Evaluating Vacancy Referrals and the Roles of Sanctions and Sickness Absence

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Abstract

Job vacancy referrals are a common active labor market policy measure to help unemployed workers to find a job. Unemployment insurance agencies may combat moral hazard by punishing refusals to apply to assigned vacancies. However, the possibility to report sick creates an additional moral hazard, since during sickness spells, minimum requirements on search behavior do not apply. This reduces the ex ante threat of sanctions. We analyze the effects of vacancy referrals and sanctions on the unemployment duration and the quality of job matches, in conjunction with the possibility to report sick. We estimate multi-spell duration models with selection on unobserved characteristics. We find that a vacancy referral increases the transition rate into work and that such accepted jobs go along with lower wages. We also find a positive effect of a vacancy referral on the probability of reporting sick. This effect is smaller at high durations, which suggests that the relative attractiveness of vacancy referrals increases over the time spent in unemployment. Overall, around 9% of sickness absence during unemployment is induced by vacancy referrals.

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1 Introduction

Job vacancy referrals are commonly used by public employment services (PES) to assist job search of the unemployed. As a policy measure, these referrals are among the most important active labor market policy (ALMP) tools (see e.g. OECD, 2013; see Crépon and van den Berg, 2016, for a discussion of different types of ALMP programs). There exist numerous studies on the effectiveness of job search assistance measures in general. These measures often consist of advice to job searchers on how to locate suitable vacancies and how to apply for jobs. The literature usually finds positive effects on the probability of leaving unemployment for a job (Card, Kluve and Weber, 2017). However, the effectiveness of job vacancy referrals in bringing unemployed back to work has not yet been evaluated.[1]

Job vacancy referrals go along with monitoring of the job search behavior of unemployment insurance (UI) recipients by the PES. A refusal to apply to an assigned vacancy can lead to a punitive UI reduction or "sanction". The same risk applies to a rejection of a job offer resulting from an assigned vacancy. By increasing the incentive to comply with the job search requirements and to apply to assigned vacancies, moral hazard should be reduced and the re-employment rate should be increased. However, individuals may avoid a sanction by reporting sick upon receiving the vacancy referral. In many countries, the possibility to report sick creates an additional layer of moral hazard. During sickness spells, the requirements on search behavior do not apply and therefore unemployed individuals cannot be sanctioned. In this paper, we analyze the effects of vacancy referrals and sanctions on the unemployment duration and the quality of job matches, taking the endogenous probability of reporting sick into account.

In the light of the importance of vacancy referrals (VRs) as a policy instrument, the evaluation of its effects seems overdue. The present paper provides this evaluation. We take sanctions and sickness absence into account because they are an integral part of the policy and its immediate response and take-up, and because the resulting findings provide interesting insights into the corresponding moral hazard issues and the determinants of reporting ill health in unemployment.[2] In particular,

[1] van den Berg, Kjærsgaard and Rosholm (2013) evaluate the effects of individual meetings between unemployed individuals and caseworkers on the transition rate to work in the weeks after the meeting. In their Danish setting, such meetings may include explicit vacancy referrals. They find a positive effect, but in the absence of data on the contents of the meetings it is not clear to what extent this result can be attributed to vacancy referrals or to other types of job search assistance and monitoring.

[2] From the point of view of the evaluation of sanction effects on exit out of unemployment, there is yet another reason to take VRs into account. In dynamic treatment evaluation, treatments are not allowed to be anticipated, in the sense that behavior at elapsed durations t does not vary with

we assess the degree of strategic sick-reporting after a job vacancy referral. If this is found to be common then that may lead to a redesign of active and passive labor market policies and/or features of the health care system.

Only a small number of studies analyze determinants of sickness absence among unemployed individuals. Larsson (2006) and Hall and Hartman (2010) analyze the use of sickness insurance and unemployment insurance in Sweden. They report a positive impact of the generosity of sickness benefits on the probability of reporting sick. Larsson (2006) additionally finds that sick reports increase as the unemployment benefits expiration date approaches. In line with this, Henningsen (2008) presents evidence that the transition rate to sickness insurance increases sharply shortly before the exhaustion of unemployment benefits among Norwegian unemployed.[3] Hofmann (2014) provides descriptive evidence that transitions into sickness absence increase after the first VR received in unemployment.[4]

Our analysis is based on administrative population register data from West Germany covering the years 2000–2002. The data contain detailed information on unemployment durations, benefits, sanctions, employment spells and daily wages. Moreover, we observe periods of sickness absence during unemployment and we observe whether or not the unemployed job seeker receives a VR in a given calendar month. The estimation is based on an inflow sample of male workers into unemployment with potentially multiple unemployment spells per individual.

We estimate discrete-time duration models for the durations until sickness absence, until a VR, until a sanction, and until employment, as well as spells in-between these events. In addition, the model includes the distributions of accepted wages and employment durations. The model allows for a number of causal effects of interest, notably the effects of a VR on sickness and on sanctions and on the transition to work and on wages and employment durations, and the effects of sanctions on the transition rate to work and on wages and employment durations. To deal with various selection effects, the model also allows for unobserved individual-specific random effects. The model is estimated by maximizing the likelihood function integrated over

the individual moment at which future treatments are realized (Abbring and van den Berg, 2003). If an individual receives a VR then his response will be affected by the knowledge that he may receive a sanction if he does not follow the referral. If VRs are not observed and are ignored in the evaluation of sanction effects then inference will be biased.

[3] A somewhat related literature has examined strategic inflow into disability as a way to avoid unemployment. This was a common phenomenon in e.g. the Netherlands due to a design failure in the disability entitlement rules; see e.g. Koning and Van Vuuren (2010).

[4] Her study does not consider sanctions, subsequent VRs, or individual labor market outcomes. Moreover, it assumes absence of unobserved confounders in the sense that the arrival of the VR is treated as an exogenous event. The current paper relaxes all these limitations and provides a comprehensive evaluation of VRs.

the random effects.

Our results show that a vacancy referral increases the transition rate into work and that such accepted jobs go along with lower wages. Additionally, we find a positive effect of a VR on sickness absence, which we interpret as evidence for strategic behavior to avoid having to apply for a VR. An additional explanation might be that the estimated effect of a VR on sickness absence reflects late reporting of real sickness. Some of the unemployed workers might be sick but they only report this sickness to the caseworker when they receive a VR. We use external data to provide additional evidence that strategic sick reporting explains at least part of the positive effect of a VR on sickness absence.

The paper is organized as follows: Section 2 describes the institutional background. Section 3 describes the data. Section 4 discusses the econometric approach. The empirical results are presented in Section 5. Section 6 concludes.

2 Institutional background

As we aim to analyze the interplay between various policies, it is important to describe the institutional setting in detail. Moreover, the policies are implemented by actors with discretionary powers (notably, by caseworkers and physicians), so our analysis requires knowledge of the range of possible actions they may take. For this purpose we conducted an extensive qualitative survey among eight individuals who are employed by the Federal Employment Office to gather insights into the daily functioning of employment agencies and active labor market programs. These eight individuals worked as caseworkers during our observation period (January 2000 until December 2002). The description below of the institutional setting refers to this observation period.

2.1 Unemployment benefits

In our observation period, UI benefits are paid to individuals who are registered as unemployed and have been working and paying social security contributions for at least twelve months within the last three years prior to unemployment. The entitlement duration depends on the duration of the prior employment period and the age of the recipient. The maximum entitlement duration is 32 months for individuals who are older than 56 years and who have been employed for at least 64 months in the seven years prior to unemployment. Up to 2005, UI benefit recipients were entitled to means-tested unemployment assistance (UA) after expiration of their UI benefits entitlement. Monthly UI benefits amounted to 67% of the previous monthly net wage for unemployed persons with dependent children and to 60% for those without, whereas the corresponding replacement ratios for UA were 57% and 53%, respectively.[5] UA entitlement was unlimited in time. For a detailed description of the UI system and its changes over time, see e.g. Konle-Seidl, Eichhorst, and Grienberger-Zingerle (2010).

2.2 Vacancy referrals

A vacancy referral (VR; also called placement referral) is a directive to apply for a specific job opening. This is usually delivered by regular mail which takes one day, but it may also be provided during a meeting with the caseworker. During our observation period, there did not exist general rules for the frequency of such meetings between the caseworker and the unemployed. However, they were rare.[6]

The corresponding job description typically contains the occupation, the working hours and the date of the potential job start, but not the wage. The jobs cover a large variety ranging from job creation schemes to regular jobs. After receiving a VR, the unemployed has to apply for the job as soon as possible. A VR does not entail that the employer is informed about the candidate in advance, or that the employer intends to hire him.

The maximum time length for the application and hiring process after a VR depends on the sector and the occupation. According to caseworker experts whom we interviewed, this length is almost always less than or equal to 2 weeks and is longer for high skilled jobs than for low skilled jobs. Usually, one VR is sent out to several job seekers. The number of job seekers who receive the same VR varies and tends to be lower for higher skilled jobs. During our observation period, the caseworkers of the PES were regularly screening the newspapers for new job ads. If they found a new vacancy which fits to their pool of unemployed job seekers, they contacted the employer to include this vacancy in their vacancy database. Usually employers agree with the inclusion in the PES database. In addition to that, some employers contacted the PES directly and asked to include a job vacancy in their database. Caseworkers try to send VRs only to job seekers who fit the job profile, to ensure that the employer is willing to cooperate with the PES in the future. The

[5] Benefits levels are capped if gross monthly pre-unemployment wages were above the so-called social security contribution ceiling. In 2000, this ceiling was at 4400 euro, corresponding to a maximum net monthly UI benefits level of around 1700 euro.

[6] One element of the Hartz-reforms introduced in 2003 was to unify and to increase the frequency of these meetings. After the Hartz-reforms, the meeting frequency ranged from every two months to every 6 months, depending on the profile of the job seeker.

caseworkers updated the status of a job vacancy after several weeks. In case the employer did not fill the vacancy yet, the caseworker searched for additional job seekers who might fit to the vacancy. In general, the number of job vacancies used for VRs for unemployed job seekers depends on the number of open vacancies in the local labor market and the willingness of the employers to share these job vacancies with the PES.

