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Extended unemployment benefits and the hazard to employment

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IFAU INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

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Extended Unemployment Benefits and the Hazard to Employment [a]

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Abstract

Previous studies estimating the effect of generosity of unemployment insurance (UI) on unemployment duration has found that as job seekers approach benefit exhaustion the probability of leaving unemployment increases sharply. Such "spikes" in the hazard rate has generally been interpreted as job seekers timing their employment to coincide with benefit exhaustion. Card, Chetty and Weber (2007*b*) argue that such spikes rather reflect flight out of the labor force as benefits run out. This paper revisits this debate by studying a 30 week UI benefit extension in Sweden and its effects on unemployment duration, duration on UI, as well as the timing of employment. As the UI extension is predicated upon a job seeker having a child below the age of 18 at the time of regular UI exhaustion this provides quasi-experimental variation which I exploit using a regression discontinuity design. I find that although increasing potential UI duration by 30 weeks increases actual take up by about 2.5 weeks, overall duration in unemployment and the probability of employment is largely unaffected. Moreover, I find no evidence of job seekers manipulating the hazard to employment such that it coincides with UI benefit exhaustion. This result is attributed to generous replacement rates offered in other assistance programs available to job seekers who exhaust their benefits.

JEL: J64, J65
Keywords: Unemployment benefits, Unemployment duration, Hazard spike

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1 Introduction

How does the generosity of unemployment insurance (UI) affect job search behavior? While providing a safety net for unexpected job loss, the provision of UI creates disincentives for job search by lowering the alternative cost to working. The question of how benefit levels and its overall generosity affects time in, and the hazard out of, unemployment has a long tradition in labor economics and been subject to extensive research. The "spike" in the hazard rate out of unemployment coinciding with UI exhaustion is a widely established empirical result since the seminal work by Katz and Meyer (1990*a*,*b*). This result has generally been attributed to shirking behavior among job seekers, holding off finding a new job until approaching benefit exhaustion. However, later work by Card, Chetty and Weber (2007*b*) challenges this view by attributing the lion's share of such spikes to flight out of the labor force. They argue that "[s]pikes are generally smaller when the spell length is measured by the time to next job than when it is defined by the time spent on the unemployment system" (p. 1). Hence, " [...] the size of the spike in re-employment rates at exhaustion in the current U.S. labor market (and many other labor markets) remains an open question. Further work on estimating these hazards using administrative measures of time to next job would be particularly valuable" (p. 16).[1] Indeed, if benefit exhaustion renders job seekers to leave the labor force, the expected cost of extending UI benefits could be exaggerated if transition to work is higher from unemployment than non-employment.

This paper contributes to the debate about the timing of re-employment and UI exhaustion, while additionally adding to the large literature on the effects of UI on job search behavior (see section 2 for a short review). In particular, I examine the effect on unemployment duration, and exit to employment, of an exogenous 30-week UI benefit extension in Sweden. For identification, I take advantage of a feature in the Swedish UI system which entitles individuals with a child below the age of 18 to 90 weeks of unemployment benefits instead of the statutory 60 weeks. As assignment to the extended UI benefit is determined by the age of a job seekers' youngest child at the time of regular UI exhaustion (60 weeks), I exploit the quasi-experimental variation generated around the age threshold using a regression discontinuity (RD) design. This allows me to estimate the casual effect of increasing potential duration of UI on actual benefit duration, unemployment duration and hazard to employment. Further, I allow the effects to vary with duration on UI and in unemployment to test whether job seekers time employment to benefit extension.

The main findings are threefold. First, while the increase in potential duration on UI increases actual duration on UI by about 2.5 weeks on average, I find no evidence of it prolonging duration in registered unemployment or negatively effecting the hazard to employment. This suggest that, the 30 week benefit extension did not prolong average unemployment duration as job seekers were on average unemployed as long but with a somewhat higher replacement

[1] The term re-employment refers here to an exit out of unemployment to any new employer whereas a recall is returning to ones previous employer.

rate. The absence of negative effects on unemployment duration and future employment is believed to be driven by job seekers access to fairly generous post-UI programs which weakens the disincentive effects of the benefit extension. Second, being eligible to 30 additional weeks of UI does not appear to have affected job search behavior prior to the actual extension period. That is, I find no evidence of job seekers lowering their search effort due to the anticipation of extended benefits. Third, I find distinct spikes in the exit out of UI at benefit exhaustion, but no such spikes are present in the hazard to employment. This therefore speaks in favor of the interpretation made in Card, Chetty and Weber (2007*b*).

The remainder of the paper is organized as follows. Section 2 briefly reviews the related literature and section 3 describes the Swedish UI system and the institutional details surrounding the benefit extension. In section 4, I outline the identification strategy, describe the data and validate the assumptions needed for casual inference. Section 5 presents the empirical results while section 6 concludes.

2 Previous Literature

There is an extensive literature on how the generosity of UI affects job search behavior where the results are largely coherent with the theoretical predictions made in Mortensen (1977).[2] For the U.S., Card and Levine (2000) studies a (temporary) program which offered a benefit extension of 13 weeks to the unemployed in New Jersey. While the number of people reaching regular benefit exhaustion appears to have increased by about 1-3 percent, exit-rates and average unemployment duration remained virtually unchanged. In their seminal study, Katz and Meyer (1990*a*) detects sharp increases in the hazard out of unemployment at the time of benefit exhaustion. Moreover, they suggest that extending the potential duration by one week prolongs unemployment duration by about 0.16 to 0.2 weeks. In more recent studies, Card et al. (2015) and Landais (2015) exploit kinks in the US benefit schedule to estimate the effect of increased UI benefits. While one additional week of potential UI is estimated to increase unemployment duration by 0.2-0.4 weeks, the elasticity with respect to the benefit level ranges between 0.2 to 0.7.[3] Moreover, Card et al. (2015) suggests that these elasticities differ substantially with overall macroeconomic conditions. This highlights the problem of policy endogeneity which many early U.S. studies of unemployment behavior have been subject to. A increase in potential duration have been induced by business cycles, estimates on unemployment duration will inevitably be biased.[4]

For Europe, Hunt (1995) evaluates a reform in Germany which resembles the one investigated in this paper. Replacement rates were cut from 63 to 56 percent for unemployed workers without

[2] A strand of literature also looks at the effect of UI generosity on job match quality (c.f. Nekoei and Weber (2017); Lalive (2007), Card, Chetty and Weber (2007*a*); Caliendo, Tatsiramos and Uhlendorff (2013)) where the evidence suggests a zero or very small positive effect.

[3] For a summary of estimated elasticities in the U.S. across studies see appendix in Card et al. (2012) Table 4.

[4] See Lalive, Van Ours and Zweimüller (2006) for a discussion of the importance of understanding policy endogeneity when estimating the effect of benefit increases.

children. While no significant changes in the flow to employment could be detected among parents, the reform seems to have had the adverse effect of increasing the likelihood of leaving the labor force. In a subsequent reform, Hunt (1995) finds that extending benefits for workers above the age of 42 increases their duration of unemployment.[5] However, the impact on the hazard to leaving the labor force appears to be larger than the hazard to employment among the older workers, thus corroborating the interpretation of hazard spikes at UI exhaustion (Card, Chetty and Weber, 2007*b*). Exploiting similar age thresholds for older workers in Germany, Schmieder, Von wachter and Bender (2012) estimates the effect of extended potential duration on non-employment duration using data covering 20 years. They find that an additional week of UI benefits yields 0.1 weeks of longer non-employment duration on average. The effect on actual UI benefit duration are estimated to be three to four times larger.

Several studies have taken advantage of various benefit discontinuities in the Austrian UI-system, rendering exogenous variation in both potential duration, replacement rates and severance pay (see e.g. Lalive, Van Ours and Zweimüller, 2006; Lalive, 2007, 2008; Card, Chetty and Weber, 2007*a*,*b*; Nekoei and Weber, 2017). The estimates on benefit extension are largely consistent across the studies, ranging from 0.05 to 0.1 additional weeks of unemployment or non-employment duration from one extra week of potential duration.[6] In other words, 10 weeks of increased potential duration tends to prolong non-employment by about 0.5 to 1 weeks (Card, Chetty and Weber, 2007*a*; Lalive, Van Ours and Zweimüller, 2006; Lalive, 2008).[7] An interesting feature in Card, Chetty and Weber (2007*a*,*b*) is also that potential duration lowers hazard rates throughout the entire spell, thus implying that people are forward looking as the benefit extension affects job search behavior in expectation of future benefits. They show that job-finding rates decrease by about 5-9 percent during the first 20 weeks when extending potential duration from 20 to 30 weeks.

A well-established empirical fact is the spike in hazard rates at the time of benefit exhaustion. This has primarily been attributed to shirking behavior among the unemployed by seemingly holding off taking a job until benefits run out.[8] Using reductions in potential benefit durations in Slovenia, van Ours and Vodopivec (2006) show that such spikes move, almost one to one, with the timing of exhaustion. While this could represent job seekers both finding jobs and moving to labor market programs or leaving the labor force, Card, Chetty and Weber (2007*b*), in contrast, shows that the spike in Austria is driven by job seekers exiting the labor force and not entering employment. The unemployment exit hazard is 2.4 times larger at exhaustion

[5]The magnitude of the effects are, however, somewhat unreliable as significant effects can only be found among 44-48 year olds whereas 49-57 year olds are unaffected.

[6]Lalive (2008) uses data on unemployment duration and finds that the effect of one week increase in potential duration for women is 0.32-0.44. This upper estimate is however biased due to manipulation of the forcing variable among women. The lower estimate, using border identification, which is less likely subject to self selection, the effect is still 4 times larger than for men. This is attributed to special rules for early retirement for women.

[7]The large difference between the Austrian and U.S. estimates (0.05-0.1 vs. 0.16-0.4) warrants some attention. As future benefits will be discounted by the probability of survival and potential duration may exhibit decreasing marginal utility one potential explanation for these results could be differences in baseline potential benefit durations.

[8]Card and Levine (2000) proposes informal contracts between the unemployed and the old employer such that recalls are timed to UI exhaustion thus rendering a spike in the hazard rate at that time.

than at the baseline period while the employment hazard is 1.15. Moreover, recalls to old jobs appear to be more common than starting new ones. In fact, Card, Chetty and Weber (2007*b*) suggests that fewer than one percent of the spells are manipulated in such a way that job finding coincides with the timing of benefit exhaustion. This is also consistent with estimates in Schmieder, Von wachter and Bender (2012) where only 8 percent of unemployed who reach benefit exhaustion return to employment whereas the majority escapes to non-employment.

