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WORKING PAPER

Is income support for part-time workers a stepping-stone to regular jobs? An application to young long-term unemployed women

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Is income support for part-time workers a stepping-stone to regular jobs?
An application to young long-term unemployed women

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Abstract:
We verify whether income support for low-paid part-time workers in Belgium increases the transition from unemployment to non-subsidised, "regular" employment. Using a sample of long-term unemployed young women, whose labour market histories are observed from 1998 to 2001, we implement the "timing of events" method to control for selection effects. Our results suggest that the policy has a significantly positive effect on the transition to regular employment.

Keywords: Active labour market policies, Evaluation, Mixed Proportional Hazard Models

Codes JEL: J64, J68, C41

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The high and persistent unemployment rate encountered in many European countries has been a major concern of policy makers for over twenty-five years. Since the Amsterdam Treaty was adopted in June 1997, the European Union (EU) explicitly recommends the implementation of "active" labour market policies (ALMP): unemployment insurance (UI) systems should be reformed to enhance employability and to favour the transition of unemployed workers towards employment. One type of ALMP consists in granting income support to low-wage and/or part-time workers, in order to reduce the part-time pay penalty, and to serve as a stepping-stone to full-time employment. Income support policies have been implemented in several European countries (including Belgium, France, Germany and the UK). This paper analyses a Belgian income support policy targeting part-time workers who earn less than the *full-time* minimum wage.

In Belgium, since 1993, unemployed workers who accept a low-paid part-time job, and who continue searching for full-time employment, receive income support. Income support is a fraction of the unemployment benefits (UB) that a worker continues to receive after accepting a part-time job. Theoretically, it is unclear whether this policy is a stepping-stone to "regular" employment, defined as either (1) full-time employment or (2) part-time employment with earnings higher than the *full-time* minimum wage. On the one hand, the labour market experience that a worker may acquire during the course of this ALMP could increase her employability. On the other hand, because of its design, this policy may create a "part-time employment trap", as the support decreases on a euro for euro basis as the worker's wage increases. The objective of the present study is to determine which of the two effects actually prevails. More specifically, we investigate whether an unemployed worker accelerates its transition to a regular job by accepting a supported part-time job – instead of rejecting it and continuing to search *directly* for regular jobs. The analysis is an application of the Timing of Events approach (Abbring and Van den Berg, 2003, 2004), adjusted for time-grouped data.

Our study analyses only a subset of the population eligible to the Belgian income support: young, long-term unemployed women. Several considerations motivate this restriction. First, the policy concerns part-time workers earning less than the *full-time* minimum wage. In order to ensure a sensible comparison of treatment and controls, we restricted the sample to a group of disadvantaged workers. Second, youth unemployment rates are very high in the EU and especially so in Belgium. Between 1995 and 2005, the average unemployment rate in the EU-15 area of the population aged between 15 and 24 was more than twice as high as the unemployment rate of the working-age population: respectively 17.9% and 8.5%. This contrast is particularly high in Belgium, where these figures were respectively 20.5% and 8.4% (Eurostat, 2006). Policy makers have therefore a particular interest in identifying policies that stimulate the integration of this age group in the labour market. Finally, we restrict our analysis to women, since the incidence of part-time work and consequently participation in income support is very low for men.

The paper is organised as follows. A first section describes the institutional context of the Belgian UI system and the policy we study. A second section surveys the empirical literature dedicated to the evaluation of income-support policies. The third and fourth sections respectively present our data and econometric model. The results of our analysis are presented in Section 5, and conclusions are given in a final section.

1. Institutional context

The Belgian UI system is particular, for the following two reasons. First, workers are in principle entitled to benefits *without any time limit*. Second, workers who, during their previous employment spell, contributed sufficiently to the insurance scheme are not the only ones entitled to benefits: *school-leavers* may also be entitled to unemployment benefits. This entitlement is acquired if the following conditions are met: (1) one is less than 30 years old; (2) one has been registered in the third year[1] of secondary education or higher; (3) one has attended classes until the end of that school year; (4) one is registered as a job-seeker at an UI office, and has searched for 9 months[2] after the registration date. Regarding the third condition, it is important to note that pupils do not have to pass the final exams. Therefore, for some of them, the highest *attained* education level may be primary education (6 years of schooling). The next level, *lower secondary*, is only attained after *successfully* completing three years of secondary education. This explains why in Table 1 the fraction of workers with a primary school education level is positive.

School-leavers are paid a flat rate benefit, the level of which depends on age and on family type. For instance, in April 2007, the monthly amount of UB of an 18-25 year old job-seeker being in charge of all the people she is living with was 889 euros. It was 368 euros if she lived together with a partner earning replacement income, and 346 euros for other cohabiting individuals. For singles, the monthly amount depends on age: 397 euros if aged 18-20, and 658 euros if older. For school leavers, the level of benefits is always lower than the level for workers that have contributed to the UI system.

Now that we have underlined the specificity of the Belgian UI, we can proceed with the description of the income support scheme of interest, referred to as the AGR (*Allocation Garantie de Revenu*). The AGR is a wage premium granted to recipients of UI benefits who are searching for a full-time job, but who temporarily accept a part-time job (i.e. a job in which the working time is at least 1/3, and less than 4/5, of a full-time job). The premium is temporary in the sense that the worker must continue searching for a full-time job in order to receive the AGR. To be eligible, a worker must: (1) formally declare to her employer that she remains available for a full-time position that would become vacant; (2) report to her unemployment agency as a full-time job seeker and remain available for a full-time job. In addition, the policy targets low-paid part-time workers: it can only be granted if the monthly gross earnings of the part-time worker are lower than the legal full-time minimum wage (in April 2007, this minimum wage amounted to 1,284 €/month if aged 21 or more[3]).

An AGR recipient who loses her part-time job will regain her entitlement to full-time unemployment benefits. Finally, note that school-leavers are not entitled to the AGR during the waiting period, since the premium is in fact a fraction of the UB that would be due if the unemployed worker did not accept the part-time position. In this study, the sample consists of young women who just completed the waiting period. This means that they are all eligible to the AGR.

[1] Students outside vocational/technical training or arts must be registered in the fourth year or higher.
[2] This waiting period is reduced to 6 months for those aged less than 18 years, and extended to 12 months for youth between 26 and 30. School-leavers older than 30 years are not entitled to benefits.
[3] The minimum wage is lower for workers below the age of 21. It increases after 6 and 12 months of labour market experience if older than 21.

The level of the AGR is computed as follows. The baseline is the amount of benefits that is due to a full-time unemployed worker. One adds a bonus, which again depends on family type (in April 2007[4], this bonus was equal to 157 euros for cohabitants in charge of dependents, 126 euros for singles and 94 euros for other cohabitants). The final amount of AGR is then computed by deducting from that sum the net monthly wage associated to the part-time job (i.e. the gross wage minus the social insurance contributions of the employee and the withholding tax on income). In summary, the AGR is computed using the formula: "AGR = baseline benefits + bonus – net wage from part-time job" and cannot be negative. This subsidy is granted for an indefinite time-period, as long as eligibility criteria are satisfied.

Note that the premium decreases with the wage earnings on a euro for euro basis. This implies that the AGR imposes an implicit marginal tax rate of 100% to any increase in earnings induced by either an increase in the number of working hours, or an increase in the hourly wage[5]. This feature of the AGR goes against the positive impact it may have (through signalling or human capital accumulation) on the integration of young workers in the labour market. It may in fact replace the "unemployment trap" by a "precarious employment trap" (Degreef, 2000).

2. Survey of the literature

Part-time work is an important feature of contemporary labour markets. It concerns mostly women, and generally results in a wage penalty, compared to full-time employment (Rodgers, 2004; Hirsch, 2005; Hardoy and Schøne, 2006 ; Manning and Petrongolo, 2008). This part-time wage gap has also been observed in Belgium (Jepsen *et al.*, 2005), the country of interest of the present study. According to Manning and Petrongolo (2008), most of the part-time wage penalty can be explained by occupational segregation: in other words, the wage gap could be reduced if better jobs were introduced for part-time workers. In that context, it is important to study an ALMP such as the AGR, which encourages part-time work: studying this policy can help determine under which conditions part-time jobs can be stepping-stones to regular employment, rather than "dead-end jobs".

Different types of income-support policies for part-time and low-wage workers have been implemented in European countries (e.g., "employment premium" in France, "Working Families Tax Credit" in the UK). Several studies suggest that this type of policies can accelerate the transition from unemployment to employment (e.g., Meyer, 1995; Cahuc, 2002; Blundell and Hoynes, 2004; Francesconi and van der Klaauw, 2004 ; Eissa and Hoynes, 2005). This conclusion does not depend on whether the support is granted to the head of household or to the individual (as in the case of the AGR). However, most of these studies concern the USA and the UK, where the minimum wage is much lower than in continental Europe. In continental European countries, the level of employment may be more sensitive to labour costs than to labour supply incentives (Cahuc, 2002). Moreover, the aforementioned studies do not specifically focus on part-time workers, but, more generally, on low-wage workers.

