WORKING PAPER 2019:12

# Unemployment insurance and youth labor market entry

Mathias von Buxhoeveden

IFAU
INSTITUTE FOR
EVALUATION OF
LABOUR MARKET AND
EDUCATION POLICY

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala.

IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

More information about IFAU and the institute's publications can be found on the website www.ifau.se

ISSN 1651-1166

# Unemployment insurance and youth labor market entry[a]

by

Mathias von Buxhoeveden[b]

30th April, 2019

**Abstract**
This paper estimates the effects of unemployment insurance (UI) benefits on job finding rates and entry level wages for unemployed high school leavers. Up to year 2007, Swedish high school-students who became unemployed shortly after graduation were entitled to UI-benefits once they became 20 years of age. Therefore, the start of an unemployment spell relative to the 20:th birthday creates potentially exogenous variation in time to treatment. I exploit this to estimate the effect of UI benefits on unemployment duration and entry level wages. The results show that there is a large and statistically significant negative effect of UI benefits on the employment hazard. There are no detectable effects on entry level wages. This would suggest that unemployment benefits induce high school leavers to postpone labor market entry but does not seem to effect job match quality.

JEL-codes: J65, J64

[a] I would like to thank Björn Öckert, Johan Vikström, Bart Cockx, seminar audiences at Uppsala University and IFAU. I gratefully acknowledge generous financial support from Forte.
[b] IFAU, mathias.vonbuxhoeveden@nek.uu.se

## Table of contents

# 1 Introduction

Labor market entry is a high stakes setting for young workers. Those who become unemployed in the school-to-work transition suffer persistent earnings penalties and continue to have lower attachment to the labor market later on (Gregg, 2001; Nordstrom Skans, 2011). In addition, the school-to-work transition determines the type of jobs school leavers are matched to, and workers who hold positions that they are overeducated for obtain a significantly lower education-earnings-premium (Leuven and Oosterbeek, 2011).[1]

Access to unemployment insurance (UI) benefits could potentially smooth the school-to-work transition, since some of the financial pressure to find a job immediately is alleviated. If young workers have access to UI benefits they can reject job offers of poor quality and focus on finding employment that match their education, which could lead to higher wages, improved career prospects and increased employment stability. On the other hand, there is a clear risk that UI benefits disincentivize job search and induce school leavers to stay unemployed longer. Quantifying the potential trade-off between longer unemployment durations and improved match quality has important implications for the design of UI-policy.

In this paper, I study the effects of UI-benefit eligibility on job finding rates and entry level wages for unemployed high school leavers. My empirical strategy is to exploit age-discontinuities in the Swedish UI-system. Between 2002 and 2006, all unemployed high school leavers became entitled to UI once they turned 20 years. Moreover, Swedish high school students typically graduate in spring the year they turn 19 years. Therefore, the start of an unemployment spell relative to the 20:th birthday creates plausibly exogenous variation in the duration until they become eligible for UI.

The experiment I have in mind can be described as follows. Consider two groups of high school students that graduate the year they turn 19 years. They are unable to find employment and register as unemployed at the public employment office in October. Now, assume that the first group is born in January and the second in February. This implies that no one can claim UI between October - December (since they are all 19 years of age

[1] Overeducated is defined as having more schooling than the job requires.

at the time). However, in January, the first group turns 20 years and becomes eligible for UI. My empirical strategy is to compare the employment hazards in January, when the first group can claim UI but the second one cannot.

I generalize this analysis, and match each group to a comparison cohort that becomes unemployed at during the same month, but are born one month later and therefore have to wait an additional month until they become eligible for UI. Again, the idea is to compare the employment hazards in the period when the first group can claim UI but the comparison group cannot.

The analysis builds on the econometric framework pioneered by Van den Berg et al. (2010). They show that such comparisons identify a causal effect of the treatment on the employment hazard under two conditions. First, the treatment and control group have to be comparable when they enter unemployment. Second, the dynamic selection has to be identical until treatment is imposed. This is achieved by imposing the "no anticipation" assumption, i.e that future UI-benefit eligibility does not effect current job search behavior. This assumption would be violated if, for instance, unemployed high school leavers adjust their job search behavior in anticipation of future UI-benefit payments.

I begin the analysis by validating the identifying assumptions through a series of falsification tests. First, I implement balancing tests and verify that the distribution of important covariates, such as high school grades, are balanced across treatment and control groups at the start of the unemployment spells. To validate the "no anticipation" assumption, I implement a series of placebo checks that test for differences in the employment hazards in the pretreatment period, when neither group can claim UI. Overall, 5.3% of the placebo tests are significant at the 5% level, which is expected by chance.

I proceed and estimate the effect of UI-benefit eligibility on the employment hazard. Variation in time to treatment for individuals born at different points of the year allows me to explore how the effect of UI-benefit eligibility varies depending on the elapsed duration until eligibility. The estimates suggest that UI-benefits reduce job finding rates, but the effect does vary depending on the waiting time until eligibility.

To gain precision, I pool all of the available treatment-control group comparisons and estimate them jointly. The results indicate that becoming eligible for UI benefits reduces monthly job finding rates by 1 percentage point. The baseline hazard is roughly 8%. Hence, the treatment effect translates into a 12.5% reduction in the conditional probability of leaving unemployment.

In the final part of the paper, I leverage the same treatment-control group comparisons and estimate the effect of UI-benefits on entry level wages. There are no detectable effects of UI on entry level wages. Altogether, these results suggest that UI-benefits induce high school leavers to postpone labor market entry, but do not seem to affect job match quality.

There is a large literature on the school-to-work transition (Kramarz and Skans, 2014; Hensvik and Nordström Skans, 2013; Wolpin, 1987; Scherer, 2004; Neumark and Wascher, 1995; Raaum and Røed, 2006). However, evidence on the effects of UI benefits in the school-to-work transition is very scarce. To the best of my knowledge, Cockx and Van Belle (2016) is the only exception. They exploit age discontinuities in the waiting period before school-leavers qualify for UI in Belgium, and find no effects on job-finding rates and post unemployment wages. However, they restrict the sample to individuals with at least a bachelors degree, whereas I consider high school graduates. Therefore, the population of interest in this paper is younger, has lower levels of education and less experience on the labor market.

In addition, this paper is the first application of the econometric framework pioneered by Van den Berg et al. (2010). This method has some attractive features. Existing methods for studying the effects of a policy change on the hazard rate rely heavily on untestable model structure. In particular, duration models that allow for unobserved heterogeneity typically impose a mixed proportional hazard structure to achieve identification. Such semi-parametric assumptions may be unappealing. The econometric framework that I use does not impose any model structure. On the other hand, the method is designed for studying short run effects, which has some important drawbacks. Importantly, it is not well-suited for studying the effects of prolonged exposure to UI.

The rest of this paper is organized as follows. The next section describes the institutional details. Section 3 presents the data sources and performs some descriptive analyses. The empirical approach is outlined in section 4. Section 5 tests the identifying assumptions. Section 6 and 7 present the main empirical results. Section 8 concludes.

## 2 Institutional framework

In Sweden, UI is typically restricted to workers who become unemployed after a sufficiently long-lasting job.[2] School-leavers who become unemployed in the school-to-work transition often fail to fulfil this condition. Motivated by this, the Swedish government introduced a special set of UI-rules in 1998. These rules exempted unemployed school-leavers from the labor market attachment criterion.[3] Hence, those who became unemployed in the school-to-work transition were eligible for UI, even if they had never worked.

School-leavers were entitled to flat rate benefits. These are paid per "working day", which means that there were 5 days of benefits paid per week. The benefit level was revised in 2002 and the rules were abolished altogether in 2007. To keep the policy regime constant, I restrict attention to the years 2002 - 2006, when the benefit level remained at 320 SEK per day. The benefit level prior to 2002 was 270 SEK per day.

Unemployed school-leavers qualified for flat rate UI if two conditions were met: (i) they were 20 years of age and (ii) they fulfilled a 90 day waiting period. The waiting period starts when the individual registers as unemployed at the Public Employment Service (PES).

The Swedish educational system is tuition free at all levels. Children typically start school the year they turn 7 years and are required by law to complete 9 years of compulsory schooling. Afterwards, about 98% choose to enrol in upper secondary school. Here, the educational system is separated into academic and vocational tracks.[4] Academic tracks are intended for students who plan to pursue further education at the university level. Students enrolled in vocational tracks typically enter the labor market immediately after high school. The vocational tracks contain specializations such as construction and nursing. Secondary school lasts for 3 years and students typically graduate in spring the year they turn 19 years.

