

Scrapping Benefits for Youth: Does it Bring Jobs or Just Misery?

Research proposal

Abstract

This research seeks to evaluate the behavioral effects (on job search, studying and home leaving) and the impact on poverty of the suppression of an unemployment benefit (the “activation allowance”) targeted at long-term unemployed youth in Belgium. The analysis is based on both a quasi-experimental (difference-in-differences) approach and the estimation of a structural behavioral model that incorporates behavioral biases.

The Great Recession of 2008 had a devastating impact on youth unemployment in Europe. The youth (under 25 years old) unemployment rate in the European Union (EU28) rose from 15.6% in 2008 to 23.7% in 2013. A similar evolution was observed in Belgium: the corresponding figures were 18.0% and 23.7% (Eurostat). Since then the figures have started to decline, but less so in Belgium. According to the last figures (2016Q3), the youth unemployment rate dropped to 18.5% in the EU28, while it is still as high as 21.8% in Belgium. Within this context the Belgian federal government has taken a number of measures, notably in unemployment insurance (UI), to combat youth unemployment. Belgium is one of the sole countries in the world in which school-leavers are eligible for non-means-tested UI based on educational qualifications without employment record. Entitlement to this “activation allowance” starts after a waiting period of twelve months conditional on registration as job seeker at the regional public employment service (PES). Because this scheme was regarded as an important disincentive to work, the Belgian government restricted qualifying conditions in several reforms between 2012 and 2015. In this research we evaluate the reform enacted on January 1, 2015. From that moment only individuals who applied for the activation allowance before the age of 25 (i.e. 24 at the first PES registration) could claim it. Before this reform the age threshold was 30 years. In addition, it was announced that from September 1 2015 onwards, adolescents younger than 21 (i.e. 20 at the first PES registration) could only claim after passing successfully six years of high school, while for those older than 21 the existing weaker educational requirements remained in force, i.e. completing (passing was not required) the sixth year of high school for those in the general track and the third year for any other track.

This reform is particularly interesting, because unlike in most existing studies the benefit level is not just altered, it is *scrapped*. Moreover, the conditioning on educational attainment is also unusual. Behavioral reactions are, hence, expected to be more pronounced and evidence more clear-cut. Moreover, since the income loss is substantial there is more at stake. However, poverty is not an inevitable outcome: the affected youth can react. Some adolescents will seek support from their parents, others leave (or have already left) home, and might claim means-tested assistance. Some may intensify job search, but might accept lower paying jobs than otherwise. Educational and training decisions will alter as well.

In this research we first aim at identifying the behavioral *reactions* of targeted youth caused by the aforementioned reform. More specifically, we evaluate the impact on job finding and job quality, training and schooling decisions, the decision to register at the PES as job seeker, take-up of means-tested assistance benefits, and on household formation. We will also study the net *global* impact (after behavioral reactions) on individual and family income, and on poverty. A main contribution is that we consider multiple reactions instead of focusing on just one as most studies do. This yields insights in the interactions between them and in the global consequences of the reform. A second objective is to get a better understanding of the *drivers* of these behavioral reactions. In particular, we will contribute to the literature by building and estimating a structural model of behavior to investigate the extent to which the identified behavioral reactions are consistent with individual rationality or suggest the presence of behavioral biases instead. Based on this model we will conduct a welfare analysis which trade-offs the *social* benefits of the reform to its *social* costs. The structural approach also allows to simulate alternative modalities of the reform to check whether these would be socially preferred to the implemented reform.

Behavioral reactions and their drivers following the suppression of UI for youth

The 2015 reform may generate diverse behavioral reactions. In the first place, standard job search theory (Mortensen 1977) predicts that suppressing the entitlement to UI should spur job search effort and induce less job offer refusals. To the extent that young job seekers are informed and rational, these behavioral reactions may already be observed during the waiting period. This would be in line with existing evidence (Tatsiramos and van Ours 2014). However, this evidence does not focus on youth. Meta-analysis and recent surveys report that active labor market policies targeted at youth are generally *less* effective than for adults (Card *et al.* 2010, 2015; Caliendo and Schmidl 2016). Our study will throw some light on whether these conclusions for activation extend to the effects of UI generosity.

