CentER

Discussion Paper

No. 2010–95

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August 2010

ISSN 0924-7815

Tilburg University

Are Short-Lived Jobs Stepping Stones to Long-Lasting Jobs?*

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August 13, 2010

Abstract

This paper assesses whether short-lived jobs (lasting one quarter or less *and* involuntarily ending in unemployment) are stepping stones to long-lasting jobs (enduring one year or more) for Belgian long-term unemployed school-leavers. We proceed in two steps. First, we estimate labour market trajectories in a multi-spell duration model that incorporates lagged duration and lagged occurrence dependence. Second, in a simulation we find that (fe)male school-leavers accepting a short-lived job are, within two years, 13.4 (9.5) percentage points more likely to find a long-lasting job than in the counterfactual in which they reject short-lived jobs.

Keywords: event history model, transition data, state dependence, short-lived jobs, stepping stone effect, long-lasting jobs.
JEL classification codes: C15, C41, J62, J64
Word count: 14,885

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*The authors acknowledge financial support from the Belgian Federal Science Policy Office (contract SO/10/073 and PAI P6/07) and intensive computing machines support from CISM, Université catholique de Louvain (FRFC 2.4502.05, *Simulation numérique. Application en physique de l'état solide, océanographie et dynamique des fluides*). Matteo Picchio is in addition grateful for the research grant he received from the Special Research Fund (FSR) of the Université catholique de Louvain. We also wish to thank Muriel Dejemeppe, Rafael Lalive, Louis-André Vallet, Bas van der Klaauw, Bruno Van der Linden, Ingrid Van Keilegom, two anonymous referees, and the participants to the XXII and XXIII Italian National Conferences of Labour Economics in Napoli and Brescia, to the COST A23 conference in The Haugue (October 2007), to the VIII Belgian Day for Labour Economists in Louvain-la-Neuve (June 2008), to the EALE conference in Amsterdam (September 2008), to the QMSS2 Seminar in Oslo (October 2008), and to seminar in Ghent University (November 2008) for their comments and suggestions.

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1 Introduction

Since the end of the 1980s, labour market regulations on temporary employment have been eased in many OECD countries, amongst which Belgium,[1] while leaving labour market protection for open-ended employment hardly affected (OECD, 2004). Research suggests that disadvantaged groups, such as youth, women, and long-term unemployed, excluded from employment by too strict regulations, may benefit most from this enhanced flexibility. There is, however, a debate on whether the increased availability of temporary jobs facilitates the integration of these disadvantaged groups in regular stable employment or spurs the development of a secondary labour market, in which the most vulnerable workers get trapped in a cycle between temporary dead-end jobs and unemployment. This article aims at providing more insights on this debate by analysing the labour market transitions of long-term unemployed school-leavers in Belgium.

Economic theory provides ambiguous predictions on this debate. On the one hand, accepting a short-term job may signal low ambition or skills reducing thereby the chances of conversion to a stable position. On the other hand, by accepting a short-term job a worker could also signal her motivation, acquire access to informal networks, and avoid deterioration of human capital, facilitating thereby the search for a longer lasting job.[2]

The degree to which permanent employment is protected may influence the stepping stone effect of temporary jobs, but again the theoretical predictions are ambiguous (Casquel and Cunyat, 2008; Ichino et al., 2008). On the one hand, the higher are firing costs for permanent jobs, the larger is the scope for using temporary jobs as a screening device, since firms attribute greater importance to the assessment of the quality of workers before locking themselves into an open-ended job relationship. On the other hand, higher firing costs may induce firms to use temporary workers as a mere flexibility buffer, if they make it difficult to adjust the number of regular employees during cyclical downturns.

Empirical evidence is more clearcut, at least if we restrict the analysis to the European labour market.[3] Studies in the UK, the Netherlands, Germany, Italy, Sweden, and

[1] In 1997 and 2002 restrictions on employment in temporary work agencies were reduced and fixed-term contracts were made renewable (OECD, 2004, pp. 119).

[2] Browning et al. (2007) argue that temporary jobs may finance consumption of liquidity constrained individuals during the subsequent unemployment spell. As such, the temporary job provides an opportunity to continue search for a "good" job. This argument is less important in Belgium, since nearly every unemployed individual is entitled to unemployment benefits.

[3] For the US, findings are mixed. Most studies find that temporary help agencies jobs lead to longer term employment and higher wages (e.g. Andersson et al., 2007; Addison and Surfield, 2009; Heinrich et al., 2009). On the basis of a social experiment, Autor and Houseman (2005) find instead that temporary help jobs generate lower earnings, less frequent employment, and potentially higher welfare recidivism over the next one to two years.

Belgium conclude that temporary employment, be it within fixed-term contracts or in temporary help agencies, are stepping stones to permanent employment.[4] In contrast, Amuedo-Dorantes et al. (2009) report that in Spain accepting employment in temporary help agencies reduces the probability of being hired on a permanent basis. This dissonant result may be due to the fact that the Spanish employment protection legislation for regular employment is one of the strictest in the EU (OECD, 2004): the use of temporary workers as a flexibility buffer rather than a screening device seems to dominate.

The existing empirical studies contain, however, some drawbacks that we try to address. First, these studies aim at determining whether or not initial job insecurity leads to more job security later on. However, being employed with an open-ended contract is an imperfect indicator of job security, since it does not guarantee that a job is *effectively* long-lasting. This depends on the institutional environment. For instance, in Belgium open-ended contracts always start with a trial period during which an employer can end a contract at low costs (maximum 7 days of notice payments).[5] In this research we therefore propose an alternative measure of job security that is based on the *effective* job tenure. In the empirical analysis discussed below, job security is attained if a worker finds a job that lasts one year or more.[6] We label these jobs as "long-lasting".

Another drawback of existing studies is that, depending on the institutional context, temporary contracts may be relatively long-lasting and the non-conversion to an open-ended job is not necessarily identified with a situation of precarious employment.[7] We therefore do not investigate the stepping stone effect of accepting a short-term employment contract,[8] but rather of accepting a "short-lived job", i.e. jobs lasting one quarter or less that are involuntarily interrupted.[9] By restricting our analysis to short-lived jobs, we aim at estimating a *lower bound* for the conversion rate of temporary jobs to

[4] See Booth et al. (2002), Zijl et al. (2004), Berton et al. (2007), Göbel and Verhofstadt (2008), Ichino et al. (2008), Picchio (2008), and Hartman et al. (2010). In Germany, Hagen (2003) finds supporting evidence for the stepping stone hypothesis of fixed-term contracts, but Kvasnicka (2009) can neither confirm nor deny this conclusion regarding employment in temporary help agencies.

[5] For white collar workers the trial period can last up to 6 months. For blue collar workers the trial period is much shorter (between 1 and 2 weeks), but their employment protection in open-ended contracts is also much weaker: maximum 35 days of notice payments if job experience is less than 5 years, while this is never less than 3 months for white collar workers.

[6] We will justify the choice of this threshold below.

[7] In the late 1990s, most of the OECD countries had no limits on the maximum duration and number of renewals of temporary jobs (OECD, 1999, § 2). In Belgium, a worker may conclude maximum four fixed-term contracts of a duration of minimum 3 months and maximum 2 years (OECD, 2007).

[8] We cannot identify the type of employment contract in the data.

[9] We identify an *involuntary* interruption by one that ends in insured unemployment. In Belgium, workers are not entitled to unemployment benefits if they voluntarily quit a job.

long-lasting jobs, taken as a measure of job security.[10] Moreover, by restricting to very short-lived jobs, we obtain more insight in the causal mechanism of the conversion rate to long-lasting jobs: signaling or informal networks are then more likely determinants than human capital formation, since the latter takes more time.

The estimation of the stepping stone effect of short-lived jobs is realized in two steps. First, we estimate the complete labour market trajectory of long-term unemployed school-leavers. Subsequently, we investigate whether workers who accept a short-lived job (the treated) are more or less likely to enter a long-lasting job (the successful outcome) than if they always rejected such jobs and continued job search until finding a more stable position (the counterfactual). This yields the average treatment effect on the treated (ATT).

Since the the treatment and the outcome are dynamic, the estimation of the ATT in the second step is not standard. In particular, the partial observation induced by the absorbing endogenous censoring state must be dealt with because the transition process from this absorbing state is not modelled.[11] We therefore propose a formal framework to define a conditional ATT (CATT), conditional on not being right censored. In addition, since the parametric model assumptions allow identification of the CATT at the individual level, we elaborate a simulation procedure to estimate not only the CATT, but also the complete distribution of individual treatment effects (Heckman et al., 1997).

The credibility of our approach hinges on the realism of the model in the first step. We distinguish between three states: insured unemployment, employment, and an absorbing censoring state for all remaining states.[12] We model the transitions between these states as well as job-to-job transitions, since job changing has been shown to be a critical component of young workers' movement to stable employment relations (Topel and Ward, 1992). In order to study the stepping stone effect of short-lived jobs, we allow for a complex form of state dependence in these transitions, depending not only on the duration in the current labour market state, but also on past history. This is realized by specifying a mixed proportional hazard (MPH) model with competing risks of exit, in which we allow the order, the type, and the duration of the previous spell to proportionally shift the baseline hazards.

Doiron and Gørgens (2008) formulate a similar event history model with lagged state dependence to evaluate policy interventions on a sample of Australian school leavers.

[10]To the extent that job-to-job transitions are voluntary, one could argue that we should consider *employment* security instead of *job* security as an indicator of success. However, sticking to an indicator of job security should provide us with a conservative measure of employment security.

[11]Exogenous right censoring is less problematic, since then subsequent transitions of censored individuals follow the same random process as for uncensored individuals. However, we avoid simulations beyond the point of exogenous censoring (at the end of 2001) to avoid extrapolations beyond the observation period.

[12]We model the exit to all remaining states as absorbing to reduce the computational problem.