[2] The labor market attachment criteria restricts UI to workers who have been employed for at least 6 out of the last 12 months prior to displacement.
[3] School leavers are defined as graduating from high school or university.
[4] There are also preparatory tracks for high school.

### Potentially confounding policies

My identification strategy consists of exploiting age-discontinuities in the Swedish UI system. In particular, I exploit that school leavers become eligible for UI once they turn 20 years. Clearly, this identification strategy requires that there are no other policy discontinuities around this threshold. Two Active Labor Market Programs (ALMP) are potential confounders.

ALMP:s in Sweden are typically restricted to individuals who are at least 20 years of age. However, at the time, some municipalities offered municipality youth programs (MYP) to job seekers between 18 and 20 years of age. The municipalities offered these on a voluntary basis and the programs typically involved a small monetary compensation,[5] education and work practice, although the precise content varied across municipalities.

Once the unemployed school leavers turned 20 years, they became eligible for other ALMP:s, such at the "youth guarantee" (YG). The YG was also administrated by the municipalities on a voluntary basis, but provision was much more restrictive compared to the MYP (Forslund and Sibbmark, 2005).

Therefore, there is a discontinuity in ALMP policies once job seekers become 20 years of age. They lose access to the MYP and become eligible for the YG. I will address this in a sensitivity analysis in section 6.

## 3 Data, sample and descriptive statistics

### Data

This paper uses data from several Swedish administrative registries. The first registry contains yearly information on enrolment in secondary school between 1995 and 2010. I use the last observed registration to identify the year of graduation.

The dataset also contains information about the high school track that students were enrolled in. Some of them cannot be classified as academic or vocation. I label these as "other high school program".

[5]Those who failed to complete high school received a monthly allowance of 1360 SEK ($\approx$ 150 USD). The monthly compensation for for those who failed to complete high school education varied across municipalities. This information is not publicly available.

I add data on grades from compulsory school. These are more informative about the ability of students compared to grades from high school, since the curriculum is identical for all students in primary school.

I further add data on unemployment outcomes from the Swedish Public Employment Service. This register covers the universe of unemployment spells between 1990 and 2015. Entry and exit from unemployment are recorded at the daily level. There is also detailed information on labor market program participation and the reason for terminating the unemployment spell (regular or subsidized employment, lost contact and so on). I complement this data with information on UI-benefit spells from 1999 onwards from the registry ASTAT. ASTAT records all UI benefit payments at the daily level.

I add data from the matched employer-employee registry RAMS. This data reports monthly earnings for all individuals employed in Swedish firms from 1985 to 2015. If an employment spell is observed the year after exit from unemployment, I take this as the post unemployment outcome. If an individual has several employment spells, I focus on the spell with the highest monthly wage.

### Sample

I restrict the sample to individuals who leave high school the year they turn 19 years, and left between 2002 and 2005.[6] The sample is further restricted to those who did not receive any income related UI benefits during their unemployment spell. This restriction is imposed since those who have fulfilled the requirements for income-related UI benefits can claim benefits prior to their 20:th birthday. Consequently, their treatment status does not change when they become 20 years of age.

The empirical strategy is to exploit age-discontinuities in the Swedish UI-system. In particular, I make use of the fact that the elapsed duration at which school leavers qualify for UI is determined by the start of an unemployment spell relative to their 20:th birthday. The empirical strategy is to match individuals born in a given month to a comparison cohort that becomes unemployed at the same time, but are born one month later, and therefore has to wait an additional month before qualifying for UI. To estimate the effect

[6] The sample includes those who left school without a high school diploma.

of UI on job finding rates, I compare the employment hazards in the month when the first group can claim UI while the control group cannot.

Therefore, I define the running variable as follows:

$$s_i = t_{1,i} - t_{0,i} + 1 \tag{1}$$

where $s_i$ is the value of the running variable for individuals $i$, $t_{1,i}$ is the calender month in which individual $i$ become 20 years of age and $t_{0,i}$ is the calender month when the unemployment spell begins. Hence, the running variable is defined as the number of calender months between the start of an unemployment spell and the individuals 20:th birthday. Adding 1 to the right hand side of equation (1) ensures that the calender month when the spell start is counted as a full month of unemployment. To ensure that all subjects are eligible for UI benefits when they become 20 years of age, I restrict the sample to those who enter unemployment at least 3 calender months prior to their 20:th birthday. Put differently, I restrict the sample to spells where the running variable is at least equal to 3. If an individual has several spells that satisfy these requirements, I restricted attention to the first spell. The sample size is reduced to 132 observations for values of the running variable greater than 13 calender months. I exclude these and restrict attention to spells where the running variable ranges from 3 to 13. The final sample contains 58641 unemployment spells.

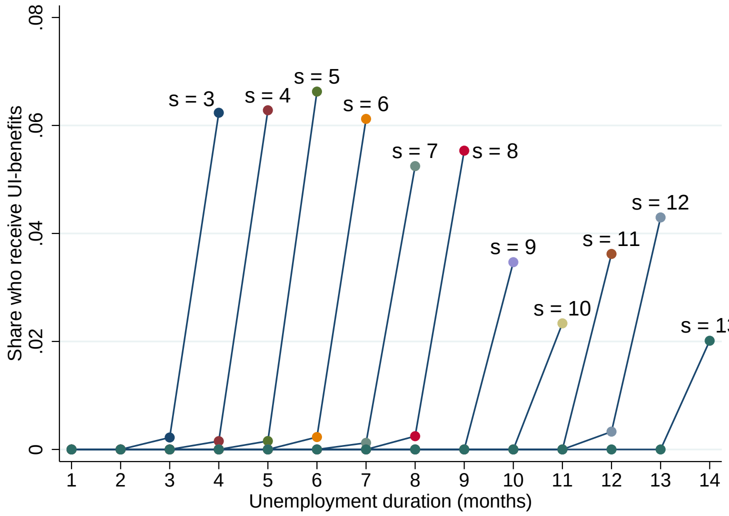
### Descriptive statistics

Descriptive statistics of the sample are collected in appendix *Table A1*. The bottom row shows the mean and standard deviations of selected covariates for the entire sample. The other rows report descriptive statistics for selected covariates separately for each value of the running variable. Note that grades from primary school have been standardized in the full population such that the mean is zero and the standard deviation is equal to one. Overall, those who enter unemployment have significantly lower grades than their peers. There is a fairly even distribution of graduates from vocational and academic programs. Moreover, the share of graduates from programs that cannot be classified as academic nor vocational is low (8%). The sample consists of approximately 50% males and females respectively.

It is interesting to note that grades from primary school are smoothly decreasing with higher values of the running variable. This is most likely driven by school starting age effects. Specifically, individuals with larger values on the running variables will, on average, be born later in the year. Moreover, individuals born later in the year typically start school when they are a bit younger. Therefore, they are often outperformed by their older peers, and these effects have been shown to persist in the labor market (Fredriksson and Öckert, 2014).

*Figure 1* shows the share of unemployed job-seekers who receive UI-benefits over the unemployment spell. Take-up rates are shown separately for cohorts with different values on the running variable, $s$. There are clear spikes in the share of individuals who receive UI-benefit payments the calender month after they become 20 years of age. However, the take-up rate is remarkably low. Only about 6% of unemployed school-leavers claim UI-benefits the calender month after they turn 20 years. This is because individuals are only eligible to apply for UI once they become 20 years of age. Those who apply before their 20:th birthday are automatically rejected and have to wait until they are 20 years before they can submit a new application. Moreover, case workers at the UI-fund have to review and approve the application before any payments are made. This process can take several months. In addition, some unemployed school-leavers may be unaware that they are eligible for UI. This explains the low initial take-up rate.

**Figure 1:** UI take-up effect




**Note**: The figure shows the take-up rate of UI over the unemployment spell for individuals with different values of the running variable. The running variables is labeled s.