[4] Since July 1st, 2005, the amount of the premium is calculated differently. However, workers who were hired before that date are still entitled to the AGR as explained in the text.
[5] Since July 1st, 2005, the bonus increases with the number of hours worked, reducing the marginal withdrawal rate if one works less than 80% of a full-time. However, the implicit marginal tax of 100% remains if a recipient accepts more than 80% or if the hourly wage increases. Our analysis concerns the period before this reform.

McCall (1996, 1997) has evaluated a system of income support for part-time workers in Canada. In this country, an unemployed worker who accepts a part-time job keeps her weekly allowance as long as her weekly wage remains below 25% of this allowance. Beyond that threshold, one (Canadian) dollar is deduced from the allowance for each additional dollar gained through part-time work. Consistent with theory, the author finds that a 50% increase in this income support tends to increase the probability of getting a part-time job (by 2% to 3%) and to decrease unemployment duration (by 2.5 to 6.2 days). These effects, however, are relatively small.

The aforementioned studies do not specify whether income supports accelerate the transition to "regular" employment (i.e., full-time or non-subsidised employment). This depends on the rate of progression of the earnings (a function of the hourly wage and/or working time): if earnings increase sufficiently, the amount of the subsidy drops to zero. Theoretically, an income support such as AGR may accelerate as well as decelerate this transition. A first argument in favour of acceleration is that a job-seeker who accepts a subsidised job signals her motivation and attachment to the labour market to employers (Gerfin et al., 2002). In addition, according to human capital theory, labour market experience and on-the-job training should lead to an increase in productivity and, *in fine*, in wages.

However, recent empirical studies have shown that returns to labour market experience are lower for low-skill workers[6]. Moreover, the income support generates an income effect that reduces the incentive to continue searching for a regular job. In addition, in the case of AGR, the 100% implicit marginal rate of taxation reinforces this "locking-in" in low-wage subsidised employment (Calmfors, 1994 ; Van Ours, 2004). Finally, the aforementioned signalling argument may also slow down the transition to a regular job. For instance, employers may believe (righteously or not) that workers who accept low-paid part-time jobs are less productive than workers who only directly accept higher paying regular jobs. In that case, accepting a low-paid part-time job sends a negative signal to employers[7].

Many researchers (e.g., Booth *et al.,* 2002; Zijl *et al.,* 2004; Autor *et al.,* 2005; D'Addio and Rosholm, 2005; Gagliarducci, 2005; Kvasnicka, 2005; Larssen *et al.,* 2005) have studied the impact of temporary jobs and employment for temporary work agencies on the transition to regular, permanent employment. These studies report mixed results. To our knowledge, few researchers have studied the impact of part-time (subsidised) work on labour market reintegration. Buddelmeyer *et al.* (1995) find that in the European Union, less than 5% of unemployed workers use part-time work as a stepping-stone to full-time employment. Blank (1998) shows that, in the USA, part-time workers tend to remain in that situation for a long time, and experience few transitions to full-time employment. However, these studies are primarily descriptive, and do not allow determining whether accepting a part-time job accelerates the transition to a full-time job. Farber (1999) suggests that in the USA, part-time work may be a phase in the transition from unemployment to full-time employment, but his analysis remains inconclusive.

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[6] Cf. Card and Robins (1999), Gladden and Taber (2000), Meghir et Whitehouse (1996), Dustmann and Meghir (2001), Card et Hyslop (2005). According to Grogger (2005), though, returns to experience are not significantly different among low-skill and high-skill workers.

[7] See Ma and Weiss (1993) and McCormick (1990) for a theoretical foundation of that argument. This argument is referred to by Burtless (1985) and Bonnal *et al.* (1994 ; 1997) in their evaluation studies. See Dubin and Rivers (1993) for a critique of that point of view.

Granier and Joutard (1999) address the same issue as we do, using a similar methodology. They estimate the impact of an income-support policy for part-time workers in France on the transition to regular employment. This policy allows a (full-time) job-seeker working less than 136 hours a month (and receiving less than 70% of her previous wage) to cumulate her labour income and unemployment benefits. The scheme is designed to prolong the worker's entitlement to the allowance proportional to her working time.

The French scheme provides more incentives to transit to regular employment than the Belgian AGR. First, the implicit withdrawal rate of the subsidy is equal to the replacement rate, and not to 100% as in Belgium. Second, since in France the duration of the entitlement to the subsidy coincides with the duration of the entitlement to the benefits, the worker who does not find a regular job before the subsidy expires, will not be entitled to any other allowance of the UI system. This contrasts sharply with the Belgian system in which the entitlement to both the subsidy and the benefits is indefinite. Granier and Joutard (1999) conclude that the investigated policy increases the transition to regular employment, especially close to the moment at which UB expire. This transition is only delayed for long-term (more than 18 months) unemployed women. It is not clear, however, whether this acceleration is caused by income support *per se*, or whether it results from the existence of a time limit on the entitlement to the benefits.

Very recently, Kyyrä (2008) and Kyyrä *et al.* (2009) study, on the basis of a timing of events methodology, the stepping-stone effect of income support for part-time workers in Finland and Denmark. In Finland the transition rate to regular jobs of part-time workers receiving income support does not significantly differ from the one of workers who remain unemployed. For part-time workers who return to unemployment, the post-treatment effect was positive in most specifications, but due to small sample size, the effect was never significantly positive. Kyyrä *et al.* (2009) find, using a much larger dataset, an important locking-in effect of these income support policies in Denmark: they reduce the transition to regular jobs when income support is due. Moreover, the post-treatment effect was only found to be positive for particular groups. Consequently, the policy reduced the expected unemployment duration only for those with short subsidized working periods (young workers and first generation Non-Western immigrants). However, care should be taken care when interpreting these findings, since, as we argue in Section 4.1, these studies can be criticized on methodological grounds.

3. Data

Our study uses administrative data centralised in a "Labour Market Data warehouse". This database is the result of a joint effort from the central databank of the Belgian Social Security (BCSS) and from various Social Insurance institutions. The Data warehouse gathers individual and longitudinal information on the labour market histories of Belgian workers. It contains quarterly information on unemployment, employment (including self-employment) and inactivity spells (identified by non-presence in any of the other spells). The database identifies ALMPs, including subsidised employment such as AGR.

The quarterly grouping of the data implies that a transition is identified only if a worker has changed labour market state between two consecutive quarters. Transitions *within* a quarter are neglected. This may seem restrictive. One may indeed be concerned that the Timing of Events methodology, used in our analysis to identify the treatment effect, cannot be applied with such data. We will discuss this issue in Section 4.2. Nevertheless, the quarterly grouping may also entail an advantage. The policy maker is not concerned with transitions to regular jobs that last only very briefly. The quarterly grouping makes it less likely that the data identify such transitions.

The sample has been selected according to three criteria. We retain: (1) women (2) aged 18-25, (3) who, in 1998, were for the first time entitled to unemployment benefits, and who, therefore, did not have any labour market experience during the 9 months waiting period following the end of their initial schooling. This leaves us with a sample of 8630 disadvantaged women. 175 of these women started working part-time with the AGR income support before December 31, 2001, the end of the observation period.

We already explained in the introduction why we restricted the analysis to disadvantaged youth. The fact that we practically implement this restriction by retaining in the sample only school-leavers that have been unemployed during 9 months is related partly to the nature of the data source, and partly to an effort to reduce the initial conditions problem. First, since we had only the above-mentioned administrative data at our disposal, we could not identify school-leavers when they enter the labour market, but only after the 9-months period of unsuccessful job search that grants them entitlements to UB[8]. Second, we selected unemployed school-leavers rather than youth with past work experience, since this avoids the initial conditions problem that workers might have participated to the AGR before the sampling date (failing to take past participation into account would bias the estimator of the treatment effect). In addition, retaining only workers at the start of the benefit entitlement ensures that the sampled workers are homogeneous in terms of their past labour market experience: They all have been unemployed during nine months since leaving school and they never worked before.

The reader may worry that this sample selection rule does not resolve all initial conditions problems, since it retains only workers with elapsed unemployment durations of nine months. It is therefore a selective sample of the inflow in unemployment. However, it is neither necessary nor possible to correct for this selectivity. The correction is not possible, since this would require information on the exits from unemployment during the first nine months, which is unavailable. Correcting for this sample selectivity is not necessary since we *define* the population of interest to be long-term unemployed. One can then view the sample as a *flow* sample of workers who are unemployed *with UB entitlement.*

Table 1 reports summary statistics for the explanatory variables. We distinguish three groups: (1) AGR recipients (the treated group), (2) individuals who experience a direct transition from unemployment to regular employment, and (3) censored individuals. As stated in the introduction, regular employment is defined here as either (1) full-time employment or (2) part-time employment with earnings higher than the *full-time* minimum wage. Censored individuals include young women who remained unemployed over the whole observation period (1998-2001). We also retain in that group women who experience a transition to inactivity (including education) or to other ALMP (such as training or temporarily subsidised employment). These transitions are ignored in order to keep the econometric model tractable, and to avoid that participation in other ALMP contaminates the control group (i.e. those women who have not been treated yet). We observe 175 AGR recipients, 3458 direct transitions, and 4997 censored individuals.