Their analysis differs from ours essentially in the following respects. First, Doiron and Gørgens (2008) specify lagged occurrence and lagged duration dependence as the cumulative number and duration of spells since first entry in the labour force. Since we are not aware of any identification result for such a model, we prefer to stick to a model with the aforementioned restrictions on the dynamics, but identified under known assumptions.[13] Second, Doiron and Gørgens (2008) explicitly model transitions from out of the labour force, whereas we treat this labour market state as an absorbing endogenous censoring state. However, since we focus on understanding the process of transition to a stable job, we allow instead for job-to-job transitions. Thirdly, since they have much preciser information on the timing of events, they analyze the data in continuous time, whereas we model the time-grouping of our data in quarterly intervals. A final major difference concerns the simulation exercise. Doiron and Gørgens (2008) simulate, assuming a stationary environment, four types of *counterfactual* treatments (policies) on particular representative persons and consider the policy impact on the dynamics of the three considered labour market states. In contrast, we do not assume stationarity and focus on the evaluation of a *realized* treatment, i.e. having accepted a short-lived job, on a specific outcome, entering a long-lasting job. In addition, we deal with an initial condition problem and, as already mentioned, modify the definition of the treatment effect as a consequence of the presence of an endogenous absorbing censoring state. Finally, we improve upon Doiron and Gørgens (2008) by constructing 95% confidence intervals that account for the precision of the estimator and by not only simulating the average treatment effect, but also its distribution.

The paper set-up is as follows. Section 2 describes the data. Section 3 discusses the econometric model. The estimation results are presented in Section 4. Section 5 provides the formal framework that defines the CATT and reports the findings of the simulation exercise. Section 6 concludes.

2 The Data

The empirical analysis is conducted on administrative records gathered by the Crossroads Bank for Social Security (CBSS).[14] The CBSS merges data from the different Belgian Social Insurance institutions and allows thereby to construct the quarterly labour market history of all Belgian workers. The sample contains all Belgian school-leavers, aged between 18 and 25 years, who, in 1998, were still unemployed nine months after graduation. In Belgium, after this "waiting period" of nine months, school-leavers are entitled

[13] Horny and Picchio (2010) show under which assumptions all the parameters of our model are nonparametrically identified.

[14] See http://www.ksz.fgov.be/En/CBSS.htm.

to unemployment benefits (UB) and, as a consequence, show up for the first time in the administrative records of the CBSS. By sampling from a population of school-leavers we drastically simplify initial conditions problems in the analysis of lagged labour market dependence, since nobody in the sample had any labour market experience prior to the sampling date. Nevertheless, the fact that all sampled individuals have been unemployed for nine months since graduation does complicate the analysis. We will discuss in Subsection 3.3 how we deal with this complication.

The sample contains 8,921 women and 6,627 men. The quarterly (un)employment history of these workers can be reconstructed for a period of (maximum) four years, from the beginning of 1998 until the end of 2001. In the analysis we distinguish between three mutually exclusive labour market states occupied at the end of each quarter: unemployed as UB recipient (u), employed (e), and an absorbing censoring state (a). This censoring state is accessed if the individual leaves the labour force, enters a training programme, returns to school, or is sanctioned and looses her entitlement to UB. We consider five possible transitions between these states: (u, e), (u, a), (e, u), (e, a) and, since the data contain a firm indicator, job-to-job transitions (e, e).

Table 1 reports descriptive statistics on the number of observed spells at individual level. Individuals occupy on average 2.5 different labour market states during the four year observation window and a maximum of 12. This multi-spell information is exploited to infer the impact of the lagged labour market outcomes on the subsequent transition intensities.

Table 1: Individual Observed Spells by Gender

	Male		Female	
# of spells (Unemployment + job spells)	Absolute frequency[(a)]	Relative frequency[(b)]	Absolute frequency[(a)]	Relative frequency[(b)]
1	6,627	100.0%	8,921	100.0%
2	3,901	58.9%	4,405	49.4%
3	2,552	38.5%	2,917	32.7%
4	1,513	22.8%	1,755	19.7%
5	876	13.2%	1,028	11.5%
6	492	7.4%	577	6.5%
7	265	4.0%	326	3.7%
8	132	2.0%	181	2.0%
9	70	1.1%	95	1.1%
More than 9	49	0.8%	70	0.8%
Total observed spells	16,477		20,275	
Average spells per individual	2.49		2.27	
Maximum number of individual spells	12		12	

[(a)] Number of individuals who experience the corresponding number of different spells.
[(b)] This frequency is the ratio of the number of individuals who experience the corresponding number of different spells to the total number of individuals in the sample.

Table 2 displays summary statistics of the covariates used in the analysis. They can be decomposed into three groups: time-invariant covariates fixed at the sampling date; spell specific variables fixed at the value attained at the start of the corresponding spell, but varying across spells; time-varying covariates which values can change every quarter.[15]

Nationality, region of residence, and education are the time-invariant covariates. Since the sample consists of long-term unemployed, sections of the population with a high unemployment risk are more represented in the sample than in the population as a whole: foreigners, low schooled youth and, since the unemployment rate in Flanders is much lower, those living in Wallonia and Brussels.[16]

The set of spell specific explanatory variables contains age, quarter of entry into the spell, household position, the monthly amount of UB, and a set of sector and firm size indicator variables. Two variables are conditioned upon in the empirical analysis, but not reported in Table 2 since their value is zero at the sampling date: the length of the previous labour market spell and an indicator for the previous spell type.

We distinguish between three types of household positions: head of household, single or cohabitant. These categories determine together with age the level of the flat UB rate to which the unemployed school-leavers are entitled to after the higher mentioned waiting period.[17] The majority of the sampled individuals (79%) reside in households in which another adult is the head of the household, "cohabitants" from now onwards.

School-leavers who worked at least one year during a time window of 18 months become entitled to a higher UB if they are laid off. If one is not the head of the household, these higher UB drop to a lower level after one year. We therefore include the monthly UB level as an explanatory variable and for those individuals for whom the UB may decline (the dummy *declining benefits*) four time-varying indicator variables as in Meyer (1990) (not reported in Table 2). If τ denotes the number of quarters remaining before UB fall to a lower level, we define these variables as follows: $\forall t \in \{1, 2, 3, 4\} : UI\ t = 1$ if $\tau \leq t$, and 0 otherwise. Note that that we include these variables to control for the (time-varying) heterogeneity induced by these features of the benefit scheme. Their coefficients cannot therefore be given a structural interpretation. For instance, since the UB is positively related to past wages (which are not modeled), the level of UB may reflect unobserved productivity rather than a disincentive for work.

We distinguish between five firm size types on the basis of the number of employees. Almost one half of those who find a job are employed in large firms and more than one

[15]The statistics of spell specific and time-varying explanatory variables are reported at the sampling date, except for the firm characteristics, which are reported at the start of the first job.

[16]Belgium is divided into three regions: Flanders, Wallonia, and Brussels.

[17]In 2000, the monthly benefit level varied between 307€ for cohabitants (more than 18 years old) not in charge of other members in the household and 790€ for household heads.

Table 2: Summary Statistics by Gender

	Male		Female	
	Mean	St.Dev.	Mean	St.Dev.
Time-invariant covariates				
Nationality				
Belgian	.891	.312	.879	.326
Non-Belgian EU	.052	.222	.054	.226
Non EU	.057	.232	.067	.250
Education				
Primary (6 to 9 years of schooling)	.121	.326	.079	.269
Lower secondary (9 to 12 years)	.280	.449	.226	.419
Higher secondary (12 to 16 years)	.422	.494	.481	.500
Higher education (16 years or more)	.126	.332	.173	.378
Other	.009	.094	.008	.088
Unknown	.042	.201	.033	.178
Region of residence				
Flanders	.201	.401	.245	.430
Wallonia	.674	.469	.641	.480
Brussels	.125	.331	.114	.317
Time-variant spell-specific covariates at sampling date				
Age	20.5	1.96	20.4	1.97
Monthly unemployment benefits (in €)	332.9	120.5	344.0	139.0
Quarter of entry				
January-February-March	.081	.273	.071	.258
April-May-June	.660	.474	.689	.463
July-August-September	.166	.372	.164	.371
October-November-December	.093	.290	.076	.264
Household Position				
Head of household	.077	.266	.108	.311
Single	.134	.341	.101	.302
Cohabitant	.789	.408	.791	.407
Firm size[(a)]				
$[1, 20)$ employees	.272	.445	.254	.435
$[20, 50)$ employees	.063	.243	.071	.257
$[50, 100)$ employees	.044	.205	.044	.205
$[100, 500)$ employees	.135	.342	.142	.349
500 or more employees	.486	.500	.489	.500
Sector[(a)]				
Agriculture	.029	.168	.018	.133
Industry & Mining	.086	.281	.039	.193
Building & Energy	.082	.274	.011	.103
Wholesale & Retail trade	.164	.370	.183	.387
Credit & Insurance	.014	.119	.017	.130
Business services	.420	.494	.343	.475
Other services & Public admin.	.205	.403	.390	.488
Time-variant covariates at sampling date				
Local unemployment rate	.184	.069	.269	.085
Observations	6,627		8,921	

[(a)] Sector and firm size figures refer to the start of the first job spell.

quarter in small firms. On the basis of the 2-digit NACE nomenclature,[18] we distinguish between seven firm sectors. Most of the workers are employed in "Business services". This sector comprises several types of services provided to firms: cleaning, call-centre activities, labour recruitment, counselling, advertising, and accounting.

Finally, in order to take into account the state of the labour market, the local unemployment rate is modelled as a time-varying explanatory variable. Since in Belgium no statistic exists on the local unemployment rate following the standard ILO definition, we rely on a non-standard statistic provided by the Belgian Unemployment Agency (ONEM): the fraction of the population insured against the risk of unemployment (thereby excluding civil servants) which is entitled to UB. At the sampling date, the average local unemployment rate for men and women is 18.4% and 26.9%, compared to 7.7% and 11.6% according to the ILO definition (http://epp.eurostat.ec.europa.eu).

3 The Econometric Model

The analysis is conducted in two steps. This section focuses on econometric issues of the first step: the specification of a multi-state multi-spell duration model in which the nature and the duration of the labour market state occupied in the past are allowed to influence the duration of stay in the current state. In a second step (Section 5), we run a microsimulation exercise on this econometric model to identify whether short-lived jobs are stepping stones to long-lasting jobs.