There are a couple of studies estimating the effect of UI generosity on duration and hazard rates in Sweden. Focusing on the presence of hazard spikes, Carling et al. (1996) estimates the transition to employment and labor market programs. Though imprecise, the estimates give evidence of spikes at exhaustion but, due to the lack of a valid control group, it is not possible to draw firm conclusions about the potential distorting effects of UI. Studying a Swedish reform in 1995, which cut replacement rates by 5 percent, Carling, Holmlund and Vejsiu (2001) finds an increased transition to employment by 10 percent, or elasticity of 1.7 which is substantially larger than any other comparable finding. The interpretation of the results are, however, muttered by accompanying changes in the UI system which increased the incentives for job search. Moreover, treatment and control groups are defined based on previous wages, which could influence the hazard to employment directly and therefore bias the estimates. Both these objections carry over to Bennmarker, Carling and Holmlund (2007) who evaluates two consecutive UI reforms in Sweden in 2001 and 2002, which increased the benefit cap. Here the overall hazard rate appears to be unaffected by the reform. However, a further analysis shows heterogeneous responses across gender with men being largely unaffected while women, in stark contrast to standard theoretical predictions, increase the employment. Bennmarker, Carling and Holmlund (2007) attributes this unexpected effect to a child care reform taking place at the same time.

3 Unemployment Compensation in Sweden

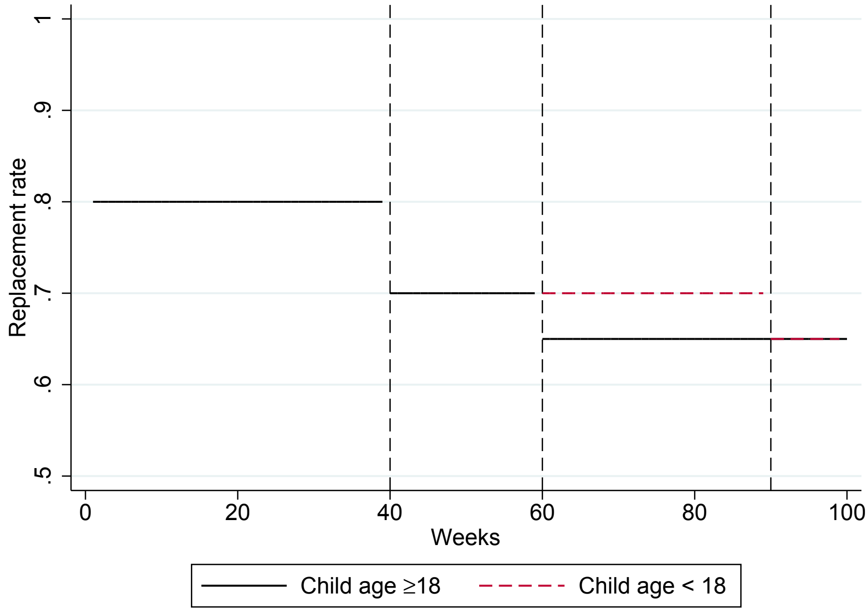
The provision of UI in Sweden is obtained through voluntary membership in branch-specific union-affiliated UI funds and the national coverage rate is about 70 percent of the labor force.[9] Job losers with sufficient work history are eligible for UI benefits with a base amount of 320 SEK per day as long as they are registered at the public employment service (PES). In order to acquire income-related UI benefits, the unemployed having been working for twelve months also have been a member of a UI fund for the same amount of time. The maximum replacement rate is then 80 percent of the workers' former wage, subject to a benefit cap of 680 SEK per day is implemented on monthly wages above 18,700 SEK.[10,11] Workers who are laid off have a seven day waiting period before receiving their first UI payment whereas voluntary quitters are

[9] Several reforms enacted in 2007, one of which increased UI-fund membership fees, led to a significant drop in the number of workers eligible for UI.

[10] In September 2015 the cap was raised to 25,025 SEK. As this is outside the sample period this does not affect my estimations.

[11] In April 2019, the SEK/US Dollar conversion rate was 9.2 and SEK/Euro conversion rate was 10.4.

Figure 1: Benefit Schedule



Notes: The figure shows replacement rates by weeks in unemployment (on UI) for job seekers entitled to income related UI with wages above and below the wage floor and cap, respectively. The solid black line depicts replacement rates for workers who at the time of regular benefit exhaustion (60 weeks) have a child above the age of 18. The dashed red show replacement rate for job seekers who are the care taker of a child below the age of 18 at week 60 on UI.

subject to a 45-day waiting period.[12]

The statutory length of a regular benefit period is 60 weeks (300 working days, 5 days a week). A job seeker may choose how many days a week he or she want to be collect UI benefits, where the maximum is 5 days per week. Therefore the duration on UI may be longer than 60 weeks if a job seeker chooses to collect UI part-time. Hence, there is a difference between the duration on UI and utilization of UI where the former refers to calender time on UI and the latter how many days/weeks are collected. Note that the two are equal if a job seeker utilizes 5 days a week.

The benefit schedule has a two-tiered structure where replacement rates are cut from 80 to 70 percent, 40 weeks into the benefit period (i.e after the job seeker has utilized 200 days of UI). However, the second tier could be extended by 30 weeks if the unemployed – at the time of regular benefit exhaustion (week 60) – is the caretaker of a child below the age of 18. Figure 1 shows the step-wise benefit schedule which, conditional on having a child, is a discontinuous function of the child's age at week 60. The extension is formally awarded at the 300th day on UI and is based on the child's age at that exact time which is checked by a third party. If a child turn 18 during the extended period, the extension has already been granted and hence there is no change in replacement rate until the extended benefit period runs out. Importantly, if individuals are forward looking and the discontinuity is salient enough, future benefits should be discounted to its present value thus making the discontinuity equally present at the start

[12] Prior to July 7 2008, there was a 5-day waiting period for involuntary quitters.

of unemployment as job losers would be able to approximate the age of their child at regular benefit exhaustion by adding to it the number of weeks remaining on UI.[13] In that case, job finding rates could be affected prior to the benefit *de facto* being awarded (Card, Chetty and Weber, 2007*b*).

job seekers who exhaust their benefits are offered to enter the Job and Development Guarantee (JDG), an active labor market program targeted towards the long-term unemployed. Participation in the program entitles the job seeker to activity support (a form of unemployment assistance) which corresponds to a replacement rate of 65 percent which is paid out by the Swedish Social Insurance Agency for an, essentially, indefinite period. Participating in the JDG is optional for individuals who are entitled to extended benefits as they can enter after their 60th week on UI but remain at 70 percent replacement rate until week 90 when the extended period ends. After that, the same rules apply.

Due to the benefit cap and the base amount, only workers with monthly wages between $10,057 - 23,015$ SEK are affected by the benefit cut. Treatment intensity thus varies directly both through the individuals' former wage, and indirectly through the probability of staying unemployed. To get a sense of the magnitude of the financial incentives one could imagine an individual who intends to uphold UI for as long as possible. In other words, the probability of staying on UI is equal to one. Fully utilizing the 30 weeks of extended benefits with 70 percent versus 65 percent would then render an additional amount of $0 - 7285.5$ SEK ($0 - 48.57$ SEK daily) depending on the former wage. Figure 2 shows the financial incentives (i.e. treatment intensity) based on former wages assuming that the job seeker stays on UI throughout. The two dashed lines depict the interval where treatment intensity is largest in percentage terms of the former wage, i.e. a 5 percentage point difference between the control and treatment group.

4 Identification Strategy

4.1 Empirical Strategy

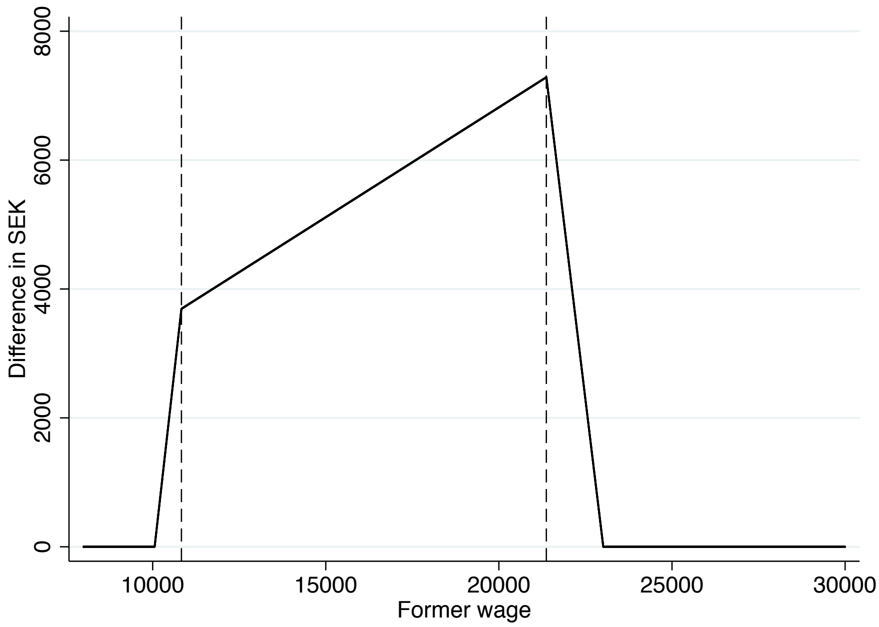
An individual's benefit level is a function of his or her former wage. Therefore, it is likely to be correlated with personal characteristics that could affect the duration of unemployment directly. In order to circumvent this omitted variable bias I take advantage of the institutional setting described in section 3 using a RD design. In the limit, close to the threshold, treatment can be thought of as randomly assigned and hence orthogonal to any remaining heterogeneity that might influence the outcome directly (Lee and Lemieux, 2010). I estimate the following baseline model

$$y_i = \alpha + \beta \mathbf{1}[ChildAge_i < 18] + f(ChildAge_i) + X_i'\delta + \varepsilon_i \tag{1}$$

where y_i is the outcome variable which represents either benefit duration (weeks on UI) or unemployment duration (weeks in registered unemployment at PES) of individual i. The forcing

[13] In this case, one can, for newly awarded benefit periods of 60 weeks, view the discontinuity as the child being 16 year and 44 ± 1 weeks at the first day of the benefit period rather than ± 18 at the time of benefit exhaustion.

