. This means that until June 2005 only those below the age of 30 participated. It implies that the RD design used in this research identifies for Wallonia not only the impact of the threat effect of the notification letter, but also the participation in the regional counseling scheme.

In Flanders, the first collective meeting takes place close to the moment at which the first interview within the new monitoring procedure takes place, i.e. after 15 and 21 months, respectively for youth aged less than 25 years and older individuals. However, it is only offered to unemployed workers who did not receive any counseling in the preceding two years. Consequently, it concerns only a small group of workers that for some reason was not treated by the preventive approach. More importantly, in 2004 this meeting did not only take place for those younger than 30 years, but also for those who were older. This means that the discontinuity at 30 years did not concern the counseling that was offered in Flanders: in Flanders the discontinuity can only reveal the threat effect of monitoring.

3 What Does Theory Predict?

Job search theory (see e.g. Mortensen, 1986; van den Berg, 1990) predicts that benefit claimants will modify their job-search intensity and acceptance behavior from the moment that they are informed of a future event that affects their welfare. Consequently, to avoid a sudden drop in their welfare induced by an intensified monitoring of job-search behavior, claimants should, from the moment of notification, accelerate their transition from unemployment to work.[11]. The empirical findings reported below for Flanders - where the notified group did not receive any specific counseling or assistance - match this prediction.

Counseling can enhance or cancel the threat effect of the notification. In Wallonia, the French speaking region in the south of Belgium, the unemployed workers are invited within two months of the notification for a collective information session. Subsequently, a caseworker of the regional Employment Office counsels them personally on their job search strategy. If necessary, this is followed-up with further counseling and with participation in various types of training. Since counseling increases the effectiveness of job search, we expect it to reinforce the threat effect of the notification (see the Appendix in Cockx and Dejemeppe (2007)).
TO BE COMPLETED

4 The Data

We exploit administrative data from several sources: (i) the federal UA informs us monthly about unemployment benefit claims, the new monitoring procedure and the return to regular education; (ii) the regional PES delivered information about participation in training and job search assistance provided to the unemployed; (iii) the Crossroads Bank for Social Security[12] matches the above information to records of all federal Social Security institutions, which allowed us to construct monthly indicators of employment (including self-employment) and starting wage for salaried employment, of sickness insurance claims and a residual state (i.e. neither being employed or UI claimant). These data are available for all sampled individuals from January 2001 until the end of 2006.

4.1 Sample selection criteria and data limitations

The empirical analysis exploits the age-discontinuity in the assignment to treatment between the 1^{st} of July 2004 and the 30^{th} of June 2005: in that period only claimants of UI less than 30 years old were dispatched a notification of the monitoring procedure. The sample contains claimants of unemployment benefits, who on the 1^{st} of July 2004 were between 25 and 34

[11] The Appendix Cockx and Dejemeppe (2007) describes a simple non-stationary job search model with this feature

[12] See www.ksz-bcss.fgov.be/

years old and who became exactly 13 months entitled to UI between May 1 and August 31 of the same year[13]. For administrative reasons, the duration criterion (13 months) is determined on the basis of payments made two months before the notification is (theoretically) dispatched. This sample allows therefore to check whether claimants anticipate the notification: in that case the exit rate during these two months would differ between the treatment and control group (see SectionXX). To estimate the threat effect of monitoring, we retain only those workers claiming UI for at least one day during the second month after which unemployment duration attained 13 months, since only for these workers a notification was sent. This dispatch occurs between July and October 2004. The descriptive statistics reported in Table 1 refer to this sample.

The RD design only holds during one year. After this period unemployed workers older than 30 years start receiving a notification. Since at least 8 months elapses between the moment at which notification is dispatched and the first monitoring interview, we can only identify the threat effect up to 8 months after notification and not the effect of monitoring itself on these data: the control group of the October cohort will start being notified from the ninth month, i.e. from July 2005.

A second drawback of the data is that the discontinuity only applies to the "flow" of workers crossing the 13 months benefit entitlement threshold after the 1^{st} of May 2004. For the "stock" of workers, who had been entitled for more than 13 months at that date, the timing of the treatment evolved gradually with age *within* each age-group. This means that workers slightly younger than 30 years in July 2004 were only notified in June 2005 whereas those in the “stock” who were slightly older than 30 years already in the subsequent month, canceling thereby the potential discontinuity *between* age groups for those in the stock. This is unfortunate, since it reduces the sample size and limits therefore the power of the RD analysis (see Section XX).

A third limitation of the data is that the the sample was selected *ex post*, in April 2006. Since administrative mistakes were corrected meanwhile, these files were not exactly the same as the original ones used to determine to whom a notification is dispatched. In addition, the computer program used for this selection was updated meanwhile to fix some minor bugs. As a consequence, roughly 15% of the unemployed workers below the age of 30 selected in this way is not notified in the month in which they should have been according to the rules. The following month, however, this incompatibility already drops to 10%,[14] indicating that the incompatibility induced by this *ex post* selection was only minor. Moreover, on the basis of this *ex post* selection, we could ensure that the control group aged more than 30 was selected on exactly the same criteria as the treatment group. Nevertheless, as a consequence, the “sharp” RD design truns into a “fuzzy” one: only a fraction (95% on average) of the sam-

[13]Aside from the age and the unemployment duration, there exist a number of additional criteria that we ignore for the sake of not overloading the reader with details.

[14]Thereafter, the figure decliens more gradually

pled workers below the age of 30 years has effectively been treated. In sectionXX we discuss the methodological implications.

4.2 Descriptive statistics

Table 1 reports descriptive statistics of the sampled population theoretically eligible for the dispatch of the notification letter. In accordance with the subsequent analysis the statistics are reported separately for Flanders and Wallonia.[15] For each of these Regions, the first column refers to the sample of unemployed workers between 25 and 29 years old (the "treated") and the second to those aged between 30 and 34 (the "controls").

First observe that, in spite of having population data, the sampled population is not very large: roughly 2,500 individuals in both regions. This is related to the inability to include the "stock" of workers with an unemployment duration of more than 13 months on May 1, 2004. This relatively small sample size has some implications for the analysis, as discussed below.

For each of the above-mentioned groups Table 1 reports information with respect to a number of observed characteristics of the unemployed workers: the starting date of the observation window (July, August, September or October 2004), the age reported in years (but measured in months) on the 1^{st} of July 2004, the gender, the nationality, the level of education, the household-type determining the benefit level (head of household, single or cohabitant), the type of entitlement (school-leaver or work experience), an indicator of participation in training (including a return to regular education) or job-search assistance during the 12-month period prior to (and including) the sample selection date, an indicator of recent employment experience in the year before sample selection and the unemployment rate by district of living. For the continuous variables, we report the average and the standard deviation, for discrete variables the proportions. Time-varying variables are evaluated at the sampling date, i.e. two months prior to the (potential) dispatch of the notification letter.