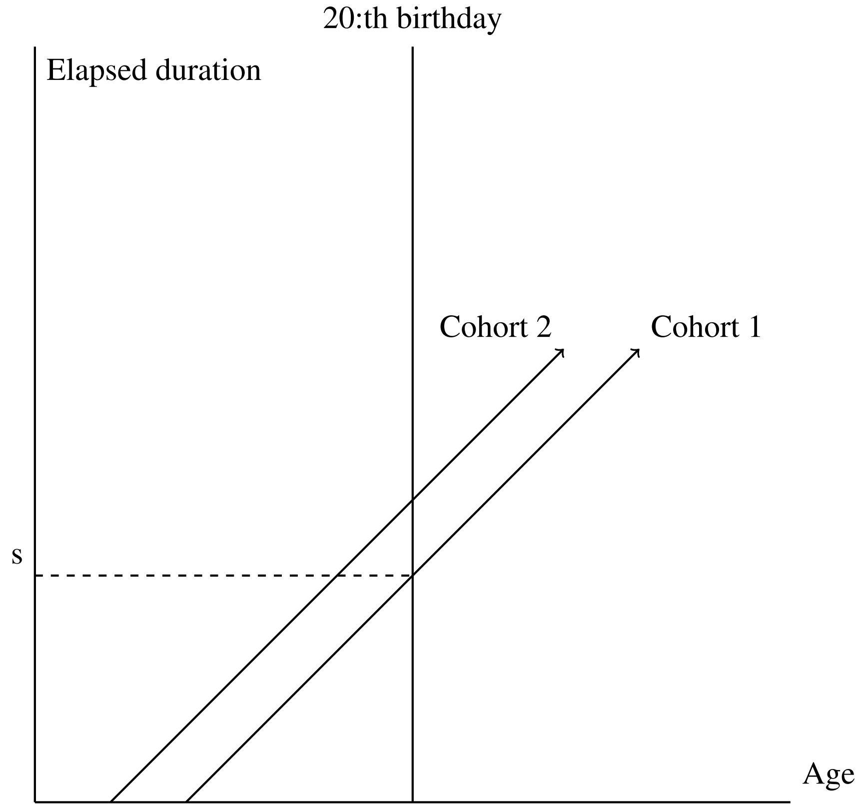
# 4 The empirical approach

*Figure 2* illustrates the approach to estimating the effect of UI on the employment hazard. Cohort 1 is marginally older at the start of the unemployment spell. They become 20 years at an elapsed duration s. Here, s corresponds to the "running variable" for cohort 1. Moreover, cohort 2 is slightly younger when they enter unemployment. They become 20 years at some elapsed duration higher than s. The idea is essentially to compare the employment hazards at an elapsed duration s, when cohort 1 can claim UI benefits whereas cohort 2 cannot.

Van den Berg et al. (2010) derive conditions under which this comparison will identify a meaningful causal effect of UI-benefit-eligibility on the employment hazard. This is a binary treatment that is assigned to commence at some elapsed duration $s \in [0, \infty)$. Here, $\infty$ corresponds to the hypothetical scenario where the individual never becomes eligible for UI, regardless of how long she remains unemployed.

For each treatment arm, there is a random variable T(s). This is the potential outcome duration if the individual becomes eligible for UI-benefits at an elapsed duration s. Un-

**Figure 2:** Identification based on two cohorts.




employed school-leavers are allowed to differ with respect to observed and unobserved characteristics, denoted X and V respectively. Treatment is assumed to be randomized conditional on $X$ and $V$. Further, treatment assignment should be orthogonal to V given X. Formally:

**Assumption 1** (Assignment). $S \perp T(s) \mid (X,V)$, and $S \perp V \mid$ X.

Assumption 1 is fulfilled if treatment is as good as random when conditioning on observed covariates X. In terms of *Figure 2*, cohort 1 and 2 have to be comparable when they enter unemployment. However, since we are interested in the causal effect of becoming eligible for UI at an elapsed duration $s$, cohort 1 and 2 have to be comparable *conditioning* on staying unemployed until $s$. To achieve this, I impose the "no anticipation" assumption. Formally:

**Assumption 2** (No anticipation). For all s $\in (0,\infty)$ and for all t $\leq$ s and all X, V $\Theta_{T(s)}(t|X,V) = \Theta_{T(\infty)}(t|X,V)$

See Abbring and Van den Berg (2003) for a detailed discussion. Here, $\Theta_{T(s)}$ is the integrated hazard for the potential duration outcome associated with treatment assignment at time s. Hence, $\Theta_{T(\infty)}$ corresponds to the integrated hazard if treatment is never assigned. Assumption 2 imposes that individuals should behave as if treatment will never be assigned up until the point when it is assigned. Put differently, unemployed school leavers should not change their job search behavior in anticipation of future UI-benefit payments. This assumption holds if treatment exposure is completely unanticipated or if individual simply do not act on information about future exposure to treatment.

Consider *Figure 2*. If assumption 1 and 2 holds, the difference in the employment hazards at an elapsed duration $s$ consistently estimates the following parameter:

$$ATTS(s,s'|X) = E[\theta_{T(s)}(s|X,V) - \theta_{T(s')}(s|X,V)|X,T(s) \geq s] \tag{2}$$

Where $\theta_{T(s)}$ and $\theta_{T(s')}$ are the hazard functions associated with the potential duration distributions T($s$) and T($s'$). This parameter is called "the average treatment effect on treated survivors". It does not depend on $s'$, as long as $s'$ is greater than $s$. We can therefore replace it with $ATTS(s|X)$, which is the instantaneous causal effect of becoming eligible for UI-benefits at an elapsed duration $s$. Furthermore, the "treated survivors" are those who are still unemployed at $s$, if they become eligible for UI at an elapsed duration $s$. Therefore, the treatment effect is averaged over subpopulations with longer potential outcome durations when the treated cohort becomes eligible for UI further into the unemployment spell.

In my setting, each cohort can be matched to several potential comparison groups. For instance, consider the cohort that becomes eligible for UI after 3 months. They could potentially be matched to a comparison group that becomes eligible for UI at $s = 4$, $s = 5$,..., $s = 13$. However, assumption 1 imposes that the treatment and control group should be comparable when they enter unemployment. Moreover, the identifying variation is

**Table 1:** Segments, Treatment and Control Groups.

| Running variable | | |
|---|---|---|
| Treatment group | Control group | Segment |
| 3 | 4 | 1 |
| 4 | 5 | 2 |
| 5 | 6 | 3 |
| 6 | 7 | 4 |
| 7 | 8 | 5 |
| 8 | 9 | 6 |
| 9 | 10 | 7 |
| 10 | 11 | 8 |
| 11 | 12 | 9 |
| 12 | 13 | 10 |

The table show the treatment and control group at each segment on the running variable

derived from the fact that school-leavers differ slightly in age when they enter unemployment. Specifically, the control group will always consist of individual born at least one month later in the year.

The descriptive evidence in appendix *Table A1* shows that grades from primary school are smoothly decreasing with higher values on the running variable. This suggests that assumption (1) is more likely to hold if each cohort is matched to a comparison group exposed to treatment at a marginally higher duration. Therefore, I will match each cohort to a comparison group that becomes eligible for UI after one additional month of unemployment.

I define a segment on the running variable as a pairing of a treatment and a control group. At each segment, the comparison group becomes eligible for UI after an additional month of unemployment. There are 10 segments on the running variable, listed in *Table 1*.

# 5 Validity of the research design

The empirical design consistently estimates the instantaneous causal effect of UI-benefit eligibility on the employment hazard if: (i) the treatment and comparison groups are comparable when they enter unemployment (random assignment), (ii) the dynamic selection is identical until treatment is imposed (no anticipation). To test this, consider the following regression model:

$$D_{ic} = \alpha_c + \tau_1 + ... + \tau_k + \gamma_1 + ... + \gamma_4 + X'_{ij}\phi_c + \varepsilon_i \tag{3}$$

$D_{ic} = 1$ if individual i belongs to the treatment group at segment $c$, $\alpha_c$ is a segment fixed effect, $\tau_1, ..., \tau_k$ are month of inflow fixed effects and $\gamma_1, ..., \gamma_4$ are dummy variables for year of graduation. Finally, $X_{ic}$ is a vector of individual background characteristics.[7] Equation (3) provides means of testing whether the treatment and control groups are comparable when they enter unemployment (assumption 1). If any elements in $\phi_c$ are statistically significant at the start of the unemployment spells, this indicates that individual background characteristics can predict treatment group status at segment $c$, and that assumption (1) is violated.

Running specification (2) after conditioning on staying unemployed until the treatment group becomes eligible for UI provides a test of assumption 2 (identical dynamic selection until treatment exposure). Intuitively, if the cohorts are comparable when they enter unemployment (assumption 1), and the dynamic selection is identical in the pre-treatment period (assumption 2), the cohorts should also be comparable after conditioning on staying unemployed until the treatment group becomes eligible for UI.

I run specification (2) separately for each segment of the running variable. This produces the results collected in appendix *Table A2*. Here, I report p-values for the Wald tests $\phi_c = 0$ at the start of the unemployment spells *and* conditional on survival until the treatment group becomes treated. The covariates tend to be strongly balanced. There are no statistically significant differences between treated and controls when they enter unemployment. However, some unbalancing do emerge when we move to higher values of the running variable *and* condition on survival until the treatment group becomes treated. Moreover, *Table A2* contains the results from twenty balancing tests. Out of those, two

[7]The vector of background characteristics includes gender, grades from primary school and dummy variables for type of high school program, i.e. academic, vocational or other.

came out statistically significant which is line with what we should expect just by chance.