Job search theory also predicts that cutting benefits has a negative impact on job quality, because liquidity constraints induce them to be less selective in job acceptance behavior (Ehrenberg and Oaxaca, 1976): more short-term jobs with reduced and/or irregular working time and less favorable compensation packages are accepted. The few existing studies find mixed evidence of UI generosity on job quality (usually measured by the wage level) and, hence, also on the presence of liquidity constraints among the unemployed. Some studies confirm the theoretical predictions, while others report zero effects (Tatsiramos and van Ours 2014). In a very recent contribution Nekoei and Weber (2017) show that the zero impact is a consequence of two offsetting forces. More generous unemployment benefits not only reduce pressure for liquidity constrained individuals to accept low wage jobs quickly. It also defers acceptance, which lengthens the unemployment spell. This sends a negative signal to employers, which, hence, puts downward pressure on wages. If one controls for this indirect negative effect, empirical findings are consistent with the theoretical prediction that UI generosity has a positive impact on job quality. This has important welfare consequences, since it implies that liquidity constraints among the unemployed matter and that reducing the UI level too much can be inefficient (Chetty 2008). Kolsrud *et al.* (2015) argue that incentive effects are more important at the start of the unemployment spell, because individuals get increasingly liquidity constrained over time. An efficient benefit profile may then actually be *increasing* over time, instead of decreasing, as is conventional wisdom (Shavell and Weiss 1979). This insight has implications for our research, as UI for school-leavers displayed *before* the 2015 reform such inclining profile: no UI during the waiting period and entitlement to a UI benefit afterwards.

Terminating the entitlement to UI may also affect study and training incentives, positively or adversely. To the extent that adolescents are sufficiently forward looking, the perspective of losing entitlement to UI could incentivize them to study longer, or to re-enter education or training. These incentives are enhanced by the diploma condition imposed in the reform on youngsters under 21. Moffitt (2007) and Hernaes *et al.* (2016) find that imposing stricter eligibility requirements on social assistance for, respectively, Norwegian youths and lone mothers in the US increases high-school completion rates. However, youngsters under 21 may have accumulated too much schooling delay, so that a high-school diploma by age 21 is an unfeasible target. In addition, by imposing an eligibility limit on UI at 25 years, young adults may be more tempted to stop studying before this age as to preserve their eligibility. *Ex post*, the withdrawal of the activation allowance may reduce re-entry in education and participation in training because such re-entrants could often keep the UI allowance during their study and were entitled to a supplement if they participated in training. These advantages disappear after the reform.

The aforementioned behavioral reactions presuppose, as is standard in economics, that individuals are perfectly informed and rational. Since the seminal work of Tversky and Kahneman (1974) insights have gradually developed that these standard economic assumptions are much too strong and the field of behavioral economics has emerged. This field informs us that individuals, amongst other, typically procrastinate in job search (Della Vigna and Paserman 2005), have biased beliefs when evaluating the prospects of job finding and offered wages (Spinnewijn 2015), and have reference dependent preferences displaying loss aversion (Della Vigna *et al.* 2016). Based on these insights, the aforementioned reactions of adolescents in anticipation of the suppression of UI may be very much muted or altered. If these biases matter the policy reform could even *reduce* the transition to employment. For instance, this could happen if adolescents mainly register as job seeker at the PES to ensure eligibility for the activation allowance. In this case the benefit withdrawal may induce school-leavers not to register or to cut off contact with the PES. As a consequence, they may forgo employment services of the PES, which, if well-designed, may counterbalance the inefficient job search actions brought about by the behavioral biases (Babcock *et al.*, 2012). This is in line with the recent finding that an increasing share of unemployed youth does not register at the PES (Desiere *et al.* 2017).

The impact of the cutting UI benefit may also crucially depend on household income. The reform we aim to analyze concerns school-leavers of whom the great majority still lives at their parents' home. Children in poor families are more at risk to be negatively affected by the withdrawal of the UI allowance than

those in rich families (Cockx and Van Belle 2016). The household situation may, however, not only be cause. It may also be effect (Billari and Liefbroer 2007; Lee and Painter 2013; Ayllón 2015). Benefit cuts could be expected to delay the establishment of an independent residence. However, for low income families, leaving the parental home could actually accelerate in case that housing costs are not too high and social assistance benefits higher than when staying at home.

Methodology for reduced form analysis identifying the behavioral reactions of the 2015 reform

A first objective of this research is to identify the causal effects of the 2015 reform on several outcomes: the aforementioned potential behavioral reactions of youth, including the overall effect on income and poverty. A main issue is selection bias: treated individuals may react differently than untreated (controls), because they are different. To solve this issue we will apply a *difference-in-differences* (DiD) approach (Meyer 1995). This eliminates the selection bias by subtracting the difference in outcomes between treated and control groups in the pre-treatment period (measuring selection bias) from the difference observed in the post-treatment period (measuring selection bias plus treatment effect). The resulting difference measures the treatment effect.