On the basis of Table 1 we can deduce that the composition of the populations varies across both, regions and age groups. This is not surprising, since the observed characteristics are often correlated with the region or the age. In Wallonia, e.g., the fraction of foreigners is known to be higher than in Flanders. Younger workers are in general more educated and are more likely to be entitled to benefits as a school-leaver, since the probability of recently completing education and working is obviously respectively higher and lower than that for older workers. The correlation of the observed characteristics with age is not problematic,

[15] Recall that for reasons of too small a sample size, we did not include Brussels in the analysis

as long as there is no discontinuity at the age of 30 (see SectionXX).
A major fraction of the long-term unemployed workers has been employed during the year prior to the selection date: between 31% and 54% according to region and age group. This apparent contradiction is related to the administrative definition of unemployment duration within the monitoring procedure. In this definition the duration counter [16] is reset to zero only if the worker has been 12 months full time employed within the preceding 15 months. As a consequence, the sample does not only contain genuinely long-term unemployed workers, but also workers with unstable labor market careers. In the empirical analysis below it will be revealed that the treatment effect differs importantly between these two groups.

Finally, the last line of Table 1 reveals the major divide in the labor market conditions between the northern and southern regions in Belgium. Even if the unemployment rates reported in Table 1 are biased upwards[17], statistics based on the standard ILO definition do not change the main picture. According to this definition 5.5% of the Flemish and 12.1% of the Walloon labor force was unemployed in 2004.[18]

4.3 The outcome variables

The benchmark outcome is an indicator of (past) transition to employment measured each month between the month of dispatch of the notification and eight months later. The indicator is only set to one in the first month since (potential) notification that the worker did not collect any UI as a full-time unemployed worker and that he is officially registered in one of the Social Security administrations as a salaried of self-employed worker.[19] In order to measure the quality of employment, we use two indicators: the gross full-time equivalent (FTE) gross monthly wage and the employment duration. Since the wage is only observable for *salaried* employment, we measure the quality on this subset only. Note that we measure employment duration and not job tenure. This duration is interrupted as soon as a worker collects at least one day of UI or if, during a month, a worker is neither employed nor collecting any UI.

Two additional outcome variables are analyzed. The first is an indicator of participation in training. This indicator comprises not only training *strictu sensu*, but also job search assis-

[16]The duration in months is obtained by dividing the number of days that benefits are claimed by 26 and rounding down to the nearest integer.

[17]The unemployment figures reported in Table 1 are based on an administrative definition of unemployment in which the denominator is underestimated, since it excludes employment in public administrations. These are, however, the only available statistics on the unemployment rate at the district level.

[18]Source: www.steunpuntwse.be/view/nl/18767

[19]We also consider as employed workers in a subsidized employment scheme of the federal UA, but not officially registered in Social Security. This is a small fraction of the total.

tance and re-enrollment in regular education. We expect that the notification of job search monitoring enhances participation in training, since it may postpone the monitoring interview while remaining entitled to UI and/or positively affect the outcome of the caseworker's evaluation of job search. Second, we observe transitions to sickness insurance, which after Xmonths may transform into disability insurance benefits. In sickness insurance the worker is entitled to a same benefit level (CHECK!). Consequently, to the extent that monitoring in sickness insurance is looser than in UI, notified workers may have an interest in reporting sick.

In principle, the threat of monitoring could induce two other types of transitions: (i) benefit claimants may under some conditions be temporarily dispensed from job search if they face particular social or family problems, e.g. raising children below the age of four; (ii) They may prefer to collect means-tested social assistance benefits. However, in our data these transitions were only of insignificant importance, so that we did not retain these in the analysis.

5 The Econometric Model

The empirical analysis aims at identifying the effect of dispatching a notification letter announcing that job search effort will be evaluated at least 8 months later on the various outcome variables described in the previous section. Identification is based on the discontinuity of the treatment at 30 years during the first year of the reform starting on July 1, 2004. RD analysis is by now well established in the economics literature (see e.g. van der Klaauw, 2002; Hahn et al., 2001; Lee and Lemieux, 2009; Imbens and Lemieux, 2008). Nevertheless, we propose a few minor contributions to this literature.

First, we propose a method to deal with a continuous running variable, age in this application, that is grouped into intervals. The method is merely a variant of the method of Lee and Card (2008) for a discrete running variable. Second, Lee and Card (2008) present a goodness-of-fit statistic to verify whether conventional least squares inference is appropriate. We generalize this statistic for the presence of specification errors, so that it can be used as specification test of the regression function and window width, as well as a falsification test for the presence of discontinuities at points other than the chosen discontinuity point. We also show that this specification test suggests a more efficient estimation procedure similar to the one that was first proposed by Amemiya and Nold (1975) for grouped discrete outcomes. However, this procedure requires many observations within each grouping of the running variable, a condition which is not satisfied in this empirical application.

In SectionXX we explained that that our RD design is fuzzy. However, throughout this section we assume that the design is sharp. There are several reasons for this choice. First, the fuzzy design is particular in that the treatment is only available at one side of the discontinuity threshold: no individual older than 30 is assigned to the treatment. Battistin and Rettore (2008) show that in this case identification assumptions of the sharp design are sufficient to identify the average treatment effect of the treated (ATT) at the discontinuity point. Second, since the fraction of treated is never lower than 85%, ignoring the fuzzy nature of the design will only lead to a slight under-estimation of the treatment effect. Finally, we don't present any methodological contibution related to the fuzziness of the design. In the presentation of the empirical results we will just report the two-stage least squares (TSLS) estimator for the preferred specifications.

We now proceed by first deriving our econometric model in the presence of a grouped continuous running variable. In a second step, we propose the goodness-of-fit test in the presence of specification error. Finally, we explain how we use this test to select the appropriate regression model (order of the polynomial) and the window width, and how it helps us in choosing the appropriate graphical presentation.

5.1 RD design with a grouped continuous running variable

Consider a continuous running variable X (age in the empirical application) which is measured in deviation from x_0 ($= 30 \times 12 = 360$ in the empirical application), the point at which the treatment status changes discontinuously: D(X)=1[X < 0]. In the data this running variable which is grouped in J equally spaced (monthly) intervals: $[-J/2, -J/2 + 1), ..., [-1, 0), [0, 1), ...[j-1, j), [j, j+1), ...[J/2-1, J/2)$. Let Y^1 and Y^0 denote the potential outcomes if an observation receives a treatment or not and assume that the conditional expectation of these potential outcomes on the continuous running variable can be expressed as:

$$E[Y^d|X = x] = (1-d)h_0(x) + dh_1(x) \tag{1}$$

for $d = 0, 1$ and where $h_0(.)$ and $h_1(.)$ are continuous functions, each determined by at most $J/2$ parameters. As Lee and Card (2008) explain, as a consequence of the grouping of the data, non-parametric identification of the treatment effect is not feasible. Using (1) and denoting the observed outcome by Y, the regression equation, as a function of the continuous running variable, can be expressed as

$$E[Y|X = x] = h_0(x) + D(x)[h_1(x) - h_0(x)] \tag{2}$$

and the treatment effect of interest is[20]

$$E[Y^1 - Y^0 | X = 0] = h_1(0) - h_0(0) \tag{3}$$

The problem is that the data on the running variable are grouped and that the functions $h_0(.)$ and $h_1(.)$ are unknown, so that this treatment effect cannot be identified without any further assumptions.

