Sick of being activated?*

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In the past few years activation policies intended to activate unemployed people have become increasingly important. However, empirical evidence on potential side effects of activation tools is missing. We argue that under certain circumstances, activation can set an incentive for unemployment insurance (UI) benefit recipients to report sick (negative side effect of moral hazard). In this paper the following question will be answered: Does the transition rate of UI benefit recipients into sickness absence increase due to an activation tool such as a job vacancy proposed by the caseworker (JVC)? We identify the effect using a piece-wise constant mixed proportional hazard specification where the arrival of a JVC is included as a time-varying covariate. For both men and women, empirical evidence indicates an increased transition into sickness absence once a JVC has been proposed. We interpret our findings as a strong hint for moral hazard behaviour.

1 Introduction

This paper studies the effect of activation on sickness absence among unemployment insurance (UI) benefit recipients. The effect is of interest due to three reasons:

First, it seems to be an empirical fact that sickness absence among employed workers is negatively related to the unemployment rate (e.g. Askildsen, Bratberg, and Nilsen (2005), Arai and Thoursie (2005), Ose and Dyrstad (2001)). While there are different explanations for this finding[1], little is known about the sickness absence behaviour of unemployed people

*Preliminary version! Please do not quote!

[1]In the ecnomic literature this finding is mainly traced back to two different effects. According to the discipline hypothesis in periods of high unemployment workers tend to either shirk less or go to work even if they are sick. According to the composition effect explanation we assume that in economically bad times firms tend to fire the "marginal worker", who is on average less healthy than his collegues or put it in a different way: in good times also the "marginal (unemployed) worker" will be hired which will increase the sickness absence rate among employed workers.

themselves. Second, in the past few years activation policies intended to activate unemployed people have become increasingly important. Besides active labour market policies (ALMP), activation strategies include regular reporting and confirmation of unemployment status, monitoring of the job-search efforts and/or action plans (Tergeist and Grubb, 2006). Empirical research in this area mostly focuses on effects of activation on the labour market performance of the individuals concerned. The main outcomes considered are transition into employment, employment stability and earnings ((Kluve, 2006)). Empirical evidence on potential side effects of activation policies is missing so far. Third, as in most OECD countries health insurance is organised as a social insurance and as an increase in sickness absence induces an increase in health expenditures, it is public costs that are involed.

Aiming to shed some light on potential interrelation effects between sickness absence and activation policies this paper answers the question: Does the transition rate of UI recipients into sickness absence increase due to the application of an activation tool such as a job vacancy proposed by the caseworker (JVC)?

The effect is assumed to arise due to a moral hazard problem based on the following regulation: each UI recipient has to comply with certain rules, e.g. to search actively for a job. Otherwise he might be sanctioned, meaning (for our observation period) a benefit cut for a period of twelve weeks (= sanction). In order to avoid a sanction we assume an incentive to report sick to exist if the UI recipient is not willing to comply with these rules. We estimate the effect of the first JVC on the transition rate into sickness absence using micro data of the German Federal Employment Agency of a sample of people who entered UI receipt in West Germany during April 2000 and March 2001. By applying a piecewise constant mixed proportional hazard model in continuous time with a time-varying indicator of the JVC we identify the effect of interest.

2 Literature review

While there is some empirical evidence of moral hazard problems among employed workers to report sick (e.g. Hesselius, Johansson, and Larsson (2005), Ichino and Riphahn (2005) Johansson and Palme (2005), Riphahn and Thalmaier (2001) or Thoursie (2004)) or in the demand for health care (e.g. Riphahn, Wambach, and Million (2003)), only three studies on the determinants of sickness absence among unemployed individuals were found: Larsson (2006) and Larsson and Runeson (2007) find empirical support of incentive effects to report sick arising due to different sizes between sickness insurance benefits and unemployment insurance benefits in Sweden; according to Henningsen (2008) results, the hazard rate for transition from unemployment to sickness insurance increases sharply in the last months before UI exhaustion for Norwegian UI recipients arising due to the possibility to prolong benefit receipt by reporting sick.

To our knowledge no study on an effect of activation policies on sickness absence among UI recipients exists.

3 Institutional setting

During our observation period, UI benefits were paid if a person had been employed in a job subject to social contribution for at least twelve months within the seven years previous to unemployment. The maximum entitlement duration depended on the duration of the previous employment period and age. The maximum duration was 32 months for people who were older than 56 years old and who had been employed for at least 64 months in the seven years previous to unemployment. Until 2005 a UI benefit recipient received additional means-tested unemployment assistance (UA) when his claims to UI benefits terminated. The monthly UI benefit amount received was 67% of the previous monthly net wage for unemployed persons with a dependent child and 60% for those without.[2] The time period of employment relevant for the calculation of the monthly UI benefits amount was twelve months.

In case of temporary disability due to sickness UI benefits are paid by the federal employment agency (FEA) for up to six weeks. Within this period the maximum entitlement duration continues to diminish. Thus, there is no financial incentive to report sick in order to prolong the UI entitlement period. If the cumulated period of sickness exceeds six weeks, then the unemployed person has to apply for sickness benefits at the health insurance.

[2]The replacement ratio for UA was 57% and 53% respectively.

Yet, there is an incentive for UI benefit recipient to report sick: in order to be eligible for the UI benefits, the recipient has to comply with certain rules that are part of activation policies: e.g. he has to actively look for a job, accept suitable job offers and take part in ALMP measures if the caseworker demands it from him. The incentive to report sick arises if the UI recipient does not want to comply with these rules.

In this study we analyse the effects of one aspect of activation policies: each UI benefit recipient who is offered a suitable JVC has to a) either apply for the job or b) take up the job in case of a successful application. A JVC is a placement proposition on a vacancy in form of a job description that is given to the UI recipient by the caseworker. Note that a JVC does not necessarily mean that a potential employer is already informed about the candidate nor even that he proposed to hire him. Refusing work, in the sense of refusing to apply for a suitable job or refusing a suitable job, could cause a sanction of twelve weeks and three weeks respectively if the job would have been temporary only. During the period of a sanction UI benefits stop *completely*.[3] We assume each UI recipient to try to avoid this financial punishment. Thus we assume an incentive to report sick to exist after the arrival of a JVC. In the following section this hypothesis will be derived from job search theory.

