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ABSTRACT

We use unique and rich register data of 88,948 sick-listed workers to investigate the effect of active labor market measures on the duration until returning to non-subsidized employment and the duration of this employment. To identify causal treatment effects, we exploit over-time variation in 98 job centers' use of active labor market measures. We find that ordinary education and especially subsidized job training have statistically significant positive employment effects. Subsidized job training has a large, statistically significant positive effect on the transition into employment but no effect on the subsequent employment duration. In contrast, ordinary education has a statistically significant positive effect on employment duration but no effect on the transition to employment. This null effect consists of a large positive effect of having completed education and a large negative lock-in effect, with low re-employment chances during program participation. Moreover, non-formal education (e.g., shorter courses) and subsidized internships have no or even negative employment effects.

Keywords: active labor market measure; effect evaluation; employment; hazard rate model; sick leave; return to work;

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Employment effects of active labor market measures for sick-listed workers

1. Introduction

Work disability, a challenge in many countries, not only reduces individual well-being and income but also reduces labor supply and forces societies to allocate considerable resources to treatments and cash transfers claims (OECD, 2010; Eurostat, 2009; Greenberg et al., 2003). In the OECD, the average employment rate of people with disabilities is slightly over half the employment rate of people without disabilities, and the costs of sickness and disability benefits correspond to nearly 2% of GDP (OECD, 2010). To increase employment rates of people with disabilities, many countries are increasingly shifting focus from passive economic compensation policies to active labor market measures (ALMM), which have become important policy tools in many EU countries (van Lin et al., 2002).

Despite the vast resources now invested in ALMM, crucial knowledge about its overall employment effects is missing. While workplace-based measures generally show positive return-to-work (RTW) effects on sick-listed workers (e.g., van Oostrom et al., 2009; Palmer et al., 2012), evidence about the effect of educational measures remains scarce and mixed. Our study provides new knowledge about the effect of educational measures.

Our study relates primarily to four other studies: Frölich et al. (2004), Rehwald et al. (2015), Markussen and Røed (2014), and Dean et al. (2015; forthcoming).[1] Markussen and Røed (2014) study the labor market effects of RTW measures for 345,000 Norwegian temporary disability insurance claimants. Markussen and Røed (2014) distinguish between four treatments— subsidized employment in ordinary firms, subsidized employment in sheltered firms, ordinary education, and vocational training courses — and to identify the treatment effect they use variation across and over time in 151 local authorities' treatment strategies as instrumental variables. Markussen and Røed (2014) they find that regular education and especially wage subsidized regular employment significantly increase employment probability. However, in contrast to subsidized employment, education has large lock-in

[1] Below we focus on the studies' findings about employment outcomes.

effects, with low re-employment chances during participation in education. Markussen and Røed (2014) also find that subsidized employment in sheltered firms and vocational training courses have negative employment effects.

Only two studies focus on sick-listed individuals. Using data on 6,300 sick-listed individuals in five Swedish counties, Frölich et al. (2004) use a nonparametric matching technique to identify the causal effect of six independent ALMM measures by assuming that they observe all variables that simultaneously affect participation in educational measures and employment. They find that both education and workplace rehabilitation reduce re-employment probability. Rehwald et al. (2015), studying labor market effects of intensified mandatory RTW treatments, use data from a randomized controlled trial in Denmark with 4,728 sick-listed individuals from 16 job centers. The treatment consists of traditional activation, paramedical measures, and graded RTW To identify the effect of these three elements, they use random variation from the trial and local variations in treatment strategies between job centers. They find positive effects of graded RTW on regular employment, while traditional activation and paramedical care have zero or adverse employment effects.

Finally, Dean et al. (2015; forthcoming) study people with cognitive disabilities (n=1,009) and mental disabilities (n=1,555) who applied for vocational rehabilitation in Virginia. Using variation across local vocational rehabilitation counselors and field offices to model the treatment propensity, they find that educational measures have negative employment effects in both the short and the long run for people with mental disabilities, but positive short- and long-run effects for people with cognitive disabilities.

We use population data from a four-year observation period of 88,948 Danish workers who in 2008 started a sick leave spell exceeding 4 weeks. We use a mixed proportional hazard-rate model with exclusion restrictions to simultaneously estimate the duration until participation in ALMM, the duration until returning to ordinary employment and the duration of the subsequent employment spell. We distinguish between four types of ALMM: ordinary education, non-formal education, wage-subsidized internships and wage-subsidized job training. Our study makes two contributions. First, this study is the first to assess the employment effect of ordinary education for sick-listed workers, taking into account the impact of unobservables on the probability of participating in ALMM and RTW. Second, in addition to the short-run employment (RTW) effect, we also estimate long-run effects (subsequent employment duration). In contrast to all previous studies, this distinction allows us to study the composition of possible employment effects, i.e. whether ALMM raises the probability of RTW or reduces the probability of ending the RTW employment—or both.

The remainder of the paper is organized as follows. Section 2 describes the institutional context of sick leave and ALMM in Denmark. Section 3 outlines our data. Section 4 describes the econometric approach. Section 5 reports empirical results. Section 6 concludes.

2. Institutional context

The Danish disability policy is publicly regulated and primarily publicly funded and administered.[2] Sickness benefit, ALMM, and disability benefit programs are administered by 98 municipalities. The sickness benefit program covers wage earners and the self-employed and insured unemployed. For wage earners, the benefit replaces 100% of the wage up to 3,515 DKK per week in 2008 (USD 625). While workers can receive the benefit for up to 52 weeks, the benefit period can be extended under certain conditions, e.g., if the worker is awaiting ALMM or has an ongoing disability or work injury claim. The employer finances benefits for the first two weeks (before June 2008) or three weeks (from June 2008)[3]; afterwards benefits are publicly financed.

The municipal job center is obligated to follow up all sickness benefit cases within eight weeks after the worker reports unfit. On average, there were about 115,000 ongoing sick leave spells exceeding eight weeks each month during 2014 (jobindsats.dk). During the sick leave period, the municipality can initiate different ALMM measures, e.g., workplace accommodations, reduced

[2] The outline of the institutional context refers to the legislation in force in 2009.
[3] The employer period was lengthened to four weeks in January 2012.

working hours with supplementary sickness benefits, job counseling, wage subsidized internships in private or public firms, wage subsidized job training in private or public firms, and educational measures ranging from courses lasting a few weeks to post-secondary education at the university level. Measures under the vocational rehabilitation program may last for up to five years. On average there were approximately 7,500 ongoing vocational rehabilitation benefit spells each month during 2014 (jobindsats.dk). If a sick-listed worker, despite medical and vocational treatment, is unable to work in an ordinary job, the municipality may refer that individual to a permanent wage-subsidized job (*flexjob*) tailored for the individual's reduced working capacity. If the disabled worker is too incapacitated to work in a *flexjob*, a disability benefit is awarded.