[8] See Section 1 for more details.

We give a brief synthetic view of the observed differences between groups, by comparing (1) censored individuals and those experiencing direct transitions, and (2) treated individuals and those experiencing direct transitions. The censored group gathers mostly inactive women, and women with long unemployment spells. There are more Belgians in the censored group than within the group that has found regular employment (85% versus 91%), while for non-EU foreigners the opposite holds (9% versus 4%). Censored women are less educated than those experiencing direct transitions: 39% of the former have less than 12 years of schooling, while this fraction is only 18% among the latter. The censored and direct transitions groups also differ with respect to family status: in the former, 14% of the women have children younger than three, whereas in the latter, this percentage is equal to 5% only. Moreover, only 67% of the women in the censored group live with their parents, versus 80% among those experiencing direct transitions. The latter may therefore face more difficulties in finding childcare during their working hours. Finally, women in the censored group live in sub-regions[9] where the unemployment rate[10] is slightly higher than average. They are less present, though, in the Walloon region than in Flanders or in Brussels; this is somehow paradoxical, since unemployment tend to be higher than average in the Walloon region.

TABLE 1 ABOUT HERE

Treated women (i.e. AGR recipients) are less educated than those experiencing direct transitions: 31% have 12 years of schooling or less, versus 18% only among the latter group. Moreover, treated women, just like those in the censored group, live less at their parents' (64% versus 80% among those experiencing direct transitions), and tend to have more young children (12% versus 5%). Compared to those experiencing direct transitions, treated women are more likely to live in areas where the unemployment rate is higher, notably in Brussels and Wallonia.

Based solely on Table 1, we cannot conclude whether the population of AGR recipients is selective with respect to observable characteristics affecting the transition to a regular job. This uncertainty appears when comparing the descriptive statistics of the AGR recipients with the total sample averages, reported in the last column of Table 1. For instance, on the one hand, a higher fraction of AGR recipients is highly educated (more than 14 years of schooling), and a lower fraction has a non-EU nationality. This suggests that AGR recipients are more employable than non-recipients. On the other hand, they are more likely to live in districts with high unemployment rates. This makes them less likely to enter regular employment than non-recipients.

4. Econometric modelling

In our study, as in any evaluation study, we face the "selection bias" problem. To estimate the impact of AGR on the rate of transition to employment, we have to compare the labour market histories of AGR recipients (treated group) to those of non-recipients (control group). By doing so, we may capture not only the effect of the AGR *per se*, but also the effect of other observed and unobserved differences between both groups. In addition, we must take the dynamic selection effect into account, since the most employable workers may have left unemployment before the treatment takes place. To solve these problems, we control for

[9] The Belgian territory is divided in 30 sub-regions.

[10] This statistic is measured by the ONEM (Belgian Unemployment Office) as a percentage of the population insured against the risk of unemployment. The retained denominator is smaller than the actual labour force. Consequently, it blows the unemployment rate up.

differences between the treated and control groups based on both observed and unobserved individual characteristics. We control for selection on observables by conditioning the hazard rates on the explanatory variables mentioned in Table 1. To control for unobserved characteristics (unobserved heterogeneity) we rely on the Timing of Events method (Abbring and Van den Berg 2003, 2004). This method exploits the fact that unobserved heterogeneity affects the transition to regular employment throughout the unemployment spell, whereas the treatment (transition into AGR, in this study) may only influence this transition from the moment at which the treatment occurs. By this “discontinuity”, one can identify the treatment effect from the selection effect without imposing “exclusion restrictions” on the observed explanatory variables. We now specify the econometric model and discuss identification of the treatment effect.

4.1. The econometric model

The Timing of Events method involves estimating a competing-risks duration model in which transition rates are proportional to observed and unobserved explanatory variables, denoted X and $V = (V_p, V_e)$ respectively.[11] In what follows, the p index always refers to the AGR policy, and the e index to regular employment. Variables X and V are independently distributed.[12] In this model, transitions to the AGR policy and to regular employment are represented by two random latent continuous durations, T_p and T_e, where t_p and t_e denote their realizations. The joint distribution of $T_e, T_p \mid X, V$ is expressed as the product of the two following conditional distributions: $T_p \mid X=x, V_p$ and $T_e \mid T_p=t_p, X=x, V_e$. These distributions are in turn completely determined by the corresponding hazard rates $\theta_p(t \mid x, V_p)$ and $\theta_e(t \mid t_p, x, V_e)$, where t is the elapsed duration in unemployment with benefit entitlement: $t=0$ at the start of benefit entitlement, nine months after leaving school. We are interested in the causal effect of t_p on $\theta_e(t \mid t_p, x, V_e)$.

Since we cannot observe V, we need further assumptions to identify the causal impact of the treatment. This is because individuals receiving the treatment are not randomly selected from the population of interest. First, if the unobserved determinants of the transitions to part-time and regular employment, i.e. V_p and V_e are dependent, then the distribution of V_e among the treated cannot be equal to the population distribution: participants will on average have high values of V_p and, by the dependence, have different values of V_e than on average in the non-participating population. Second, participants have different values of V_e because of dynamic sorting: to be able to participate in AGR, they may not have left unemployment for a regular job before t_p and must therefore have a relatively low value of V_e relative to the sampled population. Abbring and Van den Berg (2003) show under which assumptions one can identify the true causal effect of the treatment from the spurious effect induced by the aforementioned selection effects. We discuss these in Section 4.2.

We now turn to specification and the derivation of the likelihood function. The hazards are specified in the following Mixed Proportional (MPH) form:

[11] To take seasonal and business cycle effects into account, one of the explanatory variables, the variation of the unemployment rate in the district of residence since the end of 1997, is time varying, but we do not make this explicit for notational convenience. Note that the presence of time-varying exogenous covariates is helpful for identification (Brinch 2007; Richardson and Van den Berg, 2008). In addition, note that by decomposing this variable in the unemployment rate in December 31st, 1997 and its time-variation since that moment, we allow for a distinct impact of the regional and time variation in the unemployment rate.

[12] The implications of this assumption are discussed further in Section 4.2.

$$\ln \theta_p(t \mid x, V_p) = \ln \lambda_p(t) + x'\beta_p + V_p$$

(1)

$$\ln \theta_e(t \mid t_p, x, V_e) = \ln \lambda_e(t) + x'\beta_e + \delta(t \mid t_p, x).I(t>t_p) + V_e$$

where $\lambda_p(t)$ and $\lambda_e(t)$ represent the *baseline hazard* for transitions to AGR and to regular employment respectively, and where $I(.)$ is an indicator function, equal to 1 if the argument is true, and to 0 otherwise. Consequently, $\delta(t \mid t_p, x)$ measures the impact of a transition to AGR on the transition to regular employment, independently of whether the worker is still part-time employed or has returned to unemployment after participating for a while in the program. This impact may vary with elapsed unemployment duration t, with the starting time of the program t_p and with x. Consequently, the treatment effect may also depend on the elapsed time since the start of supported part-time employment $t-t_p$. Note, however, that $\delta(t \mid t_p, x)$ cannot depend on an unobserved covariate. We will discuss the consequence of this restriction in Section 4.2.

In the benchmark model we allow for an interaction with the time since the program start to capture potential "locking-in" effects of the AGR, but do not allow it to vary with other observed covariates x: $\delta(t \mid t_p, x) = \delta_0 + \delta_1.(t-t_p)$.[13] In Section 2 we indeed explained that the income support and the 100% implicit marginal implicit marginal tax rate of the AGR, reduces the incentive of AGR-recipients to accept regular jobs. This negative effect on the transition rate to regular employment should, however, diminish as time since the program start elapses, because gradually more AGR-recipients lose their jobs and are no longer entitled to the supplement. It therefore follows that a locking-in effect implies that $\delta_0<0$ and $\delta_1>0$.

Kyrrä (2008) and Kyrrä (2009) proposed a different specification to identify the locking-in effect. They define two treatment indicators by distinguishing those workers who are still receiving income support from those who returned to unemployment afterwards. However, if one does not take into account that the return to unemployment may be highly selective, then such a specification may invalidate a causal interpretation of the coefficients associated with the treatment indicators. Let us explain why this is so on the basis of an example.

Assume that the population of unemployed workers consists in "movers" and "stayers". Suppose that movers are much more likely to leave unemployment than stayers, but they are also much more likely to return to unemployment once they have been employed (irrespectively of this employment being regular or not). Consequently, if movers cannot fully be distinguished from stayers on the basis of observable characteristics, then, since, among the treated population, movers are more likely to have returned to unemployment than stayers, the post-treatment effect would be upward biased, since it reflects that movers are more likely to find a job. Conversely, there will proportionally more stayers within the population that hasn't yet returned to unemployment, leading to a downward bias of the "locking-in" effect. Since modeling this selectivity is beyond the scope of this research, we avoid such a specification of the locking-in effect.[14]

[13] In order to test for non-linear "locking-in" effects, we also estimated a model in which we allowed for an interaction with the square of ($t-t_p$). Since the estimates of this specification did not differ qualitatively from the benchmark model, we do not report these results below.