First, assume that we approximate the aforementioned functions by a polynomial of order P<J/2:

$$h_0(x) = \alpha_0 + \sum_{p=1}^{P} x^p \tilde{\gamma}_{0p} + a_0(x) \tag{4}$$

$$h_1(x) = \alpha_0 + \beta_0 + \sum_{p=1}^{P} x^p (\tilde{\gamma}_{0p} + \tilde{\gamma}_{1p}) + a_1(x) \tag{5}$$

where $a_0(x)$ and $a_1(x)$ are specification errors. Inserting these equations into (2) yields

$$E[Y|X = x] = \alpha_0 + \sum_{p=1}^{P} x^p \left(\tilde{\gamma}_{0p} + D(x)\tilde{\gamma}_{1p}\right) + D(x)\beta_0 + a(x) \tag{6}$$

where $a(x) \equiv a_0(x) + D(x)[a_1(x) - a_0(x)]$. Moreover, the treatment effect of interest simplifies to:

$$E[Y^1 - Y^0 | X = 0] = \beta_0 + a_1(0) - a_0(0) \tag{7}$$

where from (4) and (5) $a_0(0) \equiv h_0(0) - \alpha_0$ and $a_1(0) \equiv h_1(0) - \alpha_0 - \beta_0$. We assume that $a_1(0) = a_0(0)$, so that the specification error at the discontinuity point is independent of the treatment status. (Lee and Card, 2008) consider a similar case relative to the *discrete* specification errors. Our assumption differs in that it only needs to hold locally at the continuously measured cutoff point. This is less restrictive if the difference in the specification error increases as one moves away from the threshold. If, however, the assumption is not satisfied, the standard error of the treatment effect should be adjusted, as shown in Lee and Card (2008), to take the uncertainty induced by the specification errors into account. We do not consider this case here.

Second, using the law of iterated expectations, we can relate the grouped regression function

[20] As mentioned, we abstract from the fact that the RD design is fuzzy. If this is taken into account, we can only identify ATT, i.e. $E[Y^1 - Y^0 | X = 0, D(0) = 1]$.

to the continuous one as expressed in (6):

$$E[Y|j \leq X < j+1] = E\left[E[Y|X]|j \leq X < j+1\right] \tag{8}$$

If we assume that X is uniformly distributed within each interval j, so that, if $f(.|j \leq X < j+1)$ denotes the conditional density function, $f(X|j-1 \leq X < j) = 1$, we obtain using (6) and (8):

$$E[Y|j \leq X < j+1] = \alpha_0 + \sum_{p=1}^{P} \left[(j+1)^{(p+1)} - j^{(p+1)}\right](\gamma_{0p} + D_j\gamma_{1p}) + D_j\beta_0 + a_j \tag{9}$$

where $\gamma_{0p} \equiv \tilde{\gamma}_{0p}/p$, $\gamma_{1p} \equiv \tilde{\gamma}_{1p}/p$, $D_j \equiv D(j)$, and $a_j \equiv \int_j^{j+1} a_0(x)dx + D_j \int_j^{j+1}[a_1(x) - a_0(x)]dx \equiv a_{0j} + D_j[a_{1j} - a_{0j}]$. Note that the assumption that the conditional density function is uniform is innocuous, since the assumption relates to the distribution of the running variable within an interval: if this assumption is incorrect, this will be captured by the specification errors, a_j.

Equation (9) allows us to write down the regression model for the micro data. In the empirical application an observation refers to an individual i ($i = 1, 2, ..., N$) who is observed at in a particular month k ($k = 1, 2, ...K$, where K=8) after dispatch of the notification. In principle, we could allow all parameters of the regression model to vary over k. However, given that the sample size is relatively small, we impose the restriction that the polynomials in age (the gamma's) do not vary over k. The specification error a_{jk} remains unrestricted, however. This leads to the following linear regression model:

$$Y_{ijk} = \alpha_{0k} + \sum_{p=1}^{P} \left[(j+1)^{(p+1)} - j^{(p+1)}\right](\gamma_{0p} + D_j\gamma_{1p}) + D_j\beta_{0k} + a_{jk} + \epsilon_{ijk} \tag{10}$$

where $\epsilon_{ijk} \equiv Y_{ijk} - E[Y_k|j \leq X_{ijk} < j+1]$ and where $E[Y_k|j \leq X_{ijk} < j+1]$ is an obvious generalization of (9) in which we explicitly allow for a dependence on k.

For inference we assume that $E[a_{jk}|X = x] = 0$ for $-J/2 \leq x < J/2$. This is a sufficient condition for the ordinary least squares (OLS) estimator of β_0 to be consistent. However, since a_jk induces within age groups correlation across both, individuals i and time k (the specification error can be correlated over time) , and ϵ_{ijk} across time only (outcomes are typically correlated over time), consistent (for $J \to \infty$) estimation of the standard error requires to calculate the cluster robust standard errors. Using (7) and the assumption that $a_1(0) = a_0(0)$, this also provides us with a consistent estimator of the treatment effect of interest and the corresponding standard error. Note, however, that cluster robust standard are only reliable if the number of clusters are sufficiently large, i.e. not less than 40 or 50 (Wooldridge, 2003; Angrist and Lavy, 2009; Angrist and Pischke, 2009). This means that we must be careful in interpreting the standard errors if the age window is less or equal to 2 years to the left and the right of the discontinuity point of 30 years.