3. Data

3.1. Data and population

Our population comprises all Danish workers in non-subsidized employment starting a sick leave spell exceeding 4 weeks in 2008.[4] We follow this population of 91,266 individuals from 2008 through 2011 in national registers. The advantage of having access to the entire population is that it improves the possibility of identifying both unobserved heterogeneity (Gaure et al., 2007) and the effect of education. We use the "Danish Register for Evaluation of Marginalisation" (DREAM) register, with weekly individual-level recordings of all social transfer payments, and the "e-income" register, with monthly tax-based recordings of employment. For estimation purposes, we transform weekly data into monthly observations. We link the DREAM and e-income data to Statistics Denmark's registers with individual-level data on socio-economic characteristics, including information on pre-sick leave situation, previous employment, and previous medical history. We exclude 2,763 sick leave spells with missing information on baseline variables, leaving 88,948 spells for our analyses.

[4] If workers start two or more sick leave spells in 2008, we include only the first in our analysis. We only include full-time sickness spells.

3.2. Treatment variables

We distinguish between four types of treatment through ALMM: ordinary education (3.9% of the sample), non-formal education, e.g., shorter courses (15.9%), wage-subsidized internships (9.2%), and wage-subsidized job training (2.6%). We use DREAM to identify individuals in the four treatments. Ordinary education (comprising all types) includes both study-oriented education (e.g., general high school), vocationally oriented education (e.g., carpenter), and college degrees. Non-formal education includes courses targeted at enhancing employability and qualifications for people coming from unemployment or sick-leave. These courses may cover specific labor-market qualifications but may also be more informal (e.g., occupation therapist consultations, stress courses, physical exercise).

Wage-subsidized internships in public and private firms, which may last for up to 13 weeks, are primarily to assess working capacity and workplace accommodation needs.[5] Some sick-listed workers leave the sickness benefit program but remain non-working while receiving benefits. For these people, internships may also be used as a matching device between the firm and the unemployed, to assess whether they will sign an ordinary employment contract.

Wage-subsidized job training for sick-listed persons is part of a “job plan” specifying the individual’s treatments and occupational goals. Job training provides rehabilitation of professional, social, and educational skills, and is individually determined. For firms to receive a wage subsidy, a wage-subsidized position must not replace an ordinary job. Both wage level and wage subsidy regulation varies across firms and sectors.

Sick-listed workers are characterized by eight treatment variables corresponding to being either “on program” or “after program” for each of the four ALMMs. For example, for ordinary education, our “on program” variable is 0 until education begins, 1 during education, and 0 afterwards.

[5] Internships are 100% wage subsidized.

Furthermore, our “after program” variable is 0 until education ends and 1 after education. Table 1 shows descriptive statistics for the four treatments.

Table 1

Description of population of treated individuals

	Percent of population	N	Average number of spells	Median duration of first spell (months)	Median duration until beginning of first spell (months)
Ordinary education	3.91	3,476	1.52 (0.96)	5	14
Non-formal education	15.91	14,153	1.69 (0.97)	2	11
Subsidized internships	9.19	8,174	1.33 (0.64)	3	15
Subsidized job training	2.59	2,301	1.18 (0.45)	6	19

Table 1 shows that sick-listed workers on average participate in the four treatments between 1.2 and 1.7 times. The median duration of the first spell is somewhat longer for job training and ordinary education than for non-formal education or internships. Generally, non-formal education starts earlier in the sick leave spell (median duration, 11 months) than the other three. Fig. 1 reflects this difference: The hazard rate to non-formal education increases the fastest.

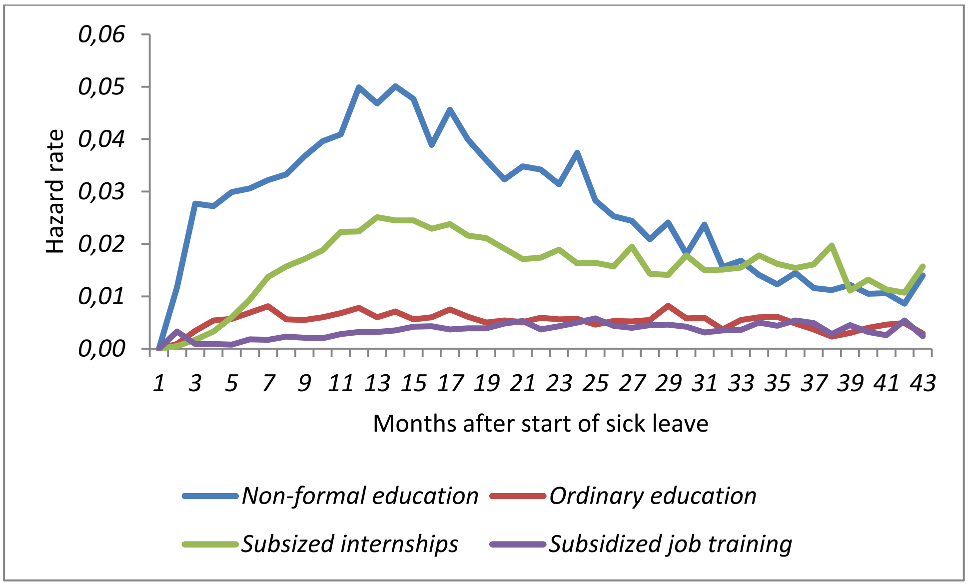

Fig. 1. Unadjusted hazard rates to educational measures

3.3. Outcome variables

Our two outcome variables are the duration until RTW and the duration of the subsequent employment spell. We measure the duration until returning to ordinary employment as the number of consecutive months from the beginning of the sick leave until the individual is employed as a wage earner and no longer receives any social transfer payment. We right censor spells when our observation period ends or sick-listed workers are awarded a disability benefit, die, or move to another country (n=4,147). We code the employment duration as the number of consecutive months from the RTW month until the individual is no longer attached to an employer or until receipt of a wage subsidy. Consequently, if a worker switches employer, the employment spell continues unless the workless period exceeds one month. About 85% of the sick-listed workers return to work during the observation period, most during the first year (see Fig. 2). Of those returning to work, 51.5% end their employment spell after about 13.6 months. The hazard rate out of employment is high immediately after RTW and then gradually decreases for the next 30 months.