The "no anticipation assumption" can also be tested by comparing the survival probabilities during the pre-treatment period, when neither group could claim UI. If the "no anticipation assumption" holds, treatment and control groups should leave unemployment to the same extent during the pre-treatment period. Appendix *Figure A1* depicts the empirical survivor functions for the treatment and control groups during the period when neither group could claim UI. The survival curves are very similar in almost every treatment-control group comparison. This supports the "no anticipation" assumption.

# 6 UI benefits and the employment hazard

This section presents estimates of the instantaneous causal effect of UI benefits on the employment hazard. Let $i$ be an indicator for the individual, $c$ index segments on the running variable and $t$ indicate the month of unemployment. Furthermore, let $s_c$ denote the value of the running variable for the treated cohort at segment $c$. Consider the following specification:

$$y_{ict} = \alpha_c + \tau_1 + ... + \tau_k + \gamma_1 + ... + \gamma_4 + \sum_{t=1}^{s_c+1} \beta_{1t} m_t + \sum_{t=1}^{s_c+1} \beta_{2t} m_t D_{ic} + X_{ic}' \phi + \varepsilon_{ict}, \quad (4)$$

where $y_{ict}$ is a dummy variable for whether individual $i$ in segment $c$ exits to employment in the t:th month of unemployment, $\alpha_c$ is a segment fixed effect, $\tau_1, ..., \tau_k$ are fixed effects for month of entry into unemployment, $\gamma_1, ..., \gamma_4$ are dummy variables for year of graduation, $m_1, ..., m_{s_c+1}$ are dummy variables for month in the unemployment spell, $D_{ic}$ is equal to one if individual $i$ belongs to the treatment group in segment $c$ and $X_{ic}$ is a vector of controls.[8] The interaction terms $m_1 D_{ic}, ..., m_{s_c+1} D_{ic}$ capture monthly differences in the employment hazards in each segment.

*Figure 1* showed clear spikes in UI benefit payments the calender month after the 20:th birthday. The treatment effect is thus estimated with the final interaction term $m_{s_c+1} D_{ic}$. The remaining interaction terms are placebo tests.

[8]The vector of controls includes gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other.

I begin the analysis by pooling all of the segments and estimate the discontinuities (the $m_{s_c+1}D_{ic}$ terms) jointly.[9] Adding fixed effects for segments and month of entry into unemployment means that the identifying variation is derived from the fact that the comparison group in each segment is one month younger than the treatment group at the start of the unemployment spell. The results are collected in *Table 2*. Standard errors are clustered by person since the same spell can appear multiple times when the discontinuities are pooled. The point estimates are strongly significant and robust across various specifications. Overall, the results in *Table 2* suggest that UI benefit eligibility decreases the conditional probability of leaving unemployment within one month by approximately 1 percentage point. Moreover, appendix *Figure A2* shows that the monthly baseline hazard is approximately 8%. Therefore, the estimated treatment effect translates into a 12.5 percent drop in the monthly job-finding probability. This is quite a substantial effect.

[9] The approach of pooling the discontinuities is analogous to the "regression discontinuity design with multiple cutoffs", see Bertanha (2016); Papay et al. (2011); Cattaneo et al. (2016).

**Table 2:** Pooled Estimates

| VARIABLES | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Treatment effect | -0.011*** | -0.011*** | -0.009*** | -0.010*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 31,392 | 31,392 | 31,392 | 31,392 |
| Clustered standard errors | ✓ | ✓ | ✓ | ✓ |
| Year of graduation FE | ✓ | ✓ | ✓ | ✓ |
| Controls | | ✓ | | ✓ |
| Month of inflow FE | | | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Estimated treatment effect after pooling all the available thresholds. The vector of controls includes gender, grades from primary school and dummy variables for type of high school program, i.e. academic, vocational or other. *** p<0.01, ** p<0.05, * p<0.1

Next, I run specification (4) separately for each segment of the running variable. The $m_{s_c+1}D_{ic}$ terms yields an estimate of ATTS($s_j+1|$X). For ease of notation, I will replace ATTS($s_j+1|$X) by the shorthand measure ATTS($t|$X). This is the instantaneous causal effect of UI-benefit eligibility on the employment hazard at an elapsed duration $t$. The remaining interaction terms $m_1D_{ic},...,m_{s_c}D_{ic}$ estimate differences in the employment hazards during the pre-treatment period, when neither group can claim UI. Therefore, these placebo checks provide further means of testing the "no anticipation" assumption.

*Figure A3* in the appendix plots the estimates of ATTS($t|$X) together with 95% confidence intervals. The point estimates do not vary systematically depending on the elapsed duration that treatment is imposed. However, precision is decreasing for higher values of

$t$. Overall, there is no evidence that the treatment effect varies depending on the elapsed duration that school-leavers becomes eligible for UI. *Table A3* in the appendix presents the full set of regression outputs.

Appendix *Figure A4* shows the distribution of the placebo estimates from my preferred specification with controls and month of inflow fixed effects. Overall, 5.3% of the placebo tests are significant at the 5% level, which is expected by chance. This provides additional support for the "no anticipation" assumption.

It is interesting to consider how job finding rates evolve further into the spell. The dynamic selection in the treated and control group will however be unaligned after treatment is first imposed. Consequently, differences in the job finding hazards further into the spell cannot be given a causal interpretation. Graphs are nonetheless provided in appendix *Figure A2*.

### Sensitivity analysis

The policy discontinuity in UI benefit eligibility coincides with a change in ALMP policies. Specifically, unemployed teenagers are allowed to participate in the MYP until they are 20 years of age. Youths in the age range 20-24 are by contrast eligible for the YG after 90 days of unemployment. Moreover, both programs are administrated by the municipalities but provision of the YG is much more restrictive. Hence, one might expect a sharp drop in ALMP participation after individuals become 20 years of age.

Appendix *Figure A5* depicts the evolution of ALMP participation rates. There is a sharp drop in ALMP participation the calender month after the 20:th birthday. If the program exerts a casual effect on job finding rates, this would confound the estimated impact of UI benefits. To address this, I repeat the main analysis but remove individuals who enroll in ALMP during the "pre treatment" period. *Figure A6* in the appendix depicts the enrolment rates after this restriction was imposed. Note there are no longer any clear discontinuities around the 20:th birthday. Next, I repeat the previous analysis using the restricted sample. Specifically, all of the discontinuities are pooled together, segment fixed effects are included and standard errors are clustered by person. This produces the results collected in appendix **??**. The main estimates do not change much when the restricted sample is used. Any potential bias induced by the discontinuities in ALMP policies should therefore be negligible.

# 7 UI-benefits and entry level wages

Mortensen (1977) laid out the theoretical arguments for a positive relationship between the generosity of UI benefits and the reservation wage. The idea is essentially that UI benefits increase the value of unemployment. Consequently, the lowest wage an unemployed worker is willing to accept (the reservation wage) will increase as the generosity of UI benefits increases. Moreover, the likelihood that a wage offer is sufficiently high for the worker to accept the job decreases as the reservation wage increases. This creates a positive relationship between the generosity of UI benefits and the duration of unemployment. This section tests whether the drop in job finding rates associated with the spikes in UI benefits is consistent with higher reservation wages.

I follow the unemployment spells throughout the calender year after high school graduation. Spells that are still in progress are treated as right censored. Note that both groups become eligible for UI benefits during the period. However, the treated cohort can start their benefit spells earlier. Consequently, if UI benefits increase worker selectivity, we would expect to observe higher post unemployment wages for the treated cohort.

Entry level wages are considered for those who left unemployment before the end of the observation window and were wage earners the year after the spell ended. For simplicity, I will refer to those who fulfill this criterion as employed. Since the outcome is only observed for a selected subset, we need to check weather sample selectivity is a concern (Heckman, 1974). Consider the following regression model:

$$y_{ic} = \alpha_c + \tau_1 + ... + \tau_k + \gamma_1 + ... + \gamma_4 + \beta_c D_{ic} + \varepsilon_{ic} \tag{5}$$

Where $y_{ic}$ is a dummy variable for observing the wage of individual $i$ in segment $c$, $D_{ic}$ is dummy variable for belonging to the treatment group in segment $c$, $\alpha_c$ is a segment fixed effect, $\tau_1, ..., \tau_k$ are fixed effects for month of entry into unemployment and $\gamma_1, ..., \gamma_4$ are dummy variables for year of graduation. I run specification (5) separately for each segment of the running variable. Point estimates of $\beta_c$ together with p-values for the hypothesis $\beta_c = 0$ are reported in appendix **??**. The estimates are economically small and insignificant. This would suggest that sample selectivity should not be major concern.