Figure 2: Treatment intensity by former wage



Notes: The figure shows the maximum difference in Swedish krona (SEK) as function of the job seekers former wage between job seekers entitled and not entitled to the extended benefit duration. The calculations assumes full discounting and a probability equal to unity of surviving on unemployment for at least 90 weeks.

variable $ChildAge_i$ is the age of individual i's child in years and months at the time of regular benefit exhaustion, which is normalized to zero and modeled flexibly with a functional form $f(\cdot)$ that allows for different slopes on either side of the threshold. The treatment indicator $\mathbf{1}[ChildAge_i < 18]$ is a dummy variable equal to unity if at the time of regular UI exhaustion the child is below the age of 18.[14,15] X_i' is a vector of individual covariates[16] that I include to increase efficiency and ε_i an error term. I estimate equation (1) semi-parametrically using a local linear regression as the forcing variable is discrete which rules out more recent estimation techniques suggested by Calonico, Cattaneo and Titiunik (2014). I use a main bandwidth of ± 18 months and confirm the robustness of the results by varying both the bandwidth and the functional form $f(\cdot)$ as suggested by Lee and Lemieux (2010).[17]

Equation (1) retrieves the reduced form, intention-to-treat (ITT), estimate of β if individuals are forward looking (as suggested by Card, Chetty and Weber (2007*a*,*b*)) as search behavior would be influenced in expectation of future possible benefit extensions. To investigate to what

[14]The age of a child at benefit exhaustion is approximated by adding the number of weeks remaining in the benefit period until exhaustion to the child's age at the time of the first UI payment in the spell. This assumes the maximum take out of 5 days a week as the sample is restricted to full-time unemployed individuals. It turns out that this is a fairly reasonable assumption, as the hazard out of employment occurs precisely after 60 or 90 weeks for about 83 percent of the sample (see Figure 5)

[15]As the sample only consists of fresh UI spells treatment status is effectively based on the child being below the age of 16 year and 8 months at the start of the unemployment spell. If an individual chooses to utilize UI at a slower pace then 5 days a week this bears the consequence of misspecifying some individuals in the control group as being treated. Nevertheless, utilizing fewer than 5 days a week would be suboptimal as it merely adds to the age of the child at regular benefit exhaustion. This does not, however, introduce bias in the estimate of β but renders it to be interpreted as an intention-to-treat (ITT) effect.

[16]The covariates are: gender, age, annual earnings in 2006 and six dummies for level of education.

[17]See the Appendix for robustness.

extent individuals react and adjust their search behavior in expectation of a future UI extension and/or time their job finding such that it coincides with benefit exhaustion, I estimate a dynamic version of equation (1) as follows:

$$\Pr(y_{it} \mid T \geq t) = \alpha_t + \beta_t \mathbf{1}[ChildAge_i < 18] + f(ChildAge_i) + X_i' \delta_t + \varepsilon_{it} \tag{2}$$

where y_{it} is either a dummy variable equal to unity if individual i leaves the UI system or a dummy for being deregistered from unemployment due to getting employed in time t, effectively censoring observations which lacks an end date or where an individual have left the unemployment register for other reasons than employment. Here β_t captures the difference in the hazard rate out of unemployment between the treated and control for those individuals who are still registered as unemployed at time $T \geq t$.

4.2 Data

I exploit data from the Swedish Public Employment Service (PES) on the universe of registered unemployment spells in Sweden from mid-2007 to the start of 2014. They contain the start and end date of each unemployment spell together with several personal characteristics such as age, gender, level of education, country of birth. I trace job seekers throughout their unemployment spell, registering different stages via search categories such as on the job search, part time unemployed or taking part in various labor market programs. To a certain extent, I observe the reason for leaving unemployment, e.g. whether a job seeker got full-time, part-time, subsidized employment, died or exited to education. However, in the final sample, about 9 percent of spells end due to a reason that is registered as "unknown" or "lost contact" and around 11 percent are right censored ongoing spells at the end of the observation window (February 18, 2014). Unemployment is defined as being registered as full-time unemployed or part of some program which does not involve subsidized employment. An unemployment spell ends with an individual leaving the unemployment register as long as she does not reappear as unemployed within 30 days. This is considered a temporary break of the unemployment spell and thus being a part of the original one. When estimating differences in hazard rates, a spell ends by the individual either entering full-time or part-time employment while the other reasons for exiting unemployment are censored.

The unemployment data is merged with data from the Swedish Unemployment Insurance Board (IAF) that contain weekly UI payments made to each individual. The register contains the start of each UI benefit spell, previous wages, paid benefit amounts and the number of days left in the UI period in any particular week. The start of an unemployment spell and a benefit period do not always coincide as claiming UI payments can be done with a lag. In order to make sure that the benefit period belongs to a particular unemployment spell, I consider benefit periods that have begun within 8 weeks prior to the start of unemployment. This also excludes voluntary quitters as the number of waiting days for voluntary quitters are 45 (9 working weeks). Finally, I use of the Swedish Multi-Generation Register which links parents to their children and contains the date of birth at the monthly level.

A UI benefit period can consist of several unemployment spells. If a spell is interrupted by e.g. temporary employment or education, reverting back to unemployment implies continuing with the previous benefit period unless the employment spell has lasted for more than 12 months. People re-entering unemployment will therefore in general have different number of weeks left on UI until benefit exhaustion.[18] Although using multiple unemployment spells for the same individual under the same benefit period more than doubles the number of observations, it severely complicates the analysis and identification. I therefore restrict the sample to newly registered benefit-entitled unemployment spells.

I impose some additional restrictions on the data. First, I restrict previous wages for which UI is based upon to $10,057 - 23,015$ SEK, as individuals below and above are only partly or completely unaffected by the treatment (see Figure 2). Second, ages are restricted to 25-59, as special rules and programs may apply to younger individuals and early retirement could be an option for older job seekers. Third, In order to not include job seekers having being granted an additional benefit period of 60 weeks but with lower benefits (65 % replacement rate) I exclude job seekers with 65% replacement rates during their benefit period. Finally, I exclude job seekers with a child who turn 18 the same month as regular UI expiration may be reached. The research design is thus a “donut” RD. This is done as I am unable to determine whether the child is exactly above or below 18 in a given month.

Table 1 show different moments of observable characteristics of job seekers in the main sample where the age of the child at predicted UI exhaustion is between 16.5 and 19.5 years. i.e a bandwidth of ± 18. The average job seeker is around 47 years old and has a little more than 2 children. No more than 26 percent have a college degree and about 10 percent will at some point during the spell become registered as having some sort of disability. The average earnings in 2006 is around 155,000 SEK where 10 percent of the sample has zero earnings. The average unemployment duration is about 46 weeks but as usual the distribution of duration is highly skewed to the right leaving the median is 26 weeks. At the median, about a third of the standard UI benefits are used (115 out of 300 days) whereas the average is about 150 days.

4.3 Identifying Assumptions

The validity of the RD-design hinges upon imperfect control over the forcing variable. As there are economic incentives to extend UI, one concern may be that job seekers can control the assignment variable and sort to the right of the cut-off into treatment. This implies that job seekers manipulate the age of their child at the time of UI exhaustion. Age, as such, is checked by a third party and hence virtually impossible to manipulate, however, a job seeker could time UI benefit entry such that is coincides with their child being just below the age of 18

[18]For this reason a benefit period can span several years. Prior to 2007 the duration on UI was in practice quasi-fixed as new benefit periods where given on a discretionary basis. Prior to 2001 unemployed could even re-qualify for new round of benefits by participating in labor market programs which in practice enabled indefinite cycling within the UI-system (Sianesi, 2008; Bennmarker, Skans and Vikman, 2013).

Table 1: Summary Statistics

	Mean	Standrad deviation	Median	Min	Max
Weeks in unemployment	45.92	55.02	26	0	363
Days used of UI	148.47	120.54	115	0	420
Age	47.73	5.18	30	47	59
Annual Earnings	15.66	10.19	17.2	0	79
# of children	2.34	1.08	2	1	11
Female	0.58	0.49	1	0	1
Disabled	0.10	0.30	0	0	1
Level of education					
< Primary School	0.09	0.28		0	1
Primary School	0.18	0.39		0	1
High school	0.48	0.50		0	1
College < 2 year	0.09	0.28		0	1
College	0.17	0.37		0	1
Ph.D	0.00	0.06		0	1

Notes: Table show moments of observable job-seeker characteristics for the main estimation sample used in the analysis with 13,162 observations. Ages of job-seekers children at onset of unemployment is thus restricted to 16.5 to 19.5 years. Annual earnings in the fourth row are presented in 10,000 SEK and refers to earnings in year 2006.

at UI benefit exhaustion. If so, this would invalidate the RD-design as it implies a selection into treatment and thus non-random assignment of prolonged potential duration on UI benefit. Formally, the identifying assumption could be written as,

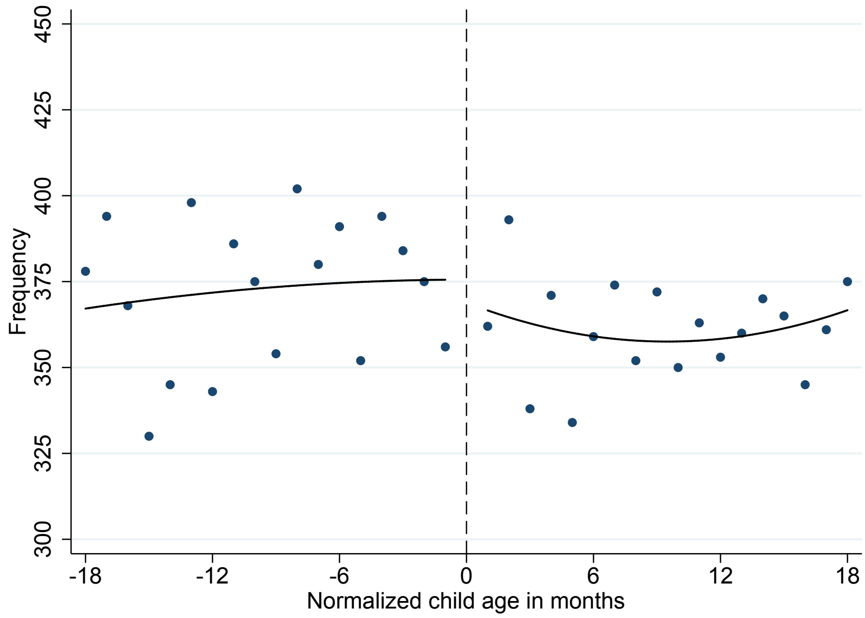
$$\lim_{\Delta \to 0^-} \mathbb{E}[\varepsilon \mid ChildAge = 18 + \Delta] = \lim_{\Delta \to 0^+} \mathbb{E}[\varepsilon \mid ChildAge = 18 + \Delta] \tag{3}$$

where ε is the error term of equation (1). Approaching the threshold, the distribution of any unobserved heterogeneity correlated with the outcome of interest is the same among those just below and above the cut-off. Although the assumption of continuity of ε can not be fully tested, its validity can be assessed by checking that the frequency of observations and that pre-determined observable characteristics varies smoothly around the threshold (Lee and Lemieux, 2010).

Figure 3 shows the frequency of observations within a 18-month bandwidth of the threshold. There is no evidence of bunching on either side of the cut-off and, in the spirit of McCrary (2008), regressing the frequency on an indicator for being below the threshold along with the control function renders insignificant estimates with a p-value of .222 and 0.74 using a first and second order polynomial, respectively. This is perhaps not surprising as the margins for timing the start of the unemployment spell such that UI exhaustion occurs just before the child's 18th birthday is virtually non-existent when having dropped voluntary quits and assigning (intention to) treatment based on the maximum 5 day UI take-out (see section 4.1).