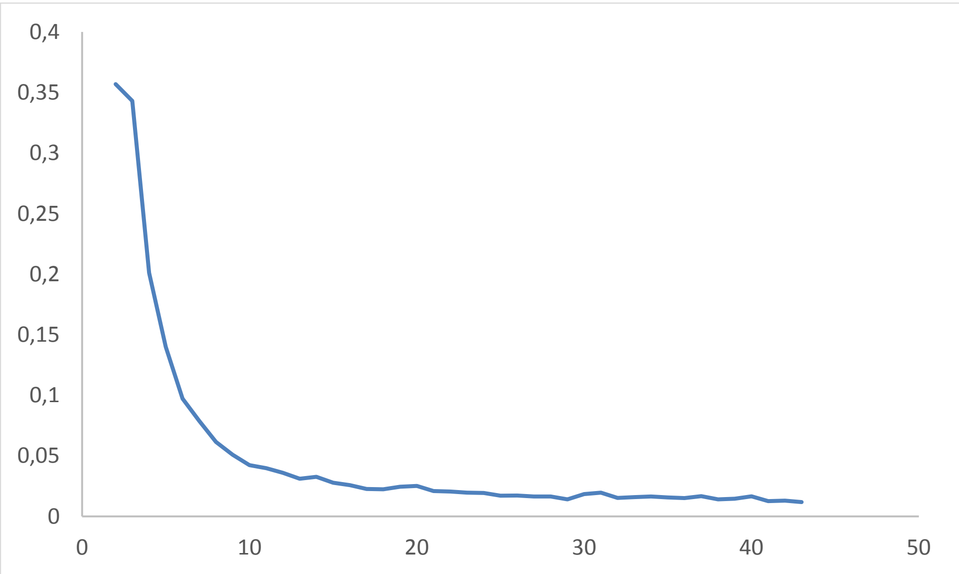

Fig. 2. Unadjusted hazard rates to returning to work

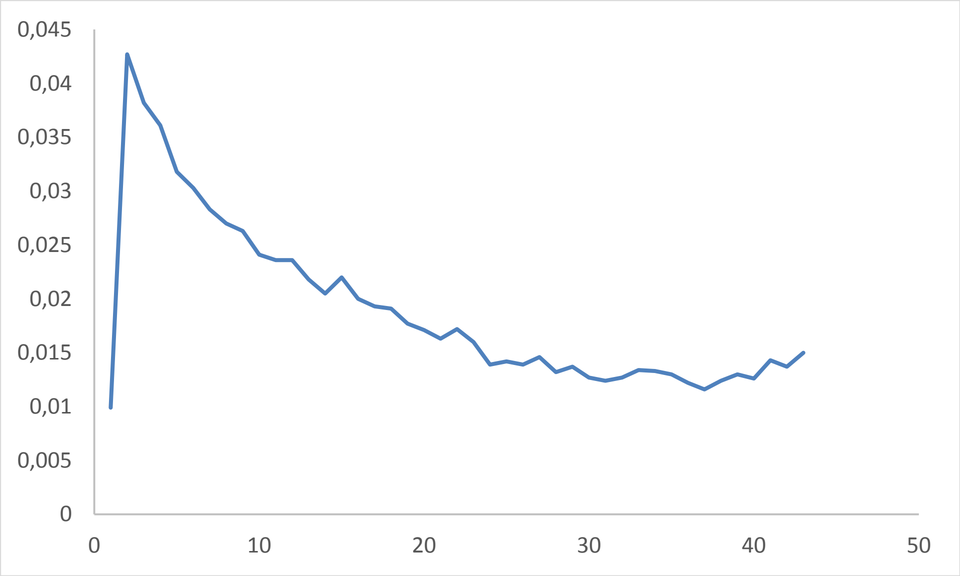

Fig. 3. Unadjusted hazard rate of ending return-to-work employment

3.4. Control variables

With our rich dataset, we can control for a wide array of background variables (table 2). Control variables are measured prior to the sickness spell (in 2007), except current age and unemployment rate, which varies by gender, age, region, and month. We also include commuting area measured in 15 dummies, commuting area interacted with the regional unemployment rate, dummy variables for the quarter of 2008 the sickness spell started, and calendar time measured every third month. Finally, beyond our exclusion-restriction variables (section 4.2), we include 96 baseline dummies measuring duration dependency in the four treatment durations and two outcome durations. Our total variables are 831.

Table 2
Descriptive statistics, control variables

	Non-treated	Ordinary education	Non-formal education	Subsidized internships	Subsidized job training
	Mean (std.dev.)	Mean (std.dev.)	Mean (std.dev.)	Mean (std.dev.)	Mean (std.dev.)
Age	38.22 (10.34)	29.45 (9.17)***	37.42 (10.18)***	38.31 (10.07)	37.79 (10.25)
Female	55.56	61.53 ***	56.78 **	60.17 ***	53.25 *
Single	52.31	76.02 ***	60.00 ***	56.53 ***	63.22 ***
Children		***	***	***	***
No Children	48.93	60.84	54.56	51.63	57.28
Children, 0-2 years	11.20	10.99	11.44	11.45	11.70
Children, 3-5 years	7.67	7.07	7.34	7.53	6.36
Children, 6-18 years	28.47	18.5	23.33	25.71	20.90
Children over 18 years	3.73	2.60	3.33	3.68	3.76
Immigrant background		***	***	***	***
Denmark	93.27	90.57	86.31	90.58	89.49
Western country	2.74	3.11	4.55	3.24	4.53
Non-Western country	3.99	6.32	9.14	6.18	6.98
Education		***	***	***	***
Primary school	31.54	45.21	42.03	39.13	41.31
High school, academic	4.45	13.83	4.94	3.94	4.33
High school, business	1.91	3.44	1.45	1.61	1.40
Vocational	41.50	27.51	37.67	40.57	40.54
Short tertiary	3.37	2.51	3.00	2.55	3.08
Medium tertiary	14.43	6.89	9.10	10.57	7.32
Long tertiary	2.81	0.60	1.82	1.62	2.02
Years employed since 1964	14.35 (9.60)	6.35 (6.93)***	11.45 (8.96)***	12.61 (9.06)***	11.93 (8.90)***
Sector, previous employment		***	***	***	***
Public administration	14.65	11.02	10.88	11.17	9.73
Other	27.76	27.04	24.59	25.23	24.03
Agriculture, fisheries, forestry	1.02	1.44	1.06	1.33	0.96
Manufacturing industry	13.71	14.88	17.76	18.68	18.92
Construction	10.36	8.17	8.66	8.07	9.92
Trade and transport	19.57	24.01	22.66	22.01	22.10
Information and communication	1.59	1.41	1.39	1.16	1.16
Financial services	1.46	0.45	1.14	1.05	1.01
Real estate	1.14	1.05	1.12	1.24	1.35
Private services	6.45	7.43	8.19	7.53	8.57
Culture	2.29	3.11	2.54	2.54	2.26
Previous medical condition					
Diabetes medication	11.93	5.21 ***	11.68	12.93 **	11.60
Heart medication	2.31	1.77 *	2.55	2.44	2.94
Lung medication	7.24	7.46	8.03 ***	8.02 *	7.66
Mental illness medication	10.19	13.02 ***	16.30 ***	17.71 ***	13.05 ***