Not following a VR to a job opening that is deemed suitable can result in a sanction. The same applies to the rejection of an offer of a job found through a VR. In our observation period, “suitability” refers to the total daily commuting time and the wage level. If the commuting time exceeds 2.5 hours then the job is not deemed suitable. Furthermore, within the first 3 months of UI benefit receipt, a job is deemed suitable if the wage is not below 80% of the previous wage; between months four and six, this threshold drops to 70%, and from the seventh month onwards, all jobs that offer a wage above the current benefit level are deemed suitable (Pollmann-Schult, 2005).

2.3 Monitoring and sanctions

In our observation period, the PES monitors whether UI and UA recipients comply with requirements and guidelines. If the agency observes that an individual violates these then it may punish the individual by way of a benefits reduction (i.e., with a sanction).

One may distinguish between 5 grounds for sanctions. (1) The individual quits his job. In this case he does not receive any benefits for the first 12 weeks of unemployment. In the case of hardship, the sanction length can be limited to 6 weeks. If the job would have ended within 4 weeks anyway, the individual is sanctioned by three weeks only. (2) The individual does not apply for a suitable job that has been proposed to him as a VR or rejects a suitable job that has been offered to him. Again, the sanction lasts for 12 weeks. If the corresponding job is temporary, the sanction period reduces to 3 weeks. Notice that the individual may intentionally prevent the employer from making an offer, e.g., by misbehaving during the interview. For the caseworker it is difficult to prove such intention; this critically depends on the quality of the contact between the caseworker and the employer. Our interviews with caseworker experts indicate that such misbehavior has been used in a number of cases as a ground to impose a sanction. (3) The individual refuses participation or (4) drops out of an ALMP measure. This involves a sanction of 12 weeks. If the scheduled length of the measure is less than 6 weeks, the unemployed worker is sanctioned for 6 weeks. Finally, (5) the individual fails to report to the regional employment agency or to show up at scheduled meetings. This includes a failure to report / show up at medical or psychological appointments with health care workers of the employment agency. Ground (5) involves a sanction for 2 weeks.

Grounds (1), (2), (3) and (4) generally lead to a sanction length of 12 weeks whereas ground (5) leads to a length of 2 weeks. We call the former "long sanctions" and the latter "short sanctions". In all cases, a sanction always involves a *complete* withdrawal of benefits during the sanction period. In this sense, sanctions amount to 100% of the benefits level. Such 100% sanctions are substantially more severe than sanctions in many other OECD countries. To prevent starvation, sanctioned individuals can apply for means-tested social assistance benefits which are not related to previous wages. To pass the means test for social assistance benefits, the unemployed individual must prove that neither own savings nor support from the immediate family can cover the living costs during the sanction period.

Violations of the guidelines are not always observed by the employment agency. Moreover, in case of an observed violation, sanctions are not imposed mechanically; instead, they occur at the discretion of the regional employment agency and the caseworkers (e.g. Müller and Oschmiansky, 2006). Whether an infringement is discovered depends on several circumstances, e.g., on the information flow between the caseworker and the human resources department of the employer offering the vacancy. It also depends on the caseload, i.e. the number of unemployed assigned to one caseworker. The interviewed caseworker experts emphasized that the caseloads between 2000 and 2002 were very high, ranging from 400 to 1000 unemployed per caseworker.

Discretion can take place at various stages of the process after a discovered violation. The caseworker must invite the unemployed individual to a hearing to give him the opportunity to justify his action. If the caseworker judges the justification as sufficient then no sanction is imposed, but if he discovers a legal infringement then he reports this to the benefits management department. Having been informed about an infringement, the benefits management department checks the evidence against the unemployed and – in case of no objection – it stops the benefit payments and sends out a letter to the unemployed informing him about the imposition of a sanction but also about the possibility of filing an objection against the sanction within one month.

In this paper we restrict attention to imposed sanctions that were not withdrawn. Once a sanction has been enforced, the unemployed has to follow the same job search requirements as before to avoid an additional sanction subsequent to the current one. When the accumulated duration of sanctions adds up to 24 weeks, the benefit recipient loses the claim to all benefits. According to the interviewed experts, some caseworkers monitor individuals more intensively after a sanction, but most of them do not increase monitoring and counseling after a sanction. Specifically, they do not send out more VR to sanctioned individuals to test their availability for work.

2.4 Sick leave during unemployment

In case of sickness, recipients of UI and UA benefits are required to call in sick to the PES and to submit a doctor's note confirming their illness. Every individual receives an information leaflet at the beginning of unemployment, in which this rule is explicitly stated. Moreover, caseworkers emphasize regularly that unemployed individuals must report sick as soon as they are sick. During the first 6 weeks of sickness, benefits continue to be paid by the PES, and the residual UI entitlement duration continues to decline. If, during an ongoing spell of benefit receipt, the accumulated period of sickness with the same diagnosed sickness exceeds 6 weeks, the unemployed person has to apply to the health insurance agency for sickness benefits. The overall number of days on sick leave due to different sicknesses can be more than 6 weeks.[7] What is important for our purposes is that reporting sick does not provide any direct financial advantages such as higher benefits or an extension of the benefit entitlement duration. Thus, there are no financial incentives per se to take sick leave in the case of a brief illness.

Incentives do arise, however, from the requirements on the benefit recipient's labor market behavior. During sickness, these requirements do not apply and therefore unemployed individuals cannot be sanctioned. First, this implies an incentive to take sick leave in the case of real sickness. Second, there is an incentive to call in sick immediately after having received a VR if the individual does not find the assigned vacancy attractive. Since the VR application periods are usually not longer than two weeks, as a rule, a sickness spell of two weeks suffices to avoid a VR.

An important feature of the German health care system is that benefit recipients can choose their physician themselves and can switch between physicians. This implies that they can search for a doctor who is willing to hand out a sick note. There is no direct way for the caseworker to check the reliability of a sick note. The caseworker can send the unemployed to the medical service of the PES (*Ärztlicher*

[7]Eligibility for sickness benefits requires a specific doctor's certificate (cf. e.g. Ziebarth and Karlsson, 2010). The health insurance can use a certified doctor of the medical service of the health insurance (*Medizinischer Dienst der Krankenversicherung*) to verify that certificate. In this paper, we focus on short-term sickness and treat observations as censored when they enter sickness benefits.

Dienst) to check general work-related health restrictions. However, along this route, sickness can only be investigated retrospectively, so the medical service cannot examine whether the physician's sick note was accurate.[8]

We should point out that holidays cannot be used as an avoidance strategy. Benefits recipients need to obtain permission from the caseworker to go on holiday, and such permission is only granted if the holiday does not interfere with job search activities. We should also point out that during sickness, the caseworker does not assign job search activities such as VR to the worker.

2.5 Theoretical considerations

In the supplementary Appendix B4 we develop a theoretical job search model that allows for vacancy referrals, sanctions and sickness absence, taking into account the relevant institutional setting and the opportunities for moral hazard behavior. This serves as a benchmark for the empirical specification and as a tool for the interpretation of the results. This subsection provides a brief summary.

As a starting point, we take a standard job search model without VRs, sanctions, and sickness (see e.g. Eckstein and van den Berg, 2007). Next, we allow VRs to arrive according to a per-period probability. At the moment it arrives, a VR does not reveal the full set of job characteristics. Instead, it reveals a subset of characteristics z that may predict the wage, but it does not yet reveal the wage which is the only characteristic that the worker cares about ex post. The value of z is a random drawing from some distribution. Upon arrival of a VR, the individual has three choices: to apply, to try to obtain a sick note to report sickness, and to do nothing and run the risk of a sanction as a punishment for not applying to the VR. We assume that in every period in which an attempt is made to obtain a sick note, the outcome is a random drawing from a Bernoulli distribution with p_{ill} being the probability of obtaining the sick note. If the individual does nothing after a VR then there is a probability p_s of being detected of not having applied. In that case a sanction is imposed. Clearly, the individual always chooses to obtain a sick note over doing nothing, if this procedure is without costs.

If the individual applies to the VR then with a probability p_r he is rejected by

[8] In an interesting study of patient-physician interactions in Norway, Markussen, Røed and Røgeberg (2013) show that the ease with which sickness absence permits are signed varies systematically across physicians. For this, they examine the outcomes of sets of individuals whose physician retired so that they were assigned to different physicians over time. In a theoretical analysis, Carlsen and Nyborg (2017) show that in equilibrium, phisicians do not extert their gatekeeper role (i.e., in verifying that the patient is ill); instead, they focus on their curative role and tend to believe their patients' sickness claims and hand out sick notes based on subjective symptoms.

the employer. If he is not rejected then he is offered a wage w. This is a random drawing from a distribution that depends on z. If the individual is rejected by the employer then he need not fear a sanction. However, if he is not rejected by the employer but rejects the offer himself then he runs the risk of a sanction.

The optimal strategy concerning the acceptance of offers has the reservation wage property. There are two reservation wages: one for regular job offers and one for offers generated by a VR. The former exceeds the latter because rejection of a regular job offer does not incur the risk of a sanction whereas the rejection of a VR-generated job offer does.

The model can be used to generate comparative statics results. For example, if $p_r \geq p_{ill}$ then the individual always prefers to apply to VR jobs, regardless of the value of z. As another example, consider the ex ante threat effects of sanctions. The threat of a sanction also implies that the individual applies to a VR more often[9] and accepts more regular offers as well as more VR-generated offers. As a result, the transition rate to work is higher due to the threat of sanctions. For the same reasons, the mean post-unemployment wage is lower. These ex ante threat effects are reduced in absolute value if $p_{ill} > 0$, compared to if $p_{ill} = 0$, because having a sick note prevents a sanction.

Notice that the probability p_{ill} of obtaining a sick note is in itself a potentially important determinant of the unemployment duration. Since it reduces the threat of a sanction, it reduces the incentive to apply to a VR job, and it makes workers more selective with respect to job offers. All this increases unemployment durations.

The model can also be used to deduce expressions for the conditional probabilities of the outcomes of interest, such as a transition to work, a sanction, and being on sick leave. In this context, the model can be extended to include a probability of being sick for other reasons and a probability of getting a sanction for a different reason (e.g., refusal to participate in a training program).