4 Theory

In order to derive a hypothesis about the reaction of a UI recipient once a JVC has arrived, we start by presenting the main features of the job search model with sanctions introduced by Abbring, van den Berg, and van Ours (1996; 2005), the latter referred to as ABO05. A basic job search model with endogenous search intensity is presented e.g. by Mortensen (1986). ABO05 extend this model by introducing sanctions. According to ABO05, we consider a situation where a UI recipient has become unemployed and currently is searching

[3]In principle we must distinguish between JVC with information on legal remedies available, i.e. JVC offering jobs that the individual had to accept as they were suitable, and those without. The latter refer to jobs that the UI recipient does not need to accept due to e.g. a too low wage or due to a too large geographical distance to the potential employer. In the empirical analysis we cannot distinguish between those two types; we do not assume an incentive to report sick after a JVC without information on legal remedies to exist. Thus our estimates will be biased towards zero in the case that these JVCs are stored in our data as well.

for a job. The expected utility of unemployment depends positively on the amount of the UI benefits and negatively on the search costs and the search intensity. The reservation wage is determined by the expected utility of unemployment. The transition rate from unemployment to employment is assumed to depend on the (exogenously given) job offer arrival rate and the search intensity and negatively on the reservation wage.

Following ABO05 we now introduce sanctions in this model. Sanctions may affect individual behaviour in two different ways. Accordingly, we distinguish between two different aspects of sanctions: the institutional aspect meaning the individual acts in a world where he might be sanctioned (*ex ante*) and the aspect of the actual imposition of a sanction (*ex post*). We consider a UI recipient in a system with sanctions. At the first sight one might assume that every UI recipient tries to avoid a sanction and therefore behaves *ex ante* in a certain way in order to prevent a sanction. If this was the case we would not observe sanctions at all. At the second sight we might think that UI recipients can perfectly anticipate when a sanction is imposed and define their choices accordingly. ABO05 argue that the results of their study as well as institutional aspects contradict such a view.

A major assumption of their model is that individuals cannot foresee *when* exactly a sanction is imposed, which corresponds to the so called no-anticipation assumption. ABO05 base this assumption on the observation of regional differences in the strictness by which sanctions are applied.

Yet, it is assumed that unemployed people do know the relationship between their behaviour and the probability of being sanctioned, i.e. an unemployed person knows that a certain type of behaviour will raise the probability of being sanctioned while another type will reduce the probability.

We apply the ABO05 model only to the situation of sanctions due to refusing a JVC. We adapt the model to our setting with respect to: First, we relax the no-anticipation assumption by arguing that certain sanctions can only be applied if the individual refuses to behave according to the caseworker's concrete demand, e.g. applying for a concrete job offer. We assume that in this situation the individual is aware of the immediate risk of being sanctioned. Second, we argue that the individual can avoid a sanction by reporting sick.

If the person refuses to apply for the job, the probability of being sanctioned due to refusing a JVC, ($p(SJVC)$), is greater than zero. If on the other hand the individual either applies for the job and respectively accepts a job offer or reports sick, the probability of a sanction is zero:

$$p(SJVC) = \begin{cases} > 0 & \text{if refusing to apply} \\ 0 & \text{if applying for / accepting job} \\ 0 & \text{if sick.} \end{cases} \tag{1}$$

Accordingly, at the moment at which a JVC arrives, the individual has three possibilities of how to react: first, he can refuse to apply for the job and risk of being sanctioned; second, he can apply for the job; and third he can report sick in order to avoid both, having to accept the job in case he would successfully apply or being sanctioned.

As ABO05 show that the utility of being unemployed after the imposition of a sanction is lower than the utility of being unemployed before the imposition of a sanction, we derive the following hypothesis: In the moment at which a JVC arrives the average probability of reporting sick increases.

5 Estimation method

In order to identify the effect of a JVC on the transition rate into sickness absence, we model the hazard into sickness absence including a time-varying covariate indicating the first JVC as described in the following. The variables of interest are the duration in UI receipt until reporting sick and three time-varying covariates, being a set of indicator after the first JVC being 1 during the two weekly period and 0 else. Those cases who leave UI benefit receipt without reporting sick are treated as right censored. In order to control for observed and unobserved heterogeneity, we estimate the following model using a piece-wise constant mixed proportional hazard (MPH) specification in continuous time:

$$\lambda(t \mid x, D^{JVC}_{t=w}, v) = exp(\sum_{j=1}^{c} \alpha_j b_j(t) + x'\beta + \sigma D^{JVC}_{t=w} + v) \tag{2}$$

where α_j are the parameters of j time intervals to be estimated and b_j the corresponding time interval indicators with $b_j(t) = 1$ if $c_{j-1} \leq t \leq c_j$ (=0 otherwise). x' is a vector of time-constant observed covariates and β the corresponding coefficient vector.

$D^{JVC}_{t=w}$ is a set of time-varying covariates:

$$D^{JVC}_{t=w} = \sigma_1 D^{JVC}_{t=JVC+1,2weeks} + \sigma_2 D^{JVC}_{t=JVC+3,4weeks} + \sigma_3 D^{JVC}_{t=JVC+5,6weeks}, \tag{3}$$

being 1 for a two-week period after the JVC has arrived ($w = JVC + 1, 2$ weeks), between week three and four after the JVC has arrived ($w = JVC + 3, 4$ weeks) and between week five and six after the JVC has arrived ($w = JVC + 5, 6$ weeks) and 0 else, with σ_1 - σ_3 being the coefficients of interest.

The term v in equation 2 captures time-constant individual-specific unobserved heterogeneity. Beside the usual regularity assumptions on the determinants of a MPH model[4], identification of the effect of a JVC relies on the assumption that the arrival of a JVC is a predictable process (Van den Berg (2001)). Predictability means that the values of all explanatory variables, including the JVC, for the hazard into sickness absence at any point in time must be known and observed at t. This means that the values of the explanatory variables, including the JVC, at t are influenced only by events that have occurred up to time t and that these events are observed.[5] In our case the caseworker will assess the aptitude of a UI recipient based on the information stored in a database, e.g. the branch of the last job, the professional education and the desired qualificational level. Since we have access to this information it is plausible that the assumption of predictability holds. According to Van den Berg (2001) given predictability models with time-varying covariates can be handeled by standard tools of survival analysis.

[4]For the regularity assumptions see Van den Berg (2001) p. 3395.

[5]As Van den Berg (2001) points out predicatability does not mean that the whole future realisations of x' can be predicted.

Following the non-parametrical approach for unobserved heterogeneity proposed by Heckman and Singer (1984), the distribution of v is modelled as masspoints, where we specify two of them $(k = 1, 2)$.