I further test the continuity assumption by regressing an indicator for being below the

Figure 3: Density around threshold



Notes: The figure shows the frequency of observations around the threshold. The solid lines are the OLS regression fit which include a second order polynomial polynomial function interacted with the threshold estimated on a bandwidth of ±18. The jump at the threshold is estimated to -6.7 with a standard error of 20.4.

threshold on several pre determined covariates along with the control function. Column (1) to (4) in Table 2 show results from these regression varying both the bandwidth and the flexibility of the control function. I find no strong evidence of selection into treatment as I am unable to predict treatment at conventional significance levels using individual job seekers characteristics by joint significance F-test. Using the linear specification (column 1 and 2), I fail to reject the null hypothesis of all coefficients being jointly equal to zero with a p-value of 0.497 and 0.491. However, among specifications allowing for a higher polynomial degree (column 3 and 4), one F-test reject the null-hypothesis at the 5-percent level. This is most likely due to the level of education estimates being highly variable and the quadratic specification over-fitting the data. Nevertheless, all estimated coefficients are small in economic terms. E.g. column 1 in Table 2 shows a linear specification for the main bandwidth of 18-months on each side of the threshold. The likelihood of treatment decreases by only 0.001–0.02 percent per 10,000 SEK in annual earnings (in year 2006).[19]

As an additional test of the continuity assumption, I plot separately the relation between the outcomes listed in Table 2 and the forcing variable in Figure A1. Column (5) and (6) in 2 show the results of these relations by regressing individual job seekers covariates separately on the treatment indicator along with the control function. There appears to be some imbalance at the threshold job seekers just below (in the treatment group) have about a 2 percentage point lower likelihood of having a college degree, significant at the 5 percent level. As higher

[19]As I restrict the sample based on the pre-unemployment wage (reported in the IAF data) I choose to balance annual earnings in the year 2006 which is the year before the first spell int he sample. Balancing pre unemployment wages also renders an exact zero.

Table 2: Balancing of Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0012	0.0020	0.0022	-0.0003	-0.0001	0.0149
	(0.0041)	(0.0037)	(0.0024)	(0.0024)	(0.0138)	(0.0196)
Age	0.0001	0.0003	0.0004	0.0001	0.0414	0.4771*
	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.1773)	(0.2761)
Annual Earnings	-0.0002	-0.0002	-0.0001	-0.0001	-0.2764	-0.5756
	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.3407)	(0.6638)
Level of education						
Primary School	0.0020	0.0021	0.0009	-0.0006	-0.0056	0.0199
	(0.0090)	(0.0079)	(0.0056)	(0.0053)	(0.0112)	(0.0143)
High school	0.0024	0.0067	-0.0036	-0.0039	-0.0140	-0.0502*
	(0.0084)	(0.0075)	(0.0059)	(0.0050)	(0.0162)	(0.0268)
Some College	0.0063	0.0045	-0.0019	-0.0007	-0.0033	0.0011
	(0.0079)	(0.0080)	(0.0041)	(0.0039)	(0.0099)	(0.0174)
College	0.0113	0.0119	0.0011	0.0010	-0.0213**	0.0241*
	(0.0079)	(0.0080)	(0.0052)	(0.0048)	(0.0089)	(0.0143)
Ph.D	0.0020	-0.0037	-0.0077	-0.0066	-0.0001	-0.0008
	(0.0391)	(0.0360)	(0.0229)	(0.0214)	(0.0018)	(0.0026)
Polynomial degree						
1st order	✓	✓			✓	
2nd order			✓	✓		✓
Bandwidth ±	18	24	18	24	18	18
p-value	.497	.491	.0297	.28	.	.
R^2	0.769	0.763	0.911	0.905	.	.
# clusters	36	48	36	48	36	36
N	13,162	17,355	13,162	17,355	13,162	13,162

Notes: The table show balance tests of baseline covariates at the threshold. Columns (1)-(4) show results from regressing the a dummy for being above the threshold on a set of baseline covariates and a polynomial control function interacted with the threshold. The excluded category for highest attained education is less than primary school. The bottom of the table displays the F-statistic and the corresponding p-value from testing the hypothesis that all coefficients being jointly equal to zero. Columns (5)-(6) report results from balancing tests where each covariate have been regressed separately on the instrument and a polynomial control function in relative ranking interacted with the threshold. Standard errors clusteredon the forcing variable and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

education is negatively correlated with both unemployment duration and the use of UI, any bias stemming from this potential imbalance should render an underestimation of the treatment effect. Nevertheless, when estimating treatment effects in the next section I control for the level of education to handle this potential imbalance. The results are not sensitive to including these controls thus suggesting that the observed imbalance is of minor importance.

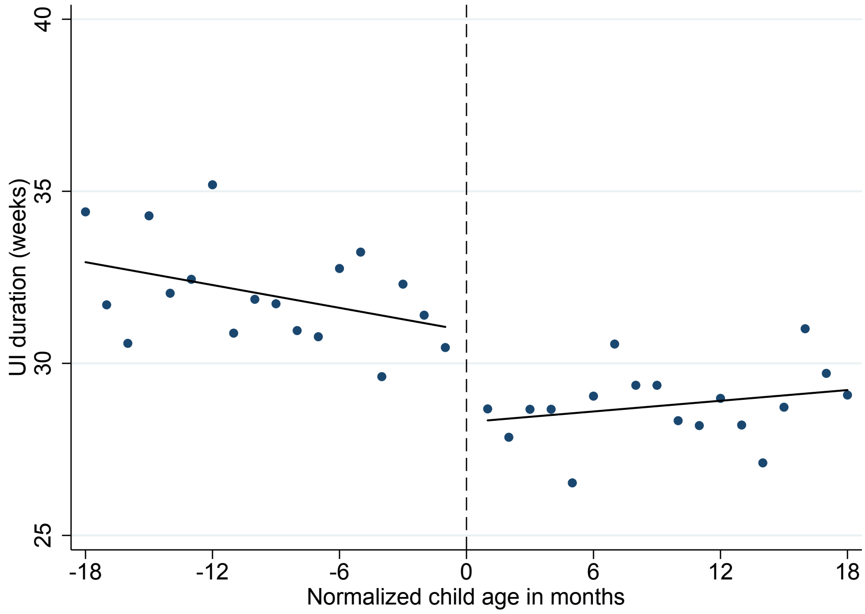
The overall take-away from this exercise is that job seekers have imprecise control of the forcing variable and failure to reject the continuity assumption. This leads me to conclude that treatment can be considered as good as randomly assigned among individuals around the threshold. If, on the other hand, a bias would exist, due to education being slightly unbalanced, it is likely to be minor. I control for all covariates listed in Table 2 in my regressions to avoid any potential bias and as can be seen in Table 3, this barely changes the estimates, thus confirming the lack of any substantial bias.

5 Results

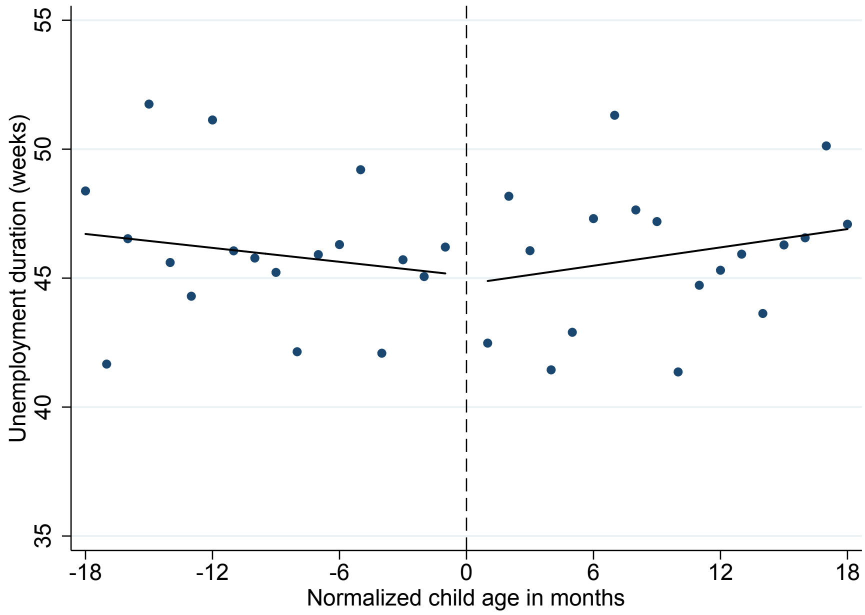
First, I present what I refer to as reduced form estimates. As the benefit extension is granted based on the age of a job seeker's child at the time of UI exhaustion, which by a forward looking individual could be foreseen, these estimates reflect an ITT-effect. That is, how having the possibility of utilizing the 30 week UI extension affect the duration on UI and in unemployment. Hence, I do not condition on job seekers *de facto* utilizing the extension. As these reduced form estimates could be influenced by dynamic selection, I follow job seekers dynamic responses to the potential extension of UI duration by analyzing survival an hazard rates to employment and its timing with respect to UI exhaustion.

Figure 4: Use of UI benefits and duration in unemployment by normalized child age

(a) UI benefit use



(b) Unemployment duration



Note: The figure show a) average utilization of UI and b) unemployment duration, both in weeks and as a function of a job seekers child age in months at regular UI exhaustion (week 60), normalized to zero for age 18 years. The regressions include a linear polynomial function interacted with the threshold. Bins are discrete and represent 1 month.

Table 3: Weeks of UI utilization and unemployment duration

	UI utilization			Unemployment duration		
	(1)	(2)	(3)	(4)	(5)	(6)
Below 18	2.512***	2.484***	2.019**	0.323	0.397	-0.939
	(0.702)	(0.714)	(0.935)	(1.755)	(1.854)	(2.808)
Control mean	28.286***	15.687***	15.978***	44.771***	20.227***	21.228***
	(0.410)	(2.818)	(2.743)	(1.403)	(5.979)	(6.235)
Polynomial degree						
1st order	✓	✓	✓	✓	✓	✓
2nd order			✓			✓
Controls		✓	✓		✓	✓
Bandwidth	18	18	18	18	18	18
# clusters	36	36	36	36	36	36
N	13,202	13,162	13,162	13,202	13,162	13,162

Notes: The table show estimates on number of weeks of utilized UI and number of weeks in unemployment. All regressions include n:th order polynomials the running variable and its interaction with the treatment indicator to allow for different slopes on each side of the threshold. When indicated regressions control for: gender, age, annual earnings in year 2006, and 5 dummies for level of education. Standard errors clustered on the forcing variable and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

5.1 Reduced Form Response to Benefit Extension

Figure 4 (A) plots UI the average utilization of UI by the forcing variable. There exist a clear discontinuous downward jump at the threshold indicating that job seekers who are eligible to the 30 week extension indeed use more weeks of UI. Table 3 show estimates of the effect of the 30 week UI extension on actual UI benefit duration and unemployment duration. As can be seen in column (1), the discontinuous in Figure 4 (A) is estimated to 2.5 weeks (standard error 0.702) which corresponds approximately to a 9 percent increase on average. This suggests that a 10 week increase of potential UI renders roughly one additional week in actual take up which corresponds to an elasticity of about 0.2 which is somewhat smaller than the UI elasticities found in Germany by Schmieder, Von wachter and Bender (2012). It is reassuring that adding covariates or allowing for higher order polynomials in column (2) and (3) hardly changes the estimates, thus bolstering confidence in the identifying assumption that treatment is orthogonal to other characteristics correlated with the outcome. Moreover, results remain stable when varying the bandwidth as can be seen in Figure A4.