An additional empirical implication of the theoretical model concerns the presence of selection effects. The per-period conditional probabilities of an exit to work, a sanction, and a sick leave, and the mean post-unemployment wage, all depend on all underlying model parameters. This is e.g. because they all depend on the reservation wages which in turn depend on the present values of the various options, which in turn depend on all parameters. By contrast, the per-period conditional probability of receiving a VR is determined outside of the theoretical model. In reality, the caseworker may fine-tune this probability to individual characteristics that are

[9] In a monitoring experiment among VR recipients, Engström, Hesselius and Holmlund (2012) find that the more intensive the monitoring, the higher the probability that the individual applies to the assigned VR.

partly unobserved to us, giving rise to a stochastic association with determinants of the other conditional probabilities. In sum, the theoretical analysis suggests that unobserved confounders affecting one specific outcome also affect the other types of outcomes for the same individual.

3 Data

3.1 Sample

We use administrative records of the German Federal Public Employment Service (*Bundesagentur für Arbeit*). More specifically, we use the integrated employment history (*IEB*) and the applicants pool database (*BewA*). The data contain detailed information on labor market outcomes that are relevant for social insurances, including participation in active labor market policies, earnings and transfer payments. The data additionally include a broad range of socio-economic characteristics including education, family status and health limitations. The data do not contain information about the exact number of working hours and periods in self-employment, in civil service, or in inactivity. A detailed description of the IEB is given by, for example, Dundler (2006).

Our starting point for the sample selection is the population of individuals who enter UI in the year 2000. From this we draw a random sample of 1.35 million individuals. Next, we omit a number of subgroups. (1) We exclude individuals who frequently move in and out of unemployment and seasonally unemployed individuals, by requiring that prior to entering unemployment, the individuals are employed subject to social security contributions for a minimum duration of 12 months. (2) We restrict attention to West Germany because during our observation period East and West Germany were substantially different in terms of economic and labor market performance. (3) We focus on male job seekers. Among unemployed primary carers of children below age 3, the job search requirements are different. The latter situation concerns more often women than men. We prefer to avoid this additional heterogeneity. Furthermore, the high share of part-timers among women renders an evaluation of wages in the first job after leaving unemployment difficult for women as we do not observe exact working hours. (4) We focus on individuals aged above 24 and below 58 years. This is motivated by the educational system and by early retirement schemes.

We terminate the observation interval for the outcome variables at December 31, 2002, since in 2003 several labor market reforms were introduced. Accordingly, duration variables are right-censored at December 31, 2002. Thus, we have an observation window of three years. As a result, the sample we use consists of 128,377 individuals. This implies that our sample corresponds to around 9% of full inflow into unemployment in 2000 in Germany, while around 8% of the observed months in unemployment in the full inflow are covered by our estimation sample.

3.2 Treatment and outcome variables

The key time events in our analysis are vacancy referrals, sanctions, sickness, and transitions from unemployment to work. At this stage it is useful to point out that the models we will estimate are in discrete time with one month as the time unit. The latter is motivated by the observation of the arrivals of VR, as explained later in this subsection.

In the sequel, we use unemployment (duration) as synonymous to (the duration of) benefit receipt. We do not distinguish between UI and UA spells, because the institutional rules with respect to VR and sanctions are the same for both types of benefit payments. If individuals leave benefit receipt without finding an unsubsidized job, or if they exit to subsidized employment or move into a specific ALMP program where they receive training measure benefits (*Unterhaltsgeld, in short UHG*), then we right-censor the unemployment spell at that moment.

We observe all VRs given to individuals in the sample. Most VR were reported at the end of a month by the employment agencies to the statistical department of the PES. Therefore, we only observe whether or not a person has received a VR in a given calendar month. No further information about the referred vacancy is available, such as the wage or the occupation.

For each sanction, we observe the day at which they are imposed and whether they are so-called long sanctions or short sanctions. Because of our interest in the impacts of VR, and to keep the analysis manageable, we ignore short sanctions. We also exclude sanctions at the beginning of an unemployment spell, given because of voluntary job loss, because the data do not enable us to control for selection due to voluntary job quits. In sum, we restrict attention to long sanctions that are either due to the rejection of a VR or its ensuing job offer, or due to the noncompliance with other ALMP measures. Unfortunately, we do not observe which of these reasons applies to any of the long sanctions in the data. However, according to statistics of the PES, sanctions related to VR were about 4 times as common as sanctions due to refusing or dropping out of a training measure (Bundesagentur für Arbeit, 2004).[10]

[10] If we observe a long sanction without preceding VR then such a sanction must be due to

In those cases in which more than one long sanction during an unemployment spell was imposed, we analyze the first sanction only and we ignore subsequent sanctions. In our sample, we observe that only around 2% of the sanctioned individuals are sanctioned again within the same spell.

As noted in Section 2, VR application periods are usually not longer than two weeks, so individuals can avoid an application to the assigned vacancy by reporting sick for two weeks or longer. Therefore, we only consider sickness absence spells during unemployment that exceed 13 days. Shorter sickness absence spells are treated as unemployment without sickness. Now one could argue that spells exceeding two weeks must signify a genuine spell of ill health since they contain more days than needed to avoid a sanction. However, if an unemployed individual repeatedly wishes to use sickness absence to avoid VRs, then it clearly makes sense to obfuscate this by randomizing the length of reported sickness spells. One can therefore not use the length of the sickness spell as a highly informative indicator of the extent to which sickness was genuine.

We need to apply a few shortcuts to fit the daily duration data on unemployment, sanctions and sickness into the straightjacket of a discrete-time model with one month as the time unit. We distinguish between mutually exclusive labor market states (notably, unemployment and employment). Within a spell of unemployment, we define sub-states and corresponding sub-spells in which the individual is sick or in which benefits are reduced due to a sanction. Sickness spells as measured in days may cover multiple months in the discrete-time frame. A benefits reduction takes 3 consecutive months. In the light of the discussion earlier in this subsection, the VR is always assigned to one specific month although as we shall see its effects may stretch over multiple months. Within all of these sub-spells, the time clock of the unemployment spell is taken to run at normal speed. If we observe a gap of up to 31 days between two employment spells or two unemployment spells, without information about the state in-between, we close the gap. We also close gaps of up to three days between two sickness spells with an intervening unemployment spell without sickness in-between.

Next, consider the observation of post-unemployment outcomes. The employment duration after job acceptance is defined from the start of the first regular job until reentry into unemployment. We define an individual as being regularly employed if he holds a job where he is paying social security contributions and does not receive any benefits from PES at the same time. With our observation window

noncompliance with other ALMP measures. Such information could be included in the empirical analysis to distinguish between different long sanctions, but this would create at least one additional layer of selectivity and causal effects in the model, which involves substantial computational cost.

of 36 months and with our population of prime-aged men with relatively favorable individual labor market histories, many employment durations in the data are right-censored. This is why we do not regard this outcome as a key outcome and we rather focus on the initial wage as the post-unemployment outcome of interest. In the data we observe the initial daily gross wage in regular employment (as mentioned above, the actual working time is not stored in the data; however, a crude measure distinguishing between part-time and full-time contracts is available, and 96% of the contracts are full-time). The wage variable is right-censored at the social security contribution ceiling. This aspect should be of limited relevance for our analysis, since almost all observed post-unemployment wages are below this threshold.[11]

Finally, to motivate our empirical approach in Section 4, it is useful to recapitulate what we do *not* observe. We do not observe the reason for a sanction, and hence we do not observe whether a specific sanction in the data is caused by the rejection of a VR that was received shortly before that. This means that, with a small probability, such a sanction may also be due to the refusal to enter a training program. Similarly, we do not observe whether a specific sickness spell is causally connected to a VR shortly before that. We also do not observe whether an accepted job has been referred to the individual through a VR or whether he found the job in a different way. Accordingly, the empirical model postulates probabilistic relations between the various events, allowing for selection on unobservables. In Section 4 we discuss the identification of the causal effects of interest.

In addition to this, we point out that the only information available about sickness absence spells concerns their starting and ending dates. In particular, we do not observe the stated diagnosis. Concerning the VRs, we do not observe the job characteristics that are observed by the individual upon receipt of the VR or after application to the VR vacancy. Thus, even if there were no wage variation in VR-based jobs, the relation between a VR and the observed wage in a job that is accepted shortly after the VR is probabilistic.

3.3 Descriptive statistics

Table 1 provides descriptive statistics for the spells and events observed in our full sample. We use all unemployment spells and subsequent employment spells within our observation interval. Overall we observe 216,708 unemployment spells, of which 56% are observed to end in a transition into regular employment. Around 71% of our sample are observed to receive at least one VR, and among them the major-

[11]In 2002, the cap was at 4500 euro per month in West Germany. Only 2.1% of our sample took up a job that paid more than 4000 euro per month.

ity receives more than one VR. Only a small share of unemployed individuals are observed to be sanctioned (around 2%). Around 10% report sick during unemployment. We observe 120,059 employment spells in our data. The average initial daily gross wage of these employment spells is €65.

Figure 1 depicts estimated monthly conditional probabilities of various events. "Conditional" refers to the elapsed unemployment duration; covariates are not included here, and in the figures for VR and sickness we do not condition on not having received a VR yet or on not having reported sick yet, respectively, so that multiple occurrences of VRs and sickness are all counted. For sanctions, the values condition on the elapsed unemployment duration as well as on not having received a sanction yet. The conditional VR probability is relatively high at the beginning of the unemployment spell (around 24%), and it decreases to around 12% after 24 months, whereas sickness becomes more common as the elapsed unemployment duration increases, from around 1% in the first month to about 5% after 24 months. The conditional sanction probability increases in the first three months to around 0.2% and does not change after that, up to month 20 where it decreases to around 0.1%. While we find some differences in the levels, the evolvement of the exit probability to employment, the probability of reporting sick, of receiving a VR, and of being sanctioned is very similar in the full inflow sample into unemployment in Germany (corresponding figures are available on request).

In Figure 2 the share of individuals receiving a VR and the share of individuals leaving unemployment for a job are displayed for each number of months before and after a sanction, among those who were observed to be sanctioned. Specifically, at any month $t < 0$, the "VR" value indicates, among those who received a first sanction, the fraction that received a VR at t months before the sanction. Note that multiple VRs can be received by the same individual. At any month $t > 0$, the "VR" and "Exit Probability to Employment" values indicate, among those who received a first sanction and who are still unemployed up to t months after the sanction, the fractions that receive a VR or that leave unemployment, respectively, at t months after the sanction. The figure shows that the observed sanction and VR events are closely connected in time. In the month before the sanction, around 65% of the individuals receive a VR, which is high compared to an average conditional VR arrival probability of around 20% per month among the non-sanctioned in the first ten months of unemployment. The figure also suggests that the monthly re-employment probability is rather high in the three months after the imposition of the sanction.