We have a look at the survival rate as it will be part of the individual likelihood contribution:

$$S_k(t \mid x, D^{JVC}_{t=w}, v_k) = exp(-\int_0^t \lambda_k(u)du) \tag{4}$$

with $k = 1, 2$ as indicator of the two masspoints of the unobserved heterogeneity distribution. The log-likelihood function is given by:

$$\begin{aligned} lnL = \sum_{i=1}^{N} ln\{p_1(\lambda(t \mid x, D^{JVC}_{t=w}, v_1))^{1-c_s} S(t \mid x, D^{JVC}_{t=w}, v_1) + \\ (1 - p_1)(\lambda(t \mid x, D^{JVC}_{t=w}, v_2))^{1-c_s} S(t \mid x, DD^{JVC}_{t=w}, v_2)\} \end{aligned} \tag{5}$$

with p_1 as probability of being member of the group with $k = 1$ and c_s as a censoring indicator if no transition into sickness absence was observed.[6]

6 Data and first descriptives

Our empirical analysis is based on administrative data of the FEA. Our sample consists of 400.000 randomly drawn persons who entered UI receipt between April 2000 and 2001 in West Germany. Employment history spells stored as integrated employment history (IEB) were merged to the inflow spells.[7] Information on transition into sickness absence as well as on the arrival of JVCs is not contained reliably in IEB.[8] Instead we use two additional sources of the data ware house of the FEA, namely information from job seekers pool database (ASU) and the applicants pool database (BewA) in order to draw daily information on the transition into sickness absence as well as daily information on the JVC

[6] $p_1 = exp(lam)/(1 + exp(lam))$ with lam being a parameter to be estimated.

[7] For further information about the data sets used see Dundler (2006).

[8] Note that in IEB only those sickness spells are included where one unemployment spell was found after the sickness spell. Thus if the individual maximum entitlement duration ends during a sickness spell and the person does not show up at the local labour market agency anymore, in this case the transition into sickness cannot be reconstructed.

received.[9] We will control for several observed characteristics that are assumed to capture heterogeneity among UI recipients and might affect either the transition into sickness absence or jointly the transition into sickness absence and the arrival rate of the first JVC: e.g. health restrictions (self assessed as well as assessed by the caseworker), cumulated employment and unemployment duration during three years before UI start, sickness absence during an unemployment spell in the past[10], information on age, variables on the household context (marital state, age of children), variables on the desired job (full-time and qualification level), (expected) commuting distance to the previous job, a dummy indicating German citizenship[11], previous wage and educational level. Additionally we control for characteristics on the regional level, e.g. regional unemployment rate. As studies on sickness absence among workers found quite different absence behaviour for women and men, we will conduct the empirical analysis separately for women and men. Table 4 in the appendix contains descriptives on the sample by gender and by sickness transition. We right-censor the data if no transition into sickness is observed until day 380. The following cross table contains the numbers of UI recipients who have a transition into sickness by those receiving the first JVC by gender:

Table 1: Number of Sickness Registrations by Number of First Job Vacancy Proposed by Caseworker (JVC)

	First JVC: no	%	yes	reporting sick within weeks 1, 2 after JVC in %	reporting sick within weeks 3, 4 after JVC in %	reporting sick within weeks 5, 6 after JVC in %	%	total	
reporting sick:	**Women:**								
no	82,501	(89.18)	55,930				(91.12)	138,431	(89.96)
yes	10,007	(10.82)	**5,449:**	19.93	12.66	8.67	(8.88)	15,456	(10.04)
total	92,508	(100.00)	61,379				(100.00)	153,887	(100.00)
	Men:								
no	137,895	(92.86)	76,534				(93.28)	214,429	(93.01)
yes	10,605	(7.14)	**5,516:**	23.48	12.85	9.54	(6.72)	16,121	(6.99)
total	148,500	(100.00)	82,050				(100.00)	230,550	(100.00)

[9] The transition into sickness was built using ASU, which is the original data source of the sickness spell contained in IEB: the end of a job seeking spell that ended due to sickness was used as the day of the transition into sickness absence. Information in BewA was used for building the arrival of the first JVC. As not the JVC arrival days itself, but the cumulated number of JVCs per person are stored in the original data, we built the arrival day by sorting all spells per person chronologically and then taking the start of the spell as the JVC arrival day where the former contained less JVCs. Of all of these arrivals during one UI spell, only the first was finally used for the empirical analysis.

[10] We include three dummies indicating whether there is a sickness spell during months 10-12 before UI start.

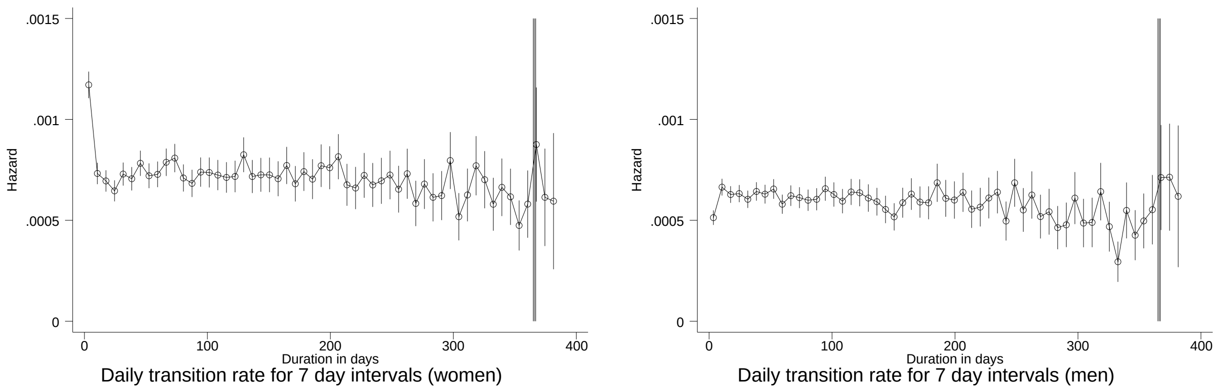
[11] UI recipients with German citizenship might be more informed about the institutional setup.

According to the figures in table 1 10.04% of our female sample reports sick during UI receipt. Around 8.88% of those female UI recipients who receive at least one JVC report sick. Among these women 19.93% do so within the first two weeks, 12.66% within week three or four and 8.67% between five and six weeks after the arrival of the first JVC respectively. These are the events we are interested in. The differences in these probabilities might be a hint for an impact of the JVC on the transition into sickness absence.

Turning to male UI recipients, we find that they report less sick compared to women: around 6.99% of our male sample. Among the 230,550 men in our sample 82,050 receive at least one JVC of whom 6.72% report sick; 23.48 % among these report sick within the first two weeks. However, compared to the cases who report sick without having been proposed a JVC before, 10.82% of for women and 7.14% of men, we do not find a hint for our hypothesis.

Though these first findings do not necessarily support our idea of an effect of the arrival of a JVC and the transition into sickness absence, we proceed with our analysis.