Visits to general practitioner	7.94	(7.78)	9.15	(8.73)***	9.41	(8.97)***	9.85	(9.28)***	8.37	(8.30)*
Visits to medical specialist	0.93	(2.73)	0.97	(2.92)	1.01	(2.91)**	1.01	(2.96)**	0.86	(3.01)
Visits to psychologist	0.10	(0.92)	0.15	(1.12)**	0.13	(1.05)**	0.13	(1.06)**	0.10	(1.08)
Visits to physiotherapist	1.67	(5.72)	1.52	(4.80)	1.72	(5.69)	1.98	(5.86)**	(1.38)	(4.47)*
No. hospital bed days with diagnosis	0.41	(2.98)	0.37	(1.57)	0.41	(1.75)	0.43	(1.79)	0.36	(1.29)
No. hospital bed days with illness	0.35	(2.65)	0.26	(1.30)*	0.33	(1.67)	0.35	(1.79)	0.25	(1.29)
Sick-listed in 2008:			***		***		***		***	
First quarter	20.69		16.47		14.07		14.15		13.77	
Second quarter	26.63		25.96		23.62		24.47		22.34	
Third quarter	25.31		29.82		30.72		30.00		29.85	
Fourth quarter	27.37		27.75		31.59		31.38		34.04	
Regional unemployment										
Regional unemployment rate at t=1 (n=98,948)	2.78	(0.98)***	2.72	(1.08)***	2.70	(0.99)***	2.69	(1.00)***	2.73	(1.03)
Regional unemployment rate at t=6 (n=85,240)	3.22	(1.33)***	3.38	(1.47)***	3.40	(1.39)***	3.34	(1.40)***	3.55	(1.47)***
Regional unemployment rate at t=12 (n=75,265)	4.41	(1.34)***	4.73	(1.50)***	4.63	(1.40)***	4.56	(1.39)***	4.77	(1.47)***
Regional unemployment rate at t=24 (n=59,619)	6.05	(1.73)***	6.54	(1.99)***	6.24	(1.81)***	6.16	(1.82)***	6.38	(1.86)***
Regional unemployment rate at t=36 (n=49,942)	6.08	(1.47)***	6.19	(1.99)***	6.29	(1.69)***	6.20	(1.68)***	6.35	(1.73)***

Differences between each group of treated and the group of non-treated workers are tested in χ^2-tests and t-tests (continuous variables). Significance levels: *** 0.1%, ** 1%, * 5%

Table 2 suggests considerable selection on observables of sick-listed workers into the four treatments. Compared to non-treated individuals, sick-listed workers in the four treatments are more often single, childless, low-educated, from a non-western country, with little employment experience, many GP visits, and using medicine for mental disorders, and living in regions with a higher unemployment rate. Several of these differences suggest that treated individuals have relatively lower a priori employment opportunities.

4. Empirical method

4.1. Econometric model

Our econometric approach relies on a multivariate mixed-proportional-hazard-rate (MMPHR) model, which is state-of-the-art within econometric evaluations of labor market programs and well suited for studying dynamic processes (van den Berg, 2001; Abbring and van den Berg, 2003). The model simultaneously estimates the sick-listed workers' transition to ALMMs, their RTW, and their transition out of this employment. These transitions occur at different times after the onset of the sick leave. Time after the first day of sick leave, *t*, is measured in months. We model transitions to the four treatments, to work, and transitions out of this employment.

First, we model state-specific transitions from state *i* into the four treatments, state *j*:

$$h_{ij}(t|x,u) = \exp(\alpha_j(t) + \beta_{1j}x + \beta_{2j}z + \sum_{s=1,s\neq j}^{s=4} \delta_{ij} d_i^{in}(t) + \sum_{s=1}^{s=4} \lambda_{ij} d_i^{post}(t) + u_j) \tag{1}$$

State *i* can be either sick-leave (0), ordinary education (1), non-formal education (2), subsidized internships (3) or subsidized job training (4). Parallel, the following state *j* can be sick-leave (0), ordinary education (1), non-formal education (2), subsidized internships (3) or subsidized job training (4). α_j is the baseline hazard rate, β_{1j} captures the effects of control variables, β_{2j} the effects of our exclusion restrictions (section 4.2), and $d_i^{in}(t)$ is a state-specific dummy variable taking the value 1 if the individual is in the program at time t and 0 otherwise. Furthermore, $d_i^{post}(t)$ is a state-specific dummy variable taking the value 1 if the individual has completed program *i* at time *t* and 0 otherwise. The estimated coefficients of the variables d_i^{in} and d_i^{post}; $i = 1,..,4$, capture the estimated effects of the four treatments. Superscript "in" indicates variables capturing the effect of the treatment when *in* treatment ("lock-in effects") (van Ours, 2004), while variables with superscript "post" capture ex-post effects of treatments (*after* the treated individual leaves treatment). When lock-in effects differ from ex post effects, distinguishing between them is essential (van Ours, 2004).

