Does an Increase in Unemployment Income Lead to Longer Unemployment Spells? Evidence Using Danish Unemployment Assistance Data

Ott-Siim Toomet

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Does an Increase in Unemployment Income Lead to Longer Unemployment Spells? Evidence Using Danish Unemployment Assistance Data[*]

Ott-Siim Toomet[†]

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Abstract

Danish unemployment assistance depends on age; it increases by 70% when unemployed individuals turn 25. This feature is used to identify the impact of income on the unemployment-to-employment hazard rate. A mixed proportional hazard framework based on a 10% representative Danish registry data set is used. The results indicate that the income effect for females is negative and significant, corresponding to an income elasticity of -0.4. The effect for males is positive but insignificant.

Contents

1. Introduction

Economic theory predicts that a high level of unemployment compensation leads to longer unemployment spells. This makes unemployed individuals more choosy and less willing to accept low-wage jobs. Moreover, if search intensity cannot be monitored (as is usually the case), the motivation to search for a job will also be lower[1].

There is considerable literature investigating the impact of unemployment benefit levels and entitlement on search behaviour and the hazard rate for leaving unemployment. The results of experimental studies mostly confirm these search-theoretical implications, although there are some indications of risk-seeking behaviour and loss-aversion (Boone et al., 2004). The empirical results reflect the fact that the real world is much more heterogeneous than a laboratory set-up can possibly allow for. The evidence from the US indicates that the effect of benefits on the out-of-unemployment hazard rate is indeed negative and significant (Card and Levine, 2000; Lee, 2000; Hotz et al., 2002; Jurajda and Tannery, 2003), and the elasticity of the hazard rate with respect to unemployment benefits is found to lie between -0.3 (Lee, 2000) and -0.9 (Meyer, 1990). There is some evidence that the effect is heterogeneous (Grogger, 2002) and diminishing in duration (Addison and Portugal, 2004).

In Europe, the results are even more mixed. A number of studies have found no significant effect at all (Stancanelli, 1999; Puhani, 2000; Bratberg and Vaage, 2000), while according to others the effect is negative and significant (Narendranathan and Stewart, 1993; Micklewright and Nagy, 1999; Carling et al., 2001; Gonzalo, 2002; Røed and Zhang, 2003). The corresponding elasticity estimates stretch from -0.1 (Arulampalam and Stewart, 1995) to -1.6 (Carling et al., 2001). There is also some evidence of a heterogeneity of effect: according to Narendranathan and Stewart (1993) and Bover et al. (2002) the negative effect of benefits diminishes quickly as the unemployment duration elapses, while according to Røed and Zhang (2003) the effect is invariant over the period of unemployment. The negative effect is stronger for young workers (Arulampalam and Stewart, 1995; Carling et al., 2001) and during economic booms (Arulampalam and Stewart, 1995).

A fundamental problem in estimating the effect of unemployment benefits is the fact that the benefit level and entitlement may be endogenous. In recent studies, it is common to use natural experiments, related to legislation and economic conditions in the particular labour market, in order to overcome the en-

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[1]Even if unemployment duration unambiguously increases, welfare and efficiency issues are less clear. Longer search periods lead to a more efficient allocation of resources and unemployment benefits may deter people from leaving the labour force (see Atkinson and Micklewright (1991) for an excellent review).

dogeneity problems. Examples include differences in benefit duration and the replacement rate over time and space (Meyer, 1990; Card and Levine, 2000; Bratberg and Vaage, 2000), different rules for fixed-term/permanent contracts (Bover et al., 2002) and the fact that the legislation may have some seasonal features (Røed and Zhang, 2003).

In the current analysis we use the age-dependence of unemployment assistance (UA) in Denmark as the identifying feature. An analogous instrument has been used before by Lemieux and Milligan (2004) based on Canadian data. Unemployment assistance is a cash transfer to unemployed people, which is distinct from unemployment insurance (UI) benefits. Contrary to the identifying features used in most of the previous studies, UA recipients may experience an exogenous *increase* of benefits when they turn 25 during the unemployment spell. The recipients of UA are a specific group of individuals where "weak" unemployed are significantly overrepresented. It is often argued that at least a part of them are not sensitive to either active labour market policies or welfare levels (Bach et al., 1998).

We use a mixed proportional hazard framework in order to investigate how dependent the unemployment-to-employment ($U \rightarrow E$) and unemployment-to-non-participation ($U \rightarrow N$) hazard rates are on age. Based on the search theory, we simulate the shape of the age effect on the $U \rightarrow E$ hazard rate, which corresponds to a fully anticipated exogenous shock on unemployment income. A corresponding Monte-Carlo analysis indicates that a semiparametric estimator may detect the effect on a sample of comparable size as that which is used in the current study. Further, we exploit a differences-in-differences estimator where individuals with children are used as a control, and those without children as a treatment group. Namely, UA depends on age only for individuals who are not responsible for children.

The results reveal no discontinuity in the age effect around age 25. However, there is evidence of a steady fall of the $U \rightarrow E$ hazard rate for females. The differences-in-differences estimate indicates the presence of a significant negative income effect for females, corresponding to an income elasticity of -0.4. The effect for males is positive but insignificant.

The paper is divided as follows: the next section describes the Danish unemployment income legislation, paying particular attention to age dependent unemployment assistance. The third section presents the econometric methodology; the fourth section describes the data and gives some descriptive statistics. The fifth section presents the simulations assessing the anticipated income effect and the corresponding Monte-Carlo analyses. The results of the estimations are given in the sixth section, the seventh section is devoted to a brief discussion and the last section concludes the paper.

2. Short Overview of Danish Unemployment Income Legislation

The Danish labour market is characterised by generous welfare payments for unemployed persons. There are two types of transfers — unemployment benefits (UB) and unemployment assistance (UA). An unemployed individual may receive unemployment benefits if he/she is a member of the Unemployment Insurance Fund and fulfils certain requirements about previous employment. Certain types of education (in particular university and vocational education) provide eligibility for UB, as does regular employment. Benefits are set at 90% of the previous wage, however, due to the presence of a lower and upper limit (correspondingly 2411 and 2940 DKK weekly in 2003), there is actually little variation in the benefits.

The requirements for UA are less strict. An individual must have lost his or her income as a result of an "event" (e.g. job loss, illness or divorce), and he/she must actively be searching for a new job. UA is not related to the previous income[2], it depends only on age and household type (Table 1). The age dependency of UA was introduced in 1994. The recipients are divided into four groups: first, individuals aged below 25 years (23 years before 1995) who are living with their parents; second, individuals aged below 25 (earlier 23) who are living alone; third, persons aged 25 (earlier 23) or above; fourth, individuals who are responsible for children. UA is different for different groups — in the case of the last group (individuals with dependent children) their benefit does not depend on age. The other groups experience a significant increase when they become 25 (earlier 23). For instance, for individuals not living with parents and who do not have children, UA increases from 4,969 to 7,711 DKK monthly when turning 25 (in 2001).

UA is means-tested in households; any additional income is subtracted from the total amount of UA the household is eligible for[3]. Both parents have the right to receive the children-specific amount of UA, given they are living together with the child.

However, not only UA but the active labour market policy also depends on age. Since 1994, individuals below 25 had to participate in an active labour market programme (ALMP) after 13 weeks of UA, while the older individuals had to participate after 12 months. This policy was changed from the 1st of

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[2]During the period 1/VII 1998 – 1/I 1999 individuals below 25 could get higher UA if their previous income exceeded a certain threshold in the last 18 months.

[3]Income below 1000 DKK monthly net of taxes is not taken into account under certain conditions.

Table 1: Unemployment assistance (DKK monthly)

Valid from	A	B	C	D	Introduced by
Until 1994: base benefits + housing allowance + child allowance					
age boundary 23 years:					
January 1st, 1994	2080	4251	6615	8825	Law 496, June 30, 1993
age boundary 25 years:					
July 1st, 1995[a]	2080	4251	6615	8825	Law 1129, Dec. 21, 1994
April 1st, 1996	2138	4370	6615	8825	Law 1113, Dec. 20, 1995
July 1st, 1998	2195	4489	6998	9317	Law 980, Dec. 17, 1997
March 1st, 2001	2195	4969	7711	10245	Law 1087, Dec. 13, 2000

[a]The new rules did not apply for individuals who received UA before that date.
*Note: A – individuals aged below 25 (23) and living with their parents; B – individuals aged below 25 (23) and living alone; C – individuals aged 25 (23) or above; D – individuals responsible for children, and pregnant women from the 12th week of pregnancy, does not depend on age. All the amounts listed above are corrected once a year according to a particular wage index (*statens reguleringprocent*).*

July 1998[4], when the age boundary for early activation was increased to 30 years. During participation in an ALMP, the amount of UA is the same as without participation for most types of ALMP. The only exception is job training, where the individual is entitled to a regular salary (the rules are slightly different for the public and private sector).

The discussion above suggests that UA recipients experience an exogenous increase in income under certain conditions. Even more, between January 1994 and June 1995, and since July 1998, the discontinuity in UA payments does not coincide with the discontinuity in the ALMP participation requirements. Hence, age can be used as an instrument for income during these periods.

3. Empirical Framework

A natural way of investigating the impact of income on search behaviour is by using a hazard rate framework. In the current study we are looking at transitions from unemployment into employment and out of the labour force. The hazard rate into employment can be described as a product of two probabilities — the probability of receiving a job offer and the probability of accepting the offer. An observable transition occurs if either the job offer is accepted (lead-

[4]The old rules still applied for individuals who started their UA-period before the 1st of July.

ing to a transition into employment) or if the individual considers it no longer worthwhile to continue the job search (leading to a transition into nonparticipation). Reduced-form duration models draw conclusions based on the actual transitions and do not attempt to disentangle the arrival rate and acceptance probability. The advantage of reduced-form models is that they are much easier to estimate and the necessary assumptions and requirements for data are relatively well-known. In order to disentangle the two above-mentioned components of the hazard rate, one has to use either more information or additional assumptions. Unfortunately, job offer rejections and reservation wages are not commonly observed in data sets, and hence various and somewhat arbitrary assumptions are required, the effect of which is not completely clear. However, although assumptions behind reduced-form models are more transparent, a corresponding arbitrariness arises when interpreting the results in a structural context. In the current analysis we have chosen the latter method.

The effect of UA on individual search behaviour is reflected by the dependence of the transition intensities on age. In particular, it should cause a discontinuity around age 25 for individuals who have no children but not for those with children. As the hazard rate can depend on age for other reasons too (true age dependence), we model the age effect in a flexible way using splines. Two types of models are estimated. First, we use the discontinuity in the age effect at age 25 as an estimate for the income effect. Second, we estimate a differences-in-differences model treating individuals who have no children as the treatment group, and those who have children as the control group.