While the benefit extension have a clear effect on actual utilization of UI, it need not necessarily affect unemployment duration if job seekers who utilize the extension would have otherwise, in absence of the extension, would continued to be unemployed but without additional benefits. Thus the effect on UI duration can be seen as a "first stage" to the effect on unemployment duration. Figure 4 (B) plots the average time registered at the PES as unemployed by the forcing variable. Here there is no evidence of the extension having an effect on average unemployment duration as it appears continuous at the threshold. The estimated jump at the threshold, displayed in columns (4) to (6) of Table 3, lies around 0.3-0.4 weeks and allowing for a more flexible functional form the estimated effect even turns negative. Allowing for a larger bandwidth increases the estimated effect on unemployment duration to around 2 to 3 weeks, although never statistically significantly different from zero at conventional levels (see Figure A4).

Thus, it appears as if the 30 extension has not caused job seekers to stay in unemployment longer but rather to the same extent but with somewhat higher benefits. Nevertheless, it is important to recall that this analysis is unable to take into account whether differences in potential duration has generated differences in e.g. time to employment. It is possible that treatment and control group have the same average length in unemployment but are leaving unemployment to different states (e.g. regular employment, subsidized employment, non-employment). To address this, in the following section I make use of the richness of the PES data which include cause of exit (see section 4.2 for description) which allows me to track job seekers throughout the unemployment spell and see why they leave unemployment.[20]

5.2 Dynamic Response

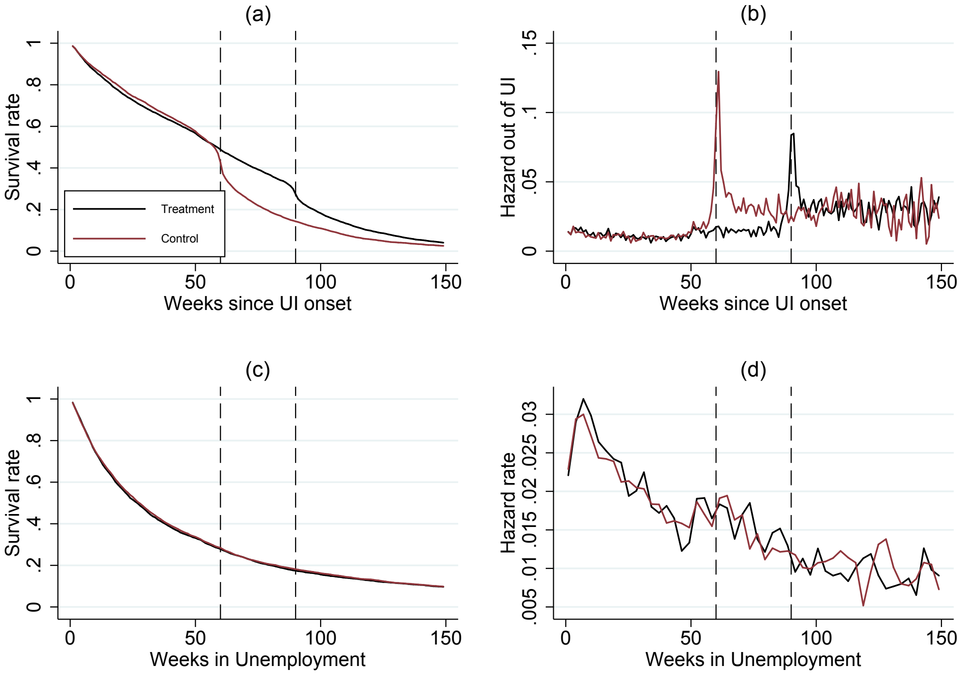
5.2.1 Graphical analysis

The top panel in Figure 5 plots (a) the probability of collecting UI and (b) the probability of leaving the UI scheme, by calender weeks since the start of UI benefits. This is done for workers within the 18 month bandwidth such that the lines corresponds survival and hazard functions for the group of job seekers below (black) and above (red) the threshold, not controlling for the running variable. After 60 weeks, about 50 percent are still collecting UI benefits at some rate. At that time, when UI exhaustion occurs for job seekers in the control group having collected UI 5 days a week, there is a spike in the hazard rate out of UI where job seekers above the threshold are about 12 percent more likely to go off UI. Similarly, there is an equivalent spike at week 90 for job seekers below the threshold who are eligible to the 30 week UI extension. These spikes are to some extent mechanical as UI is exhausted and job seekers are able transfer to the JDG where they would receive activity support from the social insurance agency. Nevertheless, it shows that the treatment and control groups are well defined as there exists a "first stage" in the form of leaving UI.

Again, leaving UI need not imply that the job seeker leaves unemployment as he may transfer into e.g. the JDG and receive activity support. This becomes evident when plotting the weekly survival and hazard out of unemployment in Figure 5 (d) where the spike at week 60 and 90 virtually non-existent. There is, however, somewhat of an increase or flattening out of the hazard rate when approaching week 60 of unemployment. But equally so for the control and treatment group. Importantly, Figure 5 (c) show that the survival functions for the treatment

[20] Unfortunately, the data does not allow me to test attrition to non-employment as it has been shown that about 45 percent of this attrition is due to finding employment while not reporting this to the PES (Bring and Carling, 2000).

Figure 5: Survival and hazard rates out of UI and unemployment



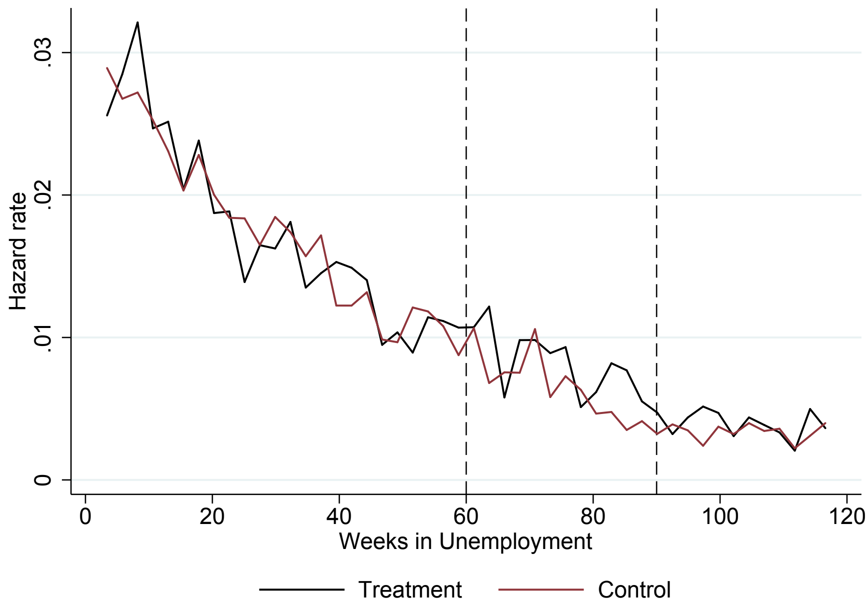
Notes: The figure shows in the upper panel a) the probability of survival on UI and b) the hazard rat out of UI as a function of elapsed weeks on UI. In the lower panel c) shows the probability of survival in registered unemployment and d) the hazard out of unemployment as a function of weeks in registered unemployment.

and the control group are literally on top of each other up until week 45 of unemployment.[21] This indicates that dynamic selection out of unemployment is less likely to have occurred as job seekers seem not to act on the possible extension and thus that the absence of effects are unlikely to be driven by compositional changes in the groups. This is also confirmed in Table 4 showing non-significant differences in the hazard rate prior to week 50.

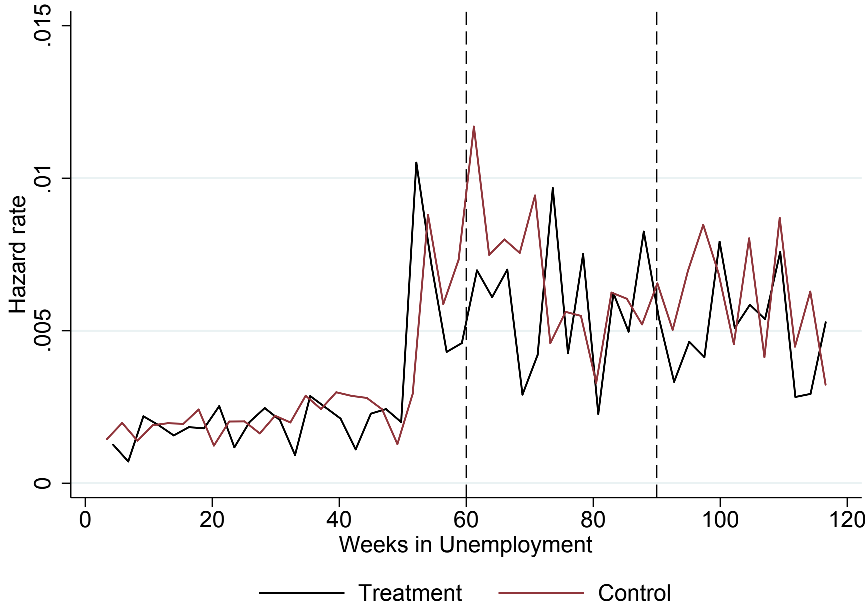
[21]Note that the likelihood of being on UI is greater than being unemployed. Whereas this can appear counter intuitive as one needs to be unemployed to collect UI benefits, leaving unemployment is defined as also having found part-time employment so job seekers keep collecting UI benefits for days they do not work.

Figure 6: Hazard rate by unemployment duration and reason for leaving unemployment

(a) Regular employment



(b) Subsidized employment



Note: The Figure plots a) hazard rate to regular employment and b) hazard rate to subsidized employment by weeks in unemployment using a rectangular kernel with a bandwidth of 1. This is plotted separately for job seekers in the treatment group (black) and control group (red). The vertical dashed lines indicate benefit exhaustion for workers utilizing UI 5 days a week in the control group (week 60) and in the treatment group (week 90).