3.1 The Specification of Transition Intensities

Since we only observe the labour market state occupied at the end of each quarter, the observed data are grouped in discrete time intervals. To avoid the dependency of parameters to the time unit of observation (Flinn and Heckman, 1982), we follow van den Berg and van der Klaauw (2001) and specify the discrete-time process as in a grouped continuous-time model. If $s = 1, \ldots, S$ denotes the spell order, counting the number of times a particular labour market state (u or e) is occupied, the transition intensity in spell s from the origin state j to the destination state k is denoted by θ^s_{jk}, with the ordered pair $(j, k) \in \mathscr{X} = \{(u, e), (u, a), (e, e), (e, u), (e, a)\}$. During spell s started at time τ_s (with $\tau_s \in \mathbb{N}_0$) and after t_s quarters in state j (with $t_s \in \mathbb{N}_0$), the transition intensity from j to k

[18] See http://ec.europa.eu/comm/competition/mergers/cases_old/index/nace_all.html for a detailed list of NACE codes. The data do not allow to further decompose this sectoral information.

is specified in the following MPH form:

$$\theta_{jk}^{s}(t_s|\mathbf{x}_{jk}(\tau_s+t_s),v_{jk}) = \exp\left[\gamma_{jk}(t_s)+\boldsymbol{\beta}_{jk}'\mathbf{x}_{jk}(\tau_s+t_s)\right]v_{jk}^{s} \tag{1}$$

for $(j,k) \in \mathscr{X}$, where $\exp\left[\gamma_{jk}(t_s)\right]$ is the piecewise constant baseline hazard capturing the duration dependence; v_{jk}^{s} is the spell- and transition-specific individual heterogeneity, a positive random number; $\mathbf{x}_{jk}(\tau_s+t_s)$ is a K_j dimensional vector of time-invariant and time-variant covariates controlling for observed heterogeneity at the transition quarter (τ_s+t_s) and including the length and type (unemployed or not) of the preceding labour market spell. The associated and conformable parameter vector to be estimated is $\boldsymbol{\beta}_{jk}$.

In addition, we impose restrictions on the form of the hazard across spells. $\gamma_{jk}(t_s)$ and $\boldsymbol{\beta}_{jk}$ are fixed across spells and the order s of the spell just shifts the baseline transition intensity proportionally across subsequent spells or even not at all if the absorbing censoring state (a) is the destination:[19]

(i) $v_{ja}^{s} = v_{ja}$ for $j=u,e$ and $s=1,\dots,S$;

(ii) $v_{jk}^{s} = v_{jk}c_{jk}^{s}$, for $(j,k) \in \{(u,e),(e,e),(e,u)\}$, where $c_{jk}^{s} = c_{jk}^{3}$ for $s=4,\dots,S$. The scaling factors c_{jk}^{1} are normalized to 1 for each $(j,k) \in \{(u,e),(e,e),(e,u)\}$.

To avoid parametric assumptions on the distribution of the unobserved heterogeneity, we follow Heckman and Singer (1984) and assume that the vector $\mathbf{v} \equiv [v_{ue}, v_{ua}, v_{ee}, v_{eu}, v_{ea}]$ is a random draw from a discrete distribution function with a finite and (*a priori*) unknown number M of points of support. The probabilities associated to the mass points sum to one and, $\forall\, m=1,\dots,M$, are denoted by

$$p_m = \Pr(v_{ue}=v_{uem}, v_{ua}=v_{uam}, v_{ee}=v_{eem}, v_{eu}=v_{eum}, v_{ea}=v_{eam}) \equiv \Pr(\mathbf{v}=\mathbf{v}_m)$$

and specified as logistic transforms:

$$p_m = \exp(\lambda_m) \Big/ \sum_{g=1}^{M} \exp(\lambda_g) \quad \text{with} \quad m=1,\dots,M \quad \text{and} \quad \lambda_M = 0.$$

A pre-specified low number of points of support may result in substantial bias. We therefore choose, as suggested by Gaure et al.'s (2007) Monte Carlo simulations, the M points of support that minimize the Akaike Information Criterion (AIC).

[19] Note that, by way of this factor of proportionality *and* the type (unemployed or not) of the preceding labour market spell, we specify lagged *occurrence* dependence.

3.2 Identification

The selection on unobservables is controlled for on the basis of a discrete distribution with an unknown number of mass points in which the correlation structure is completely flexible (Heckman and Singer, 1984). Horny and Picchio (2010) prove that, if one imposes the MPH structure, the heterogeneity distribution is non-parametrically identified together with the structural parameters of the model, including the lagged dependences. Moreover, since in this empirical application we observe multiple spells, we impose restrictions on the specification of the hazard across spells,[20] and we condition on exogenous time-varying explanatory variables, we speculate on the basis of the existing literature (Brinch, 2007; Gaure et al., 2008)[21] that the model is over-identified and that identification is therefore not only achieved by the proportionality assumption.[22]

One may find that the MPH specification is overly restrictive, since one could argue that the (lagged) occurrence and duration dependence varies with observed characteristics, such as education. However, we are unaware of any identification result that allows for such interactions.

3.3 The Likelihood Function

In the derivation of the likelihood we first ignore the initial conditions problem and assume that the sample is drawn at the start of the unemployment spell right after graduation. In a second step, we explain how what we should modify to take into account that all sampled individuals have already been uninterruptedly unemployed during three quarters.

We start with the derivation of the individual contributions of each spell to the likelihood function. The contribution of a spell s with origin state j that is incomplete because right censored at the end of the observation period is simply given by the survivor function

[20] The model would be identified even if all transition intensities were spell-specific. We only allow for a proportional shift across spells.

[21] See also Bhargava (1991) and Mroz and Savage (2006) who discuss how the time-variation of exogenous variables helps to identify the causal impacts of endogenous variables in dynamic discrete time panel data models.

[22] A concern nevertheless remains, since the higher mentioned identification result assumes that the data are measured in continuous time, whereas our data are quarterly. Gaure et al. (2007) however report from an extensive Monte Carlo analysis that, in practice, the discrete-time measurement still allows to robustly recover the true structural parameters from the observed data, to the extent that the discreteness of data measurement is explicitly taken into account in setting up the likelihood function. This is what we do in Subsection 3.3.

in that state until the end of the observation period:

$$L_{is}^{c}(t_s|\mathbf{x}_{ijk},\mathbf{v}_j^s;\Theta_j) \equiv S_j(t_s|\mathbf{x}_{ijk},\mathbf{v}_j^s)=\prod_{d=1}^{t_s}\exp\{-\sum_{(j,k)\in\mathscr{J}}\theta_{jk}^s(\tau|\mathbf{x}_{ijk}(\tau_s+d),v_{jk}^s)\}, \quad (2)$$

where $\mathscr{J}=\mathscr{E}\equiv\{(e,e),(e,u),(e,a)\}$ if $j=e$ and $\mathscr{J}=\mathscr{U}\equiv\{(u,e),(u,a)\}$ if $j=u$, Θ_j is the set of parameters if the origin state is j, and $\mathbf{v}_j^s$ collect the v_{jk}^s's with $(j,k)\in\mathscr{J}$.

Using the same notation, the contribution to the likelihood function of a complete spell s with origin state j and destination state k is derived in Appendix A-1 and takes the following form:

$$L_{is}(t_s|\mathbf{x}_{ijk},\mathbf{v}_j^s;\Theta_j)=\frac{\theta_{jk}^s(t_s|\mathbf{x}_{ijk}(\tau_s+t_s),v_{jk}^s)}{\sum_{(b,c)\in\mathscr{J}}\theta_{bc}^s(t_s|\mathbf{x}_{ibc}(\tau_s+t_s,v_{bc}^s)}\left[S_j(t_s-1|\mathbf{x}_{ij},\mathbf{v}_j^s)-S_j(t_s|\mathbf{x}_{ij},\mathbf{v}_j^s)\right]. \quad (3)$$

Conditional on the unobserved covariates, individual i's contribution to the likelihood function is given by the product over the individual i's single spell contributions. Let $L_i(\mathbf{t}_i|\mathbf{x}_i,\mathbf{v};\Theta)$ denote this product, where $\mathbf{t}_i$ collects all the individual i's labour market durations and $\mathbf{x}_i$ and $\mathbf{v}$ the associated set of observed and unobserved covariates. Integrating out the unobserved heterogeneity $\mathbf{v}$ on the basis of the above-mentioned discrete distribution yields the unconditional individual contribution to the likelihood function:

$$L_i(\mathbf{t}_i|\mathbf{x}_i;\Theta)=\sum_{m=1}^{M}p_m L_i(\mathbf{t}_i|\mathbf{x}_i,\mathbf{v}_m;\Theta). \quad (4)$$

The log-likelihood function sums the logarithm of this expression over all the individuals in the sample.

We deal with the aforementioned initial conditions problem by following the conditional likelihood approach proposed by Ridder (1984).[23] If the probability of becoming unemployed after graduation is proportional in observed and unobserved explanatory variables, the individual contribution in (4) just needs to be divided by the probability of surviving three quarters in unemployment averaged over the unobserved heterogeneity distribution:

$$L_i^0(\mathbf{t}_i|\mathbf{x}_i;\Theta)=\frac{L_i(\mathbf{t}_i|\mathbf{x}_i;\Theta)}{\sum_{m=1}^{M}p_m S_u(3|\mathbf{x}_{iu},\mathbf{v}_u)}. \quad (5)$$

[23]We prefer this approach to the approximate solution suggested by Heckman (1981) for dynamic discrete choice models and implemented by Gritz (1993) in a duration model, since one then looses the structural interpretation of the parameters regarding the first transition from unemployment. Note that neither model can be rejected against the other by a Vuong (1989) likelihood ratio test of strictly non-nested models. These findings can be obtained from the authors on request.