[14] As explained in Section 4.2, the presence of unobserved heterogeneity can also bias the locking-in effect as we specified it. However, in this case we know the direction of the bias, since it's well known that dynamic sorting biases the duration dependence downwards.

In order to verify whether the treatment effect is heterogeneous, we also estimate models in which we interact the treatment indicator with a limited number of the observed explanatory variables x. However, since there are only few transitions to the AGR in the data, we cannot estimate a model in which the treatment indicator is interacted with all these variables simultaneously.[15] To avoid biases induced by over-parameterization, we therefore allow the treatment effect to vary in only one dimension at a time. We allow the treatment to depend on (1) UB duration t; (2) on the level of education (primary and lower secondary versus higher secondary and higher education); and (3) the regional variation of the unemployment rate at the end of 1997. These interactions seem particularly interesting from a policy perspective.

In our data, time is not measured continuously, but on a quarterly basis. This time-grouping has consequences for identification, but discussion of these are delayed to Section 4.2. The time-grouping is explicitly taken into account in the specification of the baseline hazard and of the likelihood function. The baseline hazard is specified as piecewise constant within each quarter. To this purpose the time axis is divided into $K=15$ quarterly intervals. The baseline hazard can then be written:

$$(2) \qquad \ln \lambda_j(t) = \sum_{k=1}^{K} \alpha_k^j d_k^j \qquad \textit{for } j=p, e$$

where the α_k^j 's are the parameters to be estimated and where $d_k^p \,(d_k^e)$ is an indicator equal to 1 if a transition to AGR (regular employment) occurs during interval k-th quarter and to 0 otherwise. A worker who is still employed after 15 months is treated as a right censored observation. Since in the data the number of transitions to AGR is relatively small we impose that the α_k^p 's are equal for $k=1,\dots4,$ for $k=5,\dots8,$ and for $k=9,\dots15$.

The model is estimated by Maximum Likelihood, using the BHHH algorithm. We distinguish four types of likelihood contributions: (1) l_c for unemployed individuals who did not leave to any destination until the end of the k_c-th quarter[16]; (2) l_e for individuals finding a regular job in the k-th quarter; (3) l_{pc} for individuals entering AGR in the k_p-th quarter and subsequently right censored in that state; (4) l_{pe} for individuals entering AGR in the k_p-th quarter and subsequently finding a regular job in the k_e-th quarter ($k_e>k_p$). We derive these likelihood contributions taking the quarterly grouping of the data explicitly into account. We first derive these likelihood contributions conditional on the unobserved covariates V. Subsequently, we find the unconditional likelihood contributions by integrating out V on the basis of the appropriate joint distribution function.

The first mentioned likelihood contribution is given by the survival rate in unemployment at the end of k_c-th quarter, $S_u(k_c)$, where the u index denotes unemployment. The contribution can be expressed in terms of the hazard functions:

$$(3) \qquad l_c(V) = S_u(k_c) = Pr(T_p > k_c,\, T_e > k_c) = exp\left[-\sum_{j=1}^{k_c}\left[\theta_p(j|x,V_p) + \theta_e(j|j \le k_p, x, V_e)\right]\right]$$

[15] We estimated a model in which we included all the mentioned interaction effects simultaneously. None of the interactions were significantly different from zero in this case.

[16] Women experiencing a transition to inactivity or to another ALMP are censored at the end of the quarter preceding this transition. Women who are unemployed during the whole observation period are censored at the end of year 2001.

where we ignore the conditioning in the second and third term for notational convenience.

Next, the unconditional probability of a transition to destination $j=p,\ e$ in the k_j-*th* quarter can be written as the product of still being unemployed at the end of the (k_j-1)-*th* quarter and the conditional probability of a transition within the k_j-*th* quarter, conditional on not having left unemployment before the k_j-*th* quarter:

(4) $$\Pr(k_j - 1 \le T_j < k_j) = \Pr(T_p > k_j - 1, T_e > k_j - 1)\Pr(k_j - 1 \le T_j < k_j | T_p > k_j - 1, T_e > k_j - 1)$$

where we, again, ignore the conditioning on $k_j \le k_p$, x and on V for notational convenience. The first term of the product is given in Equation (3). Following Cockx (1997, p. 396-397) the second term is:

(5) $$\Pr(k_j - 1 \le T_j < k_j | T_p > k_j - 1, T_e > k_j - 1) = \frac{\theta_j(k_j)}{\theta_p(k_j) + \theta_e(k_j)}\left(1 - \exp\left[-\left(\theta_p(k_j) + \theta_e(k_j)\right)\right]\right)$$

Consequently, by replacing (3) and (5) in (4) we obtain the $Prob(k_j\text{-}1<T_j\le k_j|x,V)$ for $j=p,\ e$:

(6) $$l_j(V) = \frac{\theta_j(k_j | k_j \le k_p, x, V_p)}{\theta_p(k_j | x, V_p) + \theta_e(k_j | k_j \le k_p, x, V_e)}\ [S_u(k_j\text{-}1 \mid x,V) - S_u(k_j \mid x,V)]$$

This expression has an intuitive interpretation. The second term of the product is the probability of leaving unemployment in the k_j-*th* quarter. The first term is the conditional probability of leaving unemployment to destination j in the k_j-*th* quarter, given that one leaves unemployment in that quarter.

Finally, we derive the likelihood contributions l_{pc} and l_{pe}. The first component of these contributions coincides, since in both cases it is given by the probability to make a transition to a part-time AGR-supported job in the k_p-*th* quarter: $l_p(V)$. The second components are, however, different and given by, respectively, the conditional probability of remaining unemployed and the conditional probability of making a transition to a regular job in the k_e-*th* quarter ($k_e > k_p$). This results in the following expressions:

(7) $$\begin{aligned} l_{pc}(V) &= l_u(V)\exp\left[-\sum_{j=k_p+1}^{k_c}\theta_e\left(j | j > k_p, x, V_e\right)\right] \\ l_{pe}(V) &= l_p(V)\left\{\exp\left[-\sum_{j=k_p+1}^{k_e-1}\theta_e\left(j | j > k_p, x, V_e\right)\right] - \exp\left[-\sum_{j=k_p+1}^{k_e}\theta_e\left(j | j > k_p, x, V_e\right)\right]\right\} \end{aligned}$$

In the above expressions V is unobserved. We obtain the unconditional likelihood contributions by integrating V out:

(8) $$l_m = \int_V l_m(V)dG(V) \qquad \text{for } m=c,\ e,\ pc,\ pe$$

where $G(V)$ is the joint distribution function of the unobserved heterogeneity terms.

The unconditional log-likelihood can then be written as the sum of unconditional individual log-likelihood contributions:

$$(9) \qquad \mathcal{L} = \sum_{i=1}^{N} \left\{ J_{ei} \ln(l_{ei}) + J_{ei} \ln(l_{pi}) + J_{pci} \ln(l_{pci}) + J_{pei} \ln(l_{pei}) \right\}$$

where J_{mi} is equal to 1 if l_{mi} is the contribution of individual i to the likelihood ($m = c, e, pc, pe$), and to 0 otherwise.

Gaure *et al.* (2007) show that, in order to get unbiased estimates one has to specify the heterogeneity distribution correctly. In order to do so, we implement a non-parametric approximation of the heterogeneity distribution using a finite number of 'points of support' (Lindsay, 1983 ; Heckman and Singer, 1984).

First, we impose a one-factor loading specification for $G(V)$, in which the factor consists in two points of support. This specification is widely used in the literature. Its main drawback is that it strongly constrains the correlation between unobserved heterogeneity terms: only perfect correlation or no correlation is allowed (Van den Berg, 2001). To overcome this problem, we impose a second, more flexible specification for $G(V)$, using a discrete distribution with 4 points of supports. This flexible distribution allows for any type of correlation between V_e and V_p.