5.2 A goodness-of-fit test in the presence of specification error

In this section we propose a goodness-of-fit test in the presence of specification error and briefly discuss how this is related to a more efficient estimator of the treatment effect for sufficiently large samples. The idea is essentially that, at a grouped level, the average residual of a regression equation should be close to zero if the regression model is correctly specified. This can be implemented as an m-test (White, 1994). If we assume that the specification error is Normally distributed (with zero mean and constant variance) and independent between age groups,[21] and if groups are sufficiently large ($n_{j.} \to \infty$, where $n_{j.}$ denotes the number of observations in group jk[22]), the sum of squared grouped residuals weighted by the inverse of the variance matrix can be shown to converge, under the null hypothesis of correct specification, to a χ^2 distribution with $KJ - M$ degrees of freedom, where KJ is equal to the number of groups and M is the number of estimated parameters in the regression model. This χ^2-statistic forms the basis of our goodness-of-fit test.
More formally, let us denote the grouped residual by $\bar{e}_{jk} \equiv a_{jk} + \bar{\epsilon}_{ijk}$, where $\bar{\epsilon}_{jk} \equiv \frac{1}{n_{j.}} \sum_{i=1}^{n_{j.}} \epsilon_{ijk}$, and $\bar{e}_j \equiv [\bar{e}_{j1}...\bar{e}_{jk}...\bar{e}_{jK}]'$ then we have that

$$\sum_{j=1}^{J} \bar{e}_j' \hat{V}_j \bar{e}_j \quad \sim \quad \chi^2(KJ - M) \tag{11}$$

where $\hat{V}_j$ denotes the estimated variance-covariance matrix of $\bar{e}_j$ and where it is assumed that the residuals are not correlated across age groups j (but they can be correlated across time k). To calculate this statistic we need to construct $\hat{V}_j$. This is done in three steps.[23]

First, if we assume that the "approximation errors", ϵ_{ijk}, are i.i.d. within groups jk, then a consistent (for $n_{j.} \to \infty$) estimate of the variance ($k = l$) and covariance ($k \neq l$) is given by[24]

$$\widehat{var}(\epsilon_{ijk}\epsilon_{ijl}) \equiv \hat{\sigma}_{\epsilon jkl} = \frac{1}{(n_{j.} - 1)} \sum_{i=1}^{n_{j.}} (Y_{ijk} - \bar{Y}_{jk})(Y_{ijl} - \bar{Y}_{jl}) \equiv \frac{1}{(n_{j.} - 1)} \sum_{i=1}^{n_{j.}} \hat{\epsilon}_{ijk}\hat{\epsilon}_{ijl} \tag{12}$$

[21] The specification error is allowed to be correlated between k's.

[22] Note that $n_{j.} = n_{j1} = ... = n_{jk} = ... = n_{jK}$: the sample contains the same number of individuals in each month after dispatch of the notification

[23] See (Lee and Card, 2008) for a formal derivation.

[24] If, as in the empirical application, the outcome variable is discrete indicator of a past transition to another labor market state (implying that the indicator cannot revert to zero once it has changed to one) it is not difficult to show that $\hat{\sigma}_{\epsilon jkl} = \bar{Y}_{jk}(1 - \bar{Y}_{jl})n_j/(n_j - 1)$, reflecting the binomial distribution of the discrete indicator.

where $\bar{Y}_{jk} \equiv \frac{1}{(n_{j.}-1)} \sum_{i=1}^{n_{j.}} Y_{ijk}$.

Second, let $\hat{\bar{e}}_{jk}$ denote the (unweighted) residuals of a weighted LS regression of model (10) in which the data are grouped by age group j and by month k and in which the weights are set to $n_{j.}/(N/J)$.[25] It is not difficult to show that these residuals are equivalent to the average residuals of the micro-regression: $\hat{\bar{e}}_{jk} = (\bar{Y}_{jk} - W_{jk}\hat{\theta}_k)$, where W_{jk} is the vector of explanatory variables in (10) and where $\hat{\theta}_k$ is the vector of parameter estimates of the unweighted OLS regression on micro-data. Since the residual is a sum of a specification and a grouped approximation error ($\bar{e}_{jk} = a_{jk} + \bar{\epsilon}_{jk}$), a consistent (for $J \to \infty$) estimator of variance and covariance of the specification errors a_{jk} can be found by subtracting from the weighted (co-)variance[26] of the grouped regression residuals $\hat{\bar{e}}_{jk}$ the weighted (co-)variance of the grouped approximation errors $\bar{\epsilon}_{jk}$:

$$\widehat{var}(a_{jk}a_{jl}) \equiv \hat{\sigma}_{akl} = \frac{1}{J}\sum_{j=1}^{J} \frac{n_{j.}}{(N/J)} \left[\hat{\bar{e}}_{jk}\hat{\bar{e}}_{jl} - \frac{\hat{\sigma}_{\epsilon jkl}}{n_{j.}}\right] = \frac{1}{N}\sum_{j=1}^{J} \left[n_{j.}\hat{\bar{e}}_{jk}\hat{\bar{e}}_{jl} - \hat{\sigma}_{\epsilon jkl}\right] \qquad (13)$$

where we set $\hat{\sigma}_{akk} = 0$ if the right-hand side becomes negative and $\hat{\sigma}_{akl} = 0$ if $\hat{\sigma}_{akk} = 0$ or $\hat{\sigma}_{all} = 0$.
Finally, if $\hat{v}_{jkl}$ denotes the k^{th} row and l^{th} column of $\hat{V}_j$, we have

$$\hat{v}_{jkl} = \hat{\sigma}_{akl} + \frac{\hat{\sigma}_{\epsilon jkl}}{n_{j.}} \qquad (14)$$

A problem with this goodness-of-fit statistic is that it is known to behave poorly in small samples (in terms of $n_{j.}$). Given that in our data the average group size is only just more than 20 units, this is problem is pertinent in our empirical application. In fact, for discrete binary outcomes, as in our data, Hoel (1971) argues that the Normal approximation of the binomial distribution, on which the χ^2-test relies, is fine as long as one avoids cells with less than 5 zeros or ones. In order to implement the test[27], we therefore further group the data over wider age intervals until this condition is satisfied for every cell.[28] However, since right after the (theoretical) dispatch of the letter only few individuals have found a job and since the grouping is over j and not over k, we leave out $k = 1$ from the estimations. This also means that in the preceding formula K should be set to 7 and not to 8.

Finally, if we did not face this small sample problem, the test procedure suggests a feasible generalized least squares (GLS) estimator on the (originally) grouped data which could improve upon efficiency. The GLS estimator is a two step estimator, which either starts

[25] N/J is equal to the average group size
[26] Note that, in contrast to $\hat{V}_j$ this (co-)variance is *un*conditional on j
[27] Note that we only use this wider grouping for testing, not for estimation.
[28] In the few cases we could not fulfill this condition, we explicitly report this.

an OLS estimator on either the individual or grouped data and subsequently uses the estimated parameters of the first step to construct $\hat{V}_j$ in the same way as explained above. This estimator was first proposed by Amemiya and Nold (1975) for a grouped logit model and was labeled the "modified minimum χ^2 estimator". Cockx and Ridder (2001), Cockx and Dejemeppe (2005) and Dejemeppe (2005) applied the method for duration models and also implemented the aforementioned goodness-of-fit test.