3.1.   Hazard Rate Framework

The duration analysis was performed using a mixed proportional hazard (MPH) duration model in a competing risks grouped data framework. Transitions from unemployment (U) into employment (E) and non-participation (N) were investigated.

We specify the model in a grouped duration framework as in Moon (1991). The use of grouped durations is mainly motivated by the data availability (there is no direct daily or weekly information about labour market states). The second reason is that the use of grouped data does not impose a particular functional form onto the baseline hazard rate. The differences in survival probability across the intervals are treated as unknown parameters; the shape of the hazard rate inside the intervals remains unspecified. Previous research has indicated that these types of models are much more robust with respect to aggregation of duration than flexible parametric specifications (Bergström and Edin, 1992). There is also some Monte-Carlo evidence that grouped data models recover the model parameters well (Zhang, 2003).

We have divided duration into L intervals: $[0, \tau_1), [\tau_1, \tau_2), \ldots, [\tau_{T-1}, \tau_L)$ and denoted the interval, during which the individual i exits unemployment, or during which the observation is censored using ni. This is the only type of individual timing information we use. We have further denoted the vector of individual and spell-specific characteristics during interval n using $\boldsymbol{x}(n)$. We have assumed that the covariates are constant during the intervals (most of the variables are recorded once a year anyway). We have also assumed that $\boldsymbol{x}(T)$ is an exogenous process, in the sense defined by Lancaster (1990). Let v_i^m be an index, describing the unobserved individual characteristics, specific to the destination $m \in \{E, N\}$. We have used a bivariate 2×2-point discrete distribution $\boldsymbol{v} \equiv (v^E, v^N)' \in \{v_1^E, v_2^E\} \times \{v_1^N, v_2^N\}$ where the realisation (v_k^E, v_l^N) has the probability p_{kl}. We have assumed that the transition intensity into the final state m, $\vartheta^m(\tau | \boldsymbol{x}, v^m)$, can be separated into a duration-only dependent term $\lambda^m(\tau)$ and a covariate-only dependent term $v^m \phi^m(\boldsymbol{x})$ (a MPH specification):

$$\vartheta^m(\tau | \boldsymbol{x}, v^m) = \lambda^m(\tau) \psi^m(\boldsymbol{x}) v^m. \tag{1}$$

Additional crucial assumptions we have made are that $\boldsymbol{v}$ is independent of $\boldsymbol{x}$ and has a finite mean. We have normalised $\boldsymbol{v}$ by requiring that $v_1 = 1$. These assumptions allow us to identify $\lambda^m(\tau)$, $\boldsymbol{\beta}^m$ and the distribution of $\boldsymbol{v}$ (Elbers and Ridder, 1982; Heckman and Honoré, 1989; Abbring and van den Berg, 2003). Note that the parametric assumptions about $\phi(\boldsymbol{x}(T))$ and the distribution of v are not strictly required for identification. In addition, time-varying covariates would allow us to drop either the finite-mean assumption (Heckman and Taber, 1994) or the proportionality assumption (Brinch, 2000). Moreover, as we observe multiple spells for a certain number of individuals, most of the previous assumptions can be relaxed (Honoré, 1993; Abbring and van den Berg, 2003). However, although the model is over-identified given these assumptions, they are still maintained, as the aim in this study is not identification but a consistent estimation based on a finite sample. A correct use of these features improves the efficiency of the current estimator (Zhang, 2003).

Unfortunately, the competing-risks version of the grouped duration model needs simplifying assumptions in order to keep the model tractable. The problem lies in the fact that we cannot observe two different failure times simultaneously. Hence, we cannot distinguish between the cases where one or two separate events occur in a particular time interval. There are several approximations possible in order to overcome this problem. First, one can assume that only one event can occur in a single time interval (see e.g. Moon, 1991). This assumption is innocuous if the length of the intervals is short. Second, a specific form of the baseline hazard can be assumed (e.g. constant in intervals) which allows us to express these probabilities explicitly (see the discussion in

Thompson Jr., 1977). A third possibility is to allow transitions to occur only at the boundaries of time intervals. The current study uses the first approach. The second type of assumption is avoided because it requires the baseline hazard to be constant in single months. This may not be a realistic assumption if the job contracts tend to start around the beginning of a month. The third possibility (the case where job contracts start *only* at the boundaries of time intervals) does not seem to be plausible either.

Given the assumption that only one event can happen in a single time interval, the probability that individual i stays in unemployment during the interval n can now be written as a product of two probabilities: the probability that the individual does not find a job, and the probability that he or she does not leave the labour force. Hence, expression (1) for the transition intensities yields:

$$
\begin{aligned}
S(n|\boldsymbol{x}_i(n), \boldsymbol{v}_i) = \\
= \exp\left(-v_i^E \psi^E(\boldsymbol{x}_i(n)) \int_{\tau_{n-1}}^{\tau_n} \lambda^E(s)\, \mathrm{d}s - v_i^N \psi^N(\boldsymbol{x}_i(n)) \int_{\tau_{n-1}}^{\tau_n} \lambda^N(s)\, \mathrm{d}s \right) \equiv \\
\equiv \exp(-v_i^E z_n^E(\boldsymbol{x}_i(n)) - v_i^N z_n^N(\boldsymbol{x}_i(n))) \quad (2)
\end{aligned}
$$

where $v_i^E z_n^E(\boldsymbol{x}_i(n))$ and $v_i^N z_n^N(\boldsymbol{x}_i(n))$, defined by the equation above, are the integrated destination specific hazard rates in the interval n. We define λ_n^m as:

$$
\lambda_n^m(t_n - t_{n-1}) \equiv \int_{\tau_{n-1}}^{\tau_n} \lambda^m(s)\, \mathrm{d}s. \quad (3)
$$

λ_n^m can be interpreted as a certain average of the destination-m specific baseline hazard rate in the interval n. It is a free parameter and has to be estimated. This is a flexible way of specifying the baseline hazard. The number of intervals L may be allowed to increase and the length of intervals to decrease as the number of observations increases, and in this way a non-parametric estimator of the baseline hazard can be achieved.

According to the assumption above we ignore the possibility that different events may happen in a single time interval. Hence the contribution to the likelihood function can be expressed as a product of the exit term and the survival term: the contribution of individual i, who exits the initial state in interval n_i, to the likelihood function, conditional on $\boldsymbol{X}(0, \tau_{n_i})$, along path of $\boldsymbol{x}_i$ until the end of interval n_i, and $\boldsymbol{v}_i$, is:

$$
\begin{aligned}
\mathcal{L}(n_i|\boldsymbol{X}_i(0, \tau_{n_i}), \boldsymbol{v}_i) = \\
= \left(1 - \mathrm{e}^{-v_i^E z_n^E(\boldsymbol{x}_i(n))}\right)^{\delta_i^E} \left(1 - \mathrm{e}^{-v_i^N z_n^N(\boldsymbol{x}_i(n))}\right)^{\delta_i^N} \prod_{l=1}^{n_i - 1} S(l|\boldsymbol{x}_i(l), \boldsymbol{v}_i) \quad (4)
\end{aligned}
$$

where δ_i^m is a destination indicator, which equals one if spell j was observed to end in destination state m. If there are N_i spells observed for the individual i, the corresponding log-likelihood contribution, conditional on the observed covariates $\boldsymbol{x}_i$ is

$$\ell(\cdot|\cdot) = \log\left(\sum_{k=1}^{K} p_k \prod_{j=1}^{N_i} \mathcal{L}(n_{ij}|\boldsymbol{X}_{ij}(0, \tau_{n_{ij}}), \boldsymbol{v}_i^k)\right) \tag{5}$$

where n_{ij} is the exit interval of the j-th spell of the individual. Note that even in the presence of simplifying assumptions, this likelihood function cannot be written as a product of two independent destination-specific likelihoods anymore due to the presence of unobserved heterogeneity, which may be correlated across different destinations.

3.2. Income Effect

The previous section described the generic duration model setup. Now we describe how the age and income effects are introduced into the model. Two different specifications for isolating the income effect are used.

The first approach uses the discontinuity in the age effect at age 25. Because the 25th birthday is fully anticipated, the method gives a consistent estimate only if the individuals start to adapt to the new income level only a short while before the 25th birthday. This may be the case for instance if the individuals are myopic, or if they are heavily discounting the future for other reasons. The current estimates for the length of the anticipation effect, based on a fall in received benefits, range from 0 to 6 months (Card and Levine, 2000; Carling et al., 2001). Such a smooth adaptation may or may not be a problem, depending on the shape of the income effect and the true age effect.

Let $\boldsymbol{y}$ denote all the components of $\boldsymbol{x}$ except the age. We have specified the covariate-only dependent terms $\psi^m(\boldsymbol{x})$ in equation (1) as

$$\psi^m(\boldsymbol{x}) = \exp\left(\boldsymbol{\beta}^{m\prime}\boldsymbol{y} + g^m(age) + \gamma^m \cdot 1(age \geq 25)\right). \tag{6}$$

The function $g^m(\cdot)$ is specified in a flexible way in order to capture the true age-dependence; the possible discontinuity at age 25 is captured by the indicator $1(age \geq 25)$. The main parameter of interest in this case is γ^m. This specification allows us to interpret the discontinuity variable as the true assistance effect. However, if the discrete jump in income leads to a smooth transition, the estimated discontinuity may be a downward biased (in absolute value) estimate of the true income effect. Hence, we estimate a second model using only a flexible age effect and without any pre-defined discontinuity. $\psi^m(\boldsymbol{x})$ is

specified as:

$$\psi^m(\boldsymbol{x}) = \exp\left(\boldsymbol{\beta}^{m\prime}\boldsymbol{y} + g^m(age)\right). \qquad (7)$$

In this case, we are looking for a rapid decrease in the hazard rate around age 25. In order to isolate the income effect, we smooth the estimated age effect and look for differences in the estimated and smoothed version. Smoothing is done by regressing the estimated flexible effect using OLS on a third-degree polynomial of age. The discontinuity should appear as a decrease of the estimated effect with respect to the smoothed one around age 25. Although in this way we impose less pre-determined features on the function g, the results are not straightforward to interpret.

Note that the estimators, as described above, allow us to disentangle the income effect and the true age effect only if the true age effect is "sufficiently" smooth compared with the income effect. Because of anticipation, the requirements for the true age effect are stronger than in the typical regression-discontinuity case.