In order to get a first impression about the shape of the sickness hazard over time we have a look at the transition rate into sickness aggregated over 7 days by gender:



(a) Women

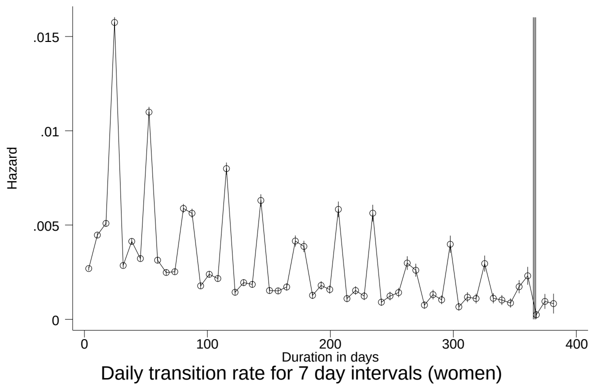
(b) Men

Figure 1: Transition rate into sickness absence

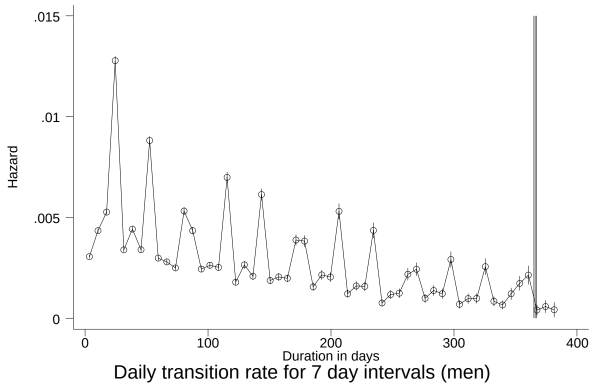
The graphs show that the (unconditional) hazard rate into sickness absence is about 0.0007 for women and 0.0006 for men, i.e. the probability of reporting sick during any day on having stayed in UI receipt until that day is around 0.07% and respectively 0.06%.

What we also see is that the hazard appears to be quite constant until a couple of weeks before the first 365 days of UI receipt is over, decreasing until day 365 and increasing slightly shortly after (the vertical line highlights one year).

We are not only interested in the event of reporting sick but also in the arrival of a JVC. In order to get a first impression about the shape of the JVC arrival rate over time we have a look at the JVC rate aggregated over 7 days:



(a) Women



(b) Men

Figure 2: JVC arrival rate

What first catches our attention is a peak of the arrival rate of the first JVC in the fourth week of UI receipt. We explain this peak by two circumstances. First, an interplay of calendar time and individual unemployment duration: around 57% of the sample start their UI spell during the first seven calendar days of a month (see table 5). The calendar time of the JVCs shows the corresponding picture: around 83% of the JVCs considered arrive between calendar day 20 and 26 of a month (ibidem). According to the IAB data department this is due to administrative reasons: most of the JVCs that were proposed to the UI recipient during the last month were delivered once a month to the statistical department of FEA. Thus instead of observing the exact JVC arrival day, these cases we observe the day until which during the previous month the JVC has arrived.[12] Second, by a "processing rhythm", meaning that UI recipients might have met or had to meet their caseworkers each four weeks in order to assess the individual situation. The caseworkers

[12]Note that in section 7 we conduct a robustness check by randomly assigning JVC arrival days if the observed day was between calendar day 20 and 26.

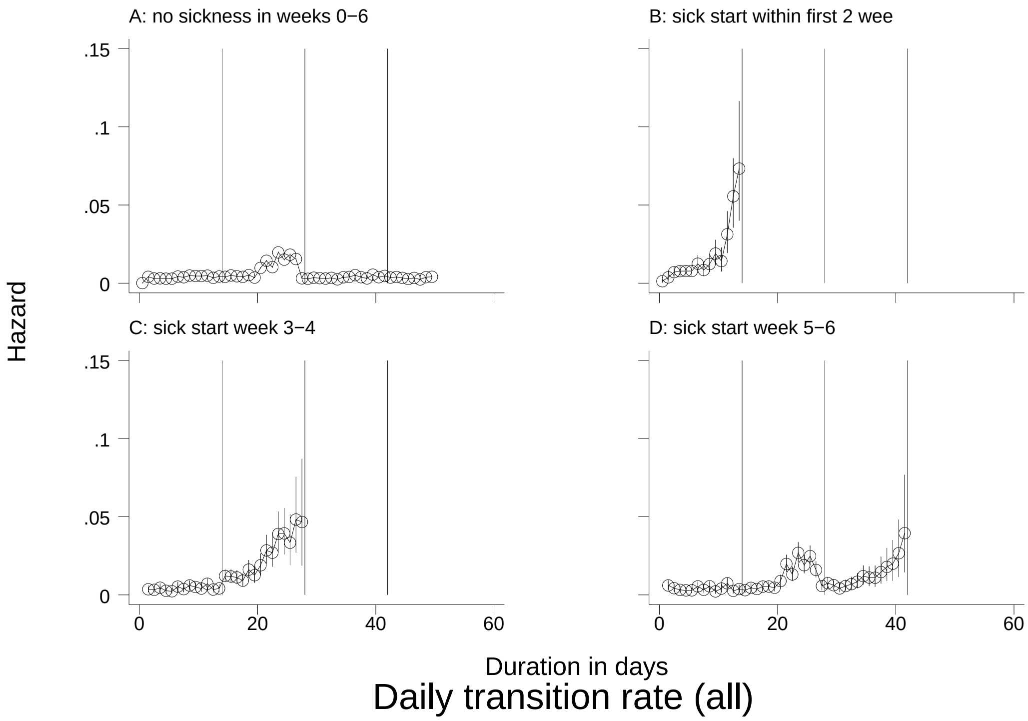
might have used these appointments for offering a JVC. What we see in the graphs is that the hazard of a JVC is quite high at the beginning and is decreasing over time. The pattern over time shows a decrease of the arrival of a JVC that might be either due to a depreciating human capital stock or due to sorting. We find the hazard of a JVC for both men and women to be around 0.005 and 0.015 (peak) at the beginning of UI receipt and is decreasing to around 0.001 and 0.025 (peak) after one year (the vertical line highlights one year).

Graphical evidence on the JVC effect

Since these two hazards separately cannot provide information on a potential correlation between the two events, we use a method proposed by Abbring, van den Berg, and van Ours (1996) in order to conduct some first graphical checks of the effect of a JVC on the transition into sickness: what we intuitively expect is that those UI recipients reporting sick during a certain period of time have a higher hazard of a JVC during a short time interval *before* that period compared to those who report sick in a short period after. Thus, as a first check of potential correlations between the arrival of a JVC and the transition into sickness absence, we have a look at the following graphs that show the JVC arrival rate by (A) those who did not leave UI into sickness absence during the first six weeks and those who left UI into sickness absence (B) during the first two weeks, (C) during week three or four and (D) during week five or six[13]:

[13]Note that loosely speaking conducting this check we hold the future constant (sickness transition) and look into the past (JVC arrival) while in the estimated model reported in the next section we hold the JVC arrival constant and look into what happens afterwards.