In the first specification, we assume that V_e can take two different values v_{e1} and v_{e2}, and that v_{pj} is defined as the product of v_{ej} and γ, a parameter to be estimated: $v_{pj} = \gamma . v_{ej}, j=1,2$. As the results, the probabilities associated to the points of support can be defined as:

$$(10) \qquad P_1 = \mathrm{Prob}(v_e = \mathrm{v}_{e1},\ v_p = \gamma . \mathrm{v}_{e1})$$
$$P_2 = \mathrm{Prob}(v_e = \mathrm{v}_{e2},\ v_p = \gamma . \mathrm{v}_{e2})$$

We specify P_1 and P_2 using a Logit model:

$$(11) \qquad P_1 = \frac{\exp \lambda}{1 + \exp \lambda} \qquad \text{and} \qquad P_2 = 1 - P_1 = \frac{1}{1 + \exp \lambda}$$

In the second specification, with 4 points of support, we assume that v_m $(m = p, e)$ can take two different values v_{m1} and v_{m2}. The four resulting probabilities are defined as follows:

$$(12) \qquad P_{11} = \mathrm{Prob}(v_e = \mathrm{v}_{e1},\ v_p = \mathrm{v}_{p1}) = p_1$$
$$P_{12} = \mathrm{Prob}(v_e = \mathrm{v}_{e1},\ v_p = \mathrm{v}_{p2}) = p_2$$
$$P_{21} = \mathrm{Prob}(v_e = \mathrm{v}_{e2},\ v_p = \mathrm{v}_{p1}) = p_3$$
$$P_{22} = \mathrm{Prob}(v_e = \mathrm{v}_{e2},\ v_p = \mathrm{v}_{p2}) = p_4$$

Probabilities p_1 to p_4 are specified using a multinomial Logit model:

$$(13) \qquad p_j = \frac{\exp \lambda_j}{1 + \sum_{i=1}^{3} \exp \lambda_i} \ \text{for } j = 1, \ldots, 3 \qquad \text{and} \qquad p_4 = 1 - \sum_{j=1}^{3} p_j = \frac{1}{1 + \sum_{i=1}^{3} \exp \lambda_i}$$

4.2. Identification of the treatment effect

Abbring and Van den Berg (2003) showed that $\delta(t|\ t_p,\ x)$ in model (1) is non-parametrically identified for single-spell data provided that:

(1) Agents neither anticipate the starting date of the treatment, nor the moment at which they are hired in a regular job; They may, however, know the *distribution* of these moments;
(2) The econometrician has sufficiently precise information concerning the timing of transitions;
(3) Observed and unobserved individual characteristics influence the rates of transitions (to subsidised employment *and* to regular employment) of untreated individuals proportionally;
(4) The treatment effect may not be heterogeneous in *un*observed characteristics of program participants;
(5) There are at least two not linearly dependent *continuous* explanatory variables;
(6) Variables X and V are independently distributed;
(7) There are no unobserved random shocks that are correlated with the timing of the treatment.

Let us discuss these assumptions in turn.

Assumption (1):

If workers anticipate the starting date of the treatment, then they could use this information to modify their behaviour in accordance. If that was the case, then these individuals should be considered as treated, from the moment they change their behaviour. Considering these workers as members of the control group would bias the treatment effect. Similarly, a worker who knows that she will be hired in regular job in the future, independently of accepting an income supported part-time job, has less interest in accepting a part-time job beforehand. Such workers artificially inflate the transition rate of non-program participants and therefore bias the treatment effect downwards.

Anticipation could e.g. occur if a worker knew that she has successfully passed a selection procedure with respect to a particular vacancy (of either a part-time or a regular job) and that the hiring occurs later. Even if such situations may occur regularly in reality, the period between communication of the hiring decision and effective hiring is in general quite short. Once both sides agree on the employment, the employer has no interest in postponing the hiring decision, since once he declares a vacancy he usually wants to fill it as soon as possible; the worker has no interest in delaying the recruitment, since, being unemployed, her interest is to be hired as soon as possible. It is therefore unlikely that the bias induced by anticipation is large.

It is important to distinguish anticipation effects from *ex ante* effects. The *ex ante* knowledge of the provided income support to part-time jobs may affect the *distribution* of transitions to regular and part-time jobs. For instance, because of the program, unemployed workers may change their search strategy by reducing search activities in the regular channel in favour of enhanced search effort for part-time jobs. Similarly, employers could reduce their offers of regular jobs in favour of part-time employment.

In order to evaluate such *ex ante* effects one needs to contrast a world *with* the policy to one *without.* Such a counterfactual analysis requires much more information and is beyond the scope of the analysis in this paper (see e.g. Abbring and Van den Berg, 2005; Blundell *et al.* 2004; Heckman *et al.* 1998). In any case, given the relatively small target group of the AGR program we expect these general equilibrium effects to be negligible.

The analysis here identifies an *ex post* effect. The *ex post* effect measures, for a given environment *with* the policy in place, the effect of the AGR program on the *individual* transition rate to a regular job. This effect is identified even in the presence of *ex ante* effects, as long as there is no anticipation.

Assumption (2):

One could argue that this condition is not satisfied, since the duration data are grouped in quarters. However, Gaure *et al.* (2007) have shown, using an extensive Monte Carlo analysis, that Abbring and Van den Berg (2003)'s method is extremely reliable, even for time-grouped data as long the time-grouping is explicitly taken into account in the formulation of the likelihood function. This is what we have done.

Assumption (3):

The assumption of proportionality is fundamental. Gaure *et al.* (2007) have shown that departures from non-proportionality can induce serious biases. In principle, we could test for departures from the MPH assumption, since in the presence of a time-varying exogenous covariate, such as the unemployment rate in the current application, this assumption is no longer required for identification (Brinch 2007).[17] Testing for such specification problems is, however, beyond the scope of the current paper.

Assumption (4):

In principle, we can allow for unobserved heterogeneity in the treatment effect if the transition rate of programme participants to regular employment is proportional in all three arguments (unemployment duration, observed and unobserved characteristics). This holds as long as this transition rate depends neither on the moment of entry into treatment, nor on the duration elapsed since that moment. Alternatively, Richardson and Van den Berg (2008) prove non-parametric identification of a model that allows for unobserved heterogeneity in the treatment effect if the last mentioned transition is proportional in the duration elapsed since entry in the programme, and in observed and unobserved characteristics, but does not depend on unemployment duration or the moment since entry. Allowing for unobserved heterogeneity in the treatment effect would complicate the analysis drastically. Moreover, in view of the limited number of program participants observed in our data, it is doubtful that we could obtain interpretable results. We therefore maintain the assumption that the treatment effect is homogeneous with respect to observables. Consequently, we must take care in interpreting the time profile of the treatment effect with the time since the start of the treatment. Richardson and Van den Berg (2008) point out that this time profile may be downwards biased by a dynamic sorting effect: Treated individuals with unobserved characteristics such that their treatment effect is high are (holding every other characteristic constant) more likely to leave unemployment quickly.

[17] The MPH is neither required if one observes multiple spells for the same individual (Abbring and Van den Berg 2003).

Assumption (5):

This is a technical sufficient condition for identification if there are no time-varying explanatory variables. It is fulfilled here, since age and the unemployment rate are two continuous explanatory variables. Note, however, that in our empirical application this condition is not essential, since the model is over-identified by including the unemployment rate as a time-varying covariate. Using an extensive Monte Carlo analysis, Gaure *et al.* (1997, p. 1186) indeed show that, with "some exogenous variation in hazard rates over calendar time, no subject-specific covariates are required in order to identify treatment and spell-duration effects".

Assumption (6):

It is unlikely that unobservable and observable covariates are independent of each other. A violation of this assumption does not affect, however, the consistency of our main parameter of interest, δ. It only means that we can no longer give a structural interpretation of the coefficients of the observed covariates, x (see Wooldridge 2005 and Crépon *et al.,* 2006, p.14 for a similar argumentation). In addition, a violation of the assumption means that we can no longer provide a structural interpretation of the variation of the treatment effect with the observed covariates x.

Assumption (7):

This assumption is not explicitly imposed in Abbring and Van den Berg (2003, 2004), but is implicit in the model. We try to avoid seasonal or business cycle shocks that are correlated with the start of the program by conditioning on a *time-varying* indicator of the local unemployment rate.[18]

5. Results

The estimation results of the benchmark model are reported in Table 2. The benchmark model includes an interaction between the treatment indicator and the time since program start (t-t_p). This specification allows testing for potential locking-in effects in the AGR policy. Table 3 reports the results for the models with a selected number of other interaction effects: (1) no interaction effect; (2) the duration of UB receipt; (3) the regional unemployment rate in December 1997; (4) the level of education.

To facilitate reading, Table 2 is divided in three panels. Panel 2.a reports the parameters regarding the transitions to regular employment and panel 2.b regarding transitions to subsidised employment (AGR policy). Panel 2.c displays goodness-of-fit statistics and information on the unobserved heterogeneity distribution. Each panel covers the results of three different specifications: (1) no correction for selection on unobservables, (2) correcting with the one-factor loading (two points of support) heterogeneity distribution, and (3) correcting with the two-factor loading (four points of support) heterogeneity distribution. For each set of results, we report the estimated coefficients, the proportional effect on the hazard by taking the exponential of the estimated coefficient, the standard error of the estimated coefficient, and the p-value. We concentrate our discussion on the impact of AGR on the transition to regular employment. Note that the treatment effect without correction for

[18] See also footnote 11.

selection on unobservables is lower than the one that accounts for such a selection. This means that participants are on average less employable in terms of unobserved characteristics than non-participants.