The transition into employment is given by:

$$h_e(t|x,u) = \exp(\alpha_e(t) + \beta_e x + \sum_{s=1}^{s=4} \delta_e d_i^{in}(t) + \sum_{s=1}^{s=4} \lambda_e d_i^{post}(t) + u_e) \tag{2}$$

Finally, we model the transition out of employment as:

$$h_o(t|x,u) = \exp(\alpha_o(t) + \beta_o x + \sum_{s=1}^{s=4} \lambda_o d_i^{post}(t) + u_o) \tag{3}$$

As noted earlier, one advantage of the MMPHR model is that it explicitly accounts for unobserved heterogeneity in individual characteristics. The distribution of unobserved heterogeneity $u = (u_1, u_2, u_3, u_4, u_e, u_o)$ follows a discrete multinomial distribution, with each mass point having probability p_k. Thus we allow for the approximation of any underlying distribution of unobserved heterogeneity without needing any parametric assumptions. The unobserved heterogeneity in the six hazard rates may be correlated, meaning that the model adjusts for the effect of unobserved characteristics on the selection into the four ALMMs.

4.2. Identification strategy

The MMPHR model allows us to estimate the effect of the four ALMMs on RTW and subsequent employment. However, some may argue that both individuals and caseworkers influence the allocation of individuals into treatments, implying that treatment choice is not necessarily exogenous. To identify the causal employment effect of the ALMMs, we rely on exclusion restrictions (ER), i.e. variables that appear in only some hazard rates (van den Berg, 2001). ERs ensure non-parametric identification of the treatment effects. Identification of parameters through ERs in duration models follow straight forward from linear models (Bowden and Turkington, 1984). The idea of exclusion restrictions is that, if the ER is warranted, variables appearing only in, e.g., hazard rates into treatment, generate as-good-as-random variation in the probability of receiving treatment.

Our identification strategy builds on studies addressing the endogeneity problem by relying on ERs that exploit variation between local authorities' treatment strategies (Aakvik et al., 2005)[6], across caseworkers' propensity to assign clients to treatments (Dean et al., 2015), and across local authorities' treatment strategies as changing over time (Markussen and Røed, 2014; Rehwald et al., 2015). Markussen and Røed (2014) and Rehwald et al. (2015) use linear probability models to estimate local treatment strategies measuring the choice of the first treatment and the speed of its implementation

[6] Aakvik et al. (2005) use local variation in the degree of rationing calculated as the percentage of treatment applicants not participating in the treatment.

(Markussen and Røed, 2014: 5). These local treatment strategies for using different ALMMS are then used as time-varying instrument variables that are assumed to affect the selection of individuals into ALMMs, without directly influencing individual employment outcomes.

In contrast, our identification is based only on over-time variation in preferences for using ALMMs within each of the 98 municipalities. As variation across municipalities in the preferences for using ALMMs appears endogenous in our data, we do not exploit this type of variation (footnote 7). Over-time variation results from random variation in either the number of sick-listed individuals needing ALMMs or the number of available treatment slots, e.g. available job training positions in local companies.

We calculate our time-varying ER variables as follows: For each type *s* of ALMM in each period *t*, we calculate the number of sick-listed individuals in the municipality starting in an ALMM as a percentage of the ongoing sickness spells exceeding 4 weeks in period *t*. This percentage, $PALMM_{s,t}$, measures the municipal preference for using a particular ALMM in period *t*. We then calculate our ER variable, $z_{s,t}$ as $PALMM_{s,t}$- $PALMM_{s,t-1}$. To avoid constructing artificial correlation between an individual who enrolls in an ALMM of type *s* in period *t* and in $z_{s,t}$, we exclude individual *i* from the calculation of $z_{s,t}$ for *i*. Thus individual-specific ER variables are calculated separately for each individual *i* at each point. That is, conditional on the *level* of municipal preferences for using ALMMs, we assume that changes over time in these preferences are exogenous to the selection into treatments. To control for the level of municipal preferences for using ALMMs, we include in the first- and the second-stage equations four variables measuring the percentage of each of the four ALMMs (the number of ongoing ALMM spells divided by that of ongoing sickness spells), using data from January through June 2007.

In small and medium sized municipalities, the nominator of the relative number of starting ALMM spells, $PALMM_{s,t}$, often becomes zero or one with monthly data, i.e., 68 and 70% of the municipal-month records contain zeros or ones for subsidized job training and ordinary education, respectively. Consequently, monthly ER variables both capture noise and identify the treatment effects from over-time variation in the number of starting ALMM spells primarily from large municipalities. Therefore, we calculate the number of starting ALMM spells (and of ongoing sickness benefit spells) during four-month periods, i.e., the ER variables become time-varying in four-months intervals. Using these periods, we reduce the number of municipal-month records with zeros and ones to 24% for ordinary education and 26% for job training (internships decrease from 23% to 7%; and non-formal education, from 19% to 5%). Fig. 4 shows considerable variation over time in the municipalities' use of ordinary education (the 98 municipalities are divided into five regions).