The slight increase in the hazard rate at regular benefit exhaustion (week 60) seems to be in line with previous studies such as Katz and Meyer (1990*a*,*b*); Carling, Holmlund and Vejsiu (2001); Bennmarker, Carling and Holmlund (2007), although much smaller in size. While there being no visible difference between treatment and control in hazard out of unemployment, there may still exist differences in the reason for exiting. In order to determine whether this small increase in the hazard is due to shrinking behavior or if job seekers become discouraged and leave the labor force I make use of the detailed PES data which provides the reason for exiting unemployment. Although, leaving unemployment for other reasons than regular or subsidized employment is very rare and constitutes about 6 percent of the sample with no significant differences across treatment and control group.[22] Figure 6 (a) plots the hazard rate to regular employment by unemployment duration.[23] There is no visible difference in the hazard to employment at week 60 when benefits are exhausted for job seekers in the control group. If anything, it appears as if job seekers entitled to the extended benefit have on average a higher likelihood of leaving for employment during the weeks 60 to 90 of unemployment. On the other hand, Figure 6 (b) show that all job seekers have a higher hazard rate to subsidized employment at the time of regular benefit exhaustion. The spike in the hazard starts at week 53 where job seekers become eligible to so-called new start jobs which is a subsidized employment where employers are exempted from paying the general payroll tax of 31.42 percent. While the spike in the hazard rate is present for both groups, it looks like job seekers in the control group have on average a higher likelihood of escaping to subsidized employment.

5.2.2 Model estimates

Using the model specified in equation (2), Table 4 quantifies the difference in hazard rates between control and treatment by unemployment duration. To make the comparison lucid, I have cut the weekly intervals into a pre-exhaustion period and then in blocks of 10 weeks. Column (1) to (4) show the results on the hazard to regular employment whereas column (5) to (8) show exit to subsidized employment. Other reasons for leaving then the ones indicated in the header of the columns are censored. Column (1) of Table 4 show the likelihood of leaving unemployment for regular employment during the first 50 weeks for job seekers in available for regular employment, that is conditional on not having left unemployment for e.g. subsidized employment which is a censored event in this case. The vast majority of job seekers find a job before UI expire and the probability of having left unemployment before week 50, for any reason, is 69.3 percent. In comparison exit to subsidized employment is only 13.5 percent in the pre exhaustion period as seen in column (5).[24] Column (2) in Table 4 show the average difference in hazard rates to regular employment between treatment and control group whereas

[22] About 12 percent of the sample are right hand censored.

[23] Regular employment is defined as finding a non-government subsidized job wither full-time, part-time or temporary employment.

[24] The reason the number of observations in column (1) and (5) in the first row of Table 4 adds up to more than the 13,202 used in the main estimation (see Table 1) is that ongoing spells (exceeding 50 weeks) are used in both samples.

column (3) estimates this difference at the threshold and column (4) adds covariates. In the pre exhaustion period (week 0-50), there are no significant differences in the hazard to either regular nor subsidized employment prior to the extension period. This suggests that, in contrast to e.g. Card, Chetty and Weber (2007*b*), that job seekers where unaware of or at least have not acted on the possible UI extension. The absence of such anticipatory behavior may also be due to the rather high replacement rate in the Job and Development Guarantee (JDG). As the difference in replacement rates transitioning from UI to activity support is at maximum a 5 percentage point drop, the optimization cost may exceed the discounted value of the losses, thereby rendering job seekers passive (c.f. Chetty, 2012). This can be be compared to e.g. Germany where the nominal replacement rate is 53 percent while the effective unemployment assistance is about 35 percent and 10 percent for men and women, respectively, due to a reduction by spousal earnings (Schmieder, Von wachter and Bender, 2012).[25]

The absence of anticipatory behavior enables comparisons of control and treatment groups, conditional on unemployment duration exceeding 50 weeks as dynamic selection is likely a minor issue. Additionally, I test for dynamic selection by balancing of covariates at the threshold among job seekers unemployed at week 60. These results are shown in in Table A1 and display no significant differences of job seekers characteristics at the threshold and therefore gives creditably to the interpretation that the estimated effects post week 60 of unemployment are indeed a casual effect of the extended UI benefits and not an artifact of dynamic selection.

[25] In Austria where Nekoei and Weber (2017) and Card, Chetty and Weber (2007*a*,*b*) study the effect of benefit increases on non-employment duration, unemployed job seekers who exhaust their benefits can apply for unemployment assistance, which is 92 percent of UI. However, as unemployment assistance is means-tested on household income, the effective replacement rate is only around 39 percent of UI.

Table 4: Hazard rate to regular and subsidized employment

	Regular employment				Subsidized employment			
Unemployment week	Mean hazard rate [# obs.] (1)	Difference in hazard rate (2)	Difference in hazard rate (at threshold) (3)	Difference in hazard rate (at threshold)* (4)	Mean hazard rate [# obs.] (5)	Difference in hazard rate (6)	Difference in hazard rate (at threshold) (7)	Difference in hazard rate (at threshold)* (8)
0-50	0.653	0.005	0.011	0.012	0.135	-0.017	0.002	0.002
	[11,515]	(0.008)	(0.015)	(0.014)	[4,616]	(0.011)	(0.024)	(0.024)
51-60	0.082	0.004	-0.011	-0.012	0.050	0.000	-0.002	-0.005
	[4,490]	(0.007)	(0.012)	(0.012)	[4,340]	(0.007)	(0.016)	(0.016)
61-69	0.071	0.008	0.024	0.023	0.058	-0.021***	-0.018	-0.016
	[3,707]	(0.009)	(0.019)	(0.020)	[3,652]	(0.007)	(0.016)	(0.015)
71-80	0.060	0.015*	-0.005	-0.006	0.046	0.002	-0.015	-0.016
	[3,048]	(0.008)	(0.015)	(0.015)	[3,003]	(0.007)	(0.016)	(0.016)
81-90	0.046	0.022***	0.013	0.013	0.048	-0.005	-0.012	-0.013
	[2,491]	(0.007)	(0.014)	(0.014)	[2,498]	(0.009)	(0.020)	(0.020)
91-100	0.032	0.013*	0.011	0.012	0.051	-0.013	-0.023*	-0.025*
	[2,064]	(0.007)	(0.014)	(0.015)	[2,105]	(0.008)	(0.013)	(0.013)
101-110	0.034	-0.003	0.009	0.007	0.048	-0.002	0.015	0.011
	[1,769]	(0.009)	(0.021)	(0.022)	[1,796]	(0.011)	(0.027)	(0.027)
111-120	0.030	0.014*	0.005	0.005	0.038	-0.003	-0.003	-0.001
	[1,517]	(0.008)	(0.016)	(0.017)	[1,530]	(0.010)	(0.013)	(0.014)
Control function			✓	✓			✓	✓
Covarites				✓				✓

Notes: Table show estimates in column (1) and (5) average hazard rate to regular and subsidized employment, respectivly, with the number of observations in each sample in hard brackets. Column (2) and (6) estiamtes the mean difference in hazard to regular and subsidized employment, respecitvly, between control and treatment group. Column (3) and (7) additionally controls linearly for the forcing variable and its interaction with treatment and column (4) and (8) adds the pre determined covarites gender, age, annual earnings in year 2006, and 5 dummies for level of education. Standard errors clusteredon the forcing variable and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

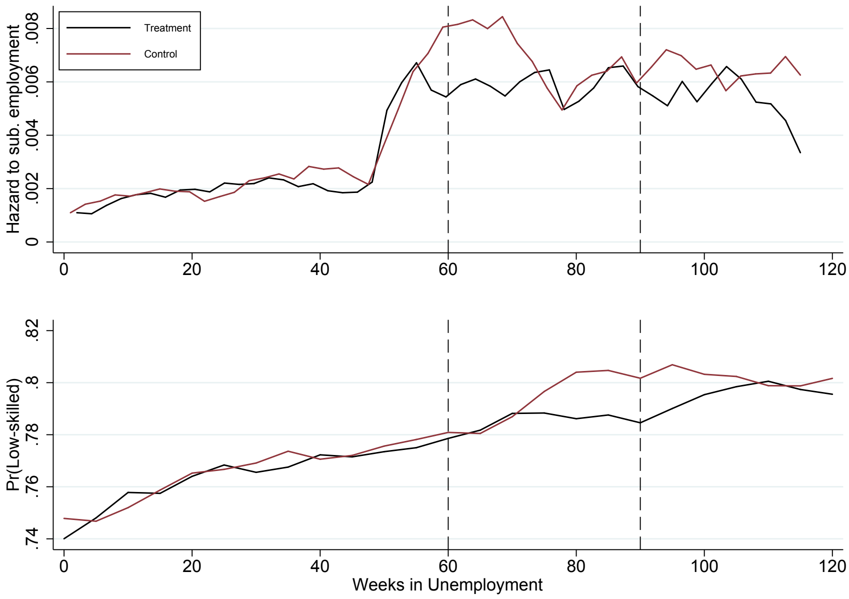
Column (2) of Table 4 show treatment effects during the (possible) benefit extension period. During week 60 to 90 of unemployment, job seekers eligible for the extension seem to in fact have between 1.5 to 2.2 percentage points higher probability of leaving unemployment for regular employment compared to job seekers eligible for the regular 60 week UI benefits. However, this difference turns insignificant in column (3) when estimated at the threshold by including the control function (column 3) and adding controls (column 4). Columns (6) to (8) show estimates of the difference in the probability of leaving unemployment for subsidized employment at different durations in the unemployment spell corresponding to Figure 6 (b). There is some suggestive evidence of job seekers in the control group are about 2 percentage points more likely to leave for subsidized employment just as the regular benefit period ends (week 61 to 69). This difference also turns insignificant when controlling for a first order polynomial in the control function and estimating the effect just at the threshold where job seekers should be as good as randomly assigned to the benefit extension. Nevertheless, while this increases the standard error the point estimate it remains about the same size.

An interesting, although weak, pattern emerges from Table 4. While job seekers eligible to the 30 week extension seem if anything somewhat less likely to exit to subsidized employment during week 61 to 100, they are more likely to exit regular employment. The upper panel of Figure 7 reproduces a more smoothed version of Figure 6 (b) and plots the hazard rate to subsidized employment for treatment and control group separately by unemployment duration. The lower panel of Figure 7 also plots the share of low-skilled job seekers, defined as the share of people with no more than high school education. job seekers with access to extended UI (treatment group) are about 2 percentage points less likely to be low-skilled between week 75-95. This could also explain the significant mean differences in the hazard to regular employment seen in column (2) to (4) in Table 4. Thus, I attribute lions share of the hazard difference to the fact that the control group is selected in such a way that it contains less able job seekers post exhaustion. Interestingly, there is no spike in the hazard to subsidized employment for the treated job seekers at extended benefit exhaustion (week 90). Rather, the treatment group seems less likely to leave for subsidized employment. This may seem surprising as the share of low-skilled individuals of the control and treatment group converges around week 110 of unemployment. However, I take this as evidence that the relatively high-skilled in the treatment group are the individuals that find regular employment. This suggests that the high-skilled individuals who entered into subsidized jobs would most likely have gotten regular employment had they remained on UI.