TABLES 2.a, 2.b, 2.c ABOUT HERE

Table 2.c reveals that the model with a two-factor loading (4 points of support) heterogeneity distribution performs best according to the Akaike Information Criterion (AIC). We therefore retain it as our preferred specification and restrict our discussion to this specification. From the before last line of Table 2.a, we learn that at the start of the program (for $t\text{-}t_p = 0$) participation in AGR has a large positive and highly significant effect on the transition rate to regular employment. As compared to the counterfactual of no participation, participation multiplies this transition rate by 2.77. This seems a huge effect. However, since in the counterfactual of no participation the transition to regular employment is low, this multiplier corresponds to a reasonable change in the conditional probability of a transition to regular employment. For instance, in the quarter following the program start, this probability is 0.08 on average in the counterfactual of no participation and it increases to 0.21 if the worker participates in the program.[19]

In contrast to Kyrrä (2008) and Kyrrä *et al.* (2009), the estimates of the treatment effect provide no evidence of a locking-in effect[20]: The treatment effect is neither negative at the start of the program nor is it significantly increasing with the time since the start of the program ($t\text{-}t_p$). In addition, the fact that the treatment effect does not significantly increase with ($t\text{-}t_p$) suggests that the positive effect is more generated by a positive signalling effect than by the accumulation of transferable human capital in the part-time job. This is consistent with the evidence (mentioned in Section 2) that the returns to experience of low-skilled or disadvantaged workers are low. It is also consistent with the study of Cockx and Picchio (2009) who study, on the same sample, the stepping-stone effect of short-lived jobs to long-lasting jobs. They find evidence that, for disadvantaged workers, this positive effect is better explained by the positive signal attached to job finding than by effects related to human capital formation. Nevertheless, the human capital formation could play a more important role than revealed at first sight. For, as mentioned above, the dependence on ($t\text{-}t_p$) can be downward biased due to a dynamic sorting induced by unobserved heterogeneity within the treated population. In view of the limited number of AGR recipients in the data, however, it would be too ambitious to account for unobserved heterogeneity in the treatment effect (see Richardson and Van den Berg, 2008).

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[19] Formally, these probabilities are calculated as follows. First, note that this probability can be written in terms of the mixture survivor functions in the following way: $Prob(k_p < T_e \leq k_p + 1 | T_e > k_p, k_p - 1 < T_p \leq k_p; x)$ $= 1 - \frac{S_m(k_p+1|x)}{S_m(k_p|x)}$, where $S_m(k_p|x) = \sum_{i=1}^{2}\sum_{j=1}^{2} P_{ij}\, exp\left[-\sum_{s=1}^{k_p}[\theta_p(s|x, v_{pi}) + \theta_e(s|x, v_{ej})]\right]$ and $S_m(k_p + 1|x) = \sum_{i=1}^{2}\sum_{j=1}^{2} P_{ij}\, exp\left[-\sum_{s=1}^{k_p}[\theta_p(s|x, v_{pi}) + \theta_e(s|x, v_{ej})] - \theta_e(k_p + 1|x, v_{ej})\right]$. Using the estimation results, we compute these probabilities for all treated individual, once conditional on treatment and once conditional on the counterfactual of no treatment. Subsequently, we calculate the averages of these two probabilities over the treated individuals.

[20] In Section 4.1 we explained why in these studies the locking-in effect might be spurious. Another reason why our results deviate from theirs is that we analyze a very specific population of disadvantaged youth: Kyrrä *et al.* (2009) also report more positive effects for youth and immigrants.

Table 3 reports a sensitivity analysis in which we allowed the treatment indicator to be interacted with some selected explanatory variables. As higher explained, data problems did not allow to include these interactions jointly. Since the interaction with (t-t_p) was not significantly different from zero, we also report in the first lines of Table 3, as a point of comparison, the model without any interaction effect and the benchmark model. Since for all interactions the model with a two-factor loading (4 points of support) heterogeneity distribution performs best according to the AIC, we limit our discussion to this specification.

TABLE 3 ABOUT HERE

Table 3 shows that only one interaction effect is significant at the 5% level: each additional quarter by which entry in a part-time job with income support is delayed increases the transition rate to regular employment by 15%. This is again consistent with the signalling hypothesis. The longer an individual remains unemployed, the more she risks to be stigmatised as being non-employable. In such circumstances, signalling employability by accepting a low-paid part-time job may pay-off increasingly.

There is some indication that accepting an AGR-supported part-time job is more effective in districts where the unemployment rate is high: the interaction is positive and just a little above the conventional significance level of 10%. It makes sense that accepting a precarious job, such as those supported by the AGR, is a more positive signal to employers if one lives in a region with few employment opportunities than if one lives in a district where the unemployment rate is low. Finally, even if there is no significant evidence that the effectiveness of AGR depends on the schooling level, the sign of the interaction effects suggests that lower schooled youth would benefit more from the AGR than higher schooled youth. This is consistent with the previous argument that the effect is stronger for workers with less employment opportunities.

We now briefly discuss the secondary results reported for the benchmark model in Table 2. It brings to light determinants of the transition to employment often quoted in the literature: nationality, education level, and location. With all three heterogeneity specifications, we find that non-EU women have a significantly lower transition rate to both regular employment and the AGR. Women who have less than 12-14 years of schooling are less likely to experience a transition to regular employment. A point noting is that, despite the indication that the effectiveness of AGR decreases with the educational level, we find that women who are more educated are also more likely to enter subsidised part-time employment. The regional variable displays disparities between Flanders on the one hand, and the Walloon region and Brussels on the other. Regional location does not influence the transition to subsidised employment, but affects the transition to regular employment: young Flemish women have a significantly higher transition rate to regular employment. This just reflects that the labour market is much more depressed in Wallonia and Brussels than in Flanders.

Conclusion

In this paper, we evaluated the stepping-stone effect of an income-support policy (known as AGR) for unemployed persons accepting to work part-time. The analysis was performed on a sample of 8630 long-term unemployed young women without prior labour market experience. The econometric model exploited the "timing of events" to take the selection on both, observables and unobservables, into account.

The findings confirm that subsidised part-time employment can be a stepping-stone to regular employment. Indeed, participation in this type of low-paid part-time employment increases considerably the transition rate to regular employment: In the quarter following the transition into part-time employment, the average conditional probability of finding regular employment increases to 0.21. If the part-time job is not taken, this probability is equal to 0.08 only. Contrary to other studies (Kyrrä 2008; Kyrrä *et al.* 2009), we do not find any evidence of a locking-in effect. In our study, the transition to regular jobs accelerates right from the start of the part-time job. This suggests that the stepping-stone effect is induced through signalling rather than human capital accumulation. The fact that the stepping-stone effect increases with unemployment duration (and, to a lesser extent, with the unemployment rate and the level of education) reinforces this interpretation: workers with fewer job opportunities are more likely to send a positive signal, even by accepting low-paid part-time employment, as evaluated in this study.

Despite this positive finding, one should keep in mind that it is valid only for the population retained for analysis. This population consists of long-term unemployed youth without any work experience. In addition, if the AGR works as a signalling device, it may only work if one can distinguish oneself from other workers by accepting such income supported part-time jobs. For instance, a policy that would impose such jobs on all disadvantaged youth is probably less effective, since participation would then no longer signal the qualities of the worker.

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Table 1 – summary statistics

VARIABLES	sub-groups			TOTAL
	Direct transitions[1]	AGR (treated)	Censored	
Age in years at the end of 1997	20.75 (2.02)	20.42 (2.03)	20.14 (1.90)	20.39 (1.98)
Nationality :				
Belgian	0.911	0.920	0.853	0.877
Non-Belgian EU	0.048	0.0457	0.059	0.055
Non EU	0.041	0.0343	0.088	0.068
Education level:				
Primary (6 to 9 years schooling)	0.039	0.074	0.107	0.079
Lower secondary (9 to 12 years)	0.139	0.234	0.281	0.223
Higher secondary (12 to 14 years of schooling)	0.495	0.457	0.469	0.479
Higher education, non-university (14 years of schooling and more)	0.187	0.160	0.080	0.124
University (16 years of schooling and more)	0.075	0.069	0.033	0.050
Other	0.005	0.006	0.010	0.008
Unknown	0.060	0	0.021	0.036
Relation to head of household:				
Head	0.066	0.177	0.114	0.096
Spouse	0.033	0.051	0.070	0.055
Child	0.802	0.640	0.674	0.724
Other	0.018	0.040	0.026	0.023
No family relationship	0.081	0.091	0.115	0.101
# persons in household:				
# of persons, [0-1) year-old	0.030	0.091	0.079	0.060
# of persons, [1-3) year-old	0.024	0.034	0.063	0.047
# of persons, [3-6) year-old	0.037	0.017	0.055	0.047
# of persons, [6-12) year-old	0.112	0.069	0.140	0.128
# of persons, [12-18) year-old	0.250	0.211	0.272	0.262
# of persons, [18-30) year-old	0.488	0.377	0.484	0.483
# of persons, [30-50) year-old	0.630	0.446	0.569	0.591
# of persons, [50-65) year-old	0.368	0.297	0.295	0.324
# of persons, [65-75) year-old	0.033	0.046	0.036	0.035
# of persons, [75+) year-old	0.019	0.034	0.014	0.016
Unemployment rate, end 1997	25.84 (8.58)	28.05 (8.12)	27.64 (8.17)	26.93 (8.38)
Region :				
Flanders	0.303	0.200	0.207	0.245
Walloon region	0.598	0.680	0.666	0.639
Brussels	0.098	0.120	0.127	0.116
Number of observations	3.458	175	4.997	8.630

[1] *This group is made of women who experience a direct transition to a regular job (full-time or paying more than the full-time minimum wage) within the observation period*

Columns: average value
In brackets: standard deviation

Note: The month of entry into unemployment is not presented here; this information is available upon request from the authors.