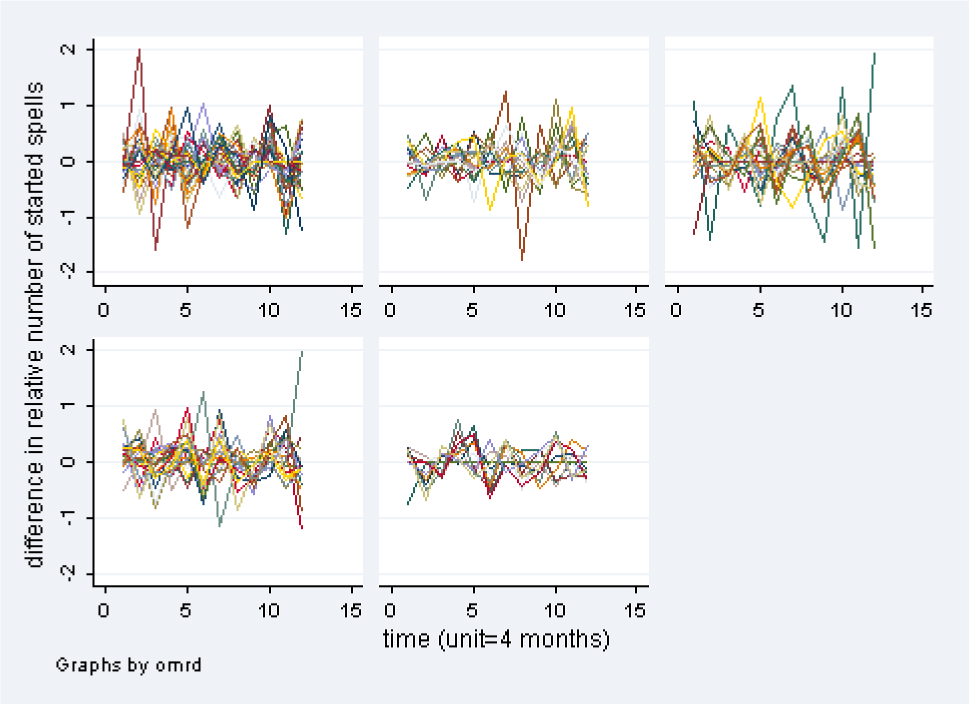

Fig. 4. Difference in relative number of started spells of ordinary education per four month

Our estimated treatment effects will be biased if the municipalities' propensity for using ALMMs correlates with unobserved third variables affecting employment outcomes. Such variables may be municipal conditions or local labor market conditions not captured by our dummies for commuting area, unemployment rate, or their interaction term. Rehwald et al. (2015) propose testing whether the ER variables are uncorrelated with a wide range of observed municipal level indicators, as a potential correlation would suggest unobserved third variables correlated with over-time changes in the municipalities' use of ALMMs. We have tested the correlation between the four ER variables and 22 municipal variables measuring the level of municipal characteristics (11 variables) and annual differences in these characteristics (11 variables) (table A1, appendix A). The test uses an OLS panel data model with random effects (containing 1,176 municipal-year-four-month records). Municipal-level characteristics are measured in year *t*-1; municipal differences are calculated as levels in year *t*-1 minus levels in year *t*-2. Six of the 88 variables are significant at a 5% level. [7] A binominal distribution test yields a 27.7% probability of observing six or more significant variables. We therefore assume that our ER variables are uncorrelated with municipal conditions.[8]

4.3 Estimation

From the hazard rates, we form the joint density (survivor function for right censored spells) of all types of spells for a single individual and subsequently the likelihood function for our sample. We maximize the joint likelihood function using the exclusion restrictions as non-parametric identification of the treatment effects (Dean et al. 2015).

5. Results and robustness tests

5.1 Results

Tables 3–5 show the results of our simultaneously estimated mixed proportional hazard-rate model of the transition to the four ALMMs, to employment, and out of employment.[9] The model has nine mass points (section 4.1).

[7] We also tested whether the relative number of ongoing monthly ALMM spells is correlated with municipal variables. Of the 88 municipal variables, 22 are significant at the 5% level.
[8] When we extend the ER variables measurement period to 6 months, they become significantly correlated with municipal characteristics. To test robustness, we estimate our model using monthly ER variables (section 5.2).
[9] The model is estimated as a cloglog model using the "Frisch" program developed by Simen Gaure from the Frisch Centre (http://folk.uio.no/sgaure/ubuntu).

Table 3 shows the first-stage results of the estimation of the effect of the four excluded variables on transitions into the four treatment types: ordinary education, non-formal education, subsidized internships, and subsidized employment, respectively. Most of the ER variables have a significant effect on the selection into the four ALMMs. The first-stage F-statistic for joint significance is 294.38 with DF=16 and thus highly significant (p=0.000). Generally, municipalities' tendency to *start new spells* with a specific ALMM has a positive correlation with individual propensity for enrollment in that *same* ALMM.

Table 3
First-stage results: over-time variation in the municipalities' propensity for starting treatments

	Transition to:			
Exclusion restriction variable	Ordinary education	Non-formal education	Subsidized internships	Subsidized job training
Differences in started ordinary education spells	0.374(0.041)***	-0.060(0.021)**	-0.046(0.029)	0.085(0.052)
Differences in started non-formal education spells	-0.002(0.004)	0.027(0.002)***	0.001(0.003)	0.004(0.005)
Differences in started subsidized internships spells	0.014(0.013)	-0.016(0.006)**	0.050(0.007)***	0.015(0.014)
Differences in started subsidized job training spells	-0.179(0.055)**	-0.030(0.024)	0.082(0.031)**	0.184(0.060)**

Note: *<0.05, **<0.01, ***<0.001. N=88,948. The four equations are estimated simultaneously with the two equations in Table 5. See Table 2 for control variables and section 3.4. for descriptions.

Tables 4-5 summarize second-stage results. Table 4 shows the estimation results for the effects on the transition into the four ALMMs of a person participating in an ALMM (OnP – on-program) or having participated (AP – after-program). A positive on-program coefficient means that the sick-listed worker relatively often switches programs directly. Transitions from being "on program" in one ALMM into the same ALMM are not meaningful. Subsidized internships and non-formal education have positive on-program and after-program coefficients to subsidized job training, indicating that these ALMMs are to some extent stepping-stones into job training. Generally, the after-program coefficients are positive, indicating that the sick-listed workers often participate in more than one measure or later enroll in the same type of measure. This finding holds particularly true for ordinary education and job training.

Table 4
Estimation results: transition to ALMMs

Treatment variables	Transition to: Ordinary education	Non-formal education	Subsidized internships	Subsidized job training
AP ordinary education	0.863 (0.068)***	-0.059 (0.042)	0.126 (0.067)	-0.006 (0.117)
AP non-formal education	0.057 (0.050)	0.397 (0.024)***	0.353 (0.028)***	0.250 (0.058)***
AP subsidized internships	0.430 (0.069)***	-0.109 (0.027)***	0.124 (0.042)**	0.501 (0.062)***
AP subsidized job training	-0.250 (0.129)	0.219 (0.047)***	0.306 (0.059)***	0.965 (0.091)***
OnP ordinary education	---	-1.575 (0.055)***	-1.555 (0.107)***	-1.150 (0.190)***
OnP non-formal education	-0.094 (0.051)	---	0.746 (0.026)***	0.429 (0.059)***
OnP subsidized internships	-0.013 (0.084)	-0.448 (0.031)***	---	1.841 (0.059)***
OnP subsidized job training	-1.733 (0.204)***	-1.832 (0.080)***	-2.029 (0.135)***	---

Note: *<0.05, **<0.01, ***<0.001. N=88,948. AP: after program; OnP: on program. The four equations are estimated simultaneously with the two equations in Table 5 (see section 3.4. for control variable descriptions). The model has nine mass points.