5.3 Model Selection and Graphical Presentation

As a consequence of the grouping of the data over the running variable, a non-parametric RD analysis is not an option. In the parametric analysis a number of choices remain open: (i) the choice of the polynomial and (ii) the choice of the window width. In addition, to be convincing, the RD design requires a graphical presentation in which the averages of the outcome variable within a number of bins to the left and the right of the cutoff point are plotted, augmented with a relatively flexible polynomial to smooth the graph (Lee and Lemieux, 2009). This involves choosing the number of bins to be graphically presented. In this section we briefly explain how we use the aforementioned goodness-of-fit test in determining these choices and explain the relationship to similar tests that have been proposed in the literature. Note that, even if we describe a method that guides us in selecting the preferred estimate, we will report for the benchmark outcome the full set of estimates so that the reader can verify the robustness of the estimates and for other outcomes the full set is available upon request.

In order to choose the polynomial and window width, we estimate each model 20 times: we choose 5 window widths adding each time a year to the left and right of the cutoff of 30 years, eventually leading to the widest window between 25 and 34 years; for each window we estimate 4 spline polynomials of degree zero up to three. We then choose within each window the polynomial that fits best according to the goodness-of-fit test by choosing the one with the highest p-value. Subsequently, we choose the widest window for which the retained estimates in the first step are not rejected at the conventional p-level of 5%. We choose this rule as a way to implement the trade-off between bias and precision. In order to increase precision we would like to include as many observations as possible by widening the observation window, but only do this to the extent that the model is not found to be misspecified.

The aforementioned testing procedure is related to one proposed by Lee and Lemieux (2009) and Lee and Card (2008) They suggest to add $J-2$ (in our case $JK-2K$) bin dummies to the polynomial in the running variable and test whether the coefficients of these bin dummies are jointly different from zero. They argue that this test can be interpreted as a falsification test of the RD design, since a rejection of the test can be interpreted as evidence for discontinuities at other thresholds of the running variable. In the case of a grouped running variable, the model augmented with bin dummies is saturated at the grouped level. In that case the test boils down to a goodness-of-fit test of the polynomial against the saturated model. This corresponds exactly to what we propose, except that we explicitly allow for a specification error and that we group the data further to ensure that the distributional assumptions of the test are satisfied.

Finally, Lee and Lemieux (2009) suggests that this goodness-of-fit test can also be a useful guide in determining the number of bins to be graphically presented aside from the smooth polynomial curve. In fact, in this test the polynomial is replaced by the set of indicator variables associated to the restricted number of bins. The test verifies whether or not the restricted grouping of the running variable can be rejected against the saturated model. It is important to note, however, that one cannot test a grouping that is finer than the one that is chosen to resolve the small sample bias. For instance, in the empirical analysis the required grouping for the test is 12 months. It then does not make sense to test whether a grouping of 3 or 6 months would be acceptable.

6 The Empirical Findings

TO BE COMPLETED (see Tables and Figures)

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Table 1 : Descriptive Statistics by Region of Living and Age Group

	Flanders		Wallonia	
	25-29 years	**30-34 years**	**25-29 years**	**30-34 years**
Number of individuals	1,311	1,165	1,310	1,069
Month of (potential) notification				
July	28.4%	28.8%	27.2%	28.9%
August	25.3%	24.0%	20.8%	22.5%
October	22.7%	24.5%	23.7%	26.0%
November	23.7%	22.8%	28.2%	22.6%
Age				
mean age in years on July 1, 2004	26.9	32.0	26.8	32.0
(standard deviation)	(1.4)	(1.4)	(1.4)	(1.4)
Sex				
women	51.9%	53.1%	45.9%	46.3%
Nationality				
Belgian	90.1%	86.8%	90.4%	85.5%
EU15 (excluding Belgian)	2.8%	5.1%	7.3%	10.1%
others	7.1%	8.2%	2.3%	4.4%
Schooling level				
primary	14.8%	22.3%	14.0%	20.4%
lower secondary	19.6%	17.7%	19.5%	21.6%
upper secondary	45.0%	42.0%	41.2%	35.2%
general degree	8.9%	9.8%	10.0%	6.3%
technical or qualifying degree	36.2%	32.2%	31.2%	28.9%
higher education	20.5%	17.8%	22.8%	15.1%
other studies	0.1%	0.3%	2.6%	7.8%
Category of insured unemployment[(a)]				
head of household	14.8%	23.4%	19.5%	25.9%
single	24.3%	22.8%	27.5%	24.0%
cohabitant	61.0%	53.7%	53.0%	50.1%
Type of entitlement to benefits[(a)]				
work experience	83.0%	98.9%	74.2%	97.9%
school-leaver	17.0%	1.1%	25.8%	2.1%
Recent participation in training[(a)(b)]	18.0%	19.1%	13.4%	11.8%
Recent work experience [(a)(c)]	53.6%	42.8%	42.6%	31.2%
mean number of days in employment[(d)]	87.3	80.7	93.1	78.2
(standard deviation)	(71.6)	(70.9)	(76.4)	(71.2)
Mean unemployment rate[(e)] **by district of living**	8.3%	8.2%	21.7%	21.0%
(standard deviation)	(1.8)	(1.8)	(5.2)	(5.4)