4 Estimation Results

We focus now on those factors that matter for the determination of the stepping stone effect of short-lived jobs: current duration dependence and lagged occurrence and duration dependence of the transitions between the two labour market states of interest: u and e.[24]

The model we report here is the one in which lagged duration dependence is log-linear. We could not reject this specification against a model in which short-lived jobs affect subsequent transitions differently than long-lasting jobs. [25]

4.1 Current Duration Dependence

Figure 1 displays the patterns of the duration dependence of three transition rates: (u, e), (e, e) and (e, u). The baseline transition from unemployment to employment, reported in the upper panel, exhibits strong negative duration dependence up to the 7^{th} quarter and is roughly constant thereafter.[26] This negative duration dependence can be caused by a number of factors: loss of skills (Pissarides, 1992), stigma effects (Vishwanath, 1989; Jackman and Layard, 1991), or a hiring strategy in which workers are ranked according to unemployment duration (Lockwood, 1991; Blanchard and Diamond, 1994).

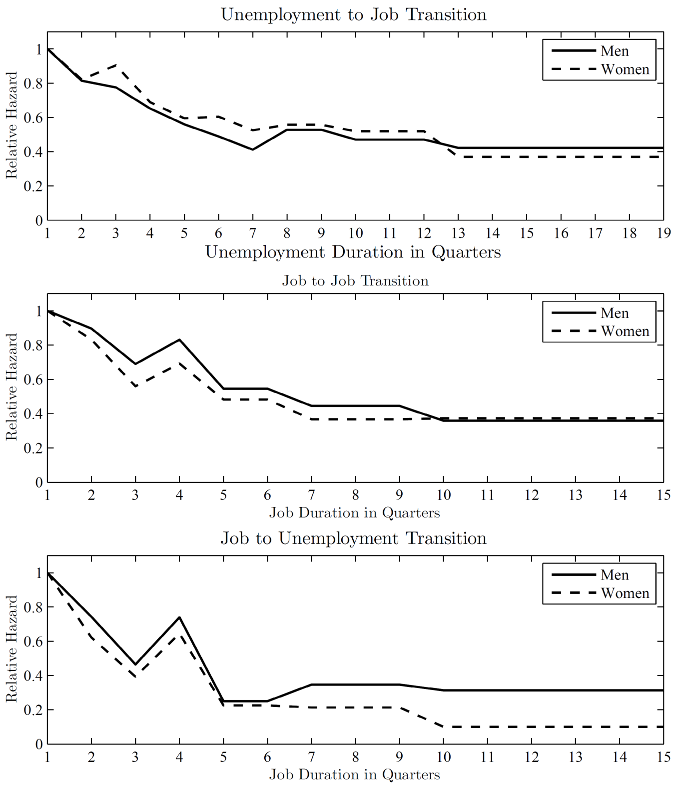
The bottom panels of Figure 1 depict the duration profiles of (e, e) and (e, u) transition intensities. Both display that the job separation rate declines with tenure, a finding that is consistent with the central facts about job mobility (e.g. Topel and Ward, 1992; Farber, 1999). The spike in the fourth quarter is probably related to the non-renewal of temporary contracts. The transition rate to unemployment declines more and much faster than the job-to-job transitions. It stabilizes after 5 quarters, whereas the job-to-job transitions continue to decline gradually. This means that dismissals essentially occur during the first year, whereas job changes are spread out over a longer time span. The evidence of the spikes and of dismissals essentially occurring during the first year supports our choice of defining long-lasting jobs as jobs lasting one year or more.

[24]Since the parameters determining the transition intensities to the absorbing censoring state a are not of direct interest, they are not reported for the sake of brevity. They are available from the authors on request.

[25]If we specify the lagged duration dependence as piecewise log-linear with two knots, the first one at two quarters and the second one at one year of lagged duration, we cannot reject a constant slope, neither for men (p-value=0.877) nor for women (p-value=0.120).

[26]This contrasts with the results of Cockx and Dejemeppe (2005), who cannot reject for Belgian men aged 28 years or younger a constant profile of the baseline hazard rate from unemployment. However, this finding could follow from their incapacity to distinguish between transitions to employment and to other destinations and from not having access to multiple spells.

Figure 1: Estimated Baseline Hazards by Gender



4.2 Lagged Occurrence and Lagged Duration Dependence

Lagged Unemployment Dependence

Do the lagged occurrence and lagged duration of unemployment generate any scarring effect (Arulampalam et al., 2001; Gregg, 2001)? The occurrence of unemployment may indeed signal low productivity, making employers more reluctant to hire and more inclined to dismiss workers (Gibbons and Katz, 1991). This negative effect may exacerbate with unemployment duration if liquidity constraints increase the pressure of accepting bad job matches and if skills depreciate progressively throughout the unemployment spell.[27] In contrast, Ehrenberg and Oaxaca (1976) argue that a longer job search may improve the quality of the match, dissolving less rapidly as a consequence.

According to the empirical findings regarding lagged duration dependence, it is the second effect that dominates. Table 3 shows that for women increasing the length of the previous unemployment spell by one quarter reduces the transition intensity from employment to unemployment by 3% and the job-to-job transition intensity by 4%. For men, only the job-to-job transition rate falls significantly by 3% per quarter of unemployment.

Therefore, if unemployment imposes a scar, it is related to its occurrence, not to its duration. The past occurrence of unemployment imposes a scar only by raising the transition rate from employment to unemployment, not by affecting job-to-job transition intensities. A young man (woman) who entered a job from unemployment instead of from another job is 27% (35%) more likely to be dismissed.[28]

Lagged Job Dependence

The hiring rate increases dramatically with the order of the unemployment spell. As compared to the hiring rate in the first unemployment spell, the hiring rate in the second unemployment spell is 38% higher for young men and even 75% higher for young women; in the third and the subsequent unemployment spells the hiring rates for men and women increase even by 70% and 98%, relative to the first spell.[29] We regard this as evidence that, for the labour market integration of long-term unemployed youth, it is essential to acquire work experience, however short. The fact that this effect is conditional on the duration of

[27]Note that the threat of benefit exhaustion may be an alternative explanation for the scarring effect of unemployment duration (Belzil, 2001; Tatsiramos, 2008), but since in Belgium benefit entitlement is indefinite, this factor is not relevant here.

[28]$27 \approx [\exp(.237) - 1] \cdot 100$ and $35 \approx [\exp(.302) - 1] \cdot 100$. Note that the effect for men is only significant at 10%.

[29]These figures are obtained from the point estimates of $\ln c^s_{ue}$ for $s = 2, 3$ reported in column (1) of Table 3 and calculated as in the previous footnote.

Table 3: The Impact of the Past on Transition Intensities

Transition	(u,e)		(e,e)		(e,u)	
	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
			Men			
Lagged unemployment duration	–	–	-.031**	.014	-.019	.013
Previous state: unemployment	–	–	-.146	.100	.237*	.130
Lagged job duration	-.004	.023	-.028	.023	-.127***	.037
Scaling factors[(a)] – The $\ln c^1_{jk}$s are normalized to zero						
$\ln c^2_{jk}$	.319***	.102	-.161	.129	-.005	.142
$\ln c^3_{jk}$	.532***	.113	-.030	.106	-.362***	.106
# of individuals	6,627		# of spells		16,447	
# of parameters	191		Log-likelihood		-41,174.1	
			Women			
Lagged unemployment duration	–	–	-.041***	.015	-.030**	.012
Previous state: unemployment	–	–	-.017	.104	.302**	.120
Lagged job duration	-.032*	.019	-.042**	.020	-.063**	.029
Scaling factors[(a)] – The $\ln c^1_{jk}$s are normalized to zero						
$\ln c^2_{jk}$	.560***	.100	-.044	.124	-.251*	.134
$\ln c^3_{jk}$	.683***	.109	-.109	.101	-.319***	.094
# of individuals	8,921		# of spells		20,275	
# of parameters	197		Log-likelihood		-51,269.2	

Notes: * Significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

[(a)] The scaling factors c^s_{jk} were introduced in Subsection 3.1. The scaling factors c^s_{jk} for $s = 2, 3$ are proportional shifts of the corresponding (j, k) transition intensities across subsequent spells, with the first unemployment and job transition intensities as references, i.e. $c^1_{jk} = 1$. For instance, $\ln c^2_{ue}$ is the shift of the hiring rate after the first job with respect to the hiring rate after graduation ($\ln c^1_{ue}$ is normalized to zero); $\ln c^3_{ue}$ is the shift of the subsequent hiring rates with respect to the hiring rate after graduation.

the past work experience suggests that there is a signalling mechanism at work: accepting a job could signal higher productivity, through, e.g., a higher motivation to work. Even if these findings demonstrate that the first work experience dramatically facilitates the subsequent job finding, they also suggest that the positive effect of work experience may fade away as youth acquires more of it. The hiring rate increases at a decreasing rate: moving from no work experience to a first job boosts the subsequent hiring rate by more than moving from the first job to the second one.

Past work experience does not only have a strong positive impact on the hiring rate. It also reinforces tenure in the subsequent job, at least from the third spell onwards. The transition rate to unemployment falls indeed by 30% for men and by 27% for women.[30]

Not only the occurrence, but also the length of the previous job may affect both future labour market transitions. On the one hand, the longer one has been employed in the past, the more time the worker has had to integrate in informal networks enhancing job finding (Ioannides and Loury, 2004) and to acquire transferable skills reinforcing the willingness of subsequent employers to hire and retain these workers. On the other hand, the longer a worker has been occupied by the same employer, the more specific human capital is lost at lay off. Consequently, the productivity of these workers in a new job may not match the higher reservation wage that these workers set to avoid a drop in their earnings (Ljungqvist and Sargent, 1998). This slows down the re-employment rate.

From Table 3 we infer that the length of the previous job does not affect the transition rate from unemployment to employment at a significance level of 5%. In contrast, the length of the previous job increases current job duration: increasing lagged job tenure by one quarter decreases the current job-to-unemployment transition intensity by 12% for men and 6% for women. For women, but not for men, this also decreases the job-to-job transitions by 4%. These findings suggest that throughout a job spell workers accumulate both specific and general skills. These factors roughly balance out in determining the speed at which a subsequent job is found. However, once the transition to a new job has been made, it is the impact related to the accumulation of general skills that matters.