Figure 3 is designed like Figure 2 but takes the first sickness absence as the reference point in time. At any month $t < 0$, the "VR" value indicates, among those who reported sick at least once during unemployment, the fraction that received a VR t months before entry into sickness. At any month $t > 0$, the "VR" and "Exit Probability to Employment" and "Sickness Absence" values indicate the fractions that receive a VR or that leave unemployment or that report sickness, respectively, t months after the first sickness. The figure shows that the observed sickness and VR events are closely connected in time. Often, VR and the beginning of the sickness spell occur in the same month. We observe similar relationships between the different events if we consider the full inflow sample into unemployment in Germany.

Table 2 lists descriptive statistics for selected covariates by different (overlapping) subsamples: by sanction status, by sickness absence and by VR receipt. The local unemployment rate and the vacancy rate are measured on a monthly basis and at the level of the catchment area of the regional PES.[12]

Figure 4 plots kernel densities of accepted wages, for individuals for whom a sanction started less than 4 months before accepting the job, and for individuals who were never sanctioned during unemployment spell. In line with the predictions of our theoretical model, the distribution of the former is strongly dominated by the distribution of the latter. Figure 5 shows a similar but smaller difference for wages after a VR during the final two months of unemployment versus wages in the absence of a VR in those months. The figure suggests that VR jobs are less attractive than jobs found in a different way. Of course, some of the wage difference by VR status captures the wage difference by sanction status, but recall that sanctions are much less common than VRs. Moreover, the observed differences in these figures might also be driven by differences in observed and unobserved characteristics. Our empirical approach aims to control for such differences.

4 Empirical approach

In this section we present the empirical model specification to be estimated and we discuss inference. For ease of exposition we abstract from potential outcomes notation and we phrase the model as a reduced-form model. The model specifies the conditional probabilities of occurrence of the various events of interest (conditional on being in the risk set for the event) and the other outcomes of interest, as listed in Section 3. These probabilities are allowed to depend on earlier events, on the

[12]There are about 140 regional employment agencies in West Germany. The local vacancy rate (or, more precisely, the V/U ratio) is the number of open vacancies registered at the regional employment agency divided by the number of unemployed workers in that region.

elapsed unemployment duration, and on individual characteristics. It is pivotal to deal with selectivity in the observed occurrence of events that potentially affect other outcomes. For this purpose we include unobserved individual-specific random effects (or unobserved covariates) in the model that jointly influence the various events and outcomes in the model. The model is estimated by maximum likelihood, where the random effects are integrated out of the likelihood function. We first discuss the various building blocks of the model and then return to the approach for inference.

4.1 Conditional probabilities of vacancy referrals, sanctions and sickness

Recall that we observe up to three types of events before exit out of unemployment: VR, the onset of a sanction, and sickness. We specify the conditional probability of each of these, as functions of covariates, events in the same period or earlier in the current unemployment spell, and the elapsed duration of unemployment. In general we denote these by θ_{VR}, θ_s, and θ_{ill}. Note that unemployed individuals can experience several of the same events during an unemployment spell. Some of the unemployed individuals in our sample receive more than one VR in a given month t. We ignore this for the reason that we do not observe which one occurred first or whether they were given simultaneously.

We assume that all systematic individual differences in θ_{VR} can be characterized by the elapsed unemployment duration t, by unobserved characteristics U_v, and by observed characteristics x evaluated at the corresponding t,

$$\theta_{VR} = \frac{\exp(\beta_{0v} + \sum_{d=2}^{k} \beta_{1dv} \mathrm{I}_{ud}(t) + x_t' \beta_{2v} + U_v)}{1 + \exp(\beta_{0v} + \sum_{d=2}^{k} \beta_{1dv} \mathrm{I}_{ud}(t) + x_t' \beta_{2v} + U_v)} \tag{1}$$

Here, the effect of unemployment-duration dependence is modeled in a flexible way by using indicator functions $\mathrm{I}_{ud}(t)$, which are equal to one iff t is within the duration interval denoted by the subscript $d = (2, ..., k)$.

For θ_s a similar specification is adopted, with an additional covariate V_t indicating whether or not the individual received a VR in t or $t-1$. In obvious notation,

$$\theta_s = \frac{\exp(\beta_{0s} + \sum_{d=2}^{k} \beta_{1ds} \mathrm{I}_{ud}(t) + x_t' \beta_{2s} + V_t \alpha_s + U_s)}{1 + \exp(\beta_{0s} + \sum_{d=2}^{k} \beta_{1ds} \mathrm{I}_{ud}(t) + x_t' \beta_{2s} + V_t \alpha_s + U_s)} \tag{2}$$

where the underlying idea is that if a VR and a sanction occur in the same period then the sanction may be due to the VR.

Analogously, in obvious notation,

$$\theta_{ill} = \frac{\exp(\beta_{0sa} + \sum_{d=2}^{k} \beta_{1dsa}\mathrm{I}_{ud}(t) + x_t'\beta_{2sa} + V_t\alpha_{sa} + U_{sa})}{1 + \exp(\beta_{0sa} + \sum_{d=2}^{k} \beta_{1dsa}\mathrm{I}_{ud}(t) + x_t'\beta_{2sa} + V_t\alpha_{sa} + U_{sa})} \quad (3)$$

This equation does not allow for state dependence in sickness, which is untenably strong. We therefore also estimate models in which this is relaxed.

4.2 Unemployment durations and post-unemployment outcomes

The conditional probability θ_u of leaving unemployment for a job has a specification that is in line with those in the previous subsection. It is specified to depend on x_t, on the elapsed unemployment duration t, on unobserved characteristics[13] U_u, on V_t, on the indicator L_t of whether the unemployed reported sick in t, and on whether a sanction has been imposed before or in t. The elapsed unemployment duration at which the first sanction occurs is denoted by t_s so that the indicator of whether a sanction has arrived at or before t can be expressed as $\mathrm{I}(t \geq t_s)$.

$$\theta_u = \frac{\exp(\beta_{0u} + \sum_{d=2}^{k} \beta_{1du}\mathrm{I}_{ud}(t) + x_t'\beta_{2u} + V_t\alpha_u + \mathrm{I}(t \geq t_s)\delta_u + L_t\kappa_u + U_u)}{1 + \exp(\beta_{0u} + \sum_{d=2}^{k} \beta_{1du}\mathrm{I}_{ud}(t) + x_t'\beta_{2u} + V_t\alpha_u + \mathrm{I}(t \geq t_s)\delta_u + L_t\kappa_u + U_u)} \quad (4)$$

Note that sanction effects are assumed to act indefinitely, contrary to effects of VRs and sickness. This is motivated by evidence in the literature that sanctions lead to prolonged intensified monitoring. However, as we have seen, caseworker experts have claimed that this is mostly absent in Germany. We therefore also estimate specifications in which the duration of the sanction effect is restricted to three months.

We capture the job match quality by the initial wage and by the monthly conditional probability of reentering unemployment. We allow the wage to depend on the relevant determinants through additive effects on the mean log wage. The realized unemployment duration t is included as a determinant, as are the observed covariates x_t in the last period t before which employment starts. Thus,

$$\ln w = \beta_{0w} + \sum_{d=2}^{k} \beta_{1dw}\mathrm{I}_{ud}(t) + x_t'\beta_{2w} + \mathrm{I}(t_s \leq t)\delta_w + V_t\alpha_w + U_w + \varepsilon_w \quad (5)$$

[13]Please note that the capital U in U_u refers to the fact that it represents unobserved covariate effects, whereas the index u refers to the fact that the outcome is unemployment.

where ε_w is assumed to be normally distributed with mean zero and variance σ_w^2 and to be independent of observed and unobserved covariates.

Finally, the conditional probability of reentering unemployment is specified as:

$$\theta_e = \frac{\exp(\beta_{0e} + \sum_{d=2}^{k} \beta_{1de}\mathrm{I}_{ed}(t_e) + \sum_{d=2}^{k} \beta_{2de}\mathrm{I}_{ud}(t) + x_t'\beta_e + \mathrm{I}(t_s \leq t)\delta_e + V_t\alpha_e + U_e)}{1 + \exp(\beta_{0e} + \sum_{d=2}^{k} \beta_{1de}\mathrm{I}_{ed}(t_e) + \sum_{d=2}^{k} \beta_{2de}\mathrm{I}_{ud}(t) + x_t'\beta_e + \mathrm{I}(t_s \leq t)\delta_e + V_t\alpha_e + U_e)} \tag{6}$$

where t_e denotes the elapsed employment duration and t is the realized previous unemployment duration.

4.3 Inference

As we have seen, it can be argued that all events and outcomes of interest are jointly influenced by common unobserved determinants. The data do not contain instrumental variables to enable causal inference in the presence of such selection effects. We therefore adopt an alternative approach, in the spirit of the "Timing of Events" approach (Abbring and van den Berg, 2003). In this approach, the identification of causal effects on duration outcomes is driven by the relative timing of the various events. Intuitively, selection effects generated by the model create a global statistical dependence between cause and effect that is present at all durations, whereas, conversely, the causal effects of interest create a local dependence, as they only work from the moment at which the cause is realized onwards. For example, if a reported sickness is typically shortly preceded by a VR, then this is evidence of a causal effect of VRs on reported sickness. The spurious selection effect does not give rise to the same type of quick succession of events. Recall that the models are estimated by maximum likelihood, where the unobservables are treated as random effects and hence are integrated out of the likelihood function. The literature provides identification proofs (see e.g. Abbring, 2008, and Abbring and Heckman, 2007; see also the discussion in Crépon et al., 2018). In the corresponding empirical literature, it has become common to analyze post-unemployment effects of events during unemployment by specifying the distributions of post-unemployment outcomes as parametric functions of previous events and random effects (see for example Van den Berg and Vikström (2014) and Caliendo, Tatsiramos, and Uhlendorff (2013)). This is also what we have done in the previous subsection. By considering the wage as a non-negative random variable not unlike a duration variable, the identification results of the literature can be applied. However, as argued by van den Berg and Vikström (2014), the linear nature of the wage equation is not equivalent to the reduced-form model structure of hazard rates of duration variables. Therefore the results on wage effects may be affected by the parametric assumptions on how treatments and events during unemployment and unobserved factors affect wages. We return to this issue later in this subsection.