Table 2a – duration model estimates: transition to regular employment

VARIABLES	No heterogeneity				Heterogeneity : 2 pts of support				heterogeneity: 4 pts of support			
	β	*Exp β*	*σ*	*p-val.*	*β*	*Exp β*	*σ*	*p-val.*	*β*	*Exp β*	*σ*	*p-val.*
Constant	-2.57	0.08	0.13	0.00								
Age in 1997	0.01	1.01	0.01	0.65	0.01	1.01	0.01	0.60	0.01	1.01	0.01	0.50
Nationality :												
Belgian (reference)												
Non-Belgian EU	-0.15	0.86	0.08	0.07	-0.14	0.87	0.08	0.09	-0.15	0.86	0.08	0.07
Non EU	-0.64	0.53	0.09	0.00	-0.67	0.51	0.09	0.00	-0.67	0.51	0.09	0.00
Education level:												
Primary	-0.88	0.41	0.09	0.00	-0.91	0.40	0.10	0.00	-0.92	0.40	0.10	0.00
Lower secondary	-0.60	0.55	0.05	0.00	-0.62	0.54	0.06	0.00	-0.62	0.54	0.06	0.00
Higher *second. (ref.)*												
Higher, non-university	0.71	2.04	0.05	0.00	0.76	2.14	0.06	0.00	0.78	2.19	0.06	0.00
University	0.76	2.13	0.07	0.00	0.80	2.23	0.08	0.00	0.81	2.24	0.09	0.00
Other	-0.70	0.49	0.23	0.00	-0.73	0.48	0.24	0.00	-0.72	0.49	0.24	0.00
Unknown	0.95	2.59	0.08	0.00	0.96	2.62	0.09	0.00	0.97	2.63	0.09	0.00
Month of entry:												
January	0.14	1.15	0.14	0.32	0.14	1.15	0.14	0.34	0.12	1.13	0.14	0.39
February	-0.13	0.88	0.15	0.40	-0.13	0.88	0.16	0.40	-0.14	0.87	0.16	0.39
March	0.13	1.13	0.10	0.22	0.12	1.13	0.11	0.24	0.14	1.15	0.11	0.18
April (reference)												
May	0.08	1.09	0.06	0.13	0.09	1.09	0.06	0.15	0.09	1.09	0.06	0.13
June	0.07	1.07	0.04	0.12	0.07	1.07	0.05	0.13	0.09	1.09	0.05	0.07
July	-0.14	0.87	0.07	0.06	-0.15	0.86	0.08	0.04	-0.14	0.87	0.08	0.06
August	-0.27	0.76	0.10	0.01	-0.29	0.75	0.10	0.00	-0.28	0.76	0.10	0.01
September	-0.19	0.83	0.11	0.09	-0.17	0.84	0.12	0.15	-0.16	0.85	0.12	0.18
October	-0.18	0.84	0.12	0.14	-0.18	0.84	0.13	0.16	-0.16	0.85	0.13	0.20
November	-0.32	0.73	0.13	0.01	-0.32	0.73	0.13	0.02	-0.24	0.78	0.14	0.08
December	-0.16	0.85	0.14	0.24	-0.17	0.85	0.14	0.23	-0.15	0.86	0.14	0.28
Relation to the head :												
Head	-0.16	0.85	0.09	0.07	-0.15	0.86	0.09	0.10	-0.15	0.86	0.09	0.11
Spouse	-0.31	0.74	0.11	0.01	-0.32	0.73	0.12	0.01	-0.31	0.73	0.12	0.01
Child (ref.)												
Other	-0.17	0.85	0.14	0.24	-0.17	0.85	0.15	0.26	-0.21	0.81	0.15	0.15
No family relationship	-0.04	0.96	0.07	0.60	-0.03	0.97	0.08	0.73	-0.03	0.97	0.08	0.73
# of persons:												
# of [0-3) year-old	-0.53	0.59	0.07	0.00	-0.55	0.58	0.08	0.00	-0.55	0.58	0.08	0.00
# of [3-6) year-old	0.05	1.06	0.08	0.49	0.06	1.06	0.08	0.46	0.07	1.07	0.08	0.43
# of [6-18) year-old	-0.09	0.92	0.02	0.00	-0.09	0.91	0.02	0.00	-0.10	0.91	0.02	0.00
# of [18-30) year-old	0.00	1.00	0.02	0.97	0.00	1.00	0.02	0.99	0.01	1.01	0.02	0.82
# of [30-50) year-old	0.13	1.14	0.03	0.00	0.14	1.16	0.04	0.00	0.15	1.16	0.04	0.00
# of [50-65) year-old	0.05	1.05	0.04	0.17	0.05	1.05	0.04	0.20	0.06	1.07	0.04	0.09
# of [65-75) year-old	-0.10	0.91	0.09	0.27	-0.11	0.90	0.09	0.22	-0.10	0.91	0.09	0.28
# of [75+) year-old	0.19	1.20	0.12	0.12	0.19	1.20	0.12	0.13	0.17	1.19	0.13	0.17
Unemp. rate, end 97	-0.02	0.98	0.00	0.00	-0.02	0.98	0.00	0.00	-0.02	0.98	0.00	0.00
Δ unemployment rate	-0.02	0.98	0.01	0.02	-0.02	0.98	0.01	0.02	-0.02	0.98	0.01	0.04
Region :												
Walloon region (ref.)												
Flanders	0.30	1.35	0.07	0.00	0.34	1.40	0.07	0.00	0.34	1.40	0.07	0.00
Brussels	0.09	1.09	0.07	0.19	0.10	1.10	0.07	0.15	0.08	1.09	0.07	0.22
Baseline : 2nd quarter	-0.15	0.86	0.06	0.01	-0.13	0.88	0.06	0.03	-0.12	0.89	0.06	0.04
3rd quarter	-0.31	0.73	0.07	0.00	-0.27	0.76	0.07	0.00	-0.26	0.77	0.07	0.00
4th quarter	-0.33	0.72	0.07	0.00	-0.27	0.76	0.08	0.00	-0.26	0.77	0.08	0.00
5th quarter	-0.38	0.69	0.08	0.00	-0.31	0.74	0.09	0.00	-0.28	0.75	0.09	0.00
6th quarter	-0.48	0.62	0.09	0.00	-0.39	0.68	0.10	0.00	-0.37	0.69	0.10	0.00
7th quarter	-0.59	0.55	0.11	0.00	-0.49	0.61	0.11	0.00	-0.47	0.63	0.11	0.00
8th quarter	-0.71	0.49	0.12	0.00	-0.61	0.55	0.13	0.00	-0.58	0.56	0.13	0.00
9th quarter	-0.81	0.45	0.14	0.00	-0.69	0.50	0.15	0.00	-0.66	0.52	0.15	0.00
10th q.	-0.92	0.40	0.17	0.00	-0.79	0.45	0.18	0.00	-0.76	0.47	0.18	0.00
11th q.	-0.75	0.47	0.16	0.00	-0.61	0.54	0.17	0.00	-0.58	0.56	0.17	0.00
12th q.	-1.51	0.22	0.27	0.00	-1.36	0.26	0.28	0.00	-1.34	0.26	0.28	0.00
13th q.	-1.27	0.28	0.25	0.00	-1.12	0.33	0.26	0.00	-1.09	0.34	0.26	0.00
14th q.	-1.76	0.17	0.33	0.00	-1.59	0.20	0.34	0.00	-1.57	0.21	0.34	0.00
15th q.	-2.59	0.08	0.64	0.00	-2.40	0.09	0.65	0.00	-2.37	0.09	0.65	0.00
Effect of AGR δ_0	**0.86**	**2.37**	**0.18**	**0.00**	**0.75**	**2.11**	**0.20**	**0.00**	**1.02**	**2.77**	**0.24**	**0.00**
Interaction-effect $\delta_1.(t - t_p)$	**-0.05**	**0.95**	**0.06**	**0.33**	**-0.06**	**0.94**	**0.06**	**0.25**	**0.04**	**1.04**	**0.08**	**0.64**