The previous section shows that UI benefit eligibility induces an immediate drop in the employment hazard. However, **??** shows that treated and controls are employed at comparable rates once the tracking period is expanded. This implies that an additional month of UI benefit eligibility does not effect the likelihood of finding a job within a year after high school graduation. The duration until a job offer is accepted will however increase marginally. To test whether this translates into higher entry level wages, consider the following regression equation:

$$w_{ic} = \alpha_c + \tau_1 + ... + \tau_k + \gamma_1 + ... + \gamma_4 + \beta_c D_{ic} + X_{ic}'\phi + \varepsilon_{ic} \tag{6}$$

Where $w_{ic}$ is the log monthly wage of individual $i$ in segment $c$, $\alpha_c$ is a segment fixed effect, $\tau_1, ..., \tau_k$ are fixed effects for month of entry into unemployment, $\gamma_1, ..., \gamma_4$ are dummy variables for year of graduation, $D_{ic}$ is a dummy variable equal to one if individual $i$ belongs to the treatment group at segment $c$, and $X_{ic}$ is a vector of controls.[10]

I once again begin the analysis by pooling the discontinuities and estimate them jointly. Standard errors are clustered by person and segment fixed effects are included in every specification. This produces the results collected in *Table 3*. The estimates are small, and statistically insignificant once fixed effects for month of entry are included.

[10]The vector of controls includes gender, grades from primary school and dummy variables for type of high school program, i.e. academic, vocational or other.

**Table 3:** UI Benefits and Entry Level Wages, Pooled Estimates

| VARIABLES | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Treatment effect | 0.008*** | 0.007*** | 0.004 | 0.004 |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 76,677 | 76,677 | 76,677 | 76,677 |
| Clustered standard errors | ✓ | ✓ | ✓ | ✓ |
| Year of graduation | ✓ | ✓ | ✓ | ✓ |
| Controls | | ✓ | | ✓ |
| Month of inflow FE | | | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Estimated treatment effect after pooling all the available thresholds. The vector of controls includes gender, grades from primary school and dummy variables for type of high school program, i.e. academic, vocational or other. *** p<0.01, ** p<0.05, * p<0.1

Next, I run specification (6) separately for each segment of the running variable. This produces the results collected in appendix *Table A6*. There is no evidence that UI benefits increase post unemployment wages. Altogether, these results suggest that the drop in job finding rates associated with the spike in UI benefit payments operates through some other channel than increased worker selectivity, for instance reduced search intensity.

# 8 Conclusion

This paper attempts to estimate the effect of UI-benefit eligibility on job-finding rates and entry level wages for unemployed high school leavers. Between 2002 and 2006, all school-leavers became eligible for UI once they turned 20 years. Therefore, the start of an unemployment spell relative to the 20:th birthday creates potentially exogenous variation in the duration until jobseekers become eligible for UI. This was exploited using the framework developed by Van den Berg et al. (2010).

I find that UI-benefit eligibility reduce the conditional probability to leave unemployment by one percentage point. The baseline hazard is roughly 8%, which means that the treatment effect translates into a 12.5% drop in the employment hazard. I further find that UI-benefit eligibility have no effect on entry-level wages. This would suggest that the impact of UI eligibility on job finding rates operates through some other channel than increased selectivity, perhaps reduced search intensity.

It is worth stressing that the estimates provided in this paper focus on short-run effects of UI-benefit eligibility on job finding rates and entry-level wages. Specifically, I focus on the effects of being exposed to UI eligibility for one month. It is not necessarily the case that the results can be generalized to prolonged exposure to UI eligibility. However, it is not possible to study long-run effects of UI on the employment hazard without imposing some untestable model structure to deal with dynamic selection. Hence, although there are some clear limitations of focusing on short-run effects of UI, it provides more robust evidence of the effects of UI on employment.

## References


Jaap H Abbring and Gerard J Van den Berg. The nonparametric identification of treatment effects in duration models. *Econometrica*, 71(5):1491–1517, 2003.

Marinho Bertanha. Regression discontinuity design with many thresholds. *Browser Download This Paper*, 2016.

Matias D Cattaneo, Rocío Titiunik, Gonzalo Vazquez-Bare, and Luke Keele. Interpreting regression discontinuity designs with multiple cutoffs. *The Journal of Politics*, 78(4): 1229–1248, 2016.

Bart Cockx and Eva Van Belle. Wating longer before claiming, and activating youth. no point? 2016.

Anders Forslund and Kristina Sibbmark. Kommunala arbetsmarknadsinsatser riktade till ungdomar mellan 18 och 24 år, 2005.

Peter Fredriksson and Björn Öckert. Life-cycle effects of age at school start. *The Economic Journal*, 124(579):977–1004, 2014.

Paul Gregg. The impact of youth unemployment on adult unemployment in the ncds. *The economic journal*, 111(475):626–653, 2001.

James Heckman. Shadow prices, market wages, and labor supply. *Econometrica: journal of the econometric society*, pages 679–694, 1974.

Lena Hensvik and Oskar Nordström Skans. Networks and youth labor market entry. Technical report, Working Paper, IFAU-Institute for Evaluation of Labour Market and Education Policy, 2013.

Francis Kramarz and Skans. When strong ties are strong: Networks and youth labour market entry. *The Review of Economic Studies*, page rdt049, 2014.

Edwin Leuven and Hessel Oosterbeek. Overeducation and mismatch in the labor market. In *Handbook of the Economics of Education*, volume 4, pages 283–326. Elsevier, 2011.

Dale T Mortensen. Unemployment insurance and job search decisions. *ILR Review*, 30 (4):505–517, 1977.

David Neumark and William Wascher. Minimum-wage effects on school and work transitions of teenagers. *The American Economic Review*, 85(2):244–249, 1995.

Oskar Nordstrom Skans. Scarring effects of the first labor market experience. 2011.

John P Papay, John B Willett, and Richard J Murnane. Extending the regression-discontinuity approach to multiple assignment variables. *Journal of Econometrics*, 161 (2):203–207, 2011.

Oddbjørn Raaum and Knut Røed. Do business cycle conditions at the time of labor market entry affect future employment prospects? *The review of economics and statistics*, 88 (2):193–210, 2006.

Stefani Scherer. Stepping-stones or traps? the consequences of labour market entry positions on future careers in west germany, great britain and italy. *Work, employment and society*, 18(2):369–394, 2004.

Gerard J Van den Berg, Antoine Bozio, and Monica Costa Dias. Policy discontinuity and duration outcomes. 2010.

Kenneth I Wolpin. Estimating a structural search model: The transition from school to work. *Econometrica: Journal of the Econometric Society*, pages 801–817, 1987.

# Appendix

**Table A1:** Descriptive Statistics

| Running variable | Grades | Academic | Vocational | Other | Male |
|---|---|---|---|---|---|
| 3 | -0.073 | 0.471 | 0.436 | 0.092 | 0.525 |
| | (0.817) | (0.499) | (0.496) | (0.289) | (0.499) |
| 4 | -0.072 | 0.471 | 0.438 | 0.090 | 0.527 |
| | (0.831) | (0.499) | (0.496) | (0.287) | (0.499) |
| 5 | -0.094 | 0.467 | 0.443 | 0.090 | 0.536 |
| | (0.840) | (0.499) | (0.497) | (0.286) | (0.499) |
| 6 | -0.113 | 0.469 | 0.449 | 0.082 | 0.533 |
| | (0.835) | (0.499) | (0.497) | (0.274) | (0.499) |
| 7 | -0.112 | 0.464 | 0.449 | 0.087 | 0.523 |
| | (0.825) | (0.499) | (0.497) | (0.282) | (0.500) |
| 8 | -0.147 | 0.450 | 0.463 | 0.087 | 0.524 |
| | (0.829) | (0.498) | (0.499) | (0.282) | (0.499) |
| 9 | -0.152 | 0.457 | 0.457 | 0.086 | 0.515 |
| | (0.853) | (0.498) | (0.498) | (0.280) | (0.500) |
| 10 | -0.182 | 0.437 | 0.479 | 0.084 | 0.501 |
| | (0.851) | (0.496) | (0.500) | (0.277) | (0.500) |
| 11 | -0.207 | 0.431 | 0.484 | 0.085 | 0.476 |
| | (0.859) | (0.495) | (0.500) | (0.278) | (0.500) |
| 12 | -0.211 | 0.426 | 0.487 | 0.087 | 0.480 |
| | (0.870) | (0.495) | (0.500) | (0.282) | (0.500) |
| 13 | -0.268 | 0.427 | 0.502 | 0.071 | 0.501 |
| | (0.886) | (0.495) | (0.500) | (0.257) | (0.500) |
| Total | -0.133 | 0.456 | 0.457 | 0.087 | 0.518 |
| | (0.842) | (0.498) | (0.498) | (0.282) | (0.500) |

Notes: The table show the mean and standard deviation (in parenthesis) of selected across the distribution of the running variable. Academic, Vocational and Other refers to academic, vocational and other high school program respectively.