4.3 Summary and the Stepping Stone Hypothesis

In a similar study on Australian youth, Doiron and Gørgens (2008) find evidence for occurrence dependence, but not for lagged duration dependence. An additional spell of employment (unemployment) increases the probability of being employed (unemployed) in the future and the size of these effects are of similar magnitude. In this study we broadly find that the direction of these lagged occurrence dependence effects are also

[30]These figures are obtained from the point estimates of $\ln c_{eu}^{3}$ reported in column (5) of Table 3.

present, but the positive impact of past employment is larger in magnitude than that of past unemployment.

In Belgium not only the occurrence of past labour market states matter. In addition, both lagged unemployment duration and lagged job tenure influence current transition intensities, be it modestly. Hence, in contrast to the Australian labour market, the depreciation and accumulation of human capital during, respectively, unemployment and employment cannot be discarded as explanatory factors of labour market transitions realized by Belgian youth.

In regard to the stepping stone hypothesis of short-lived jobs, we find contrasting forces, some in support of the hypothesis others against it. On the one hand, the pattern of lagged duration dependence contradicts it. By accepting a short-lived job the worker forgoes the opportunity of improving the job match by extending job search: the longer one has been unemployed in the past, the more likely that the current job lasts longer. On the other hand, the baseline transition intensity from unemployment to employment exhibits clear negative current duration dependence (see Figure 1). Consequently, by currently rejecting a short-lived job offer, the worker reduces her chances of finding a job later on. In addition and more importantly, past job experiences boost quite dramatically the job finding rate in the subsequent unemployment spells. This suggests that for youth the conversion rate to a long-lasting job crucially depends on the speed at which one acquires the first work experience. However, our findings also suggest that, in line with those of Gagliarducci (2005) and García Pérez and Muñoz-Bullón (2007), once this first work experience has been acquired, the stepping stone effect decreases with the number of job experiences and job interruptions.[31]

4.4 Other Coefficients

Finally, we briefly comment on the estimated coefficients of the other explanatory variables reported in Tables 4 and 5. First, the estimated coefficients of most of the observed explanatory variables are in line with expectations.[32] Second, the estimated probability masses and the location of each mass point suggest an important diversity in the impact of unobserved characteristics on the transition intensities. The discrete distribution function of the random variable $\mathbf{v}$ is found to have 4 probability masses for men and 5 for

[31]The observation period in this study is, however, too short to verify whether there exists a threshold number of work experience beyond which short-lived jobs are no longer springboards to long-lasting jobs.

[32]Recall that the coefficients of the UB variables cannot be given a structural interpretation (cf. Section 2)

women.[33]

5 Simulations

The parameter estimates discussed in the previous section did not allow us to formulate a definite conclusion on our main research question: are short-lived jobs stepping stones to long-lived jobs? In this section we provide an answer and a quantification of the effect on the basis of simulations. As the reliability of these simulations depends on the capacity of our event history model to predict the realized labour market transitions, we first report goodness-of-fit checks of the estimated model. Subsequently, since, as explained in the introduction, the presence of an absorbing censoring state requires a non-standard definition of the ATT, we propose a formal framework in which we define the conditional ATT (CATT), conditional on not being right censored and show how this is related to the conventional ATT. Finally, we report the CATT and its distribution as estimated on the basis of simulations. The different steps involved in the simulations are documented in Appendix A-2 and A-3.

5.1 Goodness-of-fit

To goodness-of-fit statistics of the model are constructed on the basis of simulations of 999 labour market histories for each individual in the sample. Replicating the simulation 999 times allows us to construct empirical 95% confidence intervals on the empirical distribution of the duration in each labour market state (u or e) and of the transitions to the possible destination states (u, e or a). The goodness-of-fit can then easily be checked by verifying whether the observed frequencies lie within the confidence intervals of the simulated ones.

The first panel of Table 6 contrasts the actual unemployment and job duration frequencies with the simulated counterparts and reports simulated confidence intervals. The model fits very well the observed frequencies of the duration distributions, in particular for women. For men, the model tends to slightly overpredict short unemployment spells and slightly underpredict long ones. The second panel of Table 6 reports the fit with respect to the destination states. The model performs less, but still reasonably well, in predicting the fractions entering a particular destination state. The job-to-job transitions are somewhat

[33]For men, the last point of support has a very small probability mass, but, since it is associated with very different (much larger) points of support in the (e,e) and (e,u) transitions, it is this specification that minimizes the AIC. Nevertheless, it should be noted that the parameters of interest are not sensitive to whether we choose the model with 3 or 4 points of support.

Table 4: Estimation Results of Systematic Parts and Individual Heterogeneity Distribution – Men

Transition	(u, e)		(e, e)		(e, u)	
Variable	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Time-invariant covariates						
Nationality - Reference: Belgian						
Non-Belgian EU	-.037	.083	.003	.122	.121	.110
Non EU	-.110	.079	.089	.124	.264**	.114
Education - Reference: Higher secondary						
Primary school	-.711***	.068	-.049	.106	.701***	.114
Lower secondary	-.503***	.052	-.048	.070	.477***	.083
Higher education	.448***	.085	.209**	.087	-.395***	.115
Other	-.666***	.194	-.211	.343	.524**	.246
Unknown	1.381***	.133	.138	.287	-3.363***	.338
Region of residence - Reference: Wallonia						
Flanders	.312***	.083	.343***	.098	-.017	.120
Brussels	.073	.061	-.172*	.092	-.071	.083
Time-variant spell-specific covariates						
Age	-.024**	.012	-.052***	.016	-.016	.017
Household position - Reference: Cohabitant						
Head of household	-.082	.149	-.021	.102	.339***	.099
Single	-.140**	.061	.104	.069	.400***	.074
Quarter of entry in the spell - Reference: April-May-June						
January-February-March	-.055	.059	.028	.071	.355***	.076
July-August-September	-.118**	.054	.014	.067	.234***	.075
October-November-December	-.189***	.058	-.050	.071	.215***	.076
Firm size - Reference: 500 or more employees						
$[1, 20)$ employees	–	–	-.200***	.063	-.335***	.066
$[20, 50)$ employees	–	–	-.217**	.094	-.268***	.100
$[50, 100)$ employees	–	–	-.268**	.119	-.218*	.122
$[100, 500)$ employees	–	–	-.206***	.072	-.241***	.076
Sector - Reference: Business services						
Agriculture	–	–	-.624***	.182	.400***	.141
Industry & Mining	–	–	-1.152***	.089	-.812***	.094
Building & Energy	–	–	-.888***	.092	-.994***	.110
Wholesale & Retail trade	–	–	-1.119***	.076	-.923***	.077
Credit & Insurance	–	–	-1.048***	.194	-1.177***	.272
Other services & Pub. Adm.	–	–	-1.430***	.078	-.912***	.076
Log unemployment benefits	-.467***	.131	–	–	–	–
Declining benefits	.246	.362	–	–	–	–
Time-variant covariates						
Local unemployment rate	-1.440***	.407	.238	.572	1.323**	.628
Quarters away of a decline in the unemployment benefit amount						
UI 4	-.075	.371	–	–	–	–
UI 3	.127	.191	–	–	–	–
UI 2	-.294	.278	–	–	–	–
UI 1	.434	.360	–	–	–	–
Individual heterogeneity distribution, $M = 4$						
Points of support						
$\ln v_{jk1}$	.183	.225	-1.146***	.224	-2.639***	.280
$\ln v_{jk2}$	-.797***	.262	-1.477***	.353	-.504*	.273
$\ln v_{jk3}$	.301	.215	-.504**	.198	-1.299***	.237
$\ln v_{jk4}$	-.258	.260	.874	.558	.925*	.482
Probability masses (logistic transform)				Resulting probabilities		
λ_1	5.563***	.766		p_1	.372	
λ_2	3.670***	.734		p_2	.056	
λ_3	5.988***	.706		p_3	.570	
				p_4	.001	

Notes: * Significant at the 10% level; ** at the 5% level; *** at the 1% level. The estimation results of the systematic parts of the (u, a) and (e, a) transitions are available on request.

Table 5: Estimation Results of Systematic Parts and Individual Heterogeneity Distribution – Women

Transition	(u,e)		(e,e)		(e,u)	
Variable	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Time-invariant covariates						
Nationality - Reference: Belgian						
Non-Belgian EU	-.063	.072	-.102	.129	-.003	.113
Non EU	-.757***	.078	-.375***	.136	.154	.110
Education - Reference: Higher secondary						
Primary school	-.969***	.083	-.166	.143	.521***	.113
Lower secondary	-.727***	.054	-.185**	.089	.329***	.069
Higher education	.774***	.059	.192***	.073	-.236***	.077
Other	-.677***	.188	-.031	.429	.823***	.253
Unknown	1.089***	.130	-.288**	.118	-2.021***	.247
Region of residence - Reference: Wallonia						
Flanders	.451***	.065	.248***	.094	-.096	.086
Brussels	.085	.061	.177**	.085	-.173**	.085
Time-variant spell-specific covariates						
Age	-.006	.010	.005	.015	-.037**	.016
Household position - Reference: Cohabitant						
Head of household	-1.358***	.202	-.213*	.110	.224***	.087
Single	-.235***	.072	-.048	.081	-.005	.072
Quarter of entry in the spell - Reference: April-May-June						
January-February-March	-.217***	.062	.077	.067	.163**	.067
July-August-September	-.073	.052	.055	.066	.204***	.065
October-November-December	-.215***	.054	-.006	.067	-.004	.068
Firm size - Reference: 500 or more employees						
$[1,20)$ employees	–	–	-.362***	.059	-.422***	.056
$[20,50)$ employees	–	–	-.243***	.082	-.428***	.086
$[50,100)$ employees	–	–	-.177	.109	-.195*	.106
$[100,500)$ employees	–	–	-.083	.070	-.274***	.068
Sector - Reference: Business services						
Agriculture	–	–	.075	.224	.881***	.134
Industry & Mining	–	–	-1.321***	.120	-.528***	.111
Building & Energy	–	–	-1.079***	.255	-.764***	.278
Wholesale & Retail trade	–	–	-1.062***	.069	-.646***	.067
Credit & Insurance	–	–	-1.143***	.161	-1.408***	.233
Other services & Pub. Adm.	–	–	-1.238***	.059	-.688***	.057
Log unemployment benefits	.519**	.207	–	–	–	–
Declining benefits	.009	.369	–	–	–	–
Time-variant covariates						
Local unemployment rate	-1.423***	.281	-1.233***	.434	.642*	.371
Quarters away of a decline in the unemployment benefit amount						
UI 4	-.475	.359	–	–	–	–
UI 3	-.223	.211	–	–	–	–
UI 2	-.723**	.353	–	–	–	–
UI 1	1.093***	.418	–	–	–	–
Individual heterogeneity distribution, $M=5$						
Points of support						
$\ln v_{jk1}$	-1.477***	.300	-1.068***	.228	-1.361***	.214
$\ln v_{jk2}$	-2.387***	.326	-.673***	.241	.005	.244
$\ln v_{jk3}$	-1.190***	.356	$-\infty$	–	.005	.271
$\ln v_{jk4}$	-.632**	.300	-.045	.221	-1.060***	.223
$\ln v_{jk5}$	-1.817***	.395	-.468	.317	$-\infty$	–
Probability masses (logistic transform)				Resulting probabilities		
λ_1	2.404***	.449		p_1	.507	
λ_2	1.549***	.463		p_2	.216	
λ_3	-.135	.694		p_3	.040	
λ_4	1.431**	.580		p_4	.191	
				p_5	.046	

Notes: * Significant at the 10% level; ** at the 5% level; *** at the 1% level. The estimation results of the systematic parts of the (u,a) and (e,a) transitions are available on request.