6 Conclusions

This paper uses a natural experiment in Sweden where job seekers with children under the age of 18 get 90 instead of 60 weeks of UI benefits, to show that although increasing potential UI duration had a positive effect on actual UI duration (estimated at 2.5 weeks, implying an elasticity of 0.2), it had no significant impact on either unemployment duration nor the

Figure 7: Hazard rate to subsidized employment and share of low-skilled by weeks in unemployment



Notes: The upper panel of the Figure plots the hazard to subsidized employment using a rectangular kernel with a bandwidth of 4. The lower panel plots the share of low-skilled job seekers by five week intervals of unemployment duration. Low-skilled is defined as having no more than high school education as a function of weeks in registered unemployment. This is done separately for job seekers in the treatment group (black) and control group (red). The vertical dashed lines indicate benefit exhaustion for workers utilizing UI 5 days a week in the control group (week 60) and in the treatment group (week 90).

hazard to employment. This stands in contrast to the previous literature which has found positive effects on unemployment duration rather consistently (see e.g. Card, Chetty and Weber, 2007*a*,*b*; Lalive, Van Ours and Zweimüller, 2006; Lalive, 2007, 2008; Landais, 2015; Nekoei and Weber, 2017; Schmieder, Von wachter and Bender, 2012). I attribute this disparity of results to the rather generous replacement rates offered in programs available to job seekers after UI exhaustion. As the disincentive effects of UI depend on the change in replacement rates, which in Sweden is 5 percentage points, this creates minor financial incentives to adjust search behavior. This highlights the importance of taking alternative benefits schemes into consideration, and their potential effects on the incentives of job search, both when designing a UI-system and when estimating its effects on e.g. unemployment duration. While the effects of UI on unemployment duration and the hazard to employment are well researched, I encourage future researchers to look into how different levels of post UI exhaustion benefits (such as unemployment assistance) affect the duration on UI and in unemployment.

The previous literature has found that the probability of leaving unemployment increases sharply at benefit exhaustion (see e.g. Katz and Meyer, 1990*a*,*b*; van Ours and Vodopivec, 2006; Carling et al., 1996) which has mainly been attributed to strategic behavior and shirking among job seekers, thus timing job-finding to benefit exhaustion. However, Card, Chetty and Weber (2007*b*) opposes this view and shows, using Austrian data, that fewer than one percent of unemployment spells are manipulated in such a way. They point out that "[s]tudies that focus on the duration of benefit receipt often find elevated hazards prior to exhaustion. In contrast, most studies that have focused on time to re-employment and used administrative data to measure job starts have found relatively small changes in exit rates at or near benefit exhaustion." (p. 15). The evidence presented in this paper speaks in favor of the interpretation in Card, Chetty and Weber (2007*b*). I find no evidence of job seekers manipulating or postponing employment such that it should coincide with benefit exhaustion. Rather, while there being a sharp increase in the hazard rate out of UI the absence of a corresponding hazard to regular full-time or part-time employment is strikingly absent. Moreover, job seekers do not appear to lower their search intensity during the unemployment spell in anticipation of future UI benefits.

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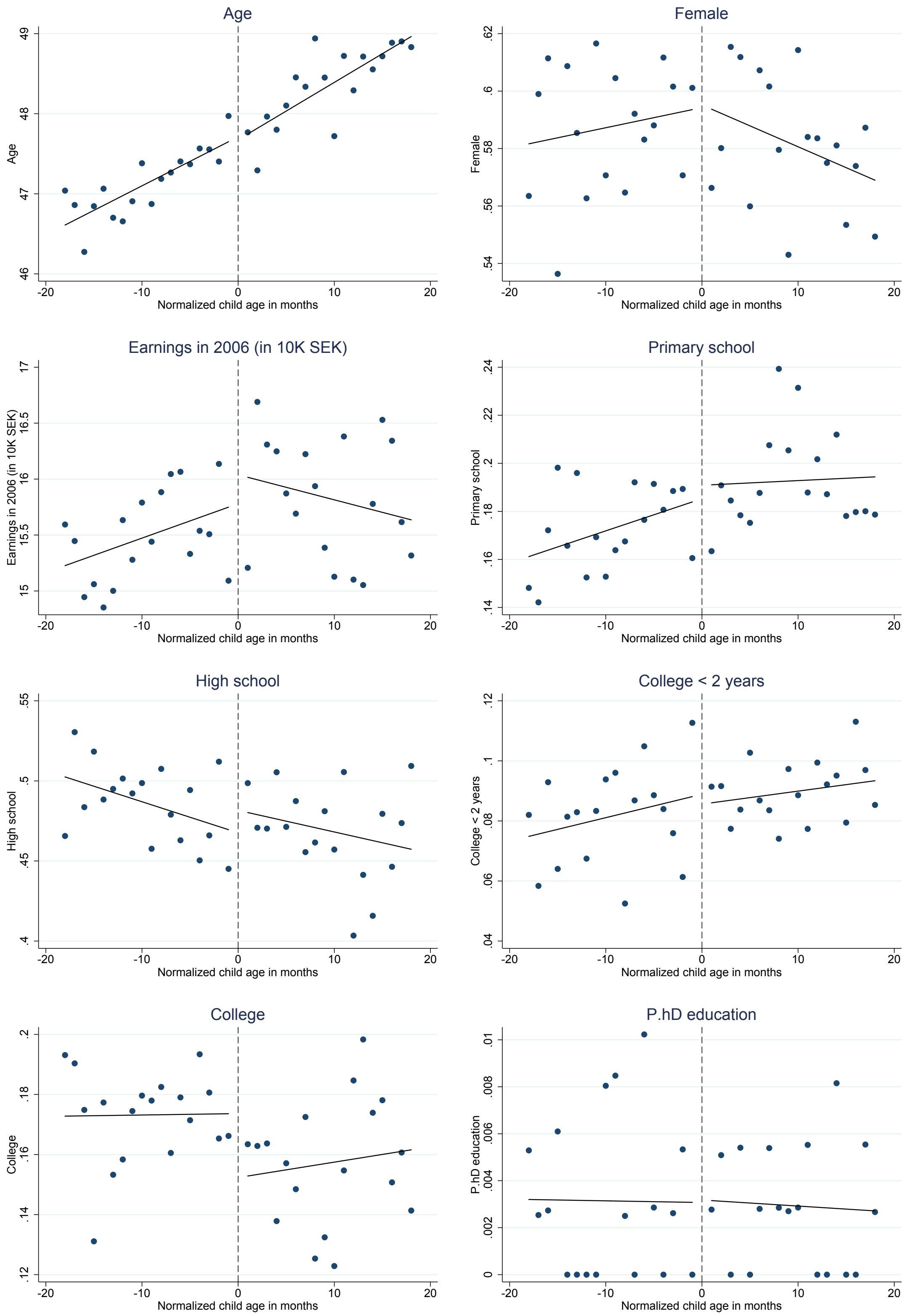
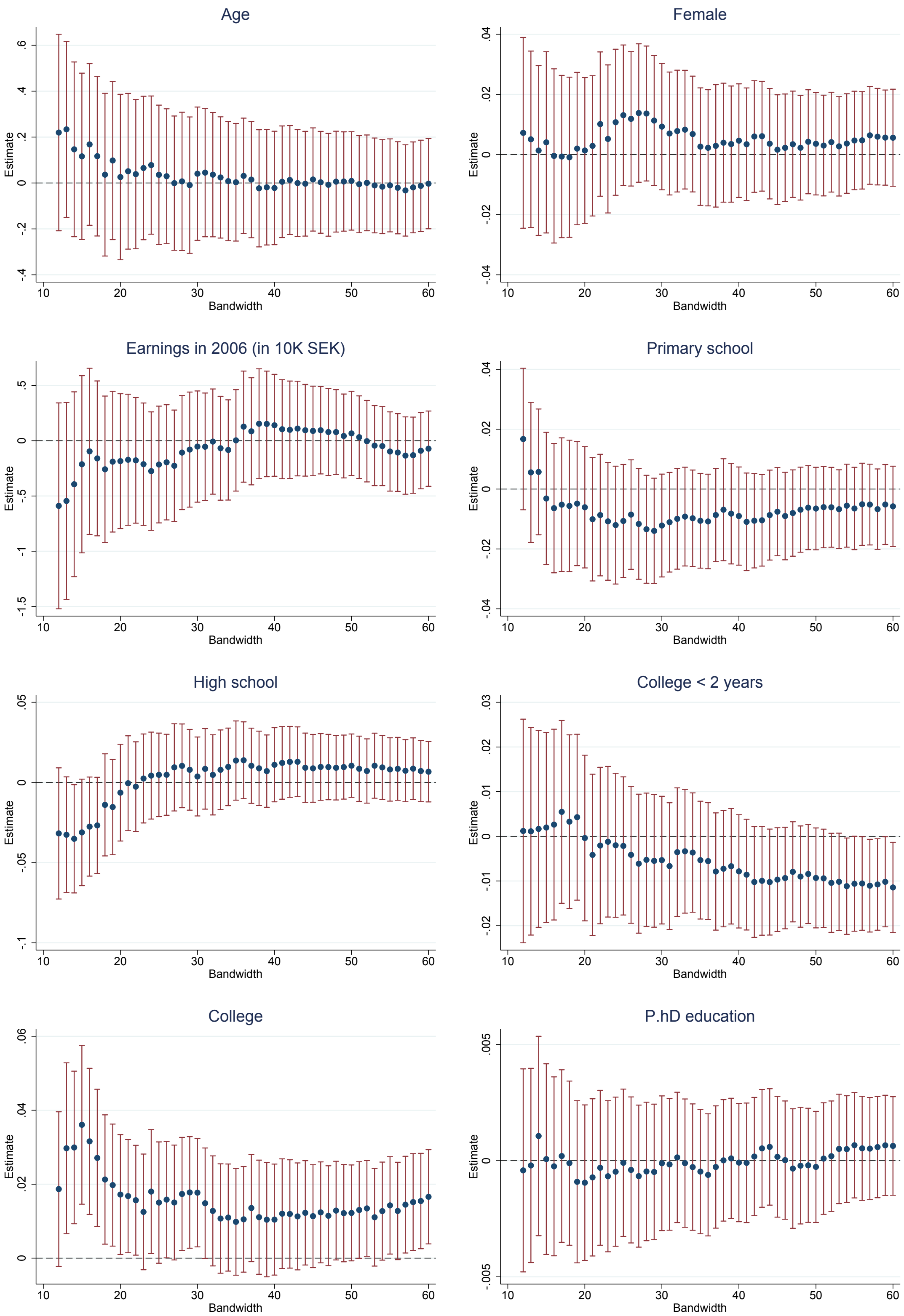
Appendix