The second approach in this study is a parametric differences-in-differences estimator. We treat the individuals who have no children as a treatment group, and those who have children as a control group. We look for differences in the age effects around age 25 for those two groups. In this case $\psi^m(\boldsymbol{x})$ in equation (1) is specified as

$$\begin{aligned}
\psi^m(\boldsymbol{x}) = \exp\big[&g^m(age) + \gamma_1^m kids + \gamma_2^m \mathbf{1}(age \geq 25) \cdot (no\ kids) + \\
&+ \gamma_3^m(Q1\ before) \cdot (no\ kids) + \gamma_4^m(Q2\ before) \cdot (no\ kids) + \\
&\qquad\qquad\qquad\qquad\qquad\qquad\qquad + \boldsymbol{\beta}^{m\prime}\boldsymbol{y}\big]. \quad (8)
\end{aligned}$$

Here $g^m(age)$ is a flexible age effect (modelled with splines) as above, but now we allow for a duration-invariant difference between individuals who have and who do not have children (γ_1^m). The income effect is reflected as a change in the difference between age effects for individuals with and without children (γ_2^m, note that using an indicator "*no kids*" means that γ_2^m counts for the effect on the individuals without kids, i.e. those who experience a change in income). γ_3^m and γ_4^m allow for an anticipation effect up to six months, *Q1 before* and *Q2 before* mean the respective number of quarters before turning 25. All the other relevant variables $\boldsymbol{y}$ are included in the linear index $\boldsymbol{\beta}^{m\prime}\boldsymbol{y}$. Needless to say, the second approach uses data for both types of individuals, those who have children and those who do not, while the first approach uses only data for the second group.

4. Data

4.1. Variables and Some Summary Statistics

The data set used in this study is a register based representative 10% sample of the Danish population aged 16–75 covering the period 1981–2001, and their corresponding labour-market histories. The data set is compiled from various administrative registers by Statistics Denmark. The sample is updated in such a way that it is representative in each of the years. The data set includes demographic characteristics, family status, income, labour market status, education and location of residence. Data for most of the variables is collected annually, at the end of year. There are some exceptions though: we observe birth year and birth month allowing us to calculate age with monthly precision. Data for unemployment benefits and assistance include monthly duration of payments (number of days). Unfortunately the amount paid is available only on an annual basis. In order to find monthly benefits and wages, we have to rely on the corresponding duration data. It is obvious that no direct inference about benefit changes during a year can be made.

The sample was restricted for spells starting between July 1998 (due to the ALMP-related legislation, see Section 2.) and December 2001 (end of the data set). This produces a flow-sample and avoids issues associated with left censoring. The sample was further restricted to age group 21 to 29 (as measured at the beginning of the spell). For each spell, the unemployment rate in the local travel-to-work area is found. The travel-to-work area is calculated based on commuting costs (not exceeding 90 DKK daily). Possible residential mobility during an unemployment spell is taken into account by allowing for the unemployment rate to be time varying. In addition, we introduce a set of regional dummies, defined by administrative borders.

The definition of variables and brief summary statistics are presented in Tables 2 and 3. The latter table confirms the widespread assessment according to which unemployment assistance recipients are a heterogeneous group where poorly motivated low-skilled individuals are overrepresented, compared to insured unemployed individuals. Even for the older age group (25–29 years), 60% have less than a high-school education while their average working experience is still under three years. Previous studies have indicated that UA recipients are more passive in job searching and that they have more often problems — such as health problems or alcohol abuse besides unemployment. Despite the low average indicators, there are still highly motivated workers in this group. Most of the UA recipients want to work (Bach et al., 1998).

One can see that there are indeed differences between the age groups. In

Table 2: The definition of the explanatory variables

Variable	Description
Demographic and family characteristics:	
age	age, years and months
female	gender (the reference group is male)
single	family status, not married or co-habiting (the reference group is married or co-habiting)
kids	children in the household (the reference group is not to have children)
Danish	ethnicity, native Danish (the reference group is immigrant)
Education levels (the reference group is less than high school):	
high school	high school, vocational education, some college
university	university degree (Bachelor, Masters, PhD)
Labour market related information:	
experience	working experience, years
ALMP during	time-varying indicator of ALMP participation during at least 50% of the time interval
ALMP post	time-varying indicator: has been in ALMP before
local u	unemployment in the local commuting area, in percentages multiplied by 10
Copenhagen	residence in Copenhagen county
East-Sealand	Roskilde, Frederiksborg (capital area)
Western Jutland	Western-Jutland
quarterly dummies (*Q4* is the reference quarter)	
year dummies (*1998* is the reference year)	

particular, the unemployment spells for individuals above 25 are two and a half months longer on average. The difference is smaller for the completed spells (1.8 and 2.0 months for $U \rightarrow E$ and $U \rightarrow N$ spells respectively). $U \rightarrow E$ transitions are a bit more common for the younger age group; the share of $U \rightarrow N$ transitions is almost the same for the two groups. We can also see that participation in ALMPs is slightly more common among younger people. The differences in education and family characteristics between the age groups are as expected: older individuals have more experience and education, and it is slightly more common for them to be married and to have children. Members of the older age group more often live in Copenhagen and less in East-Sealand and Western Jutland.

Table 3: Summary statistics for all spells, and spells for individuals below and above 25

	all			age < 25	age ≥ 25
	min	mean	max	mean	mean
Spell duration according to type (in months):					
all	0.933	6.822	42.560	5.494	8.034
$U \to E$	0.933	5.778	37.567	4.894	6.655
$U \to N$	0.933	6.743	39.567	5.682	7.731
Share of transitions:					
$U \to E$	0.000	0.427	1.000	0.446	0.410
$U \to N$	0.000	0.360	1.000	0.364	0.357
Individual- and spell-specific variables:					
age at beginning	21.037	24.603	28.878	22.687	26.353
age at end	21.122	25.164	31.211	23.139	27.013
female	0.000	0.500	1.000	0.503	0.498
kids	0.000	0.337	1.000	0.271	0.397
high school	0.000	0.347	1.000	0.315	0.376
university	0.000	0.018	1.000	0.002	0.032
Danish	0.000	0.844	1.000	0.863	0.827
single	0.000	0.607	1.000	0.639	0.578
experience	0.000	2.301	11.000	1.824	2.737
ALMP	0.000	0.283	1.000	0.306	0.263
local u	0.030	0.051	0.124	0.052	0.051
Copenhagen	0.000	0.241	1.000	0.218	0.262
East-Sealand	0.000	0.074	1.000	0.083	0.065
Western Jutland	0.000	0.106	1.000	0.113	0.100
Number of obs		7450		3556	3894

Note: Summary statistics for all spells, and spells for individuals below and above 25 in the beginning of the year when spell ends.

In order to check whether there are any visible changes in the transition pattern at age 25, we present the share of different types of transitions ($U \to E$, $U \to N$ and censored spells) by age in Table 4. Only the individuals who do not have children are included as we do not expect to see any income effect for the others. The table reveals two clear trends: first, the share of censored spells increases commencing at age 26, and second, the percentage of transitions into employment starts to decrease around the same age. There is no visible trend in the percentage of transitions into non-participation. The most important conclusion in the current context is that age 25 is not related with any particularly notable effect. None of the abovementioned trends show

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Table 4: Observed transitions from unemployment according to age and transition type

	age									
	20	21	22	23	24	25	26	27	28	29
cens	0.19	0.19	0.22	0.20	0.19	0.20	0.22	0.26	0.28	0.47
E	0.54	0.53	0.50	0.47	0.51	0.48	0.47	0.45	0.39	0.24
N	0.27	0.28	0.28	0.33	0.31	0.32	0.31	0.29	0.33	0.29
Number of obs	592	872	780	659	588	610	632	409	75	17

any special feature at that age.

This brief look at the data suggests that older individuals are indeed slower to leave unemployment. However, no indication of a distinct effect around age 25 could be found.

4.2. Relationship Between UA and Age

In this section we demonstrate that the actual payment to different types of UA-recipients in fact is in compliance with Table 1.

The current data set includes yearly income information only and consequently the monthly level of UA transfers must be found, combining the yearly payment data with corresponding welfare duration. In this section, we have used the recorded welfare payment duration as distinct from the recorded unemployment duration, for duration analysis. Note that the measurement errors in recorded unemployment spell duration, even if distributed independently from the other variables, result in a spurious negative correlation between monthly unemployment income and unemployment duration. This is because the erroneously shorter (longer) duration leads to apparently higher (lower) monthly income and apparently shorter (longer) unemployment spells. Such a bias does not arise if age is used as an instrument and the 25-year rule is fully implemented in practise. Although the level of UA depends on whether the individuals are living together with their parents, this information is not directly available and is not used in the current study.

A kernel estimate of the distribution of average monthly assistance is plotted in Figure 1 separately for three groups: A+B, C and D. These groups are defined in Table 1. Four maxima, corresponding to the four groups in the table, are clearly visible. The maximum around 2,000 (DKK monthly) corresponds to individuals below 25 who are living with their parents (group A); the next maximum slightly below 5,000 represents those who are living alone (B); the third maximum around 7,000 are those above 25 (C), and the last maximum at
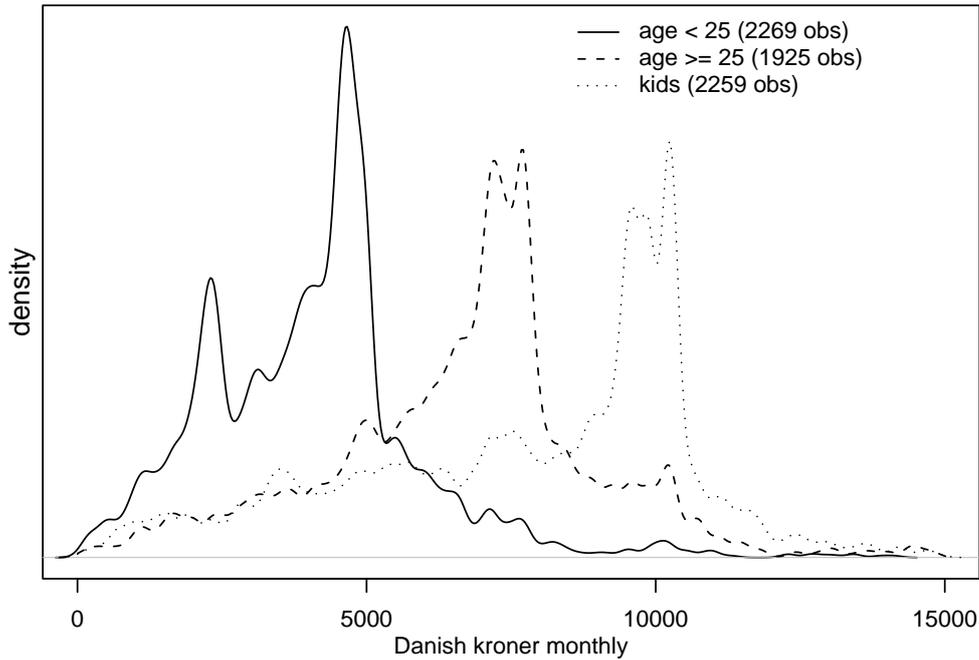
Figure 1: A kernel estimate of the density of monthly UA benefits for different recipient groups.