**Table A2:** Treatment vs. Control group. P-values for Wald Test Comparing the Distribution of Covariates

| Running variable | | | |
|---|---|---|---|
| Treatment group | Control group | Elapsed duration | p-value |
| 3 | 4 | 0 | 0.97 |
| 3 | 4 | 3 | 0.77 |
| 4 | 5 | 0 | 0.59 |
| 4 | 5 | 4 | 0.75 |
| 5 | 6 | 0 | 0.26 |
| 5 | 6 | 5 | 0.15 |
| 6 | 7 | 0 | 0.84 |
| 6 | 7 | 6 | 0.48 |
| 7 | 8 | 0 | 0.28 |
| 7 | 8 | 7 | 0.67 |
| 8 | 9 | 0 | 0.62 |
| 8 | 9 | 8 | 0.40 |
| 9 | 10 | 0 | 0.19 |
| 9 | 10 | 9 | 0.31 |
| 10 | 11 | 0 | 0.18 |
| 10 | 11 | 10 | 0.79 |
| 11 | 12 | 0 | 0.96 |
| 11 | 12 | 11 | 0.03 |
| 12 | 13 | 0 | 0.55 |
| 12 | 13 | 12 | 0.01 |

The vector of controls include gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other. The distribution of covariates are compared when subjects flow into unemployment and conditional on survival until the treatment group becomes eligible for UI benefits.