Tableau 2b - duration model estimates: transition to ALMP

VARIABLES	No heterogeneity				Heterogeneity : 2 pts of support				heterogeneity: 4 pts of support			
	β	*Exp β*	*σ*	*p-val.*	*β*	*Exp β*	*σ*	*p-val.*	*β*	*Exp β*	*σ*	*p-val.*
Constant	-5.82	0.00	0.68	0.00								
Age in 1997	-0.08	0.92	0.06	0.16	-0.08	0.92	0.06	0.17	-0.09	0.92	0.06	0.15
Nationality :												
Belgian (reference)												
Non-Belgian EU	-0.24	0.79	0.40	0.54	-0.23	0.79	0.40	0.56	-0.24	0.78	0.40	0.55
Non EU	-0.93	0.40	0.45	0.04	-0.96	0.38	0.45	0.03	-0.90	0.41	0.46	0.05
Education level:												
Primary	-0.57	0.56	0.34	0.09	-0.62	0.54	0.34	0.07	-0.54	0.58	0.34	0.12
Lower secondary	-0.28	0.76	0.21	0.19	-0.31	0.73	0.22	0.15	-0.25	0.78	0.22	0.24
Higher *second. (ref.)*												
Higher, non-university	0.93	2.53	0.28	0.00	0.99	2.70	0.29	0.00	0.88	2.40	0.31	0.00
University	1.28	3.61	0.39	0.00	1.35	3.84	0.40	0.00	1.25	3.48	0.41	0.00
Other or unknown	-1.35	0.26	1.23	0.27	-1.37	0.25	1.23	0.27	-1.38	0.25	1.24	0.27
Month of entry:												
January	0.08	1.09	0.54	0.88	0.13	1.14	0.55	0.81	0.08	1.08	0.54	0.89
February	-0.78	0.46	0.80	0.33	-0.77	0.46	0.81	0.34	-0.78	0.46	0.81	0.33
March	-0.63	0.53	0.63	0.32	-0.62	0.54	0.64	0.33	-0.65	0.52	0.65	0.32
April (reference)												
May	0.09	1.09	0.26	0.74	0.10	1.10	0.27	0.72	0.07	1.07	0.27	0.80
June	-0.47	0.63	0.24	0.05	-0.45	0.64	0.24	0.06	-0.49	0.61	0.24	0.04
July	0.26	1.29	0.27	0.35	0.25	1.29	0.27	0.36	0.26	1.29	0.28	0.35
August	-1.16	0.31	0.62	0.06	-1.17	0.31	0.62	0.06	-1.15	0.32	0.63	0.07
September	-0.08	0.92	0.45	0.85	-0.05	0.95	0.46	0.91	-0.11	0.90	0.46	0.81
October	0.48	1.61	0.40	0.23	0.49	1.64	0.40	0.22	0.47	1.61	0.41	0.25
November	-0.14	0.87	0.59	0.81	-0.13	0.88	0.59	0.83	-0.15	0.86	0.59	0.80
December	-0.14	0.87	0.56	0.80	-0.15	0.86	0.56	0.80	-0.14	0.87	0.57	0.80
Relation to the head :												
Head	-0.16	0.85	0.34	0.65	-0.16	0.85	0.35	0.64	-0.15	0.86	0.35	0.66
Spouse	-0.68	0.51	0.48	0.16	-0.72	0.49	0.48	0.14	-0.66	0.52	0.49	0.18
Child												
Other	0.25	1.28	0.52	0.63	0.26	1.29	0.53	0.63	0.27	1.31	0.54	0.61
No family relationship	-0.58	0.56	0.36	0.10	-0.58	0.56	0.36	0.10	-0.58	0.56	0.36	0.11
# of persons:												
# of [0-1) year-old	0.23	1.26	0.31	0.45	0.21	1.23	0.31	0.50	0.26	1.29	0.33	0.44
# of [1-18) year-old	-0.08	0.92	0.10	0.44	-0.09	0.92	0.10	0.41	-0.08	0.93	0.11	0.47
# of [18-30) year-old	-0.13	0.88	0.13	0.31	-0.13	0.88	0.13	0.30	-0.13	0.88	0.13	0.31
# of [30-75) year-old	-0.43	0.65	0.17	0.01	-0.43	0.65	0.17	0.01	-0.43	0.65	0.17	0.01
# of [75+) year-old	0.56	1.75	0.47	0.24	0.54	1.72	0.49	0.26	0.54	1.72	0.55	0.32
Unemp. rate, end 97	0.00	1.00	0.02	0.97	0.00	1.00	0.02	0.96	0.00	1.00	0.02	0.92
Δ unemployment rate	0.02	1.02	0.04	0.60	0.02	1.02	0.04	0.62	0.02	1.02	0.04	0.57
Region :												
Walloon region (ref.)												
Flanders	0.45	1.57	0.38	0.24	0.49	1.64	0.39	0.20	0.41	1.51	0.39	0.29
Brussels	0.23	1.26	0.31	0.46	0.25	1.28	0.31	0.42	0.23	1.25	0.31	0.47
Baseline : 5-8 quart.	0.18	1.20	0.23	0.43	0.24	1.27	0.23	0.30	0.13	1.14	0.27	0.63
9-15 quart.	0.26	1.29	0.30	0.39	0.36	1.44	0.31	0.24	0.17	1.19	0.39	0.65

Table 2c - duration model estimates: goodness-of-fit statistics and unobserved heterogeneity

GOODNESS-OF-FIT		No heterogeneity	Heterogeneity : 2 pts of support				heterogeneity: 4 pts of support			
Log-likelihood		-12102.40	-12096.50				-12092.30			
# of variables		88	90				93			
# of observations		8,630	8,630				8,630			
Akaike Information criterion		12190.4	12186.5				12185.3			
UNOBSERVED HETEROGENEITY		No heterogeneity	Heterogeneity : 2 pts of support				heterogeneity: 4 pts of support			
Points of support:			*β*	*Exp β*	*σ*	*p-val.*	*β*	*Exp β*	*σ*	*p-val.*
v_{e1}			-4.78	0.01	1.72	0.01	-2.50	0.08	0.14	0.00
v_{e2}			-2.51	0.08	0.14	0.00	-4.90	0.01	1.20	0.00
v_{p1}							-6.04	0.00	5.13	0.24
v_{p2}							-5.08	0.01	6.02	0.40
Gamma			2.28	9.74	0.30	0.00				
Probability mass (Logit):			*β*	*Exp β*	*σ*	*p-val.*	*β*	*Exp β*	*σ*	*p-val.*
Lambda / Lambda_1			-2.68	0.07	0.98	0.01	2.40	10.98	11.73	0.84
Lambda_2							-0.56	0.57	62.04	0.99
Lambda_3							-3.22	0.04	238.85	0.99
Resulting probabilities:			Heterogeneity : 2 pts of support				heterogeneity: 4 pts of support			
2 pts of support	4 pts of support									
P_1	P_{11}		0.06				0.87			
P_2	P_{12}		0.94				0.05			
	P_{21}						0.00			
	P_{22}						0.08			

Table 3 – Treatment Effects for Selected Subgroups

VARIABLES	No heterogeneity				Heterogeneity : 2 pts of support				heterogeneity: 4 pts of support			
	β	*Exp β*	*σ*	*p-val.*	*β*	*Exp β*	*σ*	*p-val.*	*β*	*Exp β*	*σ*	*p-val.*
Without interaction:												
Log-Likelihood	-12103.0				-12097.3				-12092.6			
AIC	12190.0				12186.3				12184.6			
Constant	0.73	2.08	0.12	0.00	0.58	1.79	0.15	0.00	1.05	2.86	0.23	0.00
***Quarters since transition to participation** ($t-t_p$):*[1]												
Log-Likelihood	-12102.4				-12096.5				-12092.3			
AIC	12190.4				12186.5				12185.3			
Constant	0.86	2.37	0.18	0.00	0.75	2.11	0.20	0.00	1.02	2.77	0.24	0.00
Interaction effect	-0.05	0.95	0.06	0.33	-0.06	0.94	0.06	0.25	0.04	1.04	0.08	0.64
Quarters entitled to UB before transition to participation:												
Log-Likelihood	-12098.8				-12093.7				-12088.4			
AIC	12186.8				12183.7				12181.4			
Constant	0.27	1.31	0.20	0.19	0.11	1.11	0.23	0.63	0.63	1.87	0.38	0.10
Interaction effect	0.13	1.13	0.04	0.00	0.12	1.13	0.04	0.00	0.14	1.15	0.06	0.03
Unemployment rate end 97:												
Log-Likelihood	-12101.2				-12095.2				-12091.0			
AIC	12189.2				12185.2				12184			
Constant	0.00	1.00	0.44	1.00	-0.18	0.83	0.45	0.68	0.18	1.20	0.61	0.77
Interaction-effect	0.03	1.03	0.01	0.05	0.03	1.03	0.01	0.04	0.03	1.03	0.02	0.12
Primary or lower secondary schooling:												
Log-Likelihood	-12101.4				-12095.4				-12092.1			
AIC	12189.4				12185.4				12185.1			
Constant	0.63	1.87	0.13	0.00	0.48	1.61	0.16	0.00	0.97	2.63	0.27	0.00
Interaction-effect	0.52	1.68	0.28	0.07	0.55	1.74	0.28	0.05	0.30	1.35	0.34	0.37

[1] This corresponds to the benchmark model reported in Table 2.

Interpretation:

The "*Constant*" refers to the estimate of the treatment effect in which the interaction effect is set to zero. The "*Interaction effect*" explains how the treatment effect evolves as the interaction variable changes by one unit.

For example, in the model without heterogeneity, programme participation at the start of the UB entitlement (zero quarters of entitlement) increases the logarithm of the transition rate to regular employment by 0.27, while the increase is 0.53 (=0.27+2*0.13) if the program is entered after 2 quarters of benefit receipt. Similarly, participation increases the log transition rate to regular employment by 1.15 (=0.63+0.52), for youth with at most a lower secondary schooling degree, while it would only increase by 0.63 for higher educated youth.