9,000–10,000 corresponds to parents with children (D). Note that the first two maxima are present only for the younger age group while the third one only for the older group, and the last one for individuals with children. This is in full compliance with Table 1. There is, though, a small maximum at 10,000 present for the other groups too. This is probably related to problems of registering pregnancy and childbirth as the information on children is updated only once a year. However, only a part of the observations is located close to the four maxima. The continuous part of the distribution may be related to different factors: individuals who experience a change in UA during the year (no direct information about those cases is available as we have yearly income information only), one-time supplementary benefits, errors in the recorded duration of welfare payments and different actual benefit amount due to means testing.

In order to get an idea about the strength of the instrument, we have run an OLS regression explaining unemployment assistance by age and other relevant variables. The indicator $1(age \geq 25)$ is clearly a significant predictor of UA (Table 5). All the relevant coefficients are significant and of reasonable order. Note that we have not controlled for living with parents due to data limitations. The regression includes a couple of irrelevant variables (according to the legislation) as well. *Danish* turns out to be insignificant; the others are related with

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Table 5: Unemployment assistance as a function of $1(age \geq 25)$ and other variables

coefficient	estimate	stdd
constant	3765.09*	155.65
$1(age \geq 25)$	2747.36*	108.63
kids	3331.29*	137.86
female	401.70*	91.06
Danish	38.86	120.09
single	273.25*	93.18
high school	−395.21*	90.17
university	−658.55*	325.72
$(age \geq 25) \times kids$	−2195.22*	176.22
R^2	0.23	
Number of obs	6566	

small but significant effects. Why this is, is beyond the scope of the current study, but selection effects in combination with means testing might be a possible explanation. The explanatory power of the regression is not impressive ($R^2 = 0.23$). However, part of the problem lies in measurement errors which is a minor issue if age is used as the instrument for the monthly income.

How large an effect on the hazard rate should we expect, based on these results? Table 5 suggests that turning 25 is related to a 70% pre-tax increase in UA on average (for individuals without children). We follow the other studies like Carling et al. (2001) and Røed and Zhang (2003) and ignore taxes. Using the estimate from Meyer (1990) for the elasticity of the hazard rate with respect to unemployment benefits (between -0.5 and -0.9), we would expect the multiplicative effect on the hazard rate (out from unemployment) to lie between $1 - 0.5 \times 0.7 \approx 0.65$ and $1 - 0.9 \times 0.7 \approx 0.35$. The effect on destination specific hazard rates may differ.

4.3.   Relationship Between the Hazard Rate and Age

Tables 3 and 4 suggest that there is a difference between the age groups, but there is no indication of a sharp effect at age 25. In this subsection we continue the exploratory analysis and present two simple plots. First, Kaplan-Meier hazard rates are graphed for both age groups, those below and those above 25 years. Second, we plot the age density at the end of the spell. This is related to the effect of age on the hazard rate as shown below.

The Kaplan-Meier destination-specific hazard rates into employment and non-participation are plotted in Figure 2. The hazard rates have a similar
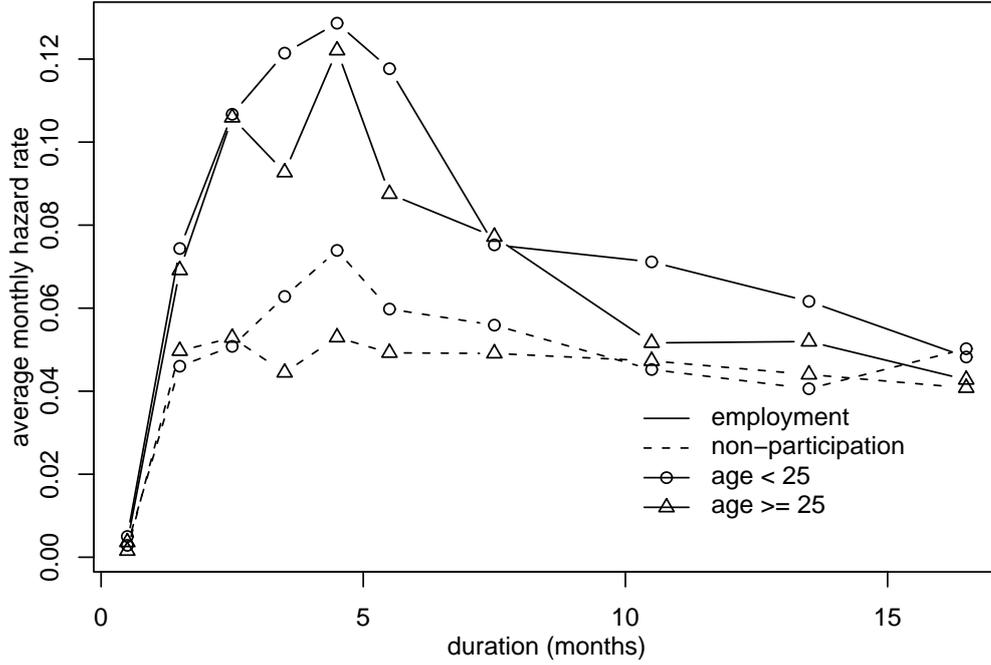
Figure 2: Kaplan-Meier estimate of the destination-specific hazard rates according to age, for individuals not responsible for children.

Note: Ages are given for the start of unemployment spells.

hump-shaped form for both age groups. Both of them, $U \to E$ and $U \to N$, are lower for the older group although the difference is not large. This fact fits well with the comparison of the average durations in Table 3. However, this simple plot does not allow us to disentangle the income and age effect.

It is easy to see that a discontinuity in the hazard rate at a certain age results in a corresponding discontinuity in the age distribution at spell end. Assume for a moment that we have a single-risk non-censoring case. We have looked at $\vartheta(\tau|a)$, the hazard rate conditional on age, and ignored the other covariates. Then with the density of completed durations τ, conditional on a^0, the age at the start of the spell, can be expressed as:

$$f(\tau|a^0) = \vartheta(\tau|a^0 + \tau) \exp\left(-\int_0^\tau \vartheta(s|a^0 + s)\, \mathrm{d}s\right). \qquad (9)$$

Let $h(a)$ be the age density at spell start and $a^1 = a^0 + \tau$ the age at the end of the spell. The joint density of τ and a^0 can now be written as:

$$f(\tau, a^0) = f(\tau|a^0)h(a^0) = f(\tau|a^1 - \tau)h(a^1 - \tau). \qquad (10)$$

The density of a^1 can be found by integrating duration τ out from the equation

above:

$$g(a^1) = \int_0^\infty \vartheta(\tau|a^1) \exp\left(-\int_0^\tau \vartheta(s|a^1 - \tau + s)\,\mathrm{d}s\right) h(a^1 - \tau)\,\mathrm{d}\tau. \quad (11)$$

This function inherits the eventual discontinuities of $\vartheta(\tau|a)$ with respect to a, given that $h(\cdot)$ is continuous at those points. Even more, if $\vartheta(\tau|a)$ is discontinuous at a^*, then:

$$\frac{\lim_{a \to a^*_-} g(a)}{\lim_{a \to a^*_+} g(a)} = \frac{\lim_{a \to a^*_-} \vartheta(\tau|a)}{\lim_{a \to a^*_+} \vartheta(\tau|a)}. \quad (12)$$

If we allow for multiple destinations (censoring can be treated as a separate destination), the result remains essentially the same. The only difference is that $\vartheta(\tau|a^1)$ under the integral in (11) is replaced by a weighted average of destination-specific hazard rates and hence (12) is not longer valid.

Both types of age densities, $h(a^0)$ and $g(a^1)$, are plotted in Figure 3. The distributions are quite similar. The main features, namely humps around age 22 and 26, and some oscillation in between them, are common for both distributions. The age-at-end curve repeats the features of age-at-start curve with a lag as the individuals get older during the spell. There is a small hollow in the age-at-end curve at age 25; however, it does not seem to be much different from a corresponding hollow in the age-at-start distribution a short while earlier.

The conclusion of the exploratory analysis in this section is similar to that of Section 4.1. Older individuals have lower hazard rates out from unemployment but there seems to be no significant feature at age 25.

5.   Some Simulations and a Monte-Carlo Sensitivity Analysis

5.1.   Anticipation Effect

The 25th birthday is perfectly anticipated. It leads rational individuals, who maximize the expected discounted income, to act on the anticipated change in the benefits before the actual benefit increases. The reservation wage and search intensity will adjust gradually to a optimal level. After the moment when this point is reached, there will be no more changes in these variables (Mortensen, 1977). The adjustments during the last months may or may not be visible in the actual data, depending on the actual shape of the anticipation effect.
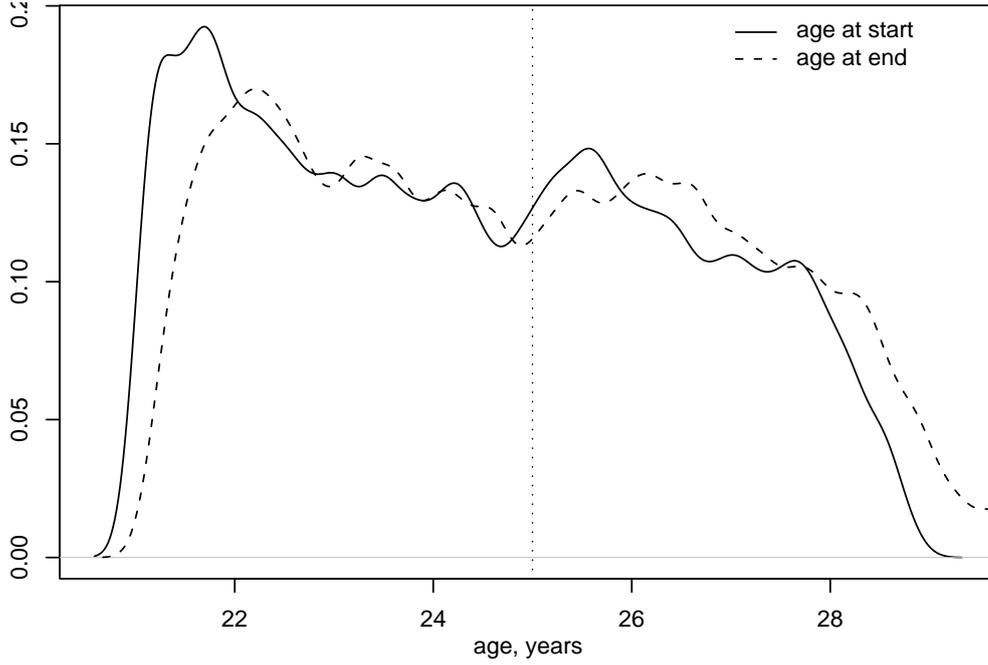
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Figure 3: A kernel estimate of age distribution at the start and end of spell for individuals not responsible for children.