Notice that equation (6) does not specify the conditional exit probability out of employment to depend on the initial wage in employment. In this sense, equation (6) should be interpreted as a reduced-form expression that implicitly includes effects running by way of the initial wage. The advantage of the expression is that it does not require assumptions on the way in which the wage enters the equation. The reduced-form nature of the equation should be kept in mind when interpreting the estimation results.

We briefly discuss important underlying identification conditions as emphasized by the literature. First, the unobserved covariates are time-invariant and independent of the observed covariates. Inference is problematic if in reality there are unobserved shocks that affect both the cause and the effect. For example, a difficult meeting between the unemployed individual and the caseworker may result in a VR as well as, with some delay, in a spell of depression for the unemployed individual. Our inferential approach would erroneously interpret the corresponding data pattern as evidence of a causal effect of VRs on sickness. Of course, the fact that we include many events during unemployment into the analysis means that our study is less sensitive to these potential problems than other studies, notably those that do not control for VRs as determinants of sanctions and sickness absence. Still, our data cannot be expected to record all individual meetings with caseworkers, and such meetings may act as a time-varying unobserved confounder for the impact of VRs on sickness absence.

A second set of assumptions concerns the degree of anticipation of future events. It is assumed that there is no anticipation of the precise moment at which a VR arrives. Also, individuals do not anticipate whether a sick note is obtained or not, or whether an offence gives rise to a sanction or not, at least not to a larger extent than is captured by the model and the observed covariates. A third set of assumptions concerns the functional forms of the model equations, with additive treatment effects and unobserved random effects. To rule out that, in particular, the specification of the wage effects drives the results, we re-estimate the model in a sensitivity analysis where we omit all data on post-unemployment outcomes.

We now provide a number of arguments for why our setting allows for inference that is not fully driven by the identification conditions listed above. First, in our setting it makes sense to impose constraints on the length of the time period that certain effects exist. For example, a given VR can only affect sanctions or sickness or job exits for a small number of months. Such constraints are typically not imposed in the models that are studied in this identification literature, and it can be expected that these constraints make inference less sensitive to functional form assumptions.

Secondly, as displayed in Table 1, our dataset contains multiple occurrences of the same types of events or outcomes in the observation window. For example, some individuals experience multiple unemployment spells. And some individuals experience multiple VRs and/or multiple sickness spells within a given spell of unemployment. By analogy to panel data analysis, this makes inference less sensitive to functional form assumptions (see e.g. Van den Berg, 2001). The same applies to the fact that we have time-varying covariates such as the local unemployment rate. Thirdly, we have a discrete time rather than a continuous time model, and the identification conditions in the literature are typically weaker for the former than for the latter, especially if some covariates are time-varying (see also Heckman and Navarro, 2007).

In the end, it remains important that the functions that act as model determinants have flexible forms. As we have seen, the expressions for conditional probabilities are expressed by logistic specifications. The results do not change if we alternatively choose log-log specifications. We specify the distribution G of unobserved covariates to have a discrete support with M support points or "classes". The unobserved covariates of different outcomes may be stochastically dependent on each other. We use a multinomial logit parameterization of the class probabilities:

$$\pi_m = \frac{\exp(\omega_m)}{\sum_{m=1}^{M} \exp(\omega_m)}, \quad m = 1, ..., M, \quad \omega_1 = 0$$

Each of the six components of the effects U of the unobserved covariates has its own value at support point m. Taking normalizations into account, this implies that for a model with $M = 2$, the distribution G is described by 7 parameters, for $M = 3$ we have 14 parameters, etc. This approach allows for flexible dependence of the various unobserved components. For a similar modeling of the multivariate unobserved heterogeneity distribution in the context of similar models see Crépon, Ferracci, Jolivet, and van den Berg (2018), and in the context of random coefficient models in the statistical literature see e.g. Aitkin (1999). Gaure, Roed, and Zhang (2007) provide Monte Carlo evidence that modeling selection this way works well in the context of Timing of Events models. In the estimation we increase the number of support points until the model fit cannot be improved by a further support point anymore, evaluated on the basis of the Akaike Criterion.

We assume that the unobserved covariates are invariant across multiple outcomes per individual. The individual likelihood contribution in case of a known given support point $U^{(m)}$ can be expressed as, in obvious notation, $l_{it}(x_{it}, U^{(m)})$. The log likelihood for the whole sample is given by[14]

$$LL = \sum_{i=1}^{N} \ln \left(\sum_{m=1}^{M} \pi_m \prod_{t=1}^{T} \left[l_{it} | x_{it}, U^{(m)} \right] \right).$$

5 Results

5.1 Main estimation results

This subsection presents the estimated parameters of our model. We note from the outset that the parameters of the causal effects capture those effects in isolation of other causal effects. Their quantitative magnitude is sometimes difficult to interpret because a given cause (such as the arrival of a VR) works directly as well as indirectly through additional causal effects further down the timeline. For example, a VR may have an instantaneous effect on re-employment for some individuals and may lead to a sanction for others, and the latter has an additional effect on the re-employment probability. To obtain a fuller picture, we use the estimated models to perform simulations of the outcome variables, in Subsection 5.2 below.

We start by estimating a model with time-invariant parameters for the causal effects of interest and compare this model with a more flexible specification. In the flexible specification we allow the VR effects to depend on the elapsed unemployment duration, and we allow the impact of a sanction to be different during the first three months after the imposition of a sanction than in the period thereafter. It turns out that the flexible specification leads to a better model fit. In the following, we will focus on the specification allowing for time-varying parameters for the causal effects of interest. The estimated causal parameters of interest of the restricted model are reported in Table A.1 in the Appendix. All parameters of the restricted model are within the range of the corresponding time-varying parameters of the flexible model.[15]

The complete set of estimated coefficients of the model with and without unobserved heterogeneity are reported in Tables A.2 and A.3 in the Appendix. In

[14] We maximize this using the BHHH algorithm with analytic first derivatives of the likelihood function with respect to the estimated parameters. The estimations are performed in MATLAB.

[15] The complete set of estimated coefficients of the restricted model is available on request.

the models, we control for the observed characteristics listed in Table 2. For each outcome we use 5 parameters to capture how the outcome depends on the elapsed duration in unemployment. Moreover, we control for the sector of the previous employment spell and the quarter in which the unemployment spell starts, and we include time-varying indicators for the current quarter, to capture seasonal effects. The estimated unobserved-heterogeneity distribution has 6 support points (M=6) leading to 35 additional parameters compared to the model without unobserved heterogeneity. A further increase of the number of support points leads to convergence to the estimated model with 6 support points. Inclusion of unobserved heterogeneity strongly improves the model fit. Compared to the specification without unobserved heterogeneity, the log likelihood increases from -1,512,594 to -1,431,175, i.e. by around 80,000.

Table 3 presents the key results for the time-varying sanction effects. We find strong evidence for time-varying sanction effects: the impact of being sanctioned on the relative probability of leaving unemployment for a job is significantly positive in the first three months after the imposition (+40%).[16] After three months, the point estimate is still positive, but not significantly different from zero anymore.

Being sanctioned leads to less stable employment spells if sanctioned individuals leave for a job within the first three months after the imposition of the sanction (+14%). The impact is close to zero and not significant if the job is taken more than three months after the sanction is imposed. The impact on the initial wages is especially high shortly after the imposition of a sanction. The wages are around 15% lower than the wages of not sanctioned individuals. The effect is much smaller if sanctioned individuals take up a job when they receive full benefit payments again (–4.5%).

We allow VR effects to be dependent on the elapsed unemployment duration. The key VR effect coefficients in Table 4 indicate that the effect of receiving a VR on the probability of being sanctioned as well as the effect on the probability of leaving unemployment for a job are rather stable over time spent in unemployment. For both probabilities, the point estimates of the VR effect in months 1-3 are not significantly different from the point estimates in months 19-36. Receiving a VR leads to an increase of around 77% in the relative probability of finding a job shortly after entering unemployment and of around 74% for individuals who are unemployed for more than 1.5 years. Notice that this is based on the VR coefficients in the conditional re-employment probability, so it only concerns the *direct* effect of a VR

[16]The corresponding coefficient of the binary variable in the logit specification is 0.340. Taking the exponent of this coefficient gives the ratio of the relative exit probabilities of individuals receiving and not receiving a VR. This leads to the number of 40%.

that is not driven by a sanction or by sickness. Somewhat loosely, it captures the effect of the VR on re-employment in the absence of sanctions, although in the absence of sanctions their ex ante effect will also be absent, which will influence the coefficients. Receiving a VR leads to a more than seven times higher relative probability of being sanctioned.

We find strong evidence for time-varying effects of receiving a VR on the relative probability of reporting sick. While the receipt of a VR leads to an increase of around 93% at the beginning of an unemployment spell, this effect drops to around 12% after more than 1.5 years of unemployment. The corresponding coefficients are significantly different from each other. This decline in the impact on reporting sick indicates that the avoidance of VRs becomes less attractive the longer job seekers are unemployed. This could be explained by a decreasing value of continuing job search over the time spent in unemployment, which could be induced by a decrease in the job offer arrival rate or by the decreasing profile of the transfer payments (after exhaustion of UI benefits individuals receive UI assistance, see Section 2 for details).