Table 6: Simulated and Actual Distributions of Durations and Transitions

	Men				Women			
	Actual frequency	Simulated frequency	95% confidence interval		Actual frequency	Simulated frequency	95% confidence interval	
Quarters	Unemployment duration							
1	.110	.129	.113	.144	**.111**	.108	.098	.119
2	**.052**	.054	.046	.062	**.048**	.047	.041	.053
3	**.033**	.028	.023	.035	**.030**	.029	.024	.034
4	.126	.138	.128	.148	**.120**	.125	.116	.134
5	**.178**	.182	.171	.194	**.166**	.169	.159	.179
6	**.115**	.113	.104	.123	**.111**	.113	.105	.123
7	**.075**	.071	.064	.079	**.077**	.077	.070	.084
8-9	**.109**	.104	.095	.114	**.107**	.106	.098	.114
10-12	**.087**	.081	.073	.089	**.090**	.090	.083	.097
13-19	.116	.099	.089	.110	**.140**	.137	.126	.146
	Job tenure							
1	**.384**	.394	.369	.421	**.374**	.384	.366	.401
2	**.169**	.178	.165	.190	**.166**	.166	.155	.178
3	**.095**	.096	.086	.105	**.095**	.094	.085	.103
4	**.086**	.086	.076	.095	**.091**	.087	.079	.095
5-6	**.081**	.079	.070	.088	**.087**	.084	.076	.092
7-9	.094	.083	.074	.093	**.090**	.088	.080	.096
10-15	**.090**	.086	.072	.100	**.097**	.097	.088	.107
Transitions	Transitions from unemployment							
(u, e)	**.595**	.629	.594	.654	**.524**	.518	.499	.535
(u, a)	**.302**	.296	.272	.336	**.371**	.377	.361	.399
Right cens.	.103	.075	.066	.086	**.106**	.105	.096	.113
	Transitions from employment							
(e, e)	.282	.346	.315	.385	.276	.301	.282	.321
(e, u)	.319	.279	.255	.304	**.335**	.332	.312	.352
(e, a)	**.150**	.146	.128	.166	**.134**	.133	.116	.148
Right cens.	**.249**	.228	.206	.249	.256	.235	.222	.248
Time elapsed since short-lived job	Probability of having found a long-lasting job after a short-lived job[(a)]							
2 quarters	**.114**	.119	.099	.139	**.140**	.140	.123	.161
3 quarters	**.197**	.214	.186	.241	.211	.234	.212	.266
4 quarters	**.269**	.299	.268	.334	**.299**	.328	.298	.363
5 quarters	**.374**	.377	.336	.415	**.387**	.401	.369	.438
6 quarters	**.452**	.445	.398	.480	**.455**	.466	.430	.503
7 quarters	**.497**	.502	.453	.538	**.539**	.520	.483	.558
8 quarters	**.536**	.551	.494	.592	**.589**	.567	.527	.604

Notes: Actual frequencies lying in the 95% confidence interval of the simulated frequencies are in bold.

[(a)] These are probabilities of having already found a long-lasting job 2 to 8 quarters after entering a short-lived job at the end of the first unemployment spell. Moreover, they are conditional on having entered a short-lived job within 7 quarters after graduation and on not being censored during this period. The reasons for this conditioning set is clarified in Subsection 5.2.

overpredicted and job-to-unemployment transitions underpredicted, but again the size of the misalignment is not so large. Finally, the third panel of Table 6 demonstrates that the model performs extremely well in predicting the outcome of interest: the probability of finding a long-lasting job within 2 to 8 quarters after accepting a short-lived job.

5.2 The Conditional ATT (CATT)

The central question of this paper is whether a short-lived job might be a stepping stone to more job security or, in contrast, trap young disadvantaged workers in a cycle between dead-end jobs and unemployment. A short-lived job is defined as a job lasting at most one quarter and involuntarily ending in unemployment.[34] Job security is attained if a worker enters a *long-lasting* job enduring at least four quarters. One may argue that the measure of job security is arbitrary. We justify this choice by the observation that the bulk of the dismissals occurs during the first year (see Figure 1). Note that our findings are hardly affected if, in a sensitivity analysis, we set the threshold of a long-lasting job to five instead of four quarters.[35]

Formally, we are interested in the causal impact of a binary treatment, being hired in a short-lived job, that is assigned in some quarter $s \in N_0 \equiv \{1, 2, ...\}$ during the first unemployment spell after school or not assigned at all, which is represented as a treatment at infinity ($s = \infty$).[36] The latter treatment corresponds to a strategy in which one rejects all the short-lived jobs as a way out of the first unemployment spell. The set of mutually exclusive treatments can thus be denoted by $\bar{N}_0 \equiv N_0 \cup \{\infty\}$. In principle, the outcome variable of interest is the potential duration $T_l^*(s) \in N_0$ between graduation and entry in a long-lasting job that would realize if the treatment is assigned in quarter s. The actual treatment is assigned according to a $\bar{N}_0$-valued random variable S and the actual outcome is $T_l \equiv T_l^*(S)$; all other outcomes are counterfactual.

Now consider a sample of individuals i with $i = 1, 2, ...I$ and assume that we observe for each of these individuals $j = 1, 2, ...J$ labour market histories. In the data we observe only one labour market history ($J = 1$) for each individual. However, in the simulations we can construct a large number of labour market histories for any given individual, since these are identified in the model through the parametric assumptions and the given time profile of the observed individual characteristics together with a given draw from the

[34]We identify the interruption of the job as involuntary when the job ends in (insured) unemployment. We exclude longer spells and jobs ending by a transition to another job as to provide a lower bound to the stepping stone effect of a short-term job.

[35]Further sensitivity analysis in which the threshold is set much higher is not feasible with the available data. This requires a longer observation period.

[36]The notation in this section is inspired by the one in Abbring and van den Berg (2003).

unobserved heterogeneity distribution associated to an individual. As shown below, this allows us to simulate average treatment effects at the individual level and, consequently, the distribution of the treatment effect.[37]

The realized potential outcomes and treatments for a particular labour market history j associated to individual i can be denoted, respectively, by $T_{lij} \equiv T^*_{lij}(S_{ij})$ and S_{ij}. All other outcomes are counterfactual, in particular the counterfactual of interest $T^*_{lij}(\infty)$, the duration to a long-lasting job if all short-lived jobs would be rejected. With this notation, the average individual treatment effect of the treated (AITT) is defined as

$$\Delta_i(s) = E_j\big[T^*_{lij}(S_{ij}) - T^*_{lij}(\infty)|S_{ij} < \infty\big], \text{ for } i = 1, \ldots, I, \tag{6}$$

where the conditional expectation is taken over the J simulated labour market histories of each sampled individual i who is eventually treated ($S_{ij} < \infty$).

In practice, this AITT cannot be identified, since it requires an observation window of infinite length. The observation window is limited by two forms of censoring: the exogenous censoring induced by the data gathering, which right censors the labour market trajectory at the end of 2001, and the endogenous censoring in the absorbing state a.

In order to face these censoring problems, we propose an alternative outcome variable by altering the conditioning set of the AITT. We label this new statistic the conditional average individual treatment effect of the treated (CAITT). The new counterfactual outcome variable is a binary indicator equal to one if an individual finds a long-lasting job within d quarters from the moment s at which (s)he is treated and zero otherwise:

$$Y^*_d(s) = \mathbb{1}(T^*_l \leq s + d). \tag{7}$$

If we denote by $T^*_a(s)$ the counterfactual duration between graduation and entry in the endogenous censoring state a that would realize if the treatment assigned in quarter s, we can define a counterfactual censoring indicator $C^*_d(s)$ as

$$C^*_d(s) = \mathbb{1}[T^*_a(s) \leq min(T^*_l(s), s + d)]. \tag{8}$$

There is no censoring if state a is entered after being hired in a long-lasting job or more than d quarters after entering a short-lived job.

The CAITT is then defined as

$$\tilde{\Delta}_{di}(m) = E_j\big[Y^*_{dij}(S_{ij}) - Y^*_{dij}(\infty)|C^*_{dij}(S_{ij}) = 0, 4 \leq S_{ij} \leq m\big], \tag{9}$$

[37] Note that the treatment effect is heterogeneous despite the MPH specification. This is because the treatment effect is a complicated nonlinear function of the estimated parameters.

for $d = 1, \ldots, D$, $i = 1, \ldots, I$, where, in our data, $m + D \leq 15$.[38] In words, the CAITT measures the increase in percentage points of individual i's probability of finding a long-lasting job within d quarters after entering a short-lived job compared to the counterfactual in which all short-lived jobs are rejected. In order to cope with the censoring problem, the CAITT is conditional on not being censored between the fifth[39] and m^{th} quarter since graduation. In the baseline simulation we set $m = 7$ and $D = 8$.[40] Note that the CAITT and the AITT are related in the following way: if one sums the CAITT over d from one to infinity in the absence of censoring, then one obtains the AITT. Finally, note that from each CAITT we can also estimate a CATT by averaging the CAITTs over the retained treated population: $\tilde{\Delta}_d(m) = E_i\left[\tilde{\Delta}_{di}(m)\right]$.