Figure 3: JVC arrival rate by different groups (concerning first six weeks sickness behaviour)



What we see in picture A is that those who did not report sick during the first six weeks experience a quite high JVC arrival during week four (vertical lines mark two weekly intervals). Those who report sick during week one or two (picture B) have a relatively higher JVC arrival rate during these two weeks compared to those reporting sick during weeks three or four (picture C) and five or six (picture D) during this first two weeks period. Picture three shows that the JVC arrival rate of those who report sick during week three or four increases during this period (around 0.05) and is about double the size than the JVC hazard of week three of the group in picture four (around 0.025).

With caution we interpret this first graphical check as being a hint of an effect of a JVC on the transition into sickness: shortly before the transition into sickness absence, the JVC arrival rate increases. The proper empirical analysis will give more information about the effect of interest.

7 Empirical evidence

In order to flexibly model potential duration dependence the baseline hazard was specified as constant for each of the first 30 days, days 31-91 ($b2$), days 92-183 ($b3$) and finally days 184-380 ($b4$).[14] The table 2 reports the estimates of the JVC, the unobserved heterogeneity terms and the duration dependence. The full table of coefficients is presented in table 6 in the appendix.

Table 2: Piecewise constant (mixed) proportional hazard model for transition into sickness absence (Sample A):

		.5 Women Coefficients	S.E.	Men Coefficients	S.E.
	JVC (weeks 1, 2)	**0.736*** **	(0.032)	**0.822*** **	(0.029)
	JVC (weeks 3, 4)	**0.334*** **	(0.041)	**0.285*** **	(0.041)
	JVC (weeks 5, 6)	**0.178*** **	(0.047)	**0.284*** **	(0.044)
Unobserved	v1	-1.277***	(0.143)	-8.825***	(0.139)
heterogeneity/	v2	-8.210***	(0.125)		
	lam	-5.481***	(0.057)		
duration	b2	0.046*	(0.026)	-0.017	(0.021)
dependence:	b3	0.066**	(0.026)	-0.015	(0.023)
	b4	0.011	(0.028)	-0.087***	(0.026)
	bic	248887.661		266086.853	
	aic	248351.782		265549.906	
	ll	-124121.891		-132722.953	
	N	150796		225461	

Note: Significance levels: ***: 1%; **: 5%; *:10%. Note that the model is specified without constant meaning for the basic model that the piecewise constant parameter of the first interval ($b1$) serves as baseline hazard for the first month while $b2 - b4$ have to be added to it when interpreting the baseline hazard of the other intervals. For the model with unobserved heterogeneity $v1$ ($v2$) can be interpreted as baseline hazard for the first month of a group of people with p_1 $(1 - p_1)$ as probability of being member of this group. Analogously to the basic model we interpret the $b2 - b4$ relative to $v1$ ($v2$). Note that for the male subsample convergence was not achieved for the model with unobserved heterogeneity terms. Thus the estimates reported result from estimation without unobserved heterogeneity.

The most interesting result is the coefficient estimate of the JVC which is 0.736 for women and 0.822 for men indicating a shift of the transition into sickness during two weeks after the JVC arrival by 108% and 128% respectively. Regarding the influence of the JVC after this first two week period, we introduced time-varying dummies that indicate the effect periods three and four and five and six weeks after the JVC arrivals. We find for both, women and men, the effect to be highest directly after the JVC arrival while during week three and four, the transition rate is shifted upwards by 40% (women) and 33% (men) respectively and in the third effect period the magnitude of the effect decreases further for

[14] The choice of the interval is mainly determined by a trade-off between most flexibility for the baseline hazard on the one hand and number of cases where we observe both events of interest (sickness and JVC arrival).

women indicating a 19%-upward shift.

As to our knowledge there is no quantitative empirical evidence on sickness absence among UI recipients in Germany, we briefly discuss the estimation results regarding the control variables:

Compared to female UI recipients below 30 years, only those above 45 years have a significantly higher hazard to report sick. Yet, according to the estimates of the age group dummies in the male sample, the sickness absence risk among men increases with age. As UI recipients can report disable in the case of sickness of a dependent child (up to the age of twelve years) we would expect the dummies indicating young children to have a positive sign; quite surprisingly, instead we find negative signs indicating having young children is associated with a lower hazard of sickness reporting for women. We assume that in these cases there is no need for an official sickness report as the caseworker might be less strict in terms of activation anyway. For men on the other hand, we do not find any significant correlation between living with a dependent child below twelve years and the individual sickness hazard. The marital state is significantly correlated with sickness absence only for female UI recipients indicating a higher hazard into sickness absence among married women compared to unmarried women. For both, men and women, our results indicate that the level of education and as well as the level of qualification are negatively correlated with sickness absence: those UI recipients with low or mid level school education as well as those who want to work in a job with low or middle level of qualification have a higher risk compared to those with higher education and higher desired job qualifications respectively. Those UI recipients who are looking for a full-time position have a higher sickness hazard than those looking for other forms of work. Male UI recipients with German citizenship are over-represented among those men who report sick which, as mentioned above, might be due to a better knowledge about the institutional set-up.

Controlling for health restrictions as reported by the caseworker as well as by the UI recipient himself at the start of the UI spell, we find latter to be significantly related to the risk of reporting sick for both men and women. In order to capture potential behaviour patterns of seasonal workers we controlled for sickness absence during month ten, eleven and twelve before the start of the UI spell. While we do not find any significant correlation for women, we find the fact whether a male UI recipient reported sick as UI recipient during these months to significantly raise the risk of reporting sick.

Regarding the variables on the individual (un)employment history, controlling for the cumulated duration in UI receipt we find cumulated number of UI benefit sanctions during the previous three years is significantly associated with a higher transition rate into reporting sick. We interpret this finding as a hint for a learning process where the UI recipient improves his knowledge about the institutional setup. Only for men we find a negative significant correlation between daily wage and the hazard into sickness absence. The indicators about the commuting distance to the previous employer also seems to capture some significant heterogeneity among UI recipients: those who worked in a firm resident within his own home municipality are less likely to report sick. However, compared to these cases, additional commuting distance is associated with a lower hazard into sickness absence which corresponds to the idea of a higher work motivation the longer the commuting distance.