**Figure A1:** Empirical Survivor Functions

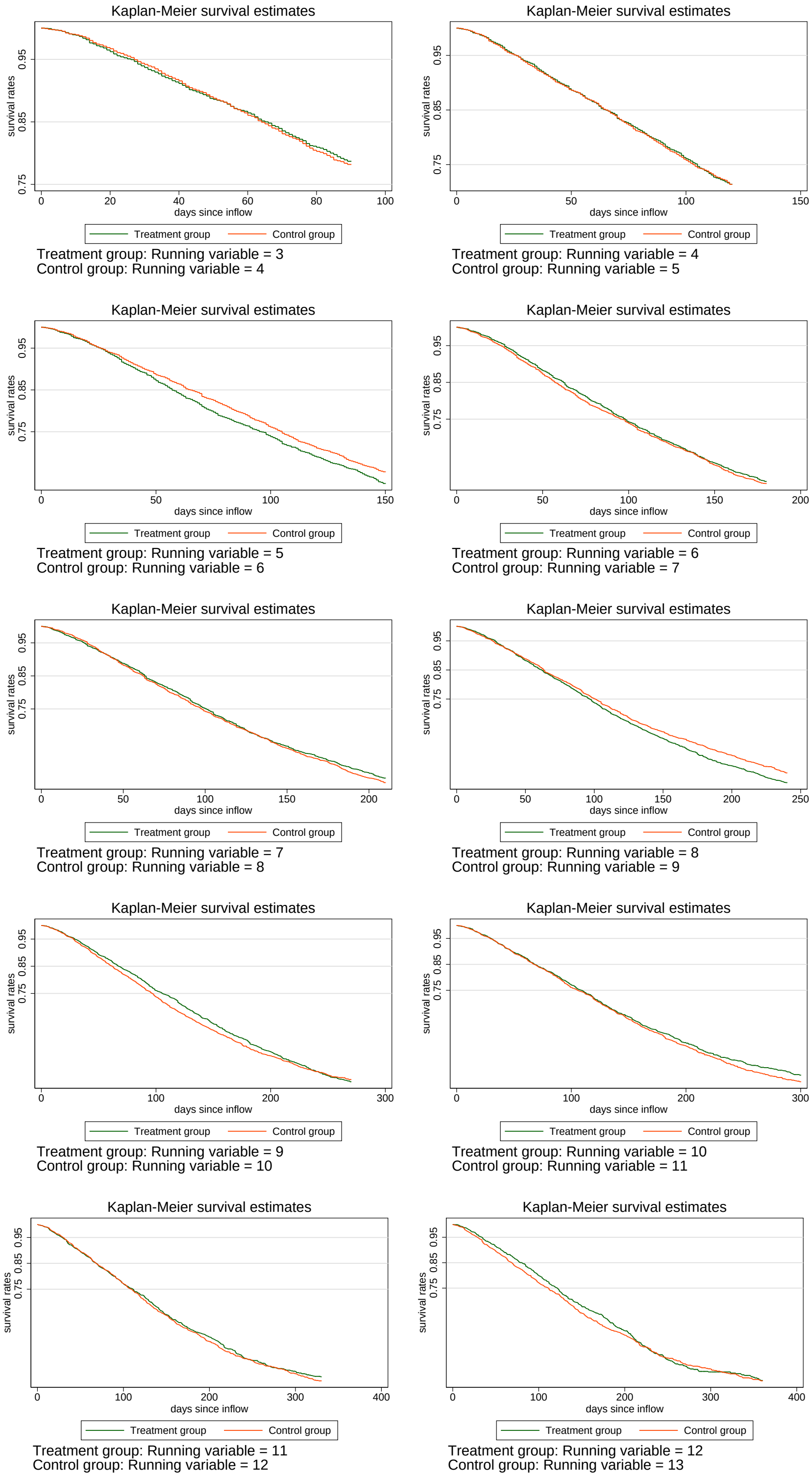


**Figure A2:** Job Finding Rates for the Treatment and Control Groups

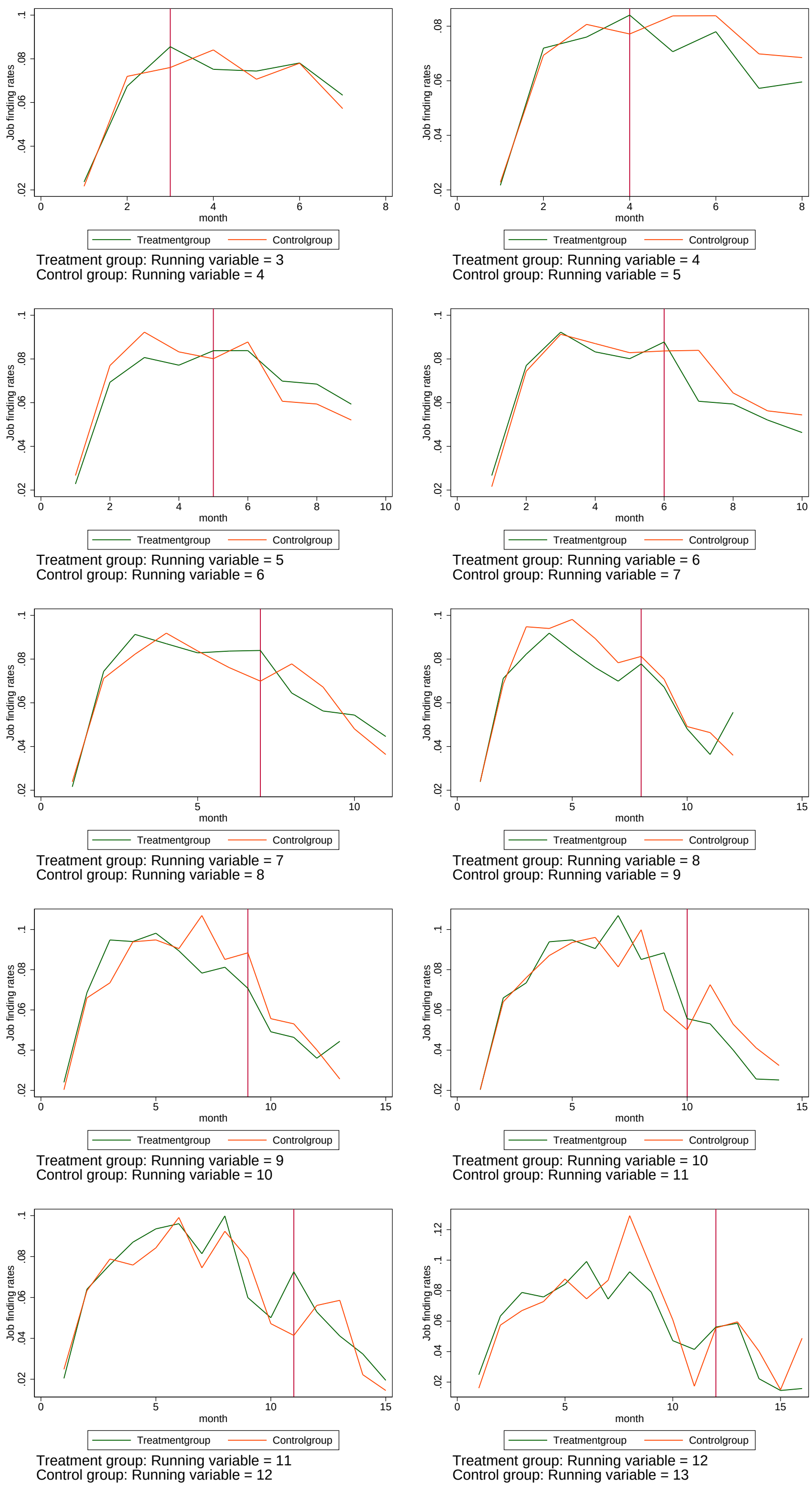


**Figure A3:** UI benefits and the Employment Hazard

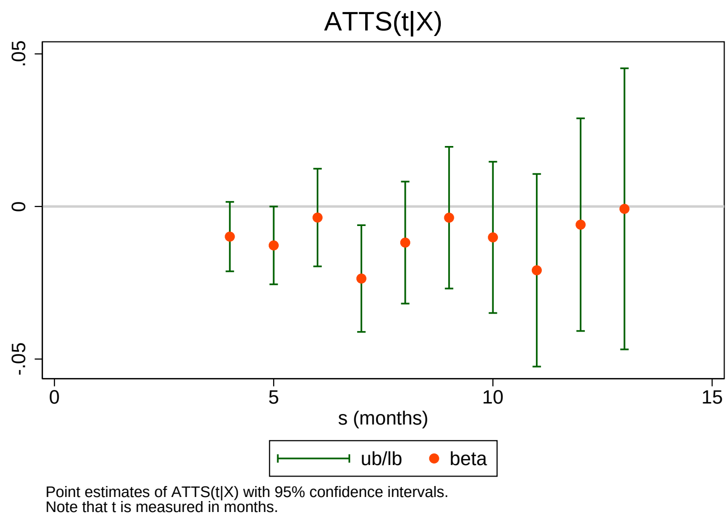
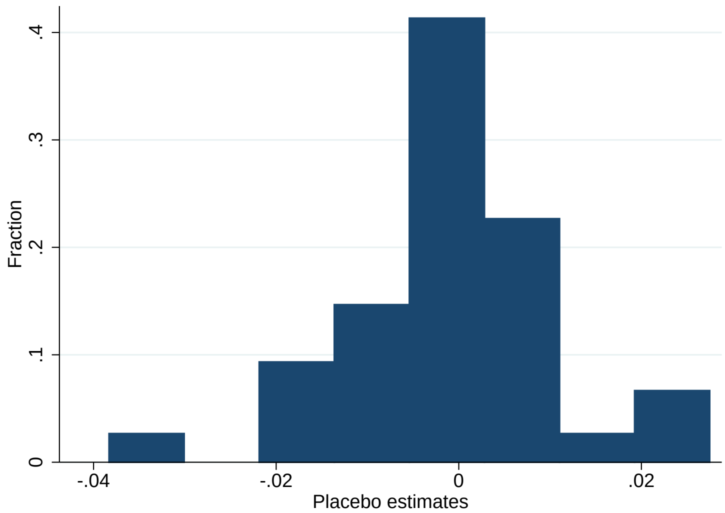


**Table A3:** UI Benefits and the Employment Hazard

| Running variable<br>Treatment/Control group | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| 3,4 | -0.009 | -0.009 | -0.010* | -0.010* |
| | (0.006) | (0.006) | (0.006) | (0.006) |
| 4,5 | -0.013** | -0.013** | -0.013** | -0.013** |
| | (0.007) | (0.006) | (0.007) | (0.007) |
| 5,6 | -0.004 | -0.005 | -0.003 | -0.004 |
| | (0.008) | (0.008) | (0.008) | (0.008) |
| 6,7 | -0.024*** | -0.023*** | -0.024*** | -0.024*** |
| | (0.009) | (0.009) | (0.009) | (0.009) |
| 7,8 | -0.013 | -0.013 | -0.012 | -0.012 |
| | (0.010) | (0.010) | (0.010) | (0.010) |
| 8,9 | -0.004 | -0.003 | -0.004 | -0.004 |
| | (0.012) | (0.012) | (0.012) | (0.012) |
| 9,10 | -0.007 | -0.008 | -0.010 | -0.010 |
| | (0.013) | (0.013) | (0.013) | (0.013) |
| 10,11 | -0.019 | -0.020 | -0.020 | -0.021 |
| | (0.016) | (0.016) | (0.016) | (0.016) |
| 11,12 | -0.003 | -0.004 | -0.005 | -0.006 |
| | (0.018) | (0.018) | (0.018) | (0.018) |
| 12,13 | -0.001 | -0.001 | -0.001 | -0.001 |
| | (0.023) | (0.023) | (0.023) | (0.023) |
| Year of graduation FE:s | ✓ | ✓ | ✓ | ✓ |
| Controls | | ✓ | | ✓ |
| Month of inflow FE:s | | | ✓ | ✓ |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table reports differences in the employment hazards in the calender month when the treatment group can claim UI-benefits and the controls cannot.

**Figure A4:** Empirical Distribution of Placebo Estimates




**Note**: The figure shows the empirical distribution of placebo estimates. These estimates are computed by comparing the employment hazards in treated and control groups during the period where no one could claim UI-benefits. Overall, 5.3% of the placebo estimates are significant at the 5% level.