5.3 Simulation Results

Table 7 reports means and selected percentiles of the CAITT distributions for $m = 7$ and $d = 2, 4, 8$, i.e. we consider the extent to which accepting short-lived jobs in the first year of observation (7-3=4) increases the probability of finding a long-lasting job within two to eight quarters after being hired in the short-lived job. These results show that, in the short-run, short-lived jobs are not stepping stones. Half a year after the transition to a short-lived job, the probability of having entered a long-lasting job is 1 percentage point (but not significantly) lower than if one would have rejected the short-lived job. However, after one year the stepping stone effect clearly emerges: the CATT is 5.2 percentage points for both men and women. After two years the CATT increases further to (9.5) 13.4 percentage points for (wo)men.

Even if there is a clear stepping stone effect of short-lived jobs, the simulations also demonstrate (see Table 7 or Figure 2) that there is substantial heterogeneity in the treatment effect. Even if the CAITTs are positive for the majority of the treated population, for roughly 40% of the treated the transition to a short-lived job decreased the likelihood of entering a long-lasting job. Furthermore, the fraction of the population that benefits from the treatment remains roughly constant with the elapsed duration d since the start of the treatment. The average effect increases with d because the magnitude of the positive CAITTs increases and the magnitude of the negative CAITTs decreases.

In order to understand the possible sources of heterogeneity in the treatment effect,

[38] m and D are chosen such that their sum does not exceed 15, i.e. the width of the observation window (16) reduced by the length of the long-lasting job (4) and augmented by 3 to take into account that m is measured from graduation and not from the start of the observation period.

[39] Since all sampled individuals are unemployed during the first three quarters, the first short-lived job cannot be occupied before the fourth quarter and censoring cannot occur therefore before the fifth quarter.

[40] In a sensitivity analysis, where we set $m = 11$ and $D = 4$, we obtain very similar results.

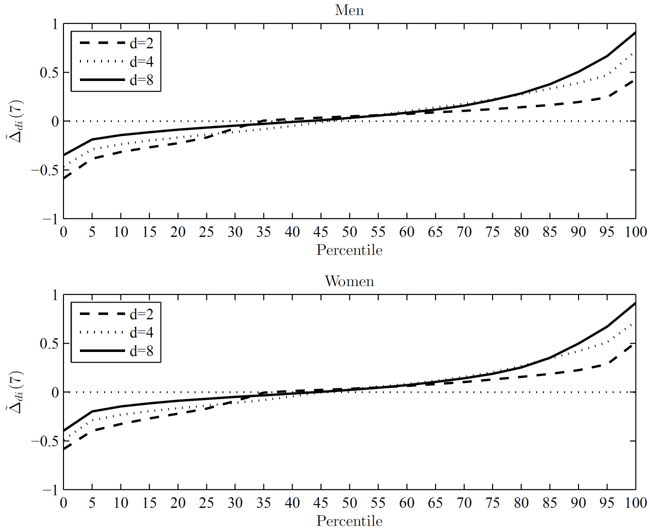
Table 7: Simulated CAITT Distributions for $m = 7$ and $d = 2, 4, 8$

Statistics	Distribution of $\tilde{\Delta}_{2i}(7)$ Mean	95% conf int		Distribution of $\tilde{\Delta}_{4i}(7)$ Mean	95% conf int		Distribution of $\tilde{\Delta}_{8i}(7)$ Mean	95% conf int	
					Men				
CATT	-.011	-.036	.016	.052	.019	.084	.134	.072	.186
	(119.)			(.299)			(.551)		
Selected percentiles of the CAITT distributions									
Minimum	-.587	-.700	-.490	-.464	-.580	-.380	-.348	-.480	-.260
5th	-.386	-.445	-.330	-.288	-.334	-.240	-.188	-.231	-.150
10th	-.317	-.370	-.260	-.237	-.280	-.190	-.143	-.180	-.101
25th	-.169	-.243	.000	-.140	-.180	-.092	-.067	-.100	-.020
50th	.048	.030	.070	.027	-.030	.070	.032	-.010	.070
75th	.123	.100	.150	.223	.180	.280	.215	.140	.290
90th	.195	.170	.240	.389	.340	.444	.504	.376	.622
95th	.243	.210	.290	.471	.414	.538	.666	.560	.770
Maximum	.429	.320	.550	.719	.600	.830	.909	.810	.980
# obs[(a)]		384.5			332.4			261.0	
					Women				
CATT	-.010	-.036	.014	.052	.020	.085	.095	.063	.142
	(.140)			(.328)			(.567)		
Selected percentiles of the CAITT distributions									
Minimum	-.583	-.680	-.490	-.489	-.610	-.400	-.396	-.530	-.300
5th	-.398	-.350	-.440	-.289	-.340	-.249	-.199	-.246	-.150
10th	-.328	-.370	-.270	-.234	-.270	-.200	-.147	-.200	-.110
25th	-.170	-.240	-.103	-.138	-.180	-.100	-.069	-.100	-.040
50th	.033	.020	.050	.028	-.020	.060	.021	-.010	.060
75th	.129	.100	.160	.203	.155	.268	.187	.140	.250
90th	.224	.190	.260	.419	.376	.472	.497	.374	.640
95th	.285	.250	.339	.509	.447	.563	.670	.586	.749
Maximum	.507	.370	.840	.724	.640	.890	.913	.840	.970
# obs[(a)]		501.7			425.6			330.4	

Notes: In parentheses we report the average probability of having entered a long-lasting job d quarters after a short-lived job, conditional on not being censored during this period and on having entered a short-lived job between the start of the observation window (4 quarters after graduation) and 7 quarters after graduation. Formally, it is the empirical counterpart of $E_i\{E_j[Y^*_{dij}(S_{ij})|C^*_{dij}(S_{ij}) = 0, 4 \leq S_{ij} \leq 7]\}$.

[(a)] This is the average number of individuals satisfying the conditioning set in (9), i.e. the average number of treated individuals.

Figure 2: Simulated CAITT Distributions for $m = 7$ and $d = 2, 4, 8$



we performed an OLS regression of the CAITTs on the observed and unobserved characteristics fixed at the beginning of the observation period. We find that the stepping stone effect is less important for youth with higher education, living in Flanders and in districts with a lower unemployment rate.

These results are in line with other European studies which generally find that short-term jobs are stepping stones to permanent jobs. We believe, however, that our findings are even stronger, since we find evidence of a stepping stone effect even for unsuccessful jobs involuntarily ending in unemployment after maximum one quarter. This claim is confirmed in a sensitivity analysis in which we include, in the group of the treated, also workers who make a job-to-job transition at the end of a job lasting maximum one quarter.[41] Two years after the start of the treatment, the CATT increases to (13.6) 19.3 percentage points for (wo)men: an increase by more than 40% as compared to the benchmark treatment definition.[42]

Finally, up to now we only considered the treatment effect on the treated. It is likely that providing a short-lived job to individuals who would otherwise have not accepted such a job might generate a lower stepping stone effect: these individuals might have rejected short-lived jobs because they knew they would be harmed by accepting such

[41] In other words, the treated are now those who find short-lived jobs, irrespective of whether this job ends in unemployment or in another job.

[42] By inspecting Table 3, one can understand the mechanism at work: a job that immediatley follows another job lasts longer than if it follows unemployment.

jobs. We cannot however directly check this hypothesis, since we do not observe such job offers. However, if this hypothesis is correct, we should find that imposing the treatment on those who are not treated should reduce the stepping stone effect. This is confirmed by a simulation in which we impose a job lasting one quarter (ending in a job-to-job transition or in unemployment) on *all* the individuals who are still unemployed one year after graduation. Two years after this imposed job, the probability of entering a long-lasting job increases by (7) 17 percentage points for (wo)men. The reader should be careful in *not* interpreting these last findings as the expected impact of a policy in which all long-term unemployed workers would be offered a short-lived job. The reason is that such a large scale policy would presumably generate general equilibrium effects, which cannot be captured by our model.

6 Conclusions

Can short-lived jobs (lasting less than one quarter and involuntarily ending in unemployment) be stepping stones to long-lasting jobs (lasting more than one year) for disadvantaged youth? This was the central question of this research. To answer this question the analysis proceeded in two steps. In a first step, a dynamic multi-state multi-spell model of labour market trajectories of Belgian long-term unemployed school-leavers was estimated using administrative data gathered between 1998 and 2001. In a second step, on the basis of the estimated model and simulations, we carried out an *a posteriori* analysis. It quantifies the effect of accepting short-lived jobs as a way out of the post-school unemployment event, as opposed to rejecting them and waiting for a better job.

The model in the first step provides evidence of the presence of lagged occurrence and lagged duration dependence in the labour market experience of Belgian disadvantaged youth. We find a positive impact of the occurrence of past employment on subsequent employment which is larger than that of past unemployment on subsequent unemployment. It implies that there are more long lasting positive impacts from employment, irrespective of its duration, than negative impacts from unemployment. This supports the stepping stone hypothesis. Nevertheless, the effect of lagged duration dependence suggests that it might not be a good idea to accept a job too quickly, since delaying exit from unemployment may improve the post-unemployment job stability (Ehrenberg and Oaxaca, 1976).

The simulation results reveal that the factors in favour of the stepping stone hypothesis dominate. By accepting short-lived jobs, rather than rejecting them as a way out of the first unemployment spell after graduation, the probability of entering a long-lasting job increases within two years by (9.5) 13.4 percentage points for (wo)men. Moreover, we claim that this effect is a lower bound, since we restricted the analysis to very short-lived jobs (lasting no longer than one quarter) that are unsuccessful in that they involuntarily end in unemployment rather than in another job. On the other hand, the simulations indicate that there is substantial heterogeneity in the effect: 40 percent of those who accepted short-lived jobs would have speeded up their labour market integration by rejecting those jobs. The stepping stone effect was found to be smaller for certain subpopulations: the higher educated, those living in Flanders and in districts with low unemployment rates.