Figure 1: Threat Effect on Transition to Employment 8 months after Notification

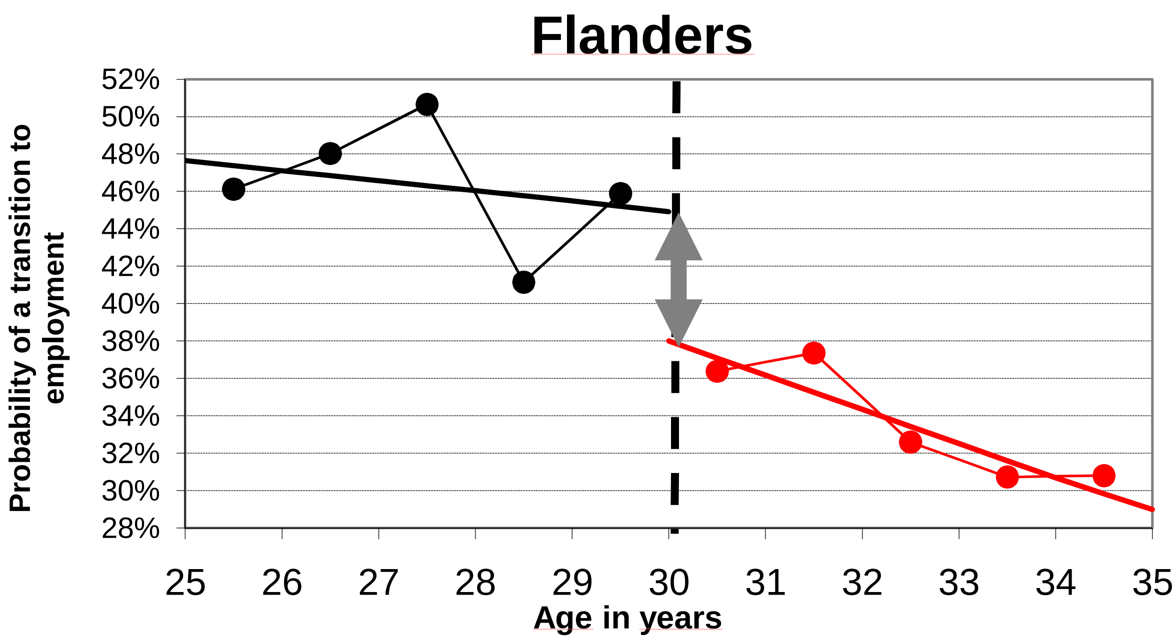

Figure 2: Threat Effect on Transition to Employment 8 months after Notification

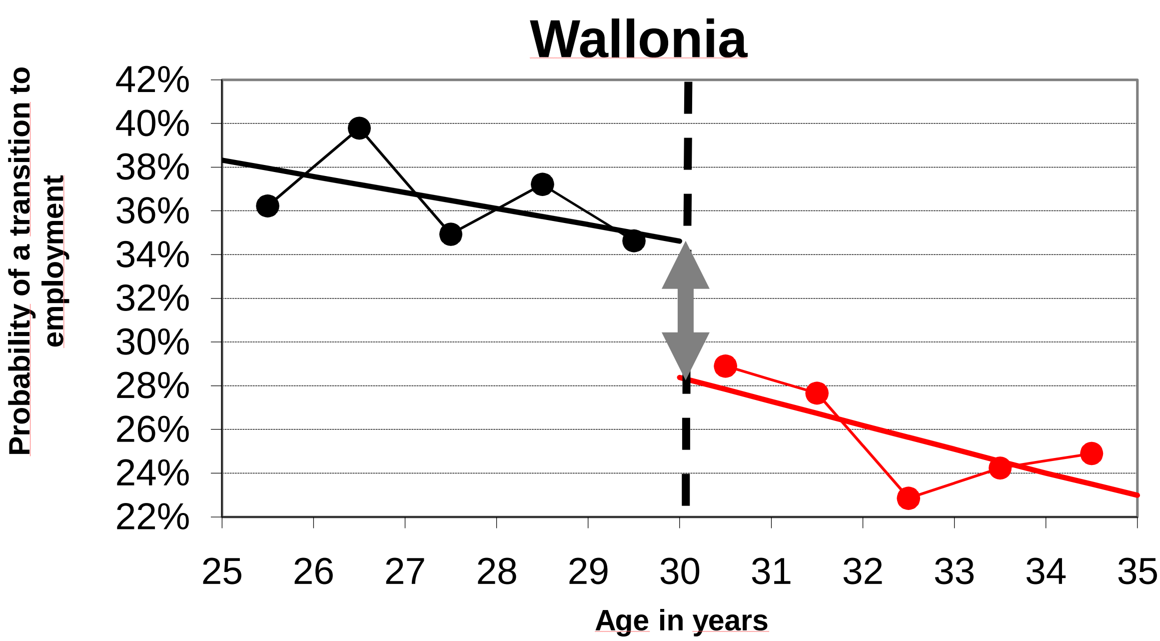

Table 2: Treatment effect of benchmark for various specifications

Window size	Flanders		Wallonia	
	Effect %points (st. err.)	Opt. Poly. Degree (p-value)	Effect % points (st. err.)	Opt. Poly. Degree (p-value)
+/- 5 years	6.9 (3.5)	1 (0.058)	6.2 (3.1)	1 (0.246)
+/- 4 years	12.1 (2.4)	0 (0.883)	10.2 (2.1)	0 (0.217)
+/- 3 years	15.4 (5.6)	2 (0.217)	3.2 (4.1)	1 (0.115)
+/- 2 years	13.5 (4.9)	1 (0.620)	5.4 (2.7)	0 (0.930)
+/- 1 year	7.1 (4.6)	0 (.)	5.5 (4.3)	0 (.)

Table 3: RD-Estimates with and without Control Variables, 8 Months after Notification

	Flanders		***Wallonia***	
	without X	***with X***	***without X***	***with X***
a(30)	**0.069****	**0.082****	**0.062****	**0.069****
(standard error)	**(0.035)**	**(0.034)**	**(0.031)**	**(0.031)**

Table 4: RD-Estimates *sharp* versus *fuzzy* design, 8 Months after Notification

	Flanders		***Wallonia***	
	sharp	***fuzzy***	***sharp***	***fuzzy***
a(30)	**0.069****	**0.077****	**0.062****	**0.067****
(standard error)	**(0.035)**	**(0.039)**	**(0.031)**	**(0.033)**

Table 5: RD-Estimates 2004 versus 2003, 8 Months after Notification

	Flanders		***Wallonia***	
	2004	***2003***	***2004***	***2003***
a(30)	**0.069****	**-0.001**	**0.062****	**0.020**
(standard error)	**(0.035)**	**(0.032)**	**(0.031)**	**(0.028)**

Table 6: Anticipation Effects 1 to 2 Months Before the Month of (Potential) Notification

	Flanders	*Wallonia*
a(30)	0.003	-0.007
(standard error)	(0.006)	(0.008)