The purpose of an ALMM is facilitating reentry into employment. Table 5 shows the effects of our four ALMMs on the transition both into and out of ordinary employment. As ordinary employment by definition excludes enrollment in an ALMM, there is no on-program effect on the employment duration (estimated by transition out of employment).

Table 5
Estimation results - transition to employment and out of employment

Treatment variables	Transition: To employment	Out of employment
AP ordinary education	0.870 (0.057)***	-0.840 (0.089)***
AP non-formal education	-0.045 (0.031)	0.404 (0.037)***
AP subsidized internships	0.007 (0.039)	0.498 (0.053)***
AP subsidized job training	0.744 (0.067)***	-0.101 (0.113)
OnP ordinary education	-1.047 (0.072)***	----
OnP non-formal education	-0.295 (0.030)***	----
OnP subsidized internships	0.045 (0.043)	----
OnP subsidized job training	0.816 (0.061)***	----

Note: *<0.05, **<0.01, ***<0.001. N=88,948. AP: after program; OnP: on program. The two equations are estimated simultaneously with the four equations in Tables 3 and 5. See section 3.4. for control variables descriptions. The model has nine mass points.

Table 5 shows a relatively large employment effect of having ended ordinary education. Thus participation in ordinary education has a positive and significant effect on the transition to non-subsidized employment, and a negative and significant effect on the transition out of it, with the latter indicating a positive effect on the employment duration. Moreover, the negative on-program effect on employment confirms that participation in ordinary education yields a significant lock-in effect that counteracts the positive effect of completing ordinary education.

Non-formal education has a negative net employment effect, i.e., a negative lock-in effect on the transition into employment and a positive effect on the transition out of employment. Subsidized internship has also a negative net employment effect, i.e. no effect on the transition to non-subsidized employment and a positive and significant effect on the transition out of it. Subsidized job training has a relatively large and positive effect on the transition to non-subsidized employment (for both on- and after-program effects) but no effect on the transition out of it.

5.2. Average treatment effects

This section presents the average treatment effects on the duration until RTW and the duration of the subsequent employment spell of each of our four treatments. For simplicity, we calculate average effects for only one treatment at a time, thus ignoring spillovers (see appendix B for an outline). Table 6 shows mean net effects of these treatments on the duration until RTW and on the duration of this employment. The mean net effect of ordinary education, consisting of both an on-program and an after-program effect, is near zero. The coefficient of 0.059 means that ordinary education on average delays the transition into employment by two days. Job training has a substantial average effect, shortening the duration until RTW by three weeks.

Table 6

Mean duration effects of treatments (measured in months)

	Transition:	
	To employment	Out of employment
Treatment variables	Net-effect (On program after program effects)	After program effect
Ordinary education	0,059	0,382
Non-formal education	0,230	-0,214
Subsidized internships	-0,003	-0,378
Subsidized job training	-0,710	-0,002

Ordinary education prolongs the subsequent employment duration by 11 days. The effect of subsidized job training is effective in shortening the duration until RTW but not the duration of the subsequent employment spell. While this result may indicate that ordinary education yields a "network" effect improving the match between the sick-listed worker and an employer, it does not provide the sick-listed worker with additional skills for improving the match quality.

In sum, our findings are in line with Markussen and Røed (2014), who also find a negative net employment effect of non-formal education, a positive net effect of ordinary education, and a positive net effect of subsidized job training. Importantly, however, our findings add to those of Markussen and Røed by suggesting that the positive effect of ordinary education stems from a stimulation of the subsequent employment duration, whereas the positive effect of subsidized job training stems from a positive effect on the transition into employment.

5.3. Robustness test

To test the robustness of our estimates, we perform three estimations. First, our choice of the interval in which we observe municipalities' start of ALMM spells may bias our estimates. Our ER variables measure differences over time in municipalities' newly started ALMM spells in four-month intervals, reflecting municipalities' preferences for using specific ALMMs. Compared to using ERs based on *monthly* data, the main advantage is that four-month intervals reduce noise, i.e., more individuals start ALMMs during a four-month interval than during one month. Consequently, another advantage is that the identification of treatment effects relies on data from both big and small municipalities. However, monthly intervals may be less prone to correlation with unobserved third variables or less likely to reflect structural conditions or local policies. To test the robustness of our treatment estimates, we estimate our model using monthly ER variables (see model II, table 7).

Table 7

Robustness tests: model with monthly exclusion restriction (ER) variables and model with time-varying health

	I. Model from Table 5[1)]		II. Model with monthly ER variables[2)]		III. Model with time-varying health dummy[3)]		IV. Model with dummy for participation in more than one treatment[4)]	
	Transition:		Transition:		Transition:		Transition:	
Treatment variables	**To employment**	**Out of employment**	**To employment**	**Out of employment**	**To employment**	**Out of employment**	**To employment**	**Out of employment**
AP ordinary education	0.870*** (0.057)	-0.840*** (0.089)	0.915*** (0.056)	-0.980*** (0.099)	0.800*** (0.049)	-0.908*** (0.082)	0.775*** (0.053)	-0.726*** (0.083)
AP non-formal education	-0.045 (0.031)	0.404*** (0.037)	-0.083** (0.029)	0.417*** (0.041)	-0.092** (0.030)	0.428*** (0.039)	-0.077* (0.031)	0.429*** (0.040)
AP subsidized internships	0.007 (0.039)	0.498*** (0.053)	-0.073* (0.037)	0.485*** (0.053)	-0.151*** (0.038)	0.435*** (0.054)	-0.103* (0.041)	0.452*** (0.057)
AP subsidized job training	0.744*** (0.067)	-0.101 (0.113)	0.859*** (0.069)	-0.623*** (0.112)	0.780*** (0.060)	-0.453*** (0.098)	0.465*** (0.063)	-0.201* (0.101)
OnP ordinary education	-1.047*** (0.072)	----	-0.986*** (0.071)	----	-1.097*** (0.067)	----	-0.974*** (0.069)	
OnP non-formal education	-0.295*** (0.030)	----	-0.362*** (0.028)	----	-0.338*** (0.031)	----	-0.277*** (0.031)	
OnP subsidized internships	0.045 (0.043)	----	-0.055 (0.040)	----	-0.002 (0.041)	----	0.036 (0.042)	
OnP subsidized job training	0.816*** (0.061)	----	0.862*** (0.064)	----	0.692*** (0.056)	----	0.649*** (0.060)	
Participating in more than one treatment	----	----	----	----	----	----	0.286*** (0.039)	-0.013 (0.057)

Note: *<0.05, **<0.01, ***<0.001. N=88,948. AP: after program; OnP: on program. Displayed equations are estimated simultaneously with the four equations in table 3. See section 3.4. for control variable descriptions.