In order to assess the magnitude of the anticipation effect, we have performed a simulation of the underlying search process. The point of departure is the dynamic search model by Berg (1990). The first difference is that we allow explicitly for job destruction. Second, unlike the common benefit-duration models, in this case unemployment income does not depend on unemployment duration but on age, which for a given individual is equivalent to calendar time. Hence, the value of unemployment does depend on calendar time, but not on unemployment duration. Consequently, the possibility of layoff leads to the calendar-time dependent value of jobs.

Let the ϱ be the discount rate, $b(t)$ be the flow of unemployment assistance benefits as a time-dependent process, δ the exogenous layoff-rate, and λ the offer arrival rate. We have abstracted from search intensity issues because search costs are difficult to calibrate. Let the wage offers be i.i.d.drawn from a known distribution $F(w)$. The value of unemployment at time t may now be written as:

$$\varrho U(t) = b(t) + \lambda \int_{w^*(t)}^{\infty} [W(t, w) - U(t)] \, \mathrm{d}F(w) + \dot{U}(t) \qquad (13)$$

and the corresponding value of a job at time t as:

$$\varrho W(t, w) = w + \delta [U(t) - W(t, w)] + \dot{W}(t, w). \qquad (14)$$

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where $w^*(t)$ is the reservation wage at time t and $\dot{U}(t)$, $\dot{W}(t,w)$ denotes the rate of change in the value functions with respect to time.

The model is quite straightforward to solve numerically. One has to solve the static problem after the 25th birthday, after which the assistance remains constant, and then calculate the previous values recursively backwards (although this may lead to cumulating errors). For the following plot we have used the following monthly parameter values: $\varrho = 0.004$ (5% annual discount rate) and $\delta \in \{0.05, 0.10, 0.15\}$. Although the last numbers may seem too high, leading to an average employment spell of only between 7 and 20 months long, we have to remember that these post-unemployment spells are far from typical employment spells. In fact, Bolvig et al. (2003) find the average duration of post-unemployment spells for UA recipients (though, including censored spells) to be 10 months. $\lambda = 0.3$ was calibrated to get a roughly correct value for the hazard rate into employment. The values for monthly benefits were chosen as given in 1998: $b_0 = 4,489$ and $b_1 = 6,998$ and the wage offers were drawn from a log-normal distribution with a mean 8,600, a standard deviation of 3,100 and truncated from above at 30,000. This distribution leads to approximately correct moments for the post-unemployment wage.

The simulated anticipation effects on the hazard rate are presented in Figure 4. The figure reveals that a higher job-destruction rate leads to a higher hazard rate and a shorter anticipation effect. The reasons are obvious — a shorter expected job duration lowers the opportunity costs of taking a low paid job and consequently increases the hazard rate. It lowers the expected discounted value of holding a job as well, exactly in the same way as the discount rate. Accordingly, a larger value for δ leads to a shorter anticipation effect. All the parameter values lead to a steady decrease of the hazard rate. The point where the hazard rate has decreased by a half of the final fall is marked by a dot in the Figure. It occurs 3–5 months before the 25th birthday. This outcome fits well with the empirical evidence: the estimates for the length of the anticipation effect stretch from 0 to 6 months (Card and Levine, 2000; Carling et al., 2001). The corresponding income elasticities and acceptance probabilities are shown in Table 6. The simulated elasticities are concordant with the estimates in the literature. However, note the modest acceptance probabilities. The previous estimates suggest a higher acceptance rate (Berg, 1990).

5.2. Monte-Carlo Sensitivity Analysis

A parametric maximum likelihood estimator would give consistent estimates given that the model is correctly specified. In our case, the preliminary data analysis suggests that there is no discontinuity at age 25, and we have
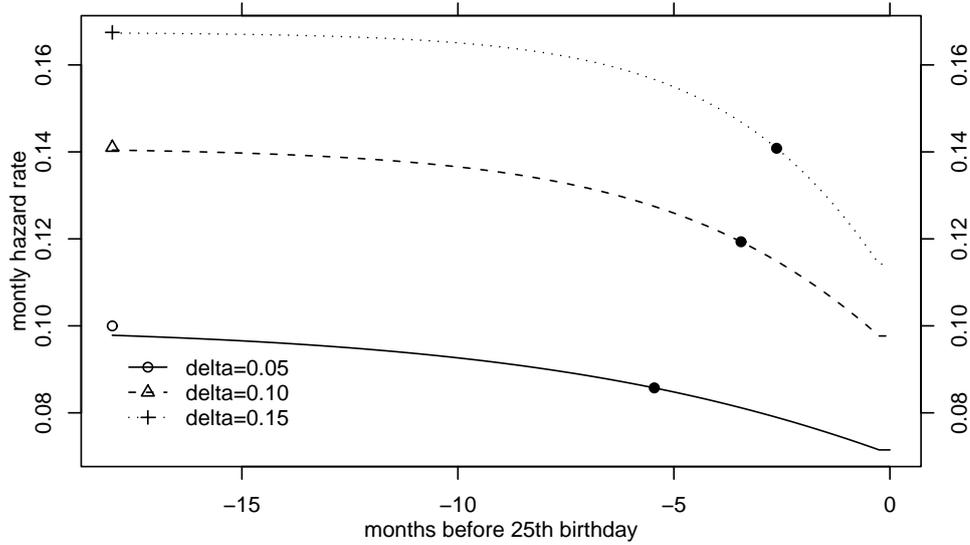
Figure 4: The dependence of the hazard rate on age — simulated anticipation effect.

Notes: Symbols on the left-hand side of the lines correspond to the respective values at $t = -\infty$, sufficiently before the 25th birthday, where the anticipated income increase has no effect on the hazard rate. Solid dots are the points where the hazard rate has decreased by a half of the total fall.

Table 6: Simulated income elasticities, acceptance probabilities and corresponding reservation wages.

model	elasticity	Accept. Probability		Reservation wage	
		at $-\infty$	at age 25	at $-\infty$	at age 25
$\delta = 0.05$	-0.510	0.333	0.238	9 420	10 400
$\delta = 0.10$	-0.550	0.470	0.326	8 320	9 490
$\delta = 0.15$	-0.569	0.558	0.381	7 700	9 010

Notes: $-\infty$ denotes point of time, sufficiently before the 25th birthday, where the anticipated income increase has no effect on the acceptance probability.

to look for a smooth transition profile instead. In this case the discontinuity based maximum likelihood estimator will in general be biased and the common asymptotic properties will not apply. The properties of the other estimator, where we look at the difference between the estimated and smoothed effect, is even more complicated. The third, differences-in-differences estimator is fully parametric and, given that our specification is correct, we can use the common asymptotic results. Hence we conduct a Monte-Carlo analysis in

order to assess the properties and sensitivity of the first two estimators.

The data generating process (DGP) combines the simulated anticipation effect, as in Figure 4, with a "true" age dependence in order to model the full age dependence of the hazard rate. The anticipation effect, corresponding to $\delta = 0.10$ in Section 5.1. is approximated as an exponential curve:

$$\log \vartheta(age) = A_0 + A_1(1 - e^{A_2 \cdot (age-25)}) \cdot 1(age < 25), \qquad (15)$$

where coefficients A_0, A_1 and A_2 are fitted using three values: firstly, with the hazard rate at $t = -\infty$ before the 25th birthday, corresponding to the stationary solution before the effect of an income increase appears; secondly, at the time where the hazard rate has decreased by half of the final fall due to the anticipation effect (dots on Figure 4); and finally, at $t = 0$, the stationary value corresponding to the new unemployment income at age 25. Here A_0 determines the overall level of the hazard, $A_1 > 0$ is the size of the income effect and $A_3 > 0$ describes the shape and duration of the anticipation effect. The resulting exponential curve approximates the simulated hazard rate well.

Next, we specify the "true" age effect as a quadratic curve where the income effect was to be added. It was chosen to be roughly similar to the estimated age dependence:

$$e(age) = 0.055(age - 30)^2. \qquad (16)$$

Thus, the final age dependence of the hazard rate appears as:

$$h(age) = \exp(e + \log \vartheta), \qquad (17)$$

where $e(age)$ and $\log \vartheta(age)$ are defined by the equations above.

The DGP creates competing risks duration data using 5 covariates: age and four uncorrelated variables, two of which were time-invariant (one dummy and one continuous) and two time-variant (one dummy and one continuous). The Weibull baseline hazard and distribution of (independent) censoring times were chosen in such a way that the resulting sample roughly produced the correct mean durations and shares of different types of transitions.

We have investigated both estimators using 100 Monte-Carlo simulations utilising the DGP as described above. Each sample consists of 2500 observations. The distribution of the estimated age effect is presented using corresponding distribution quantiles. The MC samples were estimated exactly in the same way as presented below in the results section: the base age effect was specified as a linear spline with 7 internal knots and an indicator for $age \geq 25$ for the first specification, and as a linear spline with 8 internal knots for the second one. The resulting age effect for the model with discontinuity is plotted in Figure 5. The figure reveals that the income effect, although a rather
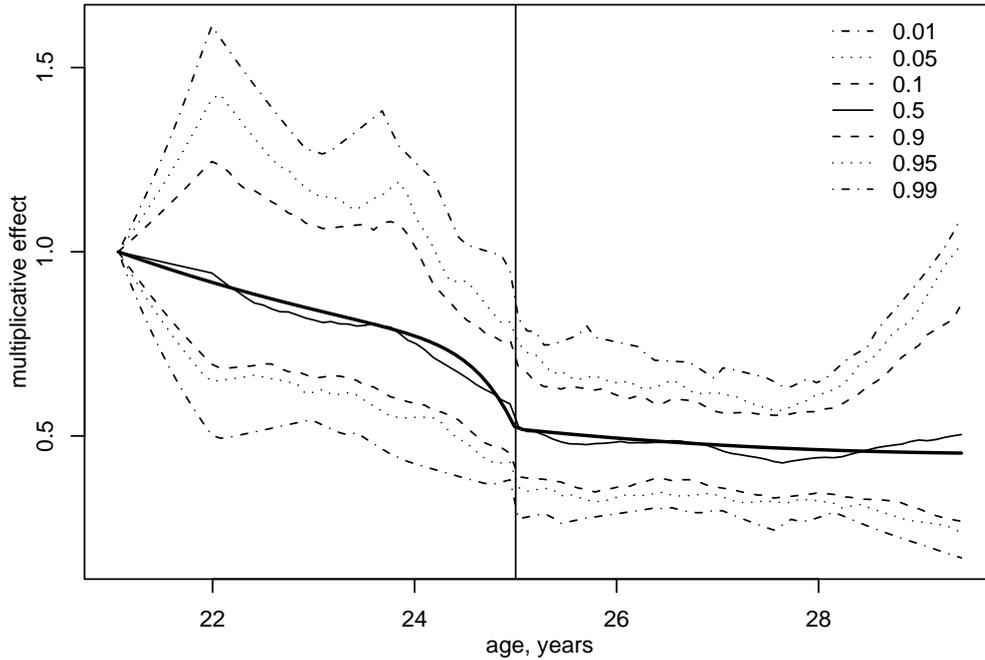
Figure 5: The simulated age effect (bold line) and point-wise quantiles of 100 Monte-Carlo estimates.

continuous one, is indeed caught by the discontinuity in the estimator. The median of the Monte-Carlo iterations approximates the true age effect quite well. However, the confidence band, based on 5% and 95% quantiles, is far larger than the discontinuity. This outcome is further reflected by the fact that the mean of the estimated discontinuity parameters is -0.126 while its standard deviation is 0.250. The mean is significantly biased toward zero; the true income-related fall in the hazard rate is 0.354. This is because the smoother part of the anticipation effect is caught by splines and not by the discontinuity.