Figure A1: Balancing of covariates



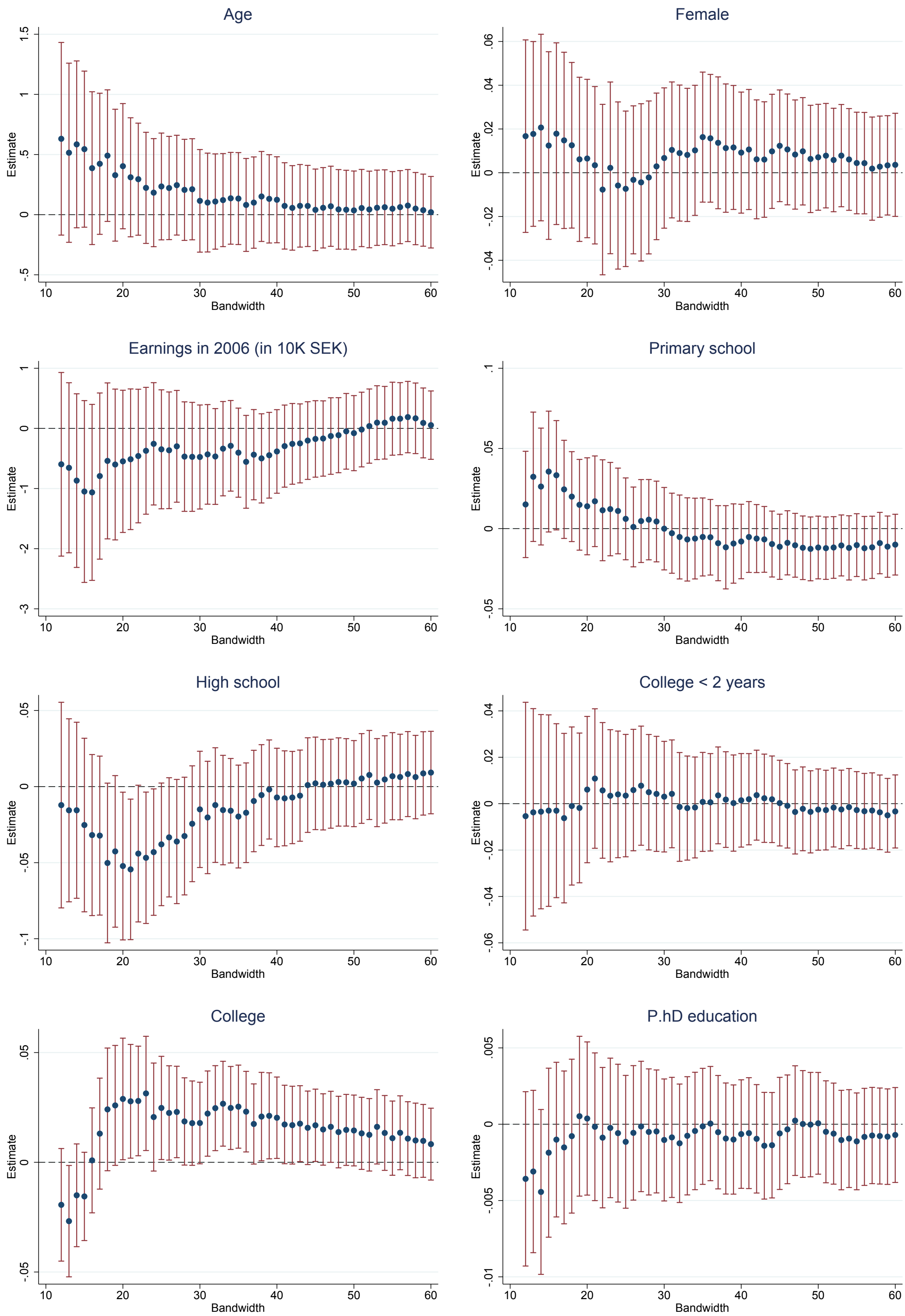
Notes: The Figure plots average job seeker characteristics by the age of the job seekers child in months at approximated benefit exhaustion. Age is normalized to 0 zero at the age of 18 and bins are discrete. Each graph is fitted with a first order polynomial on each side of the threshold and the estimated jump at the threshold can be found in Table 2 column (5), separately for each covariate.

Figure A2: Balancing of covariates by bandwidth (1^{st}-order polynomial)



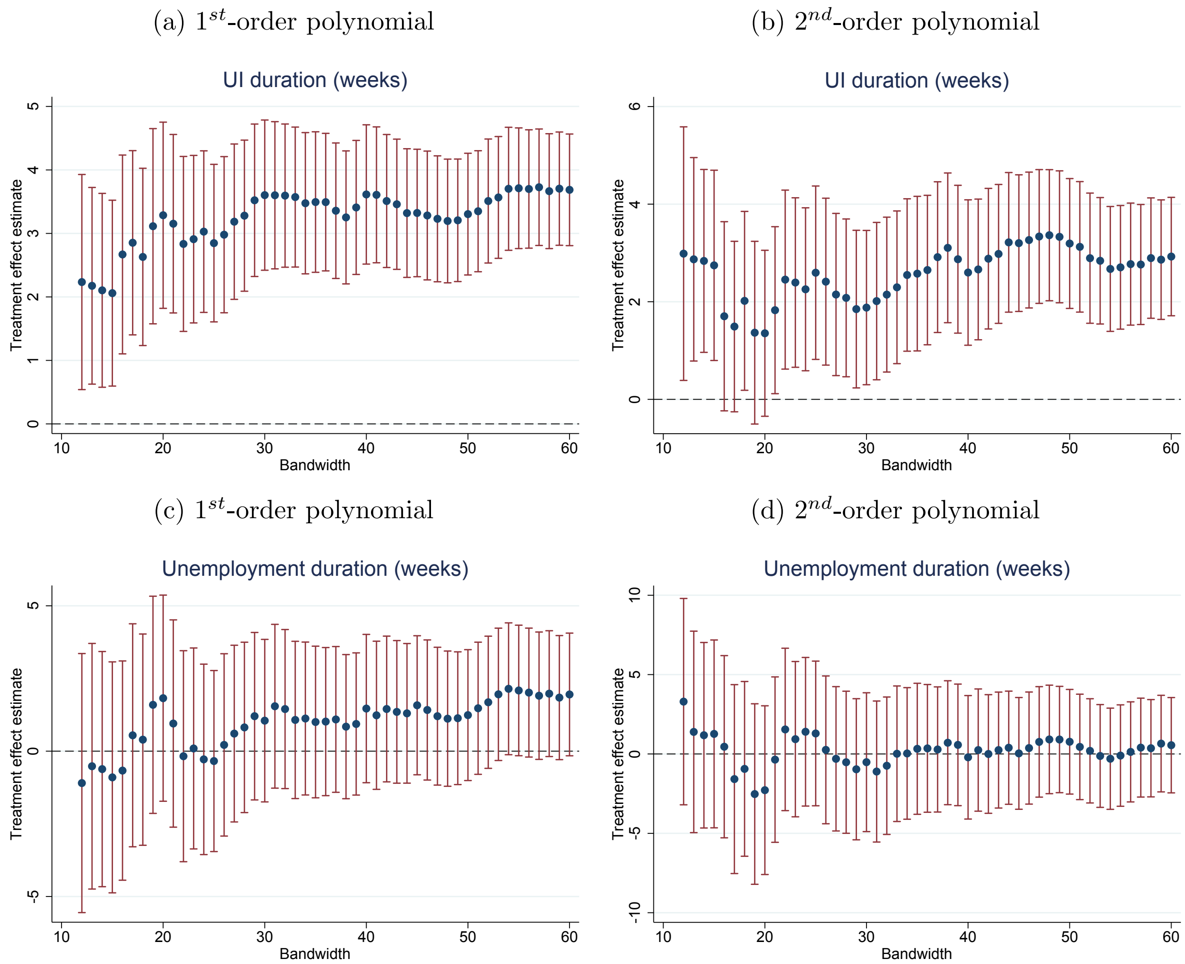
Notes: The Figure show results from balancing of job seeker characteristics at the threshold for different bandwidths. Estimates are produced by regressing the specified pre determined covariate on an indicator for being below the threshold and a first order polynomial function interacted with the threshold. Standard errors clustered on the forcing variable and shown in parentheses and the red lines show 95 percent confidence intervals.

Figure A3: Balancing of covariates by bandwidth (2^{nd}-order polynomial)



Notes: The Figure show results from balancing of job seeker characteristics at the threshold for different bandwidths. Estimates are produced by regressing the specified pre determined covariate on an indicator for being below the threshold and a second order polynomial function interacted with the threshold. Standard errors clustered on the forcing variable and shown in parentheses and the red lines show 95 percent confidence intervals.

Figure A4: Estimated treatment effects by bandwidth



Notes: The Figure show estimated treatment effects along with 95 percent confidence intervals, estimated by equation (1), as a function of bandwidth around the threshold. This is done for duration on UI in weeks and weeks in unemployment as well for the natural log of both variables. The regressions include a linear polynomial function interacted with the threshold and controls for gender, age, annual earnings in 2006 and six dummies for level of education. Standard errors in parentheses which are clustered on the forcing variable.

Table A1: Balancing of covariates on job-seekers unemployed at week 60

	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0102	-0.0062	-0.0025	-0.0035	-0.0041	-0.0312
	(0.0087)	(0.0081)	(0.0047)	(0.0044)	(0.0413)	(0.0631)
Age	-0.0002	-0.0006	0.0005	0.0004	0.0455	0.7356
	(0.0011)	(0.0009)	(0.0007)	(0.0006)	(0.5335)	(0.8421)
Annual Earnings	-0.0002	0.0000	-0.0003	-0.0002	-0.1726	-0.9851
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.4663)	(0.7023)
Level of education						
Primary School	0.0192	0.0127	0.0035	0.0083	0.0352	0.0001
	(0.0143)	(0.0133)	(0.0072)	(0.0067)	(0.0224)	(0.0341)
High school	0.0046	0.0073	0.0019	0.0004	-0.0463	-0.0457
	(0.0122)	(0.0123)	(0.0056)	(0.0058)	(0.0344)	(0.0461)
College $<$ 2 year	0.0130	0.0106	0.0109	0.0054	0.0023	0.0224
	(0.0232)	(0.0215)	(0.0133)	(0.0119)	(0.0206)	(0.0367)
College	0.0222	0.0167	0.0084	0.0120	0.0289	0.3489
	(0.0174)	(0.0160)	(0.0083)	(0.0080)	(0.0269)	(0.0396)
Ph.D	0.0405	0.0292	0.0376	0.0411	0.0022	0.0066
	(0.0600)	(0.0632)	(0.0344)	(0.0371)	(0.0042)	(0.0067)
Polynomial order						
1st order	✓	✓			✓	
2nd order			✓	✓		✓
Bandwidth $\pm$						
p-value	.349	.957	.233	.071	.	.
R^2	0.773	0.767	0.913	0.908	.	.
# clusters	36	48	36	48	36	36
N	3,281	4,319	3,281	4,319	3,281	3,281

Notes: The table show balance tests of baseline covariates at the threshold for job seekers unemployed after 60 weeks. Columns (1)-(4) show results from regressing the a dummy for being above the threshold on a set of baseline covariates and a polynomial control function interacted with the threshold. The excluded category for highest attained education is less than primary school. The bottom of the table displays the F-statistic and the corresponding p-value from testing the hypothesis that all coefficients being jointly equal to zero. Columns (5)-(6) report results from balancing tests where each covariate have been regressed separately on the instrument and a polynomial control function in relative ranking interacted with the threshold. Standard errors clusteredon the forcing variable and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.