In the model with time-invariant effects we find that the job match quality is lower if unemployed job seekers take up employment upon receiving a VR than if they take up employment at other instances (see Table A.1 in the Appendix). In our preferred flexible specification, we also allow VR effects on post-unemployment outcomes to depend on the realized unemployment duration. Admittedly, such models are demanding in terms of identification. For example, post-unemployment wages now depend on whether a VR has been received shortly before leaving unemployment as well as on the realized unemployment duration *and* on their interaction, as well as on a random effect that may depend on unobserved determinants of the unemployment duration and of VRs. Such models may be non-parametrically identified, but it cannot be ruled out that the estimates of interaction effects are mainly driven by parametric model assumptions. The identification of such a model specification may require parametric functional-form assumptions of model determinants like the unobserved heterogeneity distribution. Therefore, it cannot be ruled out that the estimates, notably those of the interaction effect, reflect such parametric functional-form assumptions. In this sense the corresponding results should be viewed with some caution. With this in mind, we find that the effect of VR on wages is largest in the beginning of the unemployment spell. We find significant effects in the first 6 months of unemployment (–3.8% in months 1 to 3 and –1.7% in months 4 to 6). Beyond 6 months, the effect on wages is not significant. While we find a rather small effect on employment stability for jobs found in months 1 to 3 and no significant impact for months 4 to 6, jobs found after the first half year of unemployment duration are significantly less stable if individuals received a VR shortly before the start of the employment spell. The increase of the relative monthly probability of reentering unemployment ranges from 17% to around 26%.

5.2 Simulations

In order to get an idea of the importance of the total effects, we perform simulations of the outcomes. These simulations are based on the estimated coefficients in the model of the previous subsection and are performed for an average unemployed worker in our sample (that is, average with respect to observed characteristics, while the outcomes are averaged over the estimated distribution of unobserved characteristics). We compare two scenarios, (i) the de facto setting with a VR effect on the probability of reporting sick, and (ii) a scenario in which this effect is absent, i.e., workers do not react with sickness absence to VR arrivals. Standard errors are computed using parametric bootstrap based on 250 draws from the covariance matrix of the estimated parameters (Skrondal and Rabe-Hesketh, 2009).

Figure 6 plots the simulated probabilities of sickness absence in each scenario as a function of the elapsed duration of unemployment. The incidence of sickness absence is increasing, in line with observed sickness patterns in Figure 1. Note that the discontinuities in the function merely reflect the piecewise-constant specification for the duration dependence of the sickness probabilities in the model. There is no reason to suspect such discontinuities in the actual sickness absence. Increasing the number of intervals for the duration dependence takes care of this imperfection, at a computational cost.

While the difference in the shares of individuals reporting sick is significant in every month of unemployment, this difference is – as expected given the time-varying coefficients – stronger at the beginning of the unemployment spell and decreasing afterwards. Overall, the share of months in which job seekers report sick increases by around 9% (from 2.9% to 3.1%) once we allow for an effect of receiving a VR. The increase is around 16% in the first 6 months of unemployment and around 4.0% in month 19 and onwards. This indicates that the moral hazard due to the possibility to report sick plays an important role in the unemployment insurance system in Germany.

It should be borne in mind that the 9% estimate only relates to the avoidance of VRs and only captures sickness and unemployment spells within our observational setting. First, the estimate does not include sickness absence to avoid those sanctions that are not related to VRs, such as sanctions for not showing up for training programs or for not showing up for a meeting with a caseworker. Clearly, such sanctions can also be avoided by reporting sick. Secondly, the 9% estimate does not include sickness of less than 14 days, even though it is possible that some VRs are avoided by such short sickness spells. These two issues make it plausible that the fraction of sickness absence in unemployment that is strategic, with the purpose to avoid sanctions, exceeds 9%. A third issue is that we do not examine sickness after 36 months of unemployment, simply because our data and estimates are restricted to the first 36 months.

The next simulation considers the overall impact of VRs on post-unemployment outcomes. Here, we have to take the indirect effect via the reduction of the unemployment duration into account. The coefficients for the lagged unemployment duration in Table A.2 indicate that the initial wages are decreasing with respect to elapsed unemployment duration. Since a VR reduces the residual unemployment duration, this indirect effect should reduce the negative impact of the VRs on wages. To proceed, we simulate the unemployment duration, the wage and the employment duration, in three scenarios, including (i) the de facto setting, and (ii) no VRs during unemployment. In addition to that we consider (iii) a scenario in which the individual does not receive any VR during the first three months of unemployment but the de facto arrival of VRs after month 3.

Recall that the highest observed unemployment and employment durations in the data equal 36 months. Our estimates do not identify the duration distributions beyond that value. Therefore, when simulating durations, we treat values right-censored at 36 months as actual realizations. This is a limitation in particular for the employment durations, as they are often much longer than 36 months. With this in mind, our simulations show that not receiving VRs increases the average unemployment duration from 13.73 to 15.99 months; see Table 5. Despite this increase, wages are higher in this scenario than in the "de facto" scenario. This indicates that the direct negative VR effect on wages dominates. The daily wages of jobs found during unemployment increase from 56.39 to 57.11 Euro. Moreover, the duration of the employment spell is slightly longer in the scenario without any VR. However, this difference is rather small (0.25 months). If individuals do not get any VRs in the first three months, the average realized wage is 57.40 Euro, and the average unemployment duration is around 14.41 months.[17] The employment spell for scenario (iii)

[17] The relatively strong wage difference compared to the de facto scenario reflects that the negative direct VR effect on wages is especially important at the beginning of an unemployment spell (see Table 4). Recall however that the latter finding is based on a heavily parameterized model specification for post-unemployment outcomes, so that this interpretation should be viewed with

lasts on average around 28.13 months, i.e. it is almost the same as in scenario (i). In sum, not receiving VRs in the first three months slightly increases unemployment durations but goes along with a 2% higher daily wage.

A third set of simulations considers sanctions. The results show that sanctioned individuals have significantly shorter unemployment spells, lower wages and shorter employment spells. For example, imposing a sanction in the third month of unemployment reduces the average unemployment duration from 14.7 to 13.1 months. The average daily wage drops from 55.0 to 48.7 Euro, and the average duration of the employment spell is around 28 months for both groups.

Because of equilibrium effects, the findings in this subsection are not directly generalizable to regime changes in the ALMP system. While the assumption of no spillovers between treated and non-treated seems to be rather weak in the case of sanctions – only a very small fraction of unemployed job seekers are sanctioned at any given point in time – this is potentially more problematic for VRs. After all, the share of unemployed workers receiving a VR is relatively high at around 19% per month. Crowding out of those who do not get a VR by those who get VRs may entail that the increased transition rate from unemployment to employment for treated individuals goes along with a decreased transition rate for non-treated individuals. Based on a French field experiment with two-stage randomization, Crépon, Duflo, Gurgand, Rathelot, and Zamora (2013) show that positive effects of a job search assistance program come partly at the expense of eligible workers who did not participate in the program. While the analysis of spillover and general equilibrium effects goes beyond the focus of this paper, it should be kept in mind that regime changes in the usage of VRs may affect those who do not receive them and that firms may react to incraesed matching rates by creating more vacancies. It seems to be implausible that the estimated effects solely reflect displacement effects by a rearrangement of the job queue. Given that the caseworkers select potential job candidates and that one VR is usually sent to several potential candidates, the effects should at least partly reflect an increased probability to fill a job vacancy if it is registered at the PES.

5.3 Additional sensitivity analysis

One may argue that the estimated effect of a VR on sickness absence may partly reflect late reporting of real sickness: unemployed who are too sick to search for a job might postpone or skip handing in a sick note to the caseworker. Only after a

caution.

VR they may report sickness to the caseworker. We argue that it is perhaps not very plausible that this phenomenon dominates our findings. First, as explained in Section 2, unemployed individuals are informed about the fact that they must report sick as soon as they are sick, and they do not suffer any negative monetary consequences of reporting sick. Second, in our empirical specification sickness absence is defined as being sick for a period of at least 14 days. We argue that individuals who are genuinely sick for over 14 days usually consult a medical doctor. In that case, handing in the sick note involves only little additional time costs to the unemployed. Third, sending out an application does not involve a lot of effort for unemployed job seekers, since they are in general obliged to have prepared an updated CV and corresponding documents for potential applications. Therefore, it seems to be plausible that, in many cases, unemployed job seekers who are sick are still able to send out an application.

To shed some more light on this, we compare the overall level of reported sickness among UI claimants in a period after a vacancy referral with the level of observed sickness absence for similar persons in employment. If at some point in time a certain fraction of individuals is sick and if our results solely reflect the increased incentive to report this sickness after receiving a vacancy referral, we should see no difference in the sickness rates between these two groups of employed and unemployed individuals. In our data, we do not observe sickness absence during employment spells. Therefore, we use a sample of working men living in West-Germany based on the German Socio-Economic Panel (SOEP) (see Wagner et al., 2007, for a description of the SOEP). Every year the interviewees are asked about the number of days they have not been able to work in the previous year due to sickness. Moreover, we have monthly information about the employment status and a number of observed characteristics including age, education, number of children, family status, nationality and state of residency (Bundesland). We apply inverse probability weighting (IPW) to make the two samples comparable. A description of the sample selection can be found in the Appendix, the results are reported in Table A.4. It turns out that the share of days spent in sickness is significantly lower in the sample of employed workers observed in the SOEP (0.018 compared to 0.020 in the sample of unemployed workers).[18] This suggests that our results do not solely reflect late reporting of real sickness.

To investigate this issue further, we analyze to what extent a sickness absence

[18]These results are based on a sample of employed workers who are at least one year employed between 2000 and 2002. We repeated this analysis for a selective sample consisting of individuals in the SOEP who have been employed during our observation period for at least one year and who have been unemployed at least once between 2000 and 2002. This sample might be more comparable to our estimation sample, but the sample size decreases from 2,193 to 177. While the estimated difference is not statistically significant anymore, we find qualitatively the same results.

spell that occurred after the receipt of a vacancy referral is correlated with a lower employment probability in the future. If some unemployed have long term and persistent health problems like mental disorders, and if some of these individuals only hand in a sick note when they receive a vacancy referral, then we would expect that reporting sick after receiving a VR is negatively correlated with future re-employment probabilities. For this, we estimate a model in which we control for the lagged receipt of a VR, reported sickness absence, and the interaction of these two events for 4, 5 and 6 months before the current month t.[19] It turns out that none of the interaction effects is statistically significant at the 5% level, and only one is significant at the 10% level. Moreover, all point estimates are positive, which suggests that underlying long-term health problems which go along with low re-employment rates are unlikely to explain the increased sickness absence after the receipt of a VR. The results are reported in Table A.5 in the Appendix.