Regarding the correlations between regional variables and the sickness reporting risk the findings are quite similar for men and women: the higher the local unemployment rate the lower the hazard into sickness absence is. Interestingly, this finding is in line with the empirical evidence on sickness absence among employed workers which might be a hint for discipline hypothesis among unemployed workers: in tighter labour markets UI recipients might tend to report sick even if they are not sick or healthy UI recipients might tend to shirk more. The vacancy rate, the caseload and the sanction rate on the other hand have a (mostly significant) positive coefficient.

We have to deal with two dimensions of time in our model, namely calendar time and process time, that both might affect the individual hazard. While we refer to effects caused by the latter as duration dependence, effects coming along with the former can be interpreted as seasonal effects. We do not control for seasonal effects in a time-varying way but instead include dummies indicating the month of the start of the UI spell in order to control for the inflow of seasonal workers. As mentioned above we use a piece-wise constant specification in order to capture flexibly potential duration dependence.

While we find unobserved heterogeneity in the female subsample, the model with the unobserved heterogeneity term did not converge in the male subsample, which might be a hint that specifying unobserved heterogeneity is not relevant in this case.[15] Calculating the probability of being member of the group with $v1$ as mass point for women we interpret *lam* as the following: with a probability of 0.4% a female UI recipient is member of a group of people who has a much "less low" baseline hazard of reporting sick than the rest of the female sample. Additionally to unobserved heterogeneity, compared to the first month of UI receipt (which serves as constant), we find a significant increase of the baseline hazard into sickness absence only during the period days 31 - 91 and days 92-183 in the sample of female UI recipients. As in the sample of male UI recipients though convergence was not achieved in using the model accounting for unobserved heterogeneity we report the estimates from a model without unobserved heterogeneity terms: regarding the indicators of duration dependence we find a significant decrease of the transition into sickness absence after the first half year.

Additionally, we control for the maximum UI entitlement duration at the start of the UI spell which varies according to the duration of contributory employment during the years previous to UI start as well as by age. We do not find it to be significantly related to the hazard into sickness absence.[16]

Sensitivity with respect to the JVC arrival day

Around 80% of the JVC arrivals concentrate on one calendar day of a month, namely between the 20th and the 26th (see table 5 and section 6). As mentioned above in these cases instead of observing the exact JVC arrival day, in around 80% we observe the day until which during the previous month the JVC has arrived. In order to check the robustness of our results, in these cases we assign a random JVC arrival day: if the calendar day of a JVC is between day 20 and day 26 of a month, we randomly substract an integer between 0 and 30. We call the sample with these randomly assigned JVC arrival days sample B.

[15] Note that different specifications, e.g. more mass points are planned to be checked in order to robustify this statement.

[16] Note that this might be a hint for our hypothesis that there is no incentive to report sick once the end of the UI entitlement period approximates as those with shorter maximum UI entitlement period are not over-represented among those who report sick. Yet, since we do not control for this variable in a time-varying way, we cannot interpret the estimate as a test of this hypothesis.

Note that due to the "processing rythm" mentioned above, we expect the true arrivals (indeed) to concentrate in the second half of a month rather than in the first. Thus, since on average the duration between JVC and transition into sickness will be too long we assume the estimates of the JVC-dummies using sample B to be rather conservative.

Table 3: Piecewise constant (mixed) proportional hazard model for transition into sickness absence (Sample B):

		Women Coefficients	S.E.	Men Coefficients	S.E.
	JVC (weeks 1, 2)	**0.360*****	(0.038)	**0.496*****	(0.034)
	JVC (weeks 3, 4)	**0.298*****	(0.042)	**0.400*****	(0.038)
	JVC (weeks 5, 6)	**0.322*****	(0.044)	**0.309*****	(0.044)
Unobserved	v1	-1.264***	(0.143)	-8.819***	(0.139)
heterogeneity/	v2	-8.180***	(0.124)		
	lam	-5.500***	(0.058)		
duration	b2	0.052**	(0.026)	-0.007	(0.021)
dependence:	b3	0.054**	(0.026)	-0.019	(0.023)
	b4	-0.007	(0.028)	-0.098***	(0.026)
	bic	249219.995		266490.582	
	aic	248684.116		265953.635	
	ll	-124288.058		-132924.818	
	N	150796		225461	

Note: Significance levels: ***: 1%; **: 5%; *:10%. Note that the model is specified without constant meaning for the basic model that the piecewise constant parameter of the first interval ($b1$) serves as baseline hazard for the first month while $b2 - b4$ have to be added to it when interpreting the baseline hazard of the other intervals. For the model with unobserved heterogeneity $v1$ ($v2$) can be interpreted as baseline hazard for the first month of a group of people with p_1 $(1 - p_1)$ as probability of being member of this group. Analogously to the basic model we interpret the $b2 - b4$ relative to $v1$ ($v2$). Note that for the male subsample convergence was not achieved for the model with unobserved heterogeneity terms. Thus the estimates reported result from estimation without unobserved heterogeneity.

The results of the robustness test yield the following results: On the one hand, the size of the estimates of σ_1 and of σ_2 become smaller, compared to the estimates reported in table 2. This is most likely due to the fact that compared to sample A, sample B has on average a longer duration from JVC until transition into sickness absence which decreases the size of the effect. On the other hand, the estimate of σ_3 increases in size compared to the estimate reported in table 2 what we trace back to the fact that in sample B σ_3 refers to a period of sample A, namely the first four weeks, where we find higher coefficient estimates compared to σ_3.

In sum, the estimates of the robustness test support our previous results indicating a rise of the hazard into sickness absence once a JVC has been proposed.

8 Conclusion

This study is the first quantitative empirical analysis of sickness absence among unemployment insurance (UI) benefit recipients in Germany. The aim of this article was to study a potential side effect of active labour market policy tools: we investigated the impact of the application of an activation tool such as a job vacancy proposed by the caseworker (JVC) on the transition into sickness absence among UI recipients. The institutional setup of the German UI system does not provide any advantages when reporting sick that might lead per se to an incentive to report sick. Yet, there is one exception: we argued that on average UI recipients have an incentive to report sick in order to avoid being punished by a financial sanction (UI benefit cut) once a JVC has arrived.

We used micro data of the German Federal Employment Agency of a sample of people who entered UI receipt in West Germany during April 2000 and March 2001. We argue that due to the informative data we use, it is plausible that the identifying assumption, the assumption of predictability, holds. Thus, we are able to identify the effect of interest using a (mixed) proportional hazard model of the transition into sickness absence with a set of time-varying indicators of the first JVC received.