To achieve a specification without discontinuity, we calculate the difference between the estimated and smoothed age effect exactly in the same way as in Section 6. below: for every MC interaction we estimate a cubic smoothing regression in the age range 22–28. The (quantiles of the) resulting distribution of differences is presented in Figure 6. The locations of spline knots are clearly visible as regular widenings of the distribution. One may see that there is indeed a large fall around age 25–26. The significance of this feature seems to be affected by the location of one knot slightly above age 25. However, similar though much smaller features appear at other ages too. An analogous simulation was conducted without any income effect, only with a quadratic "true" age effect. In this case, the corresponding picture is completely different; the median effect shows no wavy pattern and none of the quantiles investigated
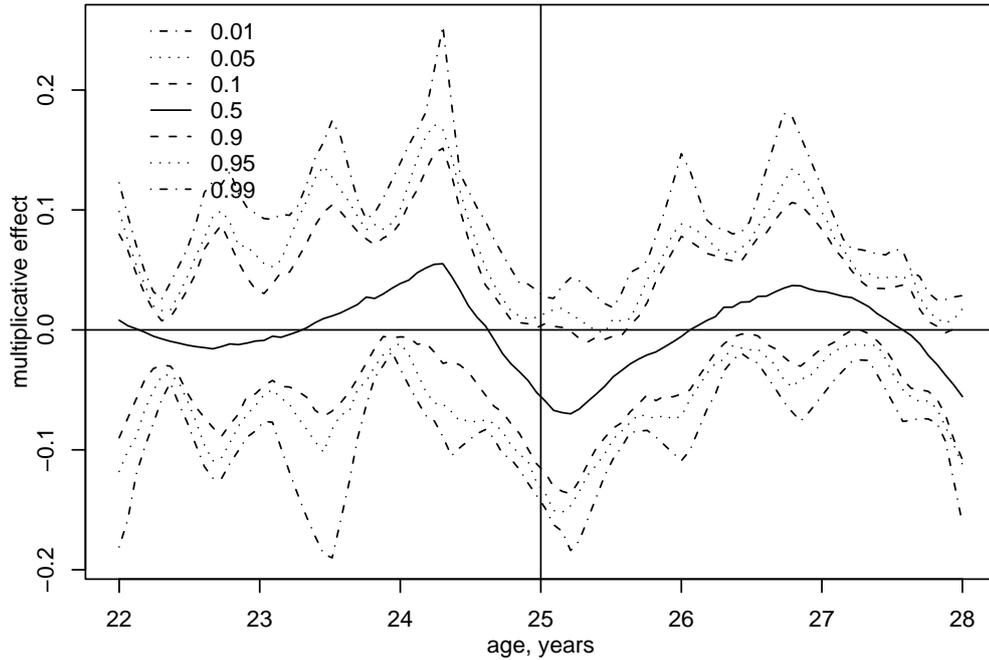
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Figure 6: The difference between smoothed and non-smoothed age effect. Quantiles of 100 Monte-Carlo estimates.

are anywhere close to zero.

In conclusion, the inference from the Monte-Carlo analysis is a bit inconclusive. The simulation suggests that the sample is too small to test the presence of a sharp fall in the age effect just before the 25th birthday. Furthermore, such an estimate would be seriously biased toward zero. However, the other specification hints that a fall in the effect may still be visible in the data. Unfortunately, there is no clear way to assess the size of the effect.

6. Results

6.1. Age Effect

6.1.1. Specification With Discontinuity

Although the Monte Carlo analysis indicates that the discontinuity associated with the anticipated income effect may not be observable on a sample of the current size, we still performed such an estimation. This is because the agents may be significantly more myopic than assumed in Section 5.1., due to other processes ignored in the previous analysis, which lead to a similar out-

Table 7: Estimated discontinuities (income effects) at age 25 and corresponding income elasticities

transition to:	employment		non-participation	
	estimate	stdd	estimate	stdd
Males				
coefficient	−0.030	0.232	0.039	0.283
elasticity	−0.042	0.322	0.057	0.421
Females				
coefficient	−0.110	0.366	0.137	0.593
elasticity	−0.149	0.468	0.210	0.971

come (e.g. on-the-job search), or simply because the effect may be stronger than suggested previously. In all these cases it is possible that a strong rapid fall in the age effect immediately before the 25th birthday may be captured by the discontinuity indicator variable. The results presented here, are based on specification (6) where age effect $g(age)$ is modelled using linear splines with 7 distinct internal knots. All the other forms for $g(\cdot)$ provided qualitatively similar results. The support for the unobserved heterogeneity only converged to one mass point, so we present the results here without unobserved heterogeneity.

In the current specification, the estimated discontinuity coefficient γ^m is associated with income elasticity:

$$\varepsilon^m = \frac{\exp(\gamma^m) - 1}{1} \frac{b}{\Delta b} = \frac{\exp(\gamma^m) - 1}{0.7} \tag{18}$$

where $\Delta b/b = 0.7$ is the experienced income increase at age 25. If the income increase is actually smaller (e.g. due to taxes or other income transfers), the coefficients remain valid, but the elasticity estimate above will be downward biased.

The estimated coefficients and corresponding income elasticities are given in Table 7. None of the effects are statistically significant. The impact on the $U \rightarrow E$ transition intensity is negative, as suggested by the theory (see Section 5.1.), but the coefficients are much smaller than the corresponding standard errors. The coefficients for the $U \rightarrow N$ transition are positive, contrary to theoretical predictions[5], but the standard errors are even larger.

Looking at the plotting of the age effect (Figure 7) reveals that the discontinuity is not capturing a more or less permanent decrease (or increase) in the

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[5]Increasing unemployment assistance leads to a higher value of unemployment and hence lower transition intensity out from it. See Rosholm and Toomet (2005) for a theoretical model.
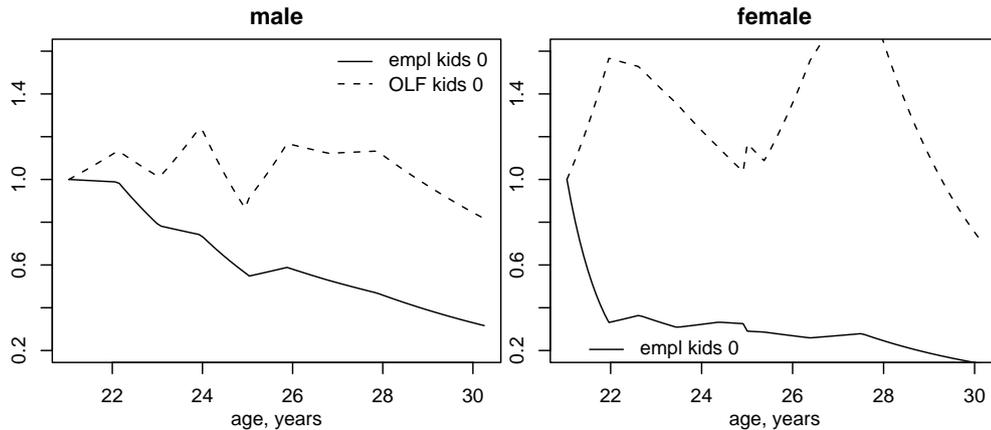
Figure 7: Multiplicative age effect modelled using linear splines. Discontinuity at age 25.

age effect, but rather a local feature. For males, the discontinuity is virtually non-existent and is followed by a much larger increase of the effect during the following year. The effect on the female $U \to N$ transition rate is positive but it is followed by a larger increase afterwards. The picture for female $U \to E$ transitions looks most in accordance with our expectations, however, this parameter, like the others, is far smaller than the corresponding standard error.

The results above reveal no sign of a distinct feature in the age effect at age 25. However, we cannot exclude the presence of weaker effects corresponding to an elasticity less than 0.6 in absolute value.

6.1.2.   Specification Without Discontinuity

As the previous analysis indicates, the income increase may not be associated with an observable discontinuity. However, given that the adjustment process in the final months before the birthday is fast enough, the impact of the increasing UA would appear as a fall in the effect during the months before the 25th birthday. Such an effect can be caught, by comparing a flexible estimate of the age effect with a smooth one.

The results here are presented for $g(\cdot)$ specified by linear splines with 8 internal knots. Different specifications provided qualitatively the same results. The algorithm was unable to find more than one support point for the distribution of unobserved heterogeneity as in the previous section, so we present the results without unobserved heterogeneity. The age effect is plotted in Figure 8. Comparing this figure with the previous specification (Figure 7) suggests that
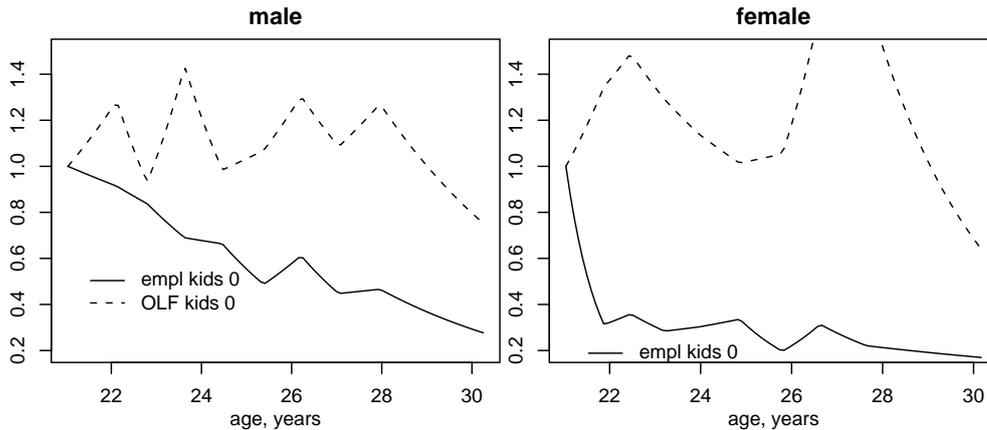
Figure 8: Multiplicative age effect. Linear spline specification without discontinuity.

the general shape of the effect is not much affected by the absence of a discontinuity. The most notable difference is a depression for females around age 25: the discontinuity (as present in the previous figure) has disappeared but a remarkable fall in the effect has appeared instead. A similar fall is present for the males too, in the current case it is deeper than in the previous figure.