In addition to that, we examine seasonal variations. If late reporting of real sickness drives the association between VRs and reported sickness and if this sickness follows a seasonal pattern – like a severe cold or a flu – then we expect a similar seasonal pattern of reporting sick in general and of reporting sick after a VR. To proceed, we estimate a logit model for the probability that an individual reports sick as a function of the calendar month and the interaction of the month and the indicator for receiving a VR (see Hofmann, 2014, for a similar approach). The results are in Table A.6 in the Appendix. While we observe clear evidence for variation of the reported sickness across seasons, we observe significant differences in the response to receiving a VR relative to behavior in January for two months only. For example, the general probability of reporting sick is significantly higher in March than in January, while the probability of reporting sick after a VR is lower in March. This evidence suggests that our results are not driven by genuine sickness that follows a seasonal pattern. Taken together, the various sensitivity analyses suggest that late reporting is not the driving force of the estimated VR effect on sickness absence and that at least part of it can be explained by strategic behavior to avoid applying for a VR.

In our main empirical specification, we assume that effects of VRs do not depend on the skill level of the job seekers. However, if this is violated, and if skilled workers leave unemployment faster than unskilled workers, then changing composition of the unemployed job seekers over time may explain the result that the VR effects on the probability of reporting sick decrease over the duration of unemployment even if the skill-specific VR effects are constant over time. Therefore, we estimate

[19] We do not consider interaction effects of 2 or 3 months before the current month to prevent that a potentially positive medium-run VR coefficient neutralizes a negative correlation of previous sickness absence after a VR with future employment probabilities.

separate models for the sample of skilled – individuals with a vocational training or an university degree – and unskilled job-seekers – individuals without vocational training or a university degree. The results indicate qualitatively similar results for both groups, and we find a decreasing effect over the duration of unemployment of a VR on the probability of reporting sick for each skill group (see Tables A.7 and A.8 in the Appendix). This indicates that a changing composition of unemployed workers over time spent in unemployment with respect to the skill level cannot explain this finding.

As noted in Subsection 3.2, we only consider sickness spells that exceed 13 days. To investigate the importance of the choice of this threshold for our results, we have estimated our model based on a minimum length of 10 days for the sickness spells. The results are reported in Tables A.9 and A.10 in the Appendix. Our main findings are stable.

To investigate whether the specification of the wage effects drives our results, we estimate the model without the data on post-unemployment outcomes. It turns out that the estimated patterns are very similar to the estimation results for the full model. Additionally, we have estimated our model using only the first unemployment spell of each individual in our sample. It turns out that the main patterns of our results are very stable with respect to the use of single spell data instead of multiple spells (the corresponding results are available upon request).

In our data, we often observe that a VR and a sanction arrive in the same month. While we do not observe the exact order of these events, we assume that receiving a VR might induce a sanction, which implies that the VR should arrive before the sanction. To investigate the robustness of our results, we perform a sensitivity analysis in which we censor observations in the month in which we observe a VR and a sanction at the same time. This implies that we keep only those sanctions which are either not related to a VR or for which the order of the events is unambiguous because the VR arrived in the month before the sanction. The results are reported in Tables A.11 and A.12 in the Appendix. This censoring is not random and we have to interpret the results with caution. We get a slightly stronger impact of sanctions on the exit rate from unemployment to work. While the point estimate of taking up a job during a sanction on employment stability is very similar, it is not statistically significant anymore. For wages, we find similar effects compared to our main specification. As expected, the estimated effects of vacancy referrals on the probability of being sanctioned drop. The impact of vacancy referrals on reporting sick as well as the effects on the three outcomes are very similar to our main estimation results. Overall, our main estimation results are robust.

One may argue that if the evaluation results for the VRs and the sanctions are insensitive to the omission of sickness absence from the model, that then the moral hazard due to sickness reporting is irrelevant for the assessment of the effectiveness of those ALMPs. We therefore perform an additional sensitivity analysis by estimating a model in which sickness absence is ignored, i.e. a model without a causal effect of sickness absence on reemployment and without any association between unobserved determinants of sickness absence and other unobserved determinants in the model.[20] This enables us to see whether ignoring reported sickness makes a large quantitative difference for the estimated causal ALMP effects. The results (available upon request) show that indeed the estimated effects differ. Indeed, it often matters more for the results if we ignore the role of sickness than if we ignore the role of selection on unobservables. Ignoring sickness absence provides a VR effect on the log wage of –3.2% which is substantially larger than the estimate of –2.6% in Table A.1. We reject the irrelevance of sickness absence.

The final sensitivity analysis deals with the time length of sickness absence. Specifically, we re-estimate the full model allowing for state dependence in sickness absence. We find a strong and positive correlation between sickness absence in month $t-1$ and the probability of reporting sick in month t. This is not surprising, given that we only consider sickness spells with a minimum length of 14 days. We still get the general pattern of decreasing effects of receiving a vacancy referral on the probability of reporting sick over the time spent in unemployment. However, the decrease in the effects is smaller and not monoton. All the other parameters of interest are very insensitive to this alternative specification.

6 Concluding remarks

Receiving a vacancy referral leads to an immediate increase in the probability of reporting sick. While part of this increase might be driven by late reporting of real sickness, this indicates that for some unemployed job seekers it is optimal to avoid applying for the assigned vacancy and to wait for better job offers. This avoidance behavior depends on the time spent in unemployment. While we find a relatively strong impact of receiving a VR on the probability of reporting sick at the beginning of an unemployment spell, the effect decreases as the unemployment duration increases. This suggests that the relative attractiveness of receiving a VR or applying

[20]This should not be confused with the simulation in Subsection 5.2 where we estimate the contribution of the moral hazard to total reported sickness in unemployment. In the latter case we switch off the causal effect on VRs on reported sickness in the estimated full model, whereas in the current case we deliberately estimate a model that is potentially misspecified.

to a VR increases over the time spent in unemployment, in comparison to the other options. Of course this may be because it becomes harder to find a job without a VR, or because it becomes harder to obtain a valid sick note after a large number of sickness spells, or because the caseworker initially tends to send out VRs of vacancies from the stock of current vacancies, which are rather unattractive. In any case, the results imply that in total around 9% of the reported sickness is driven by the arrival of a vacancy referral.

Receiving a VR also has a direct impact on the job search outcomes. Despite the avoidance behavior by using sick notes, it increases the probability of leaving unemployment for a job. Jobs taken up shortly after receiving a VR have a significantly lower wage and are somewhat less stable than jobs found without previously receiving a VR. In this sense it is not surprising that some individuals prefer not to apply to a VR job if they expect this job to have a low quality. They may prefer to try to obtain a sick note and run the corresponding risk of a sanction if they can not get the sick note. Indeed, the results suggest a strong link between the imposition of a sanction and the receipt of a VR: most of the individuals who are sanctioned received a VR shortly before that.

In line with existing evidence, we find that sanctions lead to an increased job finding probability of unemployed workers. This mainly applies to the first three months after the imposition of a sanction. Among jobs found within these months, wages are significantly lower, and subsequent employment spells are less stable. Once the unemployed job seekers receive the full benefit amount again, the previously imposed sanction has no significant impact any more.

Clearly, the moral hazard due to the possibility to report sick plays an important role in the unemployment insurance system in Germany. Taking a broader view, the picture emerges that the provision of generous unemployment insurance benefits has led to a cascade of moral hazards and counteracting interventions where, currently, these interventions are undermined by strategic sick-reporting. It is an open question whether additional interventions are able to deal with this without running the risk of opening up new moral hazards. One option could be to exclusively relegate decisions on sickness absence to the medical service of the PES and to correspondingly expand the number of health care experts in this service. However, this may lead to a shift in sickness claims that are difficult to verify, such as back pains and mental health problems.

In our paper, we are mainly interested in quantifying causal effects of policy measures on individual job search outcomes. It is difficult to use results from reduced-form analyses to study effects of counterfactual policy designs. For example, we cannot use our model estimates to predict the average unemployment duration in a world without vacancy referrals or in a world in which vacancy referrals are assigned to long-term unemployed only. For this, a structural analysis may be more appropriate. This is beyond the scope of the paper, but it would be an interesting topic for future research.

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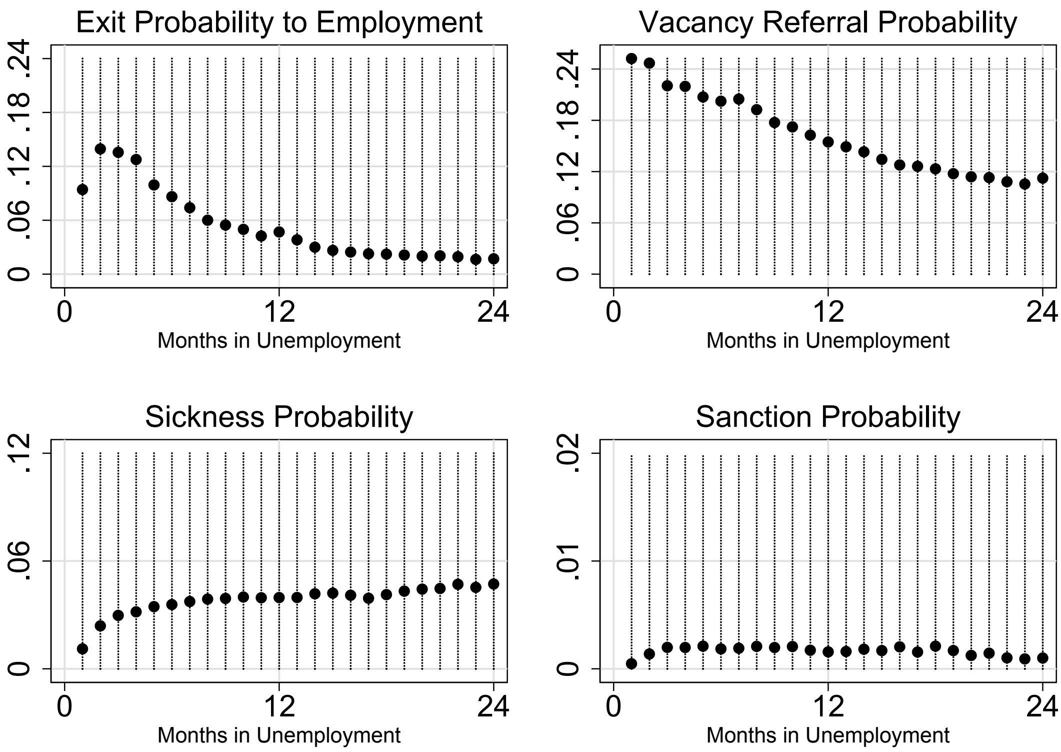
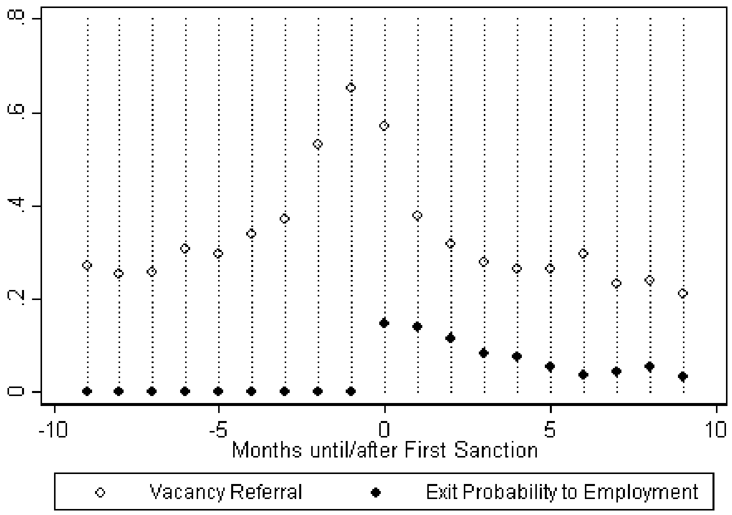
Figure 1: Empirical Conditional Probabilities of Events