According to our results both, men and women, have an increased hazard into sickness absence after the arrival of the first JVC. We checked the robustness of this result by randomly assigning JVC arrival days to cases where we suspected the reported day not to be the true day. The results range from a 19% to a 108% upward-shift of the transition rate into sickness absence after the arrival of the JVC for women and from a 128% to a 33% upward-shift for men respectively. Though we are aware that in some cases the arrival of a JVC might cause sickness due to stress, we interpret our findings as a strong hint for moral hazard behaviour: in order to avoid a sanction due to refusing work UI recipients have an incentive to report sick once a JVC is proposed to them even if they are not sick.

Instead of drawing concrete policy implications we want to point out that policy makers should be aware of potential trade-offs between activation of unemployed people and costs of the public health system.

Concluding we suggest that future research on effects of activation policies for unemployed people should keep an eye on potential side effects of activation tools such as an increased sickness absence among unemployed people.

A appendix

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Table 4: Descriptives

	women			men		
	all	sick	no sick	all	sick	no sick
sick in 10th month before UI start	.0014036	.0016175	.0013797	.0020299	.0037219	.0019027
sick in 11th month before UI start	.0014231	.0018116	.0013797	.0021644	.0041561	.0020147
sick in 12th month before UI start	.0011112	.001294	.0010908	.0013533	.0027294	.0012498
health restrictions (caseworker)	.074786	.1378035	.0676844	.0772632	.1418776	.0723567
German citizenship	.9103406	.9057788	.9108499	.8482797	.8504313	.8481179
sector: sonst. verarb. Gewerbe	.1523456	.1837474	.1488395	.1654695	.1816264	.1642548
sector: Gesundheits-Veterinär-So	.1497527	.1495212	.1497786	.0315593	.0312636	.0315815
sector: sonst. Dienstleistungen	.110997	.1001553	.1122075	.0604945	.0491905	.0613443
days in contributory employment	10.06965	8.977491	10.19159	14.02143	14.35191	13.99658
leh_into_U b	.1816008	.1808359	.1816862	.2530384	.2303207	.2547463
leh_into_U c	.1642634	.1519151	.1656421	.2230796	.2238695	.2230202
age: 30 til 34	.1752195	.1543737	.1775469	.1824593	.1565039	.1844107
age: 35 til 40	.2027332	.1866589	.2045279	.2014877	.200794	.2015399
age: 41 til 45	.1369316	.1381988	.1367902	.1232748	.1437876	.1217326
age: 46 til 50	.1153704	.1565735	.11077	.0973715	.1290243	.0949918
age: older than 50	.0996965	.1636905	.0925515	.0846628	.1440978	.0801944
previous daily wage	4.48097	4.115858	4.521736	6.503777	6.161646	6.529499
cumul. no of sanctions (3 years)	.0180717	.017663	.0181173	.0459423	.0470814	.0458567
married	.5048934	.593228	.4949271	.4698172	.5134095	.4665004
age of youngest child 0 to 3	.083009	.0945911	.0817158	.0919106	.0787792	.0928979
age of youngest child 4 to 6	.064099	.0603002	.0645231	.0573672	.058309	.0572964
age of youngest child 7 to 11	.0984619	.0910973	.0992841	.0708566	.0767942	.0704103
school low	.5320332	.645898	.5193201	.7249707	.8146517	.7182284
school middle	.2904827	.2513505	.2948867	.1545449	.1233337	.1569151
desired job: qualification low	.4056158	.4619565	.3993253	.4442203	.4827244	.4413256
desired job: qualification middle	.5579902	.5242113	.5617968	.5309438	.51334	.5322806
health restrictions (unemployed)	.1405525	.2523692	.1279515	.1420275	.2509662	.1337552
desired job: fulltime	.650072	.6009347	.6556093	.9904488	.9898392	.9904951
regional: unempl	7.580577	7.501011	7.58946	7.601974	7.53051	7.607346
regional: vacancy rate	.1693968	.171839	.1691242	.1606697	.1635393	.160454
regional: caseload	53.72266	53.70789	53.72431	53.67089	53.77222	53.66327
regional: sanction rate	.6898375	.6913464	.6896691	.6677621	.6751436	.6672071
commuting distance (in km)	2.485452	2.016582	2.537802	2.459212	2.022664	2.492032
max. duration UI receipt	.5514566	.5449017	.5521885	.6063761	.604429	.6065224
anspruch	34.58149	39.60898	34.01999	33.70371	37.59147	33.41151
days in UI receipt (3 years)	6.153749	5.19758	6.260507	8.758102	8.480597	8.778965
start UI receipt Jan	.1217842	.1262293	.1212879	.1764563	.1572483	.1779004
start UI receipt Feb	.0591928	.0546066	.0597048	.0752462	.0731965	.0754002
start UI receipt Mar	.0571458	.0559006	.0572849	.0590761	.0597978	.0590219
start UI receipt Apr	.1004763	.0989907	.1006422	.0811928	.0795856	.0813136
start UI receipt May	.0739244	.0777692	.0734951	.0633182	.0650704	.0631864
start UI receipt Jun	.0698045	.068323	.0699699	.0563956	.0524781	.0566901
start UI receipt Jul	.1012171	.1017728	.1011551	.0700239	.0733205	.069776
start UI receipt Aug	.0841137	.0813923	.0844175	.0620343	.0641399	.061876
start UI receipt Sep	.07731	.0841744	.0765435	.0614834	.0692885	.0608966
start UI receipt Oct	.0911773	.098926	.0903121	.0723834	.0819428	.0716647
start UI receipt Nov	.0878632	.0822981	.0884845	.0832227	.092364	.0825355

Source: Integrated employment history (IEB), job seekers database (ASU) and applicants database (BewA)