These findings need to be qualified for a number of reasons. First, the strategy of accepting short-lived jobs may have less beneficial long-run consequences on other dimensions of the quality of a job, such as on the level of wages. Second, as mentioned in Subsection 4.2, our findings also suggest, in line with those of Gagliarducci (2005) and García Pérez and Muñoz-Bullón (2007), that, once a first work experience has been acquired, the stepping stone effect may be less important or even disappear. Thirdly, in our simulation we estimated the treatment effect on the treated. This stepping stone effect cannot be extrapolated from the non-treated population. As a matter of fact, those who rejected short-lived jobs may have done so because they knew they would be harmed. Finally, if the stepping stone effect is generated by signaling higher productivity or motivation, which is likely if we restrict our attention to short-term employment,[43] then policies stimulating the transition to short-term jobs might not generate the positive effects we report here. By doing so, the signal may indeed become less reliable, meaning that employers may no longer use it in their hiring decisions.

Appendix

A-1 Deriving the Likelihood Function

In this appendix we specify the contribution to the likelihood function of a completed spell s of which the origin state is j. Suppose that after t_s quarters spent in the origin state j, a transition to the destination state k is observed, with $(j, k) \in \mathscr{X}$. Denote D_{jk} an indicator variable equal to 1 if a (j, k) transition is observed and 0 otherwise. We suppress the set of observed and unobserved characteristics, but in what follows we are implicitly conditioning on them.

The contribution to the likelihood function is the unconditional probability of jointly observing the departure from j and the transition to k after a sojourn of t_s quarters in the origin state j, i.e. $\Pr(t_s - 1 \leq T_j < t_s, D_{jk} = 1)$. Since we have quarterly information, we do not exactly know when the transition occurs within two consecutive quarters. Hence, we model the probability of

[43] Other mechanisms, such as those related to the accumulation of human capital, take more time.

observing the departure within two consecutive quarters. This probability can be rewritten as

$$\Pr(T_j \geq t_s - 1)\Pr(t_s - 1 \leq T_j < t_s, D_{jk} = 1 | T_j \geq t_s - 1), \tag{A-1}$$

which is the product of the survivor function and of a conditional probability.

The survivor function in state j for $t_s - 1$ quarters is given by

$$\begin{aligned}\Pr(T_j \geq t_s - 1) &= \exp\left\{ -\int_0^{t_s-1} \sum_{(j,k)\in\mathscr{J}} \theta_{jk}^s(\tau)d\tau \right\} \\ &= \exp\left\{ -\int_0^1 \sum_{(j,k)\in\mathscr{J}} \theta_{jk}^s(\tau)d\tau - \int_1^2 \sum_{(j,k)\in\mathscr{J}} \theta_{jk}^s(\tau)d\tau - \ldots - \int_{t_s-2}^{t_s-1} \sum_{(j,k)\in\mathscr{J}} \theta_{jk}^s(\tau)d\tau \right\},\end{aligned}$$

where $\mathscr{J} = \mathscr{E}$ if $j = e$ and $\mathscr{J} = \mathscr{U}$ if $j = u$. We assume now that the transition intensities are constant within two consecutive quarters, since we do not have information on what happens within each interval. Under this assumption, we can specify the discrete time process as a continuous time model and the hazard functions can be taken out of the integrals, yielding

$$\Pr(T_j \geq t_s - 1) = \prod_{\tau=1}^{t_s-1} \exp\left\{ - \sum_{(j,k)\in\mathscr{J}} \theta_{jk}^s(\tau) \right\} \equiv S_j(t_s - 1). \tag{A-2}$$

The conditional probability in (A-1) can be written as

$$\Pr(t_s - 1 \leq T_j < t_s, D_{jk} = 1 | T_j \geq t_s - 1) = \frac{\int_{t_s-1}^{t_s} \theta_{jk}^s(\tau) \exp\left\{ - \int_0^\tau \sum_{(j,k)\in\mathscr{J}} \theta_{jk}^s(r)dr \right\} d\tau}{\exp\left\{ - \int_0^{t_s-1} \sum_{(j,k)\in\mathscr{J}} \theta_{jk}^s(r)dr \right\}} \tag{A-3}$$

and exploiting again the assumption that the transition intensities are constant within two consecutive quarters, Eq. (A-3) can be rewritten, following Cockx (1997), as

$$\left[1 - \exp\left\{ - \sum_{(j,k)\in\mathscr{J}} \theta_{jk}^s(t_s) \right\} \right] \times \frac{\theta_{jk}^s(t_s)}{\sum_{(b,c)\in\mathscr{J}} \theta_{bc}^s(t_s)}. \tag{A-4}$$

Multiplying (A-2) by (A-4) and reintroducing the set of observed and observed characteristics yield Eq. (3), which is the contribution to the likelihood function of a complete spell s.

A-2 Goodness-of-Fit

As a consequence of stock sampling, a complication arises in performing simulations. Since all sampled individuals have already been unemployed for three quarters at the start of the observation period, the distribution of unobserved heterogeneity must be modified along the lines of the adjustment of the likelihood function. This means that the probability p_{im} that individual i is of type

m and is therefore assigned the vector of location points $\hat{\mathbf{v}}_m \equiv [\hat{v}_{uem}, \hat{v}_{uam}, \hat{v}_{eem}, \hat{v}_{eum}, \hat{v}_{eam}]$ for $m = 1, \ldots, \widehat{M}$ can be estimated by

$$\hat{p}_{im} = \frac{\widehat{S}_u(3|\mathbf{x}_{iu}; \widehat{\Theta}_u, \hat{\mathbf{v}}^1_{um})\hat{p}_m}{\sum_{r=1}^{\widehat{M}} \widehat{S}_u(3|\mathbf{x}_{iu}; \widehat{\Theta}_u, \hat{\mathbf{v}}^1_{ur})\hat{p}_r}, \tag{A-5}$$

where $\widehat{M} = 4$ for men and $\widehat{M} = 5$ for women. Observe that this distribution depends on the values of the observed explanatory variables at the sampling date.

The simulation then proceeds according to the following steps:

1. Draw a vector of parameter estimates assuming that the estimator is Normally distributed around the point estimates with a variance-covariance matrix equal to the estimated one.
2. Assign to each individual the observed explanatory variables at the sampling date and a vector of unobserved characteristics drawn with the probability as given in Eq. (A-5).
3. Simulate the transition from u to e or a by a sequence of quarterly transition lotteries starting from the 4^{th} quarter (the observation period). These transition lotteries are based on the empirical counterparts of the probability of leaving state u for k ($k = e, a$), conditional on surviving in state u until the end of the previous quarter. Their form is given by Eq. (A-4). In this process, the time-varying variables, for example the local unemployment rate, are adjusted to their new values at the beginning of each quarter.
4. If a transition to the censoring state a occurs, the simulation for that individual is halted. If there is a transition to e, assign new values to the unemployment rate and the spell specific time-varying variables. The age, quarter of entry, and household position of each individual are assigned the values as reported at the calendar time corresponding to the quarter of entry in the simulated employment spell. The vector of firm characteristics corresponds to that of the firm that was randomly drawn from the set of firms that hired workers with the same origin state (u or e) and elapsed duration in that origin state.
5. Simulate the transitions from e and from all subsequent states according to a similar sequence of quarterly lotteries as described for the unemployment state in point 3 and adjust the time-varying explanatory variables following the procedure described in point 4. In each new unemployment spell the amount of UB is adjusted according to the rules, using information on the corresponding household position, age, and labour market history.
6. The simulation procedure is halted once the end of the observation period is reached, i.e. in December 2001, 13 to 16 quarters after the sampling date.
7. Repeat for each individual points 1 to 6 999 times to obtain 999 independent labour market histories for each sampled individual.

A-3 The Simulation with regard to the Stepping Stone Hypothesis

The simulation procedure to test the stepping stone hypothesis goes as follows:

1. Simulate the labour market history for all individuals in the sample as in Appendix A-2.
2. Retain those individuals who entered a short-lived job within m quarters after graduation and who were not endogenously censored within d quarters after entering a short-lived job.
3. For each retained individual we re-simulate, conditional on not being endogenously censored,[44] $J = 100$ times the labour market history until the end of the observation window, once with and once without imposing the counterfactual job search strategy in which all short-lived jobs are rejected as a way out of the first unemployment spell after graduation. In the counterfactual case, the job-seeker who is imposed the rejection of a short-lived job is allowed to continue searching for new jobs in the same quarter.[45]
4. Calculate for each d up to $D = 8$ the empirical counterpart of the CAITT for each retained individual by taking the difference in the outcome variable between the two counterfactuals. This provides us with one estimation of the CAITT distribution.
5. Calculate and store the mean of the CAITT (which is an estimate of the CATT) and a number of selected percentiles of the CAITT distribution.
6. As to construct 95% empirical confidence intervals of the CATT and the selected percentiles of the CAITT distribution, we repeat steps 1 to 5 119 independent times.

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[44] This conditioning is realized by adjusting the transition probability in Eq. (A-4) in the following way

$$\frac{\left[1-\exp\left\{-\sum_{(j,k)\in\mathscr{J}}\theta^s_{jk}(t_s)\right\}\right]\times\frac{\theta^s_{jk}(t_s)}{\sum_{(b,c)\in\mathscr{J}}\theta^s_{bc}(t_s)}}{1-\left[1-\exp\left\{-\sum_{(j,k)\in\mathscr{J}}\theta^s_{jk}(t_s)\right\}\right]\times\frac{\theta^s_{ja}(t_s)}{\sum_{(b,c)\in\mathscr{J}}\theta^s_{bc}(t_s)}}.$$

The denominator is the conditional set, i.e. the conditional probability of not ending the quarter in a.

[45] In these simulations we retain the parameter draw of the first step, so that both observed and unobserved characteristics are fixed at individual level over these simulations.

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