**Figure A5:** ALMP Participation Rates for the Treatment and Control Groups

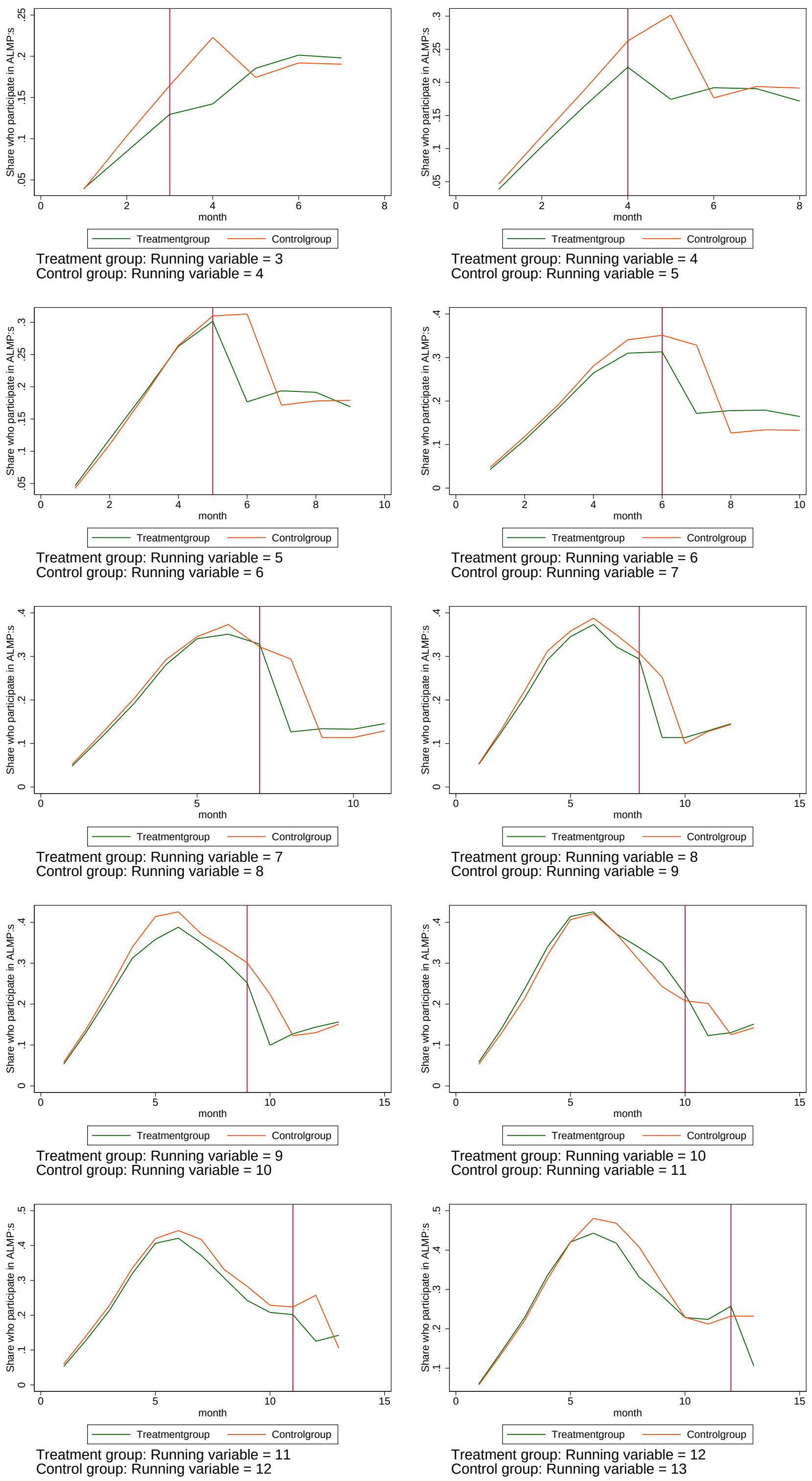


**Figure A6:** ALMP Participation Rates, with Censoring

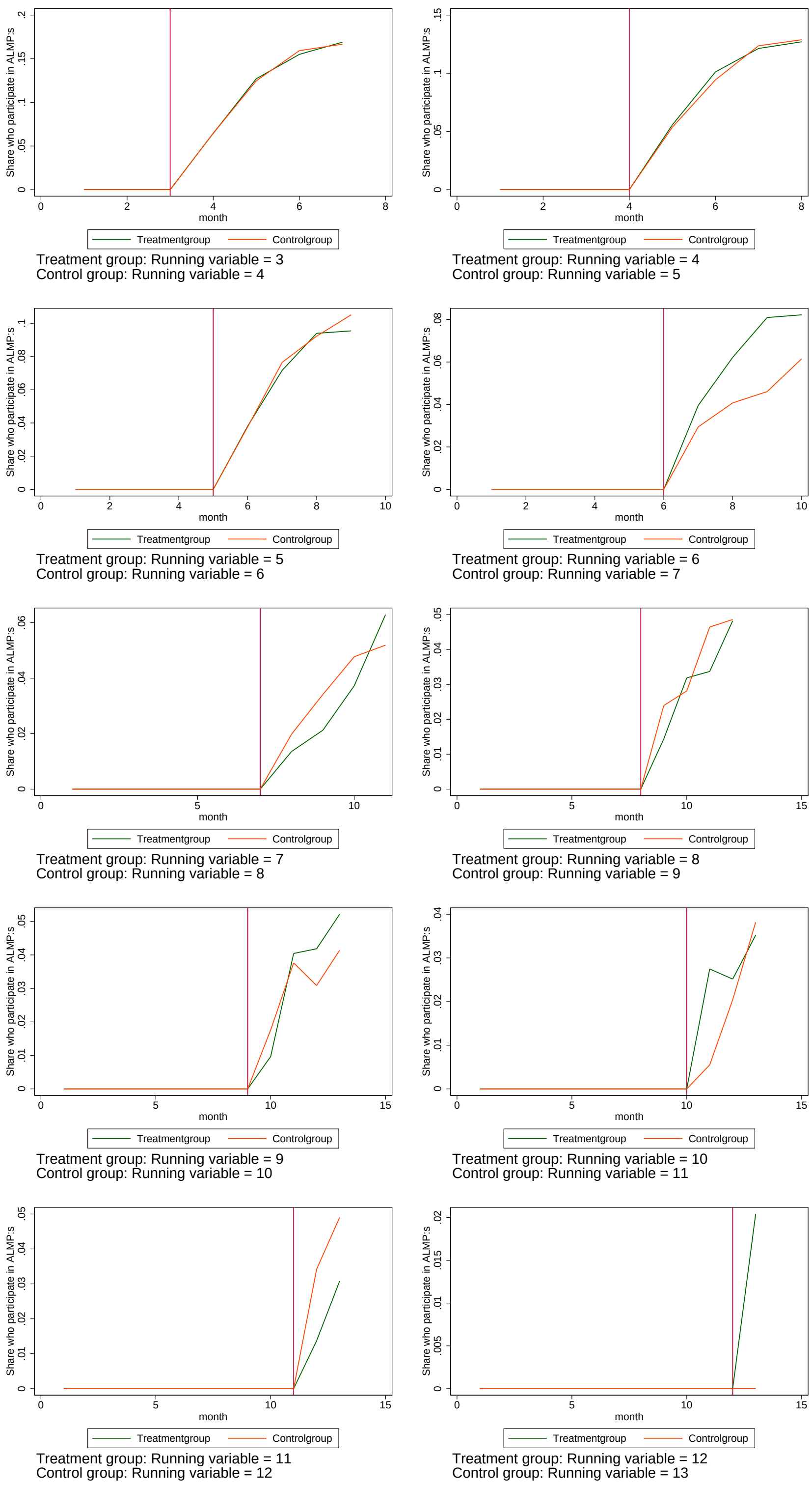


**Table A4:** Pooled Estimate, Robustness Test

| VARIABLES | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Treatment effect | -0.009** | -0.009** | -0.008** | -0.008** |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Observations | 20,161 | 20,161 | 20,161 | 20,161 |
| Clustered standard errors | ✓ | ✓ | ✓ | ✓ |
| Year of graduation FE | ✓ | ✓ | ✓ | ✓ |
| Controls | | ✓ | | ✓ |
| Month of inflow FE | | | ✓ | ✓ |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Estimated treatment effect after pooling all the available thresholds. The vector of controls include gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other. *** p<0.01, ** p<0.05, * p<0.1

**Table A5:** Treatment vs. Control group. P-values for Wald Test Comparing the Likelihood of Employment.

| Running variable | | | |
|---|---|---|---|
| Treatment group | Control group | $\beta_j$ | p-value |
| 3 | 4 | 0.0017 | 0.82 |
| 4 | 5 | 0.0096 | 0.19 |
| 5 | 6 | 0.0116 | 0.13 |
| 6 | 7 | 0.0119 | 0.13 |
| 7 | 8 | -0.007 | 0.39 |
| 8 | 9 | 0.0038 | 0.65 |
| 9 | 10 | 0.0031 | 0.74 |
| 10 | 11 | 0.0062 | 0.53 |
| 11 | 12 | -0.0006 | 0.95 |
| 12 | 13 | -0.0001 | 0.99 |

Table A6: UI Benefits and Wages

| Running variable | | | | |
|---|---|---|---|---|
| Treatment/Control group | Model 1 | Model 2 | Model 3 | Model 4 |
| 3,4 | 0.019 | 0.020 | 0.012 | 0.013 |
| | (0.022) | (0.021) | (0.022) | (0.022) |
| 4,5 | -0.014 | -0.014 | -0.021 | -0.020 |
| | (0.021) | (0.020) | (0.021) | (0.021) |
| 5,6 | 0.011 | 0.011 | 0.008 | 0.006 |
| | (0.021) | (0.021) | (0.022) | (0.021) |
| 6,7 | 0.012 | 0.008 | 0.011 | 0.008 |
| | (0.022) | (0.021) | (0.022) | (0.022) |
| 7,8 | -0.007 | -0.003 | 0.006 | 0.009 |
| | (0.022) | (0.022) | (0.023) | (0.022) |
| 8,9 | 0.010 | 0.005 | 0.021 | 0.016 |
| | (0.024) | (0.024) | (0.024) | (0.024) |
| 9,10 | 0.048* | 0.041 | 0.026 | 0.030 |
| | (0.026) | (0.026) | (0.027) | (0.027) |
| 10,11 | -0.020 | -0.025 | -0.030 | -0.034 |
| | (0.028) | (0.028) | (0.029) | (0.029) |
| 11,12 | -0.008 | -0.005 | -0.014 | -0.010 |
| | (0.030) | (0.029) | (0.031) | (0.031) |
| 12,13 | 0.047 | 0.051 | 0.040 | 0.039 |
| | (0.037) | (0.036) | (0.039) | (0.039) |
| Year of graduation FE:s | ✓ | ✓ | ✓ | ✓ |
| Controls | | ✓ | | ✓ |
| Month of inflow FE:s | | | ✓ | ✓ |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table reports differences in log monthly wages between treatment and control groups.