Table 5: Distribution of calender days

calendar day	UI start in %			first JVC in %*		
	all	women	men	all	women	men
1	41.94	49.64	36.81	0.40	0.38	0.42
2	3.36	1.68	4.49	0.67	0.60	0.73
3	2.22	1.93	2.42	0.47	0.48	0.46
4	2.79	2.55	2.94	0.61	0.58	0.63
5	2.52	2.32	2.65	0.73	0.67	0.77
6	2.44	2.19	2.61	0.72	0.70	0.73
7	1.78	1.68	1.84	0.67	0.66	0.68
8	2.16	1.96	2.30	0.79	0.78	0.81
9	1.94	1.66	2.12	0.75	0.70	0.78
10	1.70	1.57	1.79	0.68	0.65	0.70
11	1.75	1.63	1.83	0.86	0.86	0.86
12	1.67	1.57	1.74	0.95	0.92	0.98
13	2.00	1.72	2.19	0.86	0.81	0.90
14	1.52	1.42	1.59	0.82	0.81	0.83
15	2.56	2.42	2.65	0.86	0.82	0.90
16	5.74	5.35	6.00	0.81	0.76	0.85
17	1.74	1.62	1.82	0.69	0.72	0.66
18	1.66	1.48	1.78	0.87	0.85	0.89
19	1.77	1.53	1.94	1.06	1.07	1.06
20	1.89	1.45	2.18	8.37	7.78	8.80
21	1.80	1.38	2.08	11.21	11.76	10.79
22	2.05	1.48	2.43	7.68	7.12	8.09
23	2.03	1.37	2.47	17.68	17.77	17.61
24	1.33	1.19	1.42	16.46	17.14	15.95
25	1.16	1.06	1.22	11.33	11.31	11.34
26	1.14	1.09	1.17	10.79	11.32	10.39
27	1.33	1.25	1.39	0.48	0.44	0.51
28	1.10	1.03	1.15	0.44	0.42	0.46
29	1.18	1.24	1.15	0.48	0.44	0.50
30	1.02	0.90	1.10	0.48	0.42	0.52
31	0.70	0.66	0.72	0.33	0.29	0.36

*Note that in our robustness check we assigned random JVC arrival days if the original day was between calendar day 20 and 26.

Table 6: Piecewise constant (mixed) proportional hazard model for transition into sickness absence (Sample A):

		Women		Men	
		Coefficients	S.E.	Coefficients	S.E.
	JVC (weeks 1, 2)	0.736***	(0.032)	0.822***	(0.029)
	JVC (weeks 3, 4)	0.334***	(0.041)	0.285***	(0.041)
	JVC (weeks 5, 6)	0.178***	(0.047)	0.284***	(0.044)
Socio-demographic	age: 30 til 34	-0.014	(0.030)	0.097***	(0.027)
variables:	age: 35 til 40	0.002	(0.029)	0.202***	(0.026)
	age: 41 til 45	0.043	(0.032)	0.282***	(0.029)
	age: 46 til 50	0.173***	(0.034)	0.243***	(0.033)
	age: older than 50	0.101**	(0.039)	0.292***	(0.037)
	age of youngest child 0 to 3	-0.174***	(0.040)	-0.047	(0.033)
	age of youngest child 4 to 6	-0.129***	(0.039)	0.044	(0.036)
	age of youngest child 7 to 11	-0.114***	(0.032)	0.001	(0.032)
	married	0.069***	(0.019)	0.025	(0.020)
	school low	0.492***	(0.032)	0.645***	(0.037)
	school middle	0.316***	(0.034)	0.423***	(0.042)
	desired job: qualification low	0.167***	(0.040)	0.150***	(0.039)
	desired job: qualification middle	0.132***	(0.039)	0.198***	(0.039)
	desired job: fulltime	0.079***	(0.020)	0.214***	(0.080)
	German citizenship	-0.014	(0.030)	0.077***	(0.024)
Sickness	health restrictions (unemployed)	0.419***	(0.027)	0.313***	(0.026)
indicators:	health restrictions (caseworker)	-0.042	(0.033)	-0.041	(0.032)
	sick in 10th month before UI start	0.251	(0.240)	0.420***	(0.149)
	sick in 11th month before UI start	0.244	(0.241)	0.550***	(0.149)
	sick in 12th month before UI start	0.188	(0.277)	0.450**	(0.178)
(Un)Employment	days in contributory employment (3 years)	0.000	(0.000)	0.000	(0.000)
history:	days in UI receipt (3 years)	0.001	(0.001)	0.004***	(0.001)
	cumul. no of sanctions (3 years)	0.110**	(0.050)	0.106***	(0.029)
	previous daily wage	-0.004	(0.003)	-0.009***	(0.003)
	commuting distance (in km)	-0.005***	(0.001)	-0.010***	(0.002)
	any distance	0.081***	(0.018)	0.072***	(0.017)
	sector: sonst. verarb. Gewerbe	0.080***	(0.023)	0.027	(0.021)
	sector: Gesundheits-Veterinär-Sozialwesen	0.034	(0.025)	-0.070	(0.046)
	sector: sonst. Dienstleistungen	-0.026	(0.029)	-0.177***	(0.037)
Regional variables:	regional: unemployment rate	-0.030***	(0.006)	-0.053***	(0.006)
	regional: vacancy rate	0.304***	(0.114)	0.309***	(0.114)
	regional: caseload	0.003	(0.002)	0.005***	(0.002)
	regional: sanction rate	0.088**	(0.040)	0.037	(0.039)
UI spell:	start UI receipt Jan	0.124***	(0.040)	0.066**	(0.031)
	start UI receipt Feb	0.017	(0.048)	0.169***	(0.037)
	start UI receipt Mar	0.024	(0.048)	0.115***	(0.040)
	start UI receipt Apr	0.096**	(0.042)	0.166***	(0.037)
	start UI receipt May	0.121***	(0.044)	0.152***	(0.039)
	start UI receipt Jun	0.083*	(0.045)	0.042	(0.042)
	start UI receipt Jul	0.100**	(0.041)	0.154***	(0.037)
	start UI receipt Aug	0.118***	(0.043)	0.167***	(0.039)
	start UI receipt Sep	0.157***	(0.043)	0.169***	(0.038)
	start UI receipt Oct	0.118***	(0.041)	0.104***	(0.036)
	start UI receipt Nov	0.035	(0.043)	0.058*	(0.034)
	max. duration UI receipt	0.001	(0.001)	0.002***	(0.001)
Unobserved	v1 / b1	-1.277***	(0.143)	-8.825***	(0.139)
heterogeneity/	v2	-8.210***	(0.125)		
duration	lam	-5.481***	(0.057)		
dependence:	b2	0.046*	(0.026)	-0.017	(0.021)
	b3	0.066**	(0.026)	-0.015	(0.023)
	b4	0.011	(0.028)	-0.087***	(0.026)
	bic	248887.661		266086.853	
	aic	248351.782		265549.906	
	ll	-124121.891		-132722.953	
	N	150796		225461	

Note: Significance levels: ***: 1%; **: 5%; *:10%. Note that the model is specified without constant meaning for the basic model that the piecewise constant parameter of the first interval ($b1$) serves as baseline hazard for the first month while $b2 - b4$ have to be added to it when interpreting the baseline hazard of the other intervals. For the model with unobserved heterogeneity $v1$ ($v2$) can be interpreted as baseline hazard for the first month of a group of people with p_1 ($1 - p_1$) as probability of being member of this group. Analogously to the basic model we interpret the $b2 - b4$ relative to $v1$ ($v2$). Note that for the male subsample convergence was not achieved for the model with unobserved heterogeneity terms. Thus the estimates reported result from estimation without unobserved heterogeneity.