To evaluate the impact of the reform, we consider two treatment groups, i.e. groups that are no longer eligible for the activation allowance after the reform: (i) adolescents younger than 21 who did not (yet) complete high school at the moment of the reform, and (ii) young adults older than 24 entering unemployment (or older than 23 for those who did not yet leave education). For unemployed job seekers the age threshold is 24 instead of 25, to account for the waiting period of one year. For those in education the decision to continue one more year of schooling is critical one year earlier, at age 23, because the extra year of schooling needs to be taken into account to determine eligibility for UI.

These two treatment groups can be contrasted to a control group aged between 21 and 23 (or 24) at the reform date that was not affected by the reform. As to apply the DiD method we sample the same age groups three years prior to the reform. This choice leaves a sufficient time span to follow-up individuals during the pre-treatment period and enables placebo tests on identifying assumptions.

Since the decision to leave education can only be affected for those who did not yet leave education by the reform, the analysis distinguishes between those who left education by the reform and those who did not. For the first group we can ignore the impact of the reform on the school-leaving decision. This facilitates the analysis and is a good starting point to study the impact on transitions from unemployment. Since those in the control group grow older and the pre-treatment period is finite, some of individuals in the control and pre-treatment groups may eventually get treated. Implementing the DiD within a duration model that allows for a time-varying treatment status can accommodate for this (Lammers *et al.* 2013).

What if the placebo tests fail? We can consider alternative control groups. If transitions from unemployment are outcomes of interest, both treatment groups could be contrasted to individuals of the same age entering unemployment after sufficient (one year) work experience: this group is entitled to UI irrespectively of age or diploma and can, hence, serve as control group. For the age group under 21 those who did acquire a high school diploma are neither affected by the reform. It is also possible to combine control groups. This adds a third dimension (e.g. diploma) to time and age, so that second-order interaction effects (time-age, time-diploma, age-diploma) can be controlled for and the treatment effect is identified by a triple differences model (Yelowitz 1995). We could also apply a *conditional* DiD that controls for observed compositional differences between groups (e.g. Albanese and Cockx 2015).

Analysis based on a structural model of behavior

A drawback of the reduced form approach is that it does not reveal the behavioral mechanisms that generate the treatment effects and that it is not possible to perform counterfactual policy simulations of alternative policy designs. We therefore build a structural model of behavior that we estimate on the control samples and externally validate on the treated sample (Todd and Wolpin 2006). Since this approach is challenging, we proceed stepwise. We focus on the second treatment, starting with the group that has left education by 2015 and assuming perfect rationality. This is the case for which we can build on acquired experience (Cockx *et al.* 2016). Subsequently, we extend the model to allow for behavioral biases, as in Paserman (2008) (procrastination) or Della Vigna *et al.* (2016) (reference dependence). Finally, we use the estimated model to evaluate the impact of the reform and investigate whether alternative policy designs (such as inclining benefit profiles) can improve social welfare.

Data

The policy reform has potential effects on educational, labor market outcomes, household formation, income and poverty. We therefore propose to integrate a large sample of individual data from the

following sources: the Flemish administration of education (yearly study progression, mother's education, language spoken at home), the Flemish PES (monthly register of unemployment), the federal Datawarehouse of the Crossroads Bank for Social Security (employment history and wages, child allowances, UI and other social security benefits, personal and household information), and the personal tax registers (after-tax income). The integration of these datasets is complex, but feasible. For instance, the "Policy Research Centre Work and Social Economy" has recently accomplished this for the first three mentioned sources. The integration of the personal tax registers may be less obvious. If this is not possible, we can use gross income as proxy for after-tax income, which is available in the Datawarehouse. A complication is that approval is required by two distinct privacy commissions: the Flemish and the federal. We therefore anticipate that the data collection may take about one year.

Timing

	Year 1	Year 2	Year 3	Year 4
Data gathering and cleaning				
Training (methods and doctoral school) of the researcher				
Review of institutions and scientific literature				
Reduced form analysis 1 (ignoring school-leaving)				
Reduced form analysis 2 (incl. school-leaving)				
Structural Model: theory				
Structural Model: estimation				

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