(1) The model has nine mass points.

(2) The model has eight mass points.

(3) The model has ten mass points.

(4) The model has ten mass points.

All coefficients in the model with monthly IVs (model II) have the same signs and, in most cases, the same magnitude as our preferred model (model I). The differences between the coefficients in the two models are not statistically significant.

Second, our estimates may also be biased if sick-listed employee health changes over time. Although mixed-proportional-hazard-rate models adjust for time-invariant unobserved heterogeneity, if unobserved health components change over time, this “dynamic” endogeneity may potentially bias treatment estimates in duration model, e.g. if individuals with unobserved health improvements are treated more (or less) often and also relatively more often RTW. We therefore include a time-varying dummy variable that equals 1 during sickness benefit receipt and 0 otherwise. As this (endogenous) variable captures changes in both in health status and benefit receipt, we also test whether changes in replacement rates significantly alter the estimated treatment effects. Model III in Table 7 shows the treatment estimates of a model with the time-varying benefit indicator.

The treatment estimates in the model with a time-varying health indicator are largely the same as those in our preferred model (model I), suggesting that our estimates are not significantly affected by unobserved changes in health or replacement rates. However, the after-program effect of subsidized job training on the transition out of employment is larger in the robustness test (-0.453) than in our preferred model (-0.101), indicating that we may underestimate the employment effect of subsidized job training.

Third, the estimates of our econometric model capture the effect of program participation without considering that some individuals participate in more than one treatment. If the total effect of participation in, for example, two treatments is bigger than the sum of the individual effects of each of the two treatments, this simplification may potentially lead to either under- or overestimation of treatment effects. In other words, we disregard possible synergy effects.[10] To test this source of

[10] In theory, we could estimate the effects of different sequences of treatments; however, doing so is unfeasible for computational reasons.

potential bias, we re-estimate our model including a dummy variable that equals 1 when the individual participates in more than one treatment and 0 otherwise (model IV, table 7).

The estimates in model IV suggest that participation in more than one treatment has a significant and positive effect on RTW (0.286) and no effect on the transition out of employment. This relative modest net employment effect, coupled with the on-program and after-program treatment coefficients being relatively close to the coefficients in the preferred model (model I), suggest that ignoring sequential treatments is not a serious problem.

6. Conclusion

Using register data of 88,948 workers sick-listed for over four weeks, we study how participation in active labor market measures (ALMM) affect the probability of returning to ordinary employment and the probability of ending this employment. We focus on four ALMM types: ordinary education, non-formal education, wage-subsidized internships, and wage-subsidized employment. We use a multivariate mixed-proportional-hazard-rate model to simultaneously estimate the transition into the four types of ALMMs, and we investigate the effect of ALMMs on the duration until returning to non-subsidized employment and the duration of this employment. To identify causal treatment effects, we exploit over-time variation in 98 job centers' use of ALMMs.

Our results indicate that ordinary education and particularly subsidized job training have a positive employment effect. While the positive effect of ordinary education results from a positive effect on the duration of the subsequent employment spell, the positive effect of subsidized job training results from a positive effect on the transition into employment. Moreover, non-formal education (e.g., shorter courses) and wage-subsidized internships have negative employment effects.

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Appendix A.

Table A.1. Description of municipal-level variables in testing the exclusion restrictions

Description
Average age
Yearly number of newborns per 1,000 females aged 15-49
Percentage of population with Danish origin
Life expectancy at birth
Municipality in-migration in percentage of population
Municipality out-migration in percentage of population
Percentage of population with less than 11 years schooling
Percentage of employed persons working in primary sector
Labor force participation (in percentage)
Unemployment (full-time equivalents) in percentage of labor force
Sick-listed persons (full-time equivalents) in percentage of population

APPENDIX B

We calculate the mean difference in the expected duration until RTW and the subsequent duration of employment conditional on treatment status and some observed covariates x. We obtain the expected duration until RTW from the survivor function[11] conditional on treatment from time t to $t + s$ where s is the length of the treatment.

$$
\begin{aligned}
E\left(T \middle| x, d_i^{in}(t), d_i^{post}(t+s)\right) \quad = & \quad \int_0^t exp\big(-h_e(T|x, d_i^{in}=0, d_i^{post}=0\big)dt \\
+ & \quad \int_t^{t+s} exp\big(-h_e(T|x, d_i^{in}=1, d_i^{post}=0\big)dt \qquad (1) \\
+ & \quad \int_{t+s}^{\infty} exp\big(-h_e(T|x, d_i^{in}=1, d_i^{post}=1\big)dt,
\end{aligned}
$$

State i can be any of the four treatments—sick-leave, ordinary education, non-formal education, wage subsidized internships and wage subsidized job training, All notation follows section 4.1. In our simulation we set t to 5 (months) and s to 5 (months) in all calculations, irrespective of which treatment we are analyzing. To calculate the differences in expected durations with and without treatment, we calculate (1) with and without treatment dummies switched on (at T = 5 and 10 months, respectively). The differences in expected duration are the average treatment effect, conditional on treatment timing.

We factor in the effects from unobserved heterogeneity in terms of discrete random effects in the model. To do so, we use (1) to calculate expectations with and without treatment for each of the mass points, and then calculate a combined expected duration with a weighted average using the estimated mass point weights as weights.

For the treatment effects on the subsequent employment spell, we make similar calculations. However, for employment spells calculations are easier because the treatment is either switched on at the beginning and throughout the spell or it is not. Thus, when calculating treatment effects for the employment spells, we need make no assumption about the timing of the treatment.

[11] In calculating the marginal effects, we ignore the possibility of exiting into anything other than employment. Thus we ignore possible effects from one treatment spilling over to entry into other treatments.

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