In order to test the significance of the features in the figure, we plotted the difference between the flexible and smoothed effect (Figure 6.1.2.). The estimate of $g(\cdot)$ is smoothed by fitting a cubic curve onto it using OLS in the age range 22–28. A limited range was used in order to avoid problems related to the instability of the spline estimator at the boundaries. Ninety-five per cent confidence bounds are calculated using the delta method. In general, the confidence bands for $U \rightarrow E$ transitions are narrower than those for $U \rightarrow N$ transitions. This is related to the number of corresponding completed spells. None of the effects on $U \rightarrow N$ transitions appears to be significant. In the case of $U \rightarrow E$ transitions, the male and female curves look qualitatively similar: both have a minimum between age 25 and 26. However, due to the smaller standard errors, the minimum turns out to be significant for females only. Different specifications of $g(\cdot)$ slightly affect the location of the minimum (between 25 and 25.5 years) and its significance (it becomes insignificant in some cases). Note that the smallest effect occurs not at age 25 but around half a year later. This is in concordance with the Monte-Carlo simulation, which suggests that the true age effect at 25 appears as a minimum around half a year later.

6.1.3. Parametric Differences in Differences Estimate

Differences-in-differences estimates were conducted using specification (8) and exploiting the fact that only individuals who are not responsible for chil-
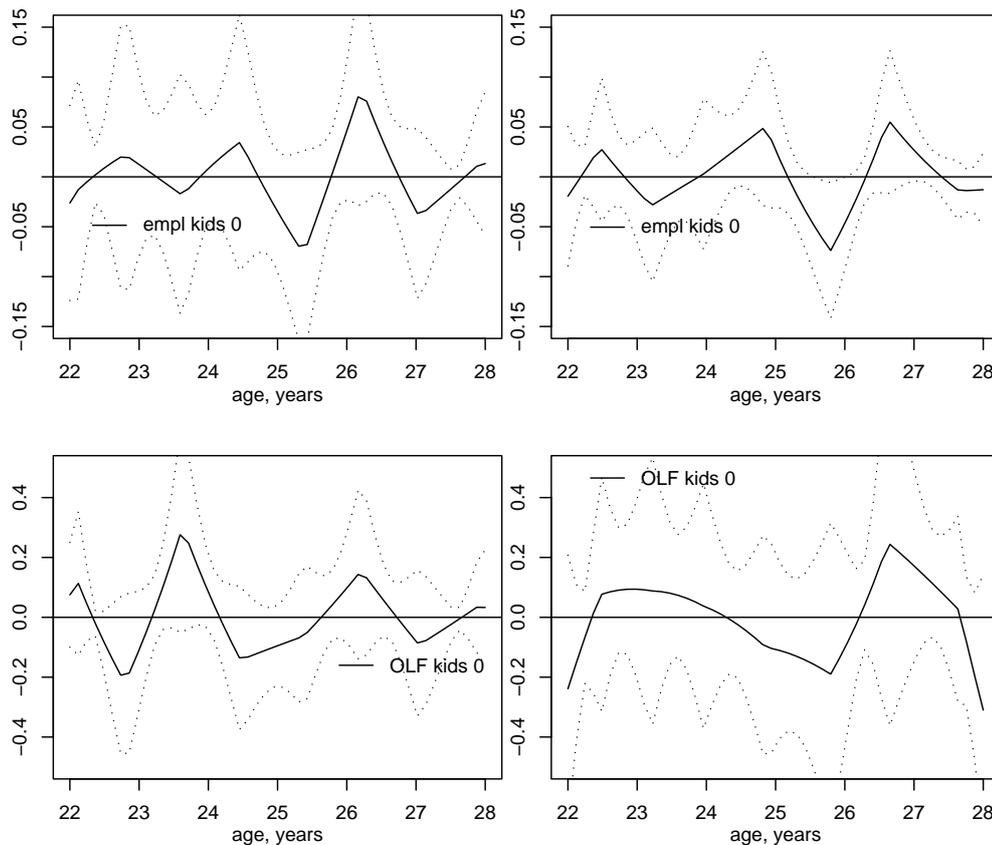
Figure 9: Difference between the flexible and smooth age effect.

Notes: 95% confidence bands. Transitions into employment (upper panel) and to non-participation (lower panel). Males on the left and females right panel.

dren experience the income increase. Although income remains constant for the "control" group, individuals with children, the results must be treated with caution. First, child birth is not a completely exogenous process; and second, these two groups are already structurally different and a simple comparison reveals additional points of difference (Table 8).

The most striking difference, although not at all surprising, appears to be in the family situation. For males, more than 80% of those without children are single while this is true for only slightly more than 10% of those who have children. After all, single fathers are not a common sight in Denmark. The case of females is analogous, although the differences are significantly smaller. The next most important difference lies in ethnic background. Ethnic Danes are overrepresented among those who have no children. Education reveals

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Table 8: The characteristics (mean values) of the treatment and control groups for the differences-in-differences estimate

	Males			Females		
children:	No	Yes	t	No	Yes	t
high school	0.325	0.291	1.746	0.475	0.293	11.476
university	0.015	0.017	−0.435	0.039	0.004	7.100
Danish	0.881	0.650	11.942	0.901	0.806	8.338
single	0.826	0.116	50.192	0.641	0.411	14.414
ALMP	0.282	0.304	−1.128	0.259	0.300	−2.815
local u	0.052	0.052	0.040	0.051	0.052	−1.461
Copenhagen	0.264	0.219	2.509	0.276	0.186	6.525
East-Sealand	0.076	0.073	0.251	0.073	0.072	0.211
Western Jutland	0.099	0.122	−1.652	0.113	0.106	0.694
Number of obs	3051	672		1690	2040	

Notes: t-values of mean equality test.

a clear pattern: more education, fewer children. The effect is stronger for females. The other differences are less important: there are fewer children in Copenhagen, and those who have children more often participate in ALMPs. Both effects are significant for females only. A number of other variables do not reveal any significant difference. We do not present the corresponding numbers for durations, as these are outcomes of the age effect.

As is common with this type of estimator, the difference in fixed effects (i.e. different hazard rate levels) does not bias the results. The main source of bias lies in the fact that the groups may experience different true age effects. Another possible problem may arise if there are only a few individuals with children at a particular age (lack of common support). Unfortunately, we cannot limit the analysis to a narrower subgroup due to the low number of observations. We performed specification tests (see below) in order to assess the first problem. The tests did not reject the hypothesis of a common age effect for both groups. However, this remains a possible weakness of the analysis below.

The model's outcome was virtually not affected by way of the specification of $g(\cdot)$; in the following we discuss the case of 7-knot linear splines. The results, related to the income effect, are presented in Table 9. We limit the discussion to transitions to employment, as the relevant variables for the $U \rightarrow N$ transition are not significant. The effects of the other variables and the baseline hazard are discussed in Section 6.2.

In the probability distribution of the unobserved components, some of the

Table 9: Differences-in-differences estimate. The coefficients related to the income effect

transition to:	employment		out of labour force	
	estimate	stdd	estimate	stdd
	Males			
no kids $\times$ $\mathbb{1}(age \geq 25)$	0.208	0.137	0.090	0.177
elasticity	0.331	0.242	0.134	0.276
Q1 before	−0.144	0.184	−0.097	0.244
Q2 before	−0.320	0.204	−0.011	0.214
LRT: $\chi^2(8)$	10.724		6.918	
Number of obs/Number of individuals	3520/2461			
	Females			
no kids $\times$ $\mathbb{1}(age \geq 25)$	−0.322*	0.131	−0.003	0.137
elasticity	−0.393	0.136	−0.005	0.196
Q1 before	−0.322	0.307	0.445	0.288
Q2 before	0.098	0.237	0.159	0.339
LRT: $\chi^2(8)$	5.472		4.884	
Number of obs/Number of individuals	3577/2620			
	Single females			
no kids $\times$ $\mathbb{1}(age \geq 25)$	−0.295	0.193	−0.167	0.198
elasticity	−0.365	0.205	−0.219	0.239
Q1 before	0.295	0.329	−0.078	0.360
Q2 before	−0.779*	0.375	−1.211	0.647
LRT: $\chi^2(8)$	6.838		4.816	
Number of obs/Number of individuals	1845/1431			

probabilities converged to zero. In order to overcome this problem, we specified a less flexible distribution where v^E and v^N were required to be perfectly correlated (correlation $= +1$)[6]. Corresponding coefficients are reported below. However, in order to perform the specification test we had to rely on models with no unobserved heterogeneity because otherwise the value or the probability of an additional point of support converged to zero.

The main effect for males, *no kids* $\times$ $\mathbb{1}(age >= 25)$, is positive but insignificant. This outcome means that there is no distinct difference in the time trends of the job-search behaviour between males with and without children around the time when they turn 25. The anticipation effect (*Q1 before* and *Q2 before*) is large and negative but it is not significant either. Figure 10 (left panel) illustrates the case. Note that we have eliminated the differences in level (fixed effects) using a suitable scaling, allowing us to focus on the differences in trends. The age effect of men without children falls significantly during the

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[6]The main results are robust with respect to specification of the distribution of unobserved heterogeneity.
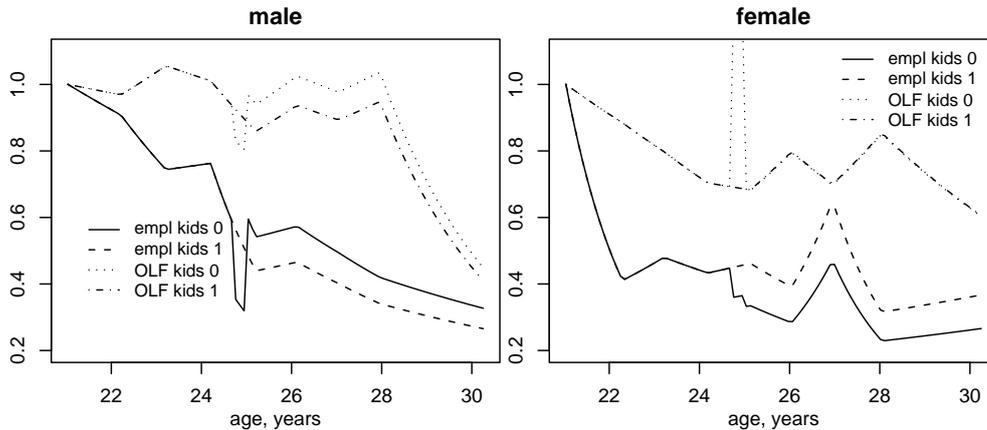
Figure 10: Differences-in-differences estimate. Age effect.

last two quarters before the 25th birthday, but rises thereafter slightly above the level of those with children. Females, on the contrary, have a negative significant income effect, corresponding to an income elasticity of -0.4. However, in their case too, the anticipation effect turns out to be weak and insignificant.