Figure 3: Threat Effect on Transition to Employment 8 months after Notification

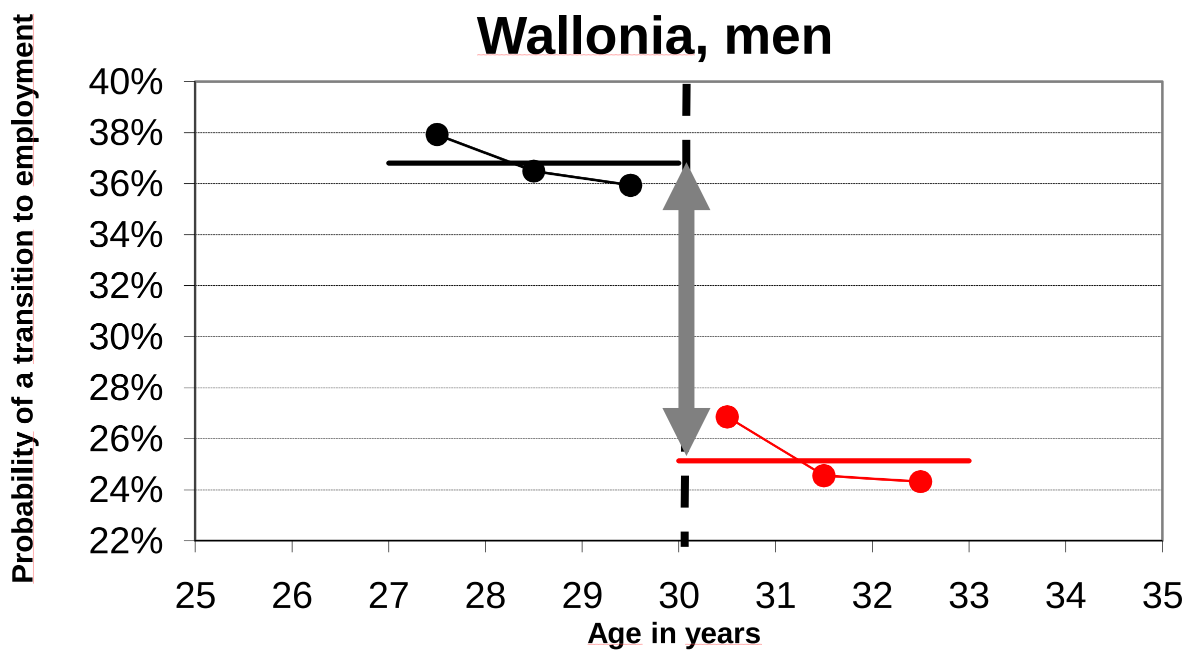

Figure 4: Threat Effect on Transition to Employment 8 months after Notification

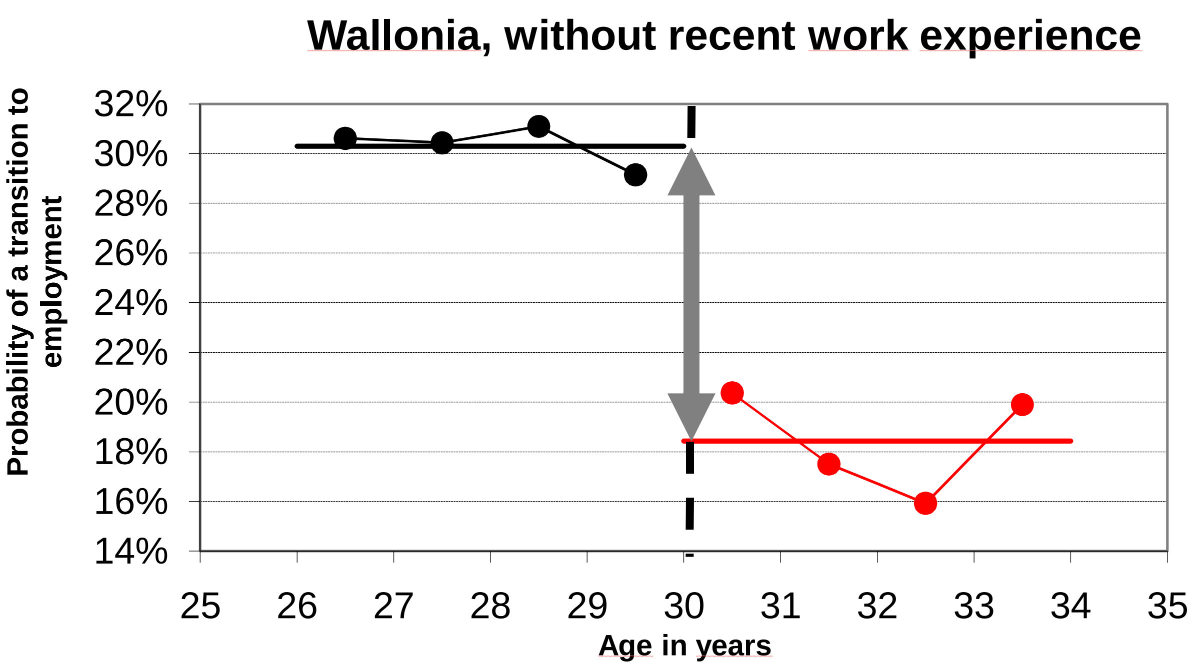

Figure 5: Threat Effect on Transition to Training 8 months after Notification

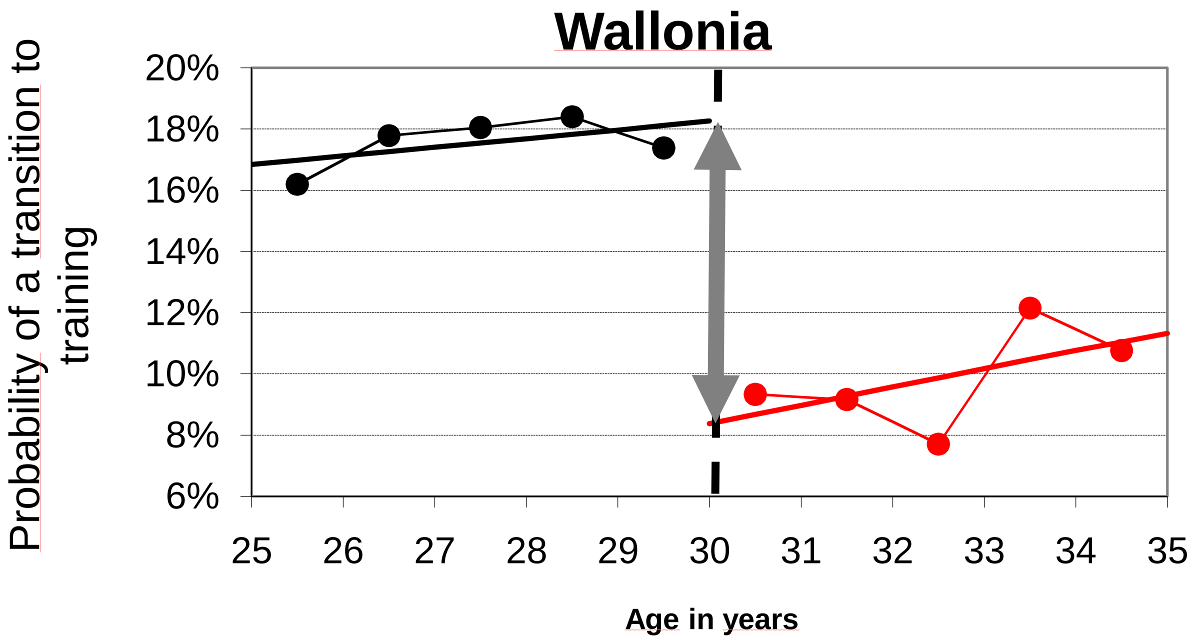

Figure 6: Threat Effect on Transition to Inactivity 8 months after Notification

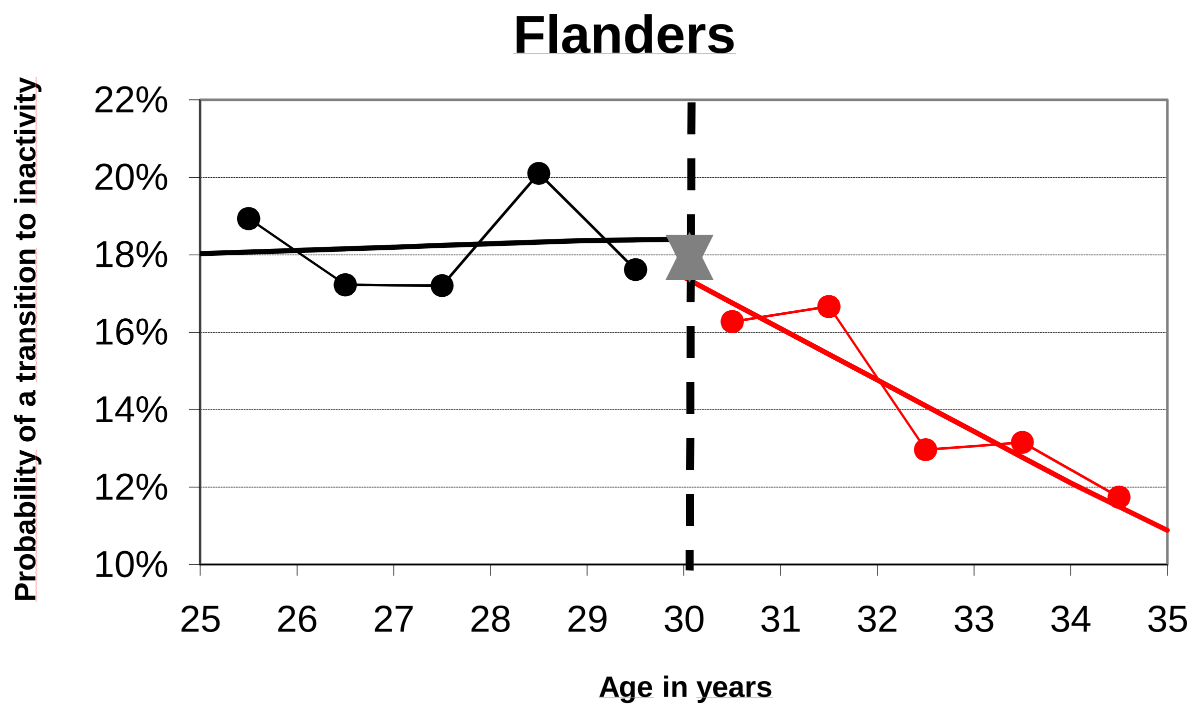

Figure 7: Threat Effect on Transition to Inactivity 8 months after Notification

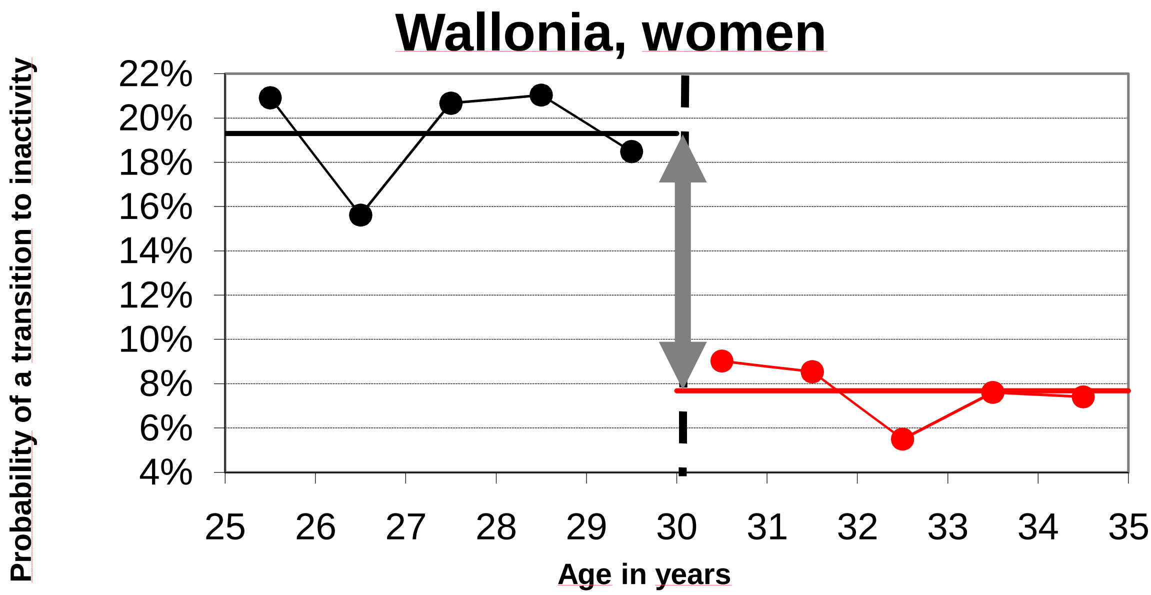

Table 6: Quality of Employment: Employment Duration

	Salaried employment		duration < 50%		duration >= 50%	
	Flanders	*Wallonia*	*Flanders*	*Wallonia*	*Flanders*	*Wallonia*
Outcome without treatment	0.348	0.269	0.162	0.126	0.185	0.144
(standard error)	(0.024)	(0.019)	(0.020)	(0.015)	(0.019)	(0.016)
a(30)	0.045	0.055**	0.060**	0.061***	-0.015	-0.006
(standard error)	(0.031)	(0.026)	(0.027)	(0.022)	(0.025)	(0.021)
P-value fit	0.358	0.687	0.483	0.402	0.057	0.923
P for F equality test	0.077*	0.059*				

Specification with 2 years age window, no polynomial in age and without control variables.

Median employment duration (in months) Flanders = 10; in Wallonia = 9.4

Table 7: Quality of Employment: Wage

	Salaried employment		With wage < 50%		With wage >= 50%	
	Flanders	*Wallonia*	*Flanders*	*Wallonia*	*Flanders*	*Wallonia*
Outcome without treatment	**0.348**	**0.269**	**0.185**	**0.139**	**0.162**	**0.130**
(standard error)	**(0.024)**	**(0.019)**	**(0.017)**	**(0.017)**	**(0.019)**	**(0.015)**
a(30)	**0.045**	**0.055****	**0.043**	**0.024**	**0.002**	**0.031**
(standard error)	**(0.031)**	**(0.026)**	**(0.027)**	**(0.022)**	**(0.025)**	**(0.022)**
P-value fit	**0.358**	**0.687**	**0.144**	**0.637**	**0.979**	**0.638**
P of F equality test	**0.332**	**0.845**				

 # Specification with 2 years age window, no polynomial in age and without control variables.

Median FTE monthly gross wage in Flanders = 1964€ ; in Wallonia = 1911€