In order to investigate the robustness of the results, we re-estimated the model on a subsample of single females (there were too few single males with children in order to do a similar exercise with males). This is in order to avoid possible interference of spouses' labour supply and related problems with means testing. The results (third part of the table) are similar to those of the full sample of all females. The income effect is only slightly smaller in absolute value, but as the sample is nearly halved, none of the effects turns out to be significant. The results suggest that the estimate above is not an artefact of family-related income and labour supply issues.

The differences-in-differences estimates above are subject to specific assumptions about the treatment and control group. In particular, we assume that individuals with and without children share the same age effect when controlled for the time-invariant effect of children, the anticipation effect, and income. Put differently, the specification assumes an age-invariant difference between those two groups. However, it is possible that the age effect differs in a more general way. In order to test this hypothesis, we allowed the intergroup difference, after controlling for the effect above, to depend on the age in a flexible way, using a similar spline function as used for modelling $g(\cdot)$. The likelihood ratio test does not reject the hypothesis of a duration-invariant difference for any of the samples and any of the transitions (the 95% critical value for $\chi^2(8)$ is 15.51). The main coefficient of interest, *no kids* $\times 1(age >= 25)$, remains qualitatively the same.

6.2. Other Effects

In Table 10, we report the effect of individual-specific and spell-specific characteristics corresponding to the models in Section 6.1.3. The respective results for the other types of models were roughly similar.

Table 10: Estimated individual and regional effects, corresponding to model specification (8).

transition to:	employment		out of labour force	
	estimate	stdd	estimate	stdd
	Males			
Danish	0.368*	0.097	0.008	0.100
high school	0.351*	0.064	−0.252*	0.089
university	0.825*	0.232	−1.458*	0.586
experience	0.171*	0.015	−0.094*	0.021
kids	−0.067	0.135	−0.006	0.180
single	−0.173*	0.079	0.158	0.100
ALMP during	−0.971*	0.113	−1.030*	0.128
ALMP post	2.365*	0.121	2.493*	0.149
local u (%, ×10)	0.015	0.297	0.094	0.347
Copenhagen	0.455*	0.076	0.481*	0.092
East-Sealand	0.323*	0.122	0.096	0.164
Western Jutland	0.380*	0.108	0.301*	0.135
v_2	5.089*	0.702	9.857*	1.801
p_{22}	0.807*	0.020	0.807*	0.020
	Females			
Danish	0.221	0.116	−0.077	0.089
high school	0.522*	0.070	−0.111	0.079
university	1.099*	0.209	−0.708	0.392
experience	0.259*	0.019	−0.019	0.025
kids	−0.852*	0.103	0.341*	0.101
single	−0.053	0.065	0.083	0.066
ALMP during	−0.952*	0.132	−1.124*	0.128
ALMP post	3.067*	0.163	2.819*	0.134
local u (%, ×10)	0.164	0.339	−0.012	0.294
Copenhagen	0.419*	0.086	0.377*	0.083
East-Sealand	0.590*	0.116	0.004	0.147
Western Jutland	0.003	0.129	−0.034	0.121
v_2	0.074*	0.016	0.091*	0.017
p_{22}	0.138*	0.015	0.138*	0.015

*Notes: * – statistically significant at 95% level*

There are more significant effects for the $U \rightarrow E$ transition. This is partly

because the corresponding standard errors are smaller, and partly because the coefficients for the $U \rightarrow N$ transition are smaller in absolute value (especially for females).

Ethnic Danes clearly have a higher $U \rightarrow E$ hazard rate although the effect is not significant for females. Education is associated with the transition intensity in the expected way: more education leads to faster transitions to jobs and to a lower propensity to leave the labour force. The same is true for working experience (the coefficient corresponds to an additional year of experience).

Children in the household do not affect males' hazard rates, but make females much less inclined to move to a job and much more prone to leave the labour force. Being single is associated with a lower labour-market orientation for males: single men have lower transition intensities into employment, and the transition intensity into non-participation is higher. However, the latter feature is not significant.

The ALMPs show a significant locking-in effect and a strong positive post-program effect. The size of the post-program effect exceeds that found in the previous studies (between -1 and $+1$, depending on the type of the program Bolvig et al. (2003); Rosholm and Skipper (2003)). The programs seem to have a qualitatively similar effect on the transitions to employment and non-participation.

The recipients of social assistance are not sensitive with respect to local unemployment. The regional dummies, included in the model, reveal several regional differences. Individuals are leaving unemployment faster for both destination states in the Copenhagen area. In East-Sealand only the effect on the $U \rightarrow E$ hazard rate is present. Western-Jutland is similar to Copenhagen with respect to males; female transition intensities do not differ from the national average in that region.

A 2×2-mass-point perfectly correlated (correlation $= +1$) distribution was used for unobserved heterogeneity. The distributions appear to be essentially similar for both genders and both transition types: the mass points are rather different, the value of one is 5–14 times that of the other, and most of the probability (80–90%) is put on the mass point with the largest value.

The baseline hazard rates are presented in Figure 11. The figure reveals that the hazard rates increase during the first months of unemployment and stabilise thereafter at around 0.08 (monthly).
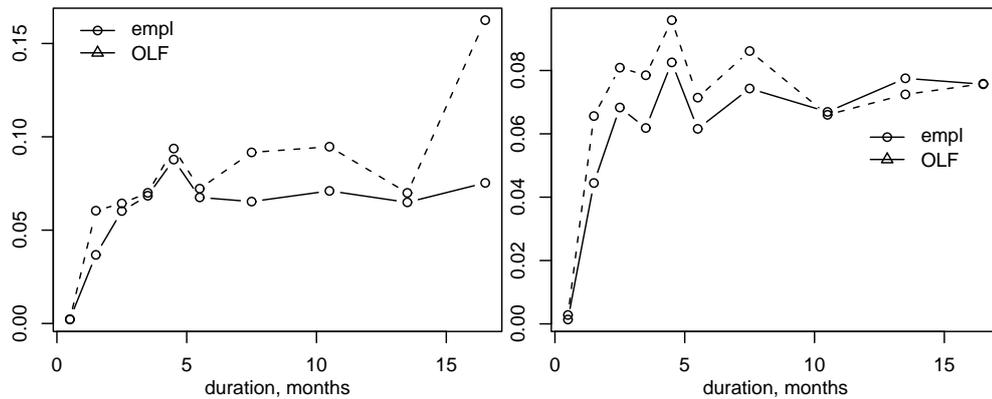
Figure 11: Baseline hazard rates. Males on the left and females right panel.

7. Discussion

A 70% increase in unemployment assistance at the age 25 does not seem to have a large effect on the transition rate out of unemployment. The 30% fall in the female hazard rate corresponds to an elasticity of –0.4, which is in line with results by Røed and Zhang (2003), based on a sample of insured female Norwegian workers. The current results suggest that females among the UA recipients are much more sensitive to financial incentives than males. Although this outcome is not common, it is far from unique (Achdut et al., 2004). There is also some evidence from the US that the most welfare-dependent individuals are less sensitive to income-related incentives (Grogger, 2002).

Income elasticity is related to the location of the reservation wage relative to wage offer distribution. If the reservation wage lies sufficiently below the bulk of wage offers, an increase in the benefits may not lead to a significant fall in the observed transition rate. This seems to be the case for males, but not for females. The sample, unemployment assistance recipients, is believed to contain mostly "weak" unemployed workers, individuals with low skills and low work motivation. It may be the case that for males, the fact that one has been caught by the last-resource security net has a significant stigmatising effect. Hence, some males accept most of the "reasonable" job offers anyway, regardless of the benefits, while others may not be interested in working at all. Although childcare related problems may hinder females in finding a suitable job, related unemployment may not be as stigmatising as in the case of males and hence they may have more job offers to choose from.

The sample of UA recipients is heterogeneous. This fact is confirmed in the current study by the large difference in the values of estimated support

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points for unobserved heterogeneity. Although these values cannot be directly interpreted as different individual "types", the result indicates that there are people in the sample who virtually never leave unemployment. This group may not be sensitive to financial incentives for various reasons.

The shape of the age effect in the semiparametric analysis (Section 6.1.2.) is in concordance with the corresponding Monte-Carlo simulations, namely that the smallest effect appears around half a year after the 25th birthday. In fact, the measured effect may be delayed compared to the effect on the underlying search behaviour for several reasons. First, there is usually a certain delay between finding and starting a job. Second, the sample composition may change over time following the income increase. There may also be some issues with bounded rationality where adaptation to the anticipated changes occurs less than perfectly in advance. In that case the adaptation should continue after the environment has changed.

8. Conclusions

We analysed the impact of an exogenous increase in income for Danish unemployment assistance (UA) recipients. According to the legislation, UA increases by around 70% when individuals turn 25. However, those who have children receive a higher amount independent of their age.

We use a competing risks duration model framework to analyse the age effect on the transition intensity from unemployment into employment and non-participation. The age effect is modelled in a flexible way using splines in order to disentangle the true age-dependency of the hazard rate from the income effect. We perform a simulation exercise and a related Monte-Carlo study in order to assess the properties of the income effect and to investigate the sensitivity and properties of the estimators.

A 10% register data set of the Danish working age population has been used. The analysis is limited to individuals aged 21–29 when starting their unemployment assistance spell, and to the time period 1998–2001. Exploratory data analysis suggests that the unemployment to employment hazard rate is falling in age, but there is no distinct feature around age 25.

The econometric analysis confirms the preliminary findings. There is no significant discontinuity around age 25, though, as the Monte-Carlo analysis suggests, it may be too small to be detected on the current sample. However, there is a smooth negative effect for females, corresponding to indications from the Monte-Carlo simulation. No effect appears to be present for males. The parametric analysis, using the individuals who are responsible for children

as a control group, confirms these results. The income effect for males is positive though insignificant, the effect for females is negative, significant, and corresponds to an income elasticity of around -0.4.

Most of the other effects are as expected: more education is associated with faster transitions into employment, females with small children are hindered in finding employment, and there are certain regional differences in the labour market. The baseline hazards are increasing during the first months of the unemployment spell and roughly constant thereafter.

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