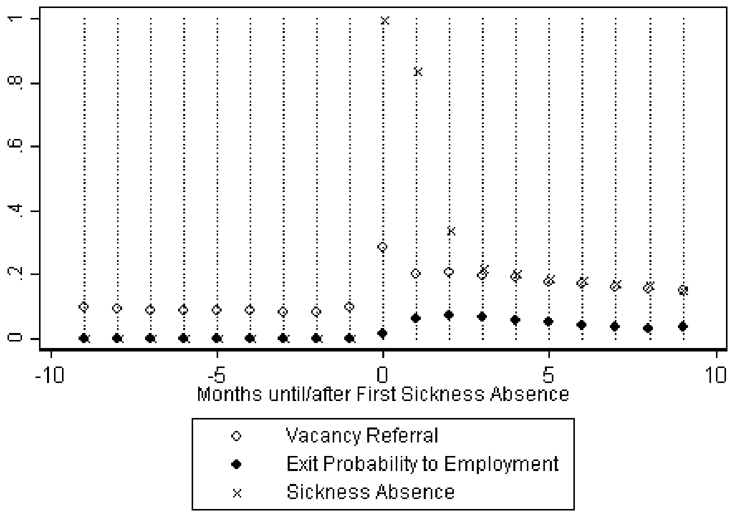
Note: in each panel, values at month t are conditional on survival in unemployment up to t. No conditioning on covariates. Based on 216,708 unemployment spells; number of individuals: 128,377.

Figure 2: Timing of Vacancy Referrals and Transitions to Employment, Relative to the First Moment a Sanction is Imposed.



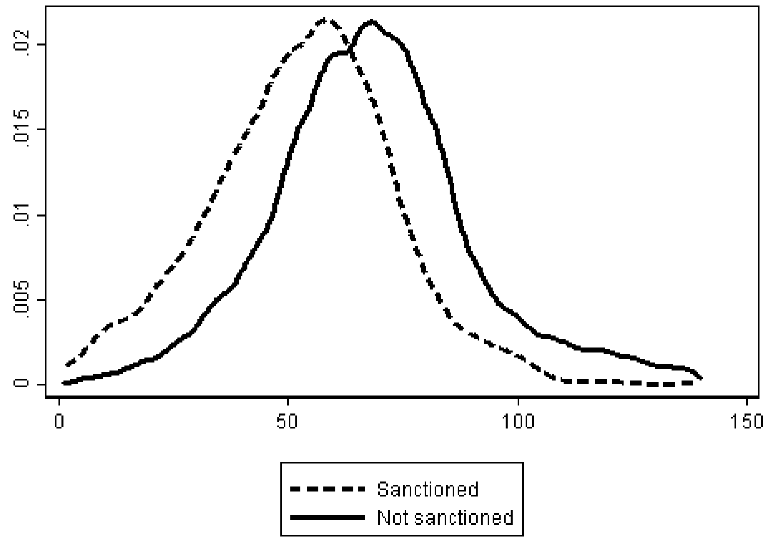
Notes: Month 0 defines the month in which the first sanction is imposed to an individual. The figure only uses individuals who have been sanctioned during their unemployment spell (n=2,061). At any month $t < 0$, the "VR" value indicates, among those who received a first sanction, the fraction that received a VR t months before the sanction. By construction, the fraction moving into employment is zero before month 0. At any month $t > 0$, the "VR" and "Exit Probability to Employment" values indicate, among those who received a first sanction and who are still unemployed up to t months after the sanction, the fractions that receive a VR or that leave unemployment, respectively, t months after the sanction.

Figure 3: Timing of Vacancy Referrals, Sickness Absence and Transitions to Employment, Relative to the Moment of First Sickness Absence.



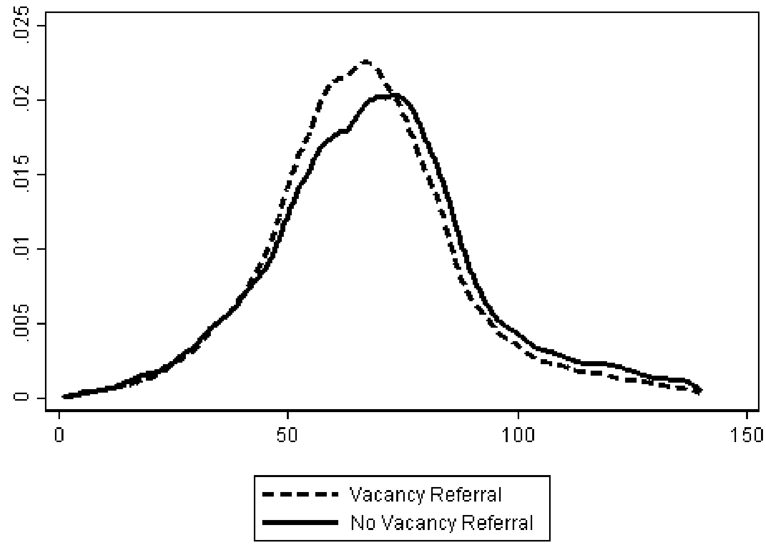
Notes: Month 0 defines the first month in which an individual reports sick for the first time during the unemployment spell. The figure only uses individuals who reported sick at least once during their unemployment spell (n=13,348). At any month $t < 0$, the "VR" value indicates, among those who reported sick at least once during unemployment, the fraction that received a VR t months before entry into sickness. By construction, the fractions moving into employment and sickness are zero before month 0. At any month $t > 0$, the "VR" and "Exit Probability to Employment" and "Sickness Absence" values indicate, among those who reported sick at least once and who are still unemployed up to t months after the first sickness, the fractions that receive a VR or that leave unemployment or that report sickness, respectively, t months after the first sickness.

Figure 4: Wages by Sanction



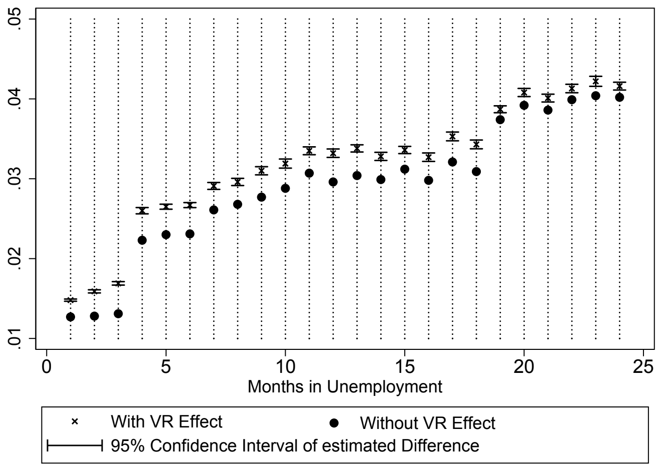
Notes: based on 668 (118,996) spells of sanctioned (not sanctioned) individuals. Sanctioned: sanction started during the final three months of unemployment spell. Not sanctioned: never sanctioned during unemployment spell. Wages are truncated at the social security contribution ceiling.

Figure 5: Wages by Vacancy Referral



Notes: based on 55,998 (64,028) spells of individuals who received (did not receive) a VR. Receive VR: individual received a VR during the final two months of unemployment spell. No VR: individual did not receive a VR during the final two months of unemployment spell. Wages are truncated at the social security contribution ceiling.

Figure 6: Simulated sickness probabilities



Notes: Simulations are based on the estimated coefficients and are performed for the average unemployed worker in our sample. Share 1: situation in which we allow for an effect of receiving a vacancy referral on the probability of reporting sick. Share 2: situation in which we set this effect to zero, i.e., we impose that workers do not react on the arrival of a VR by reporting sick. The confidence intervals refer to the estimated difference between the two shares.

Table 1: Number of observations and transitions

Unemployment	
No. months	1,395,863
No. spells	216,708
% exits to employment	55.55
% 1 spell	53.03
% 2 spells	30.31
% 3 spells	12.57
% > 3 spells	4.08
Vacancy Referrals (VR)	
No. VR arrivals	277,965
% individuals received VR	70.90
% 1 VR	34.94
% 2 VRs	21.50
% 3 VRs	13.75
% > 3 VRs	29.80
Sanctions (S)	
No.	2,101
% unemployment spells with S	0.97
% sanctioned individuals	1.61
Sickness absence (SA)	
No. months in SA	41,909
% individuals in SA	10.40
% 1 month SA	16.20
% 2 months SA	44.57
% 3 months SA	14.11
% > 3 months SA	25.12
Employment	
No. spells	120,059
initial daily gross wage	65
% exits to unemployment	44.39
% 1 spell	69.28
% 2 spells	22.57
% 3 spells	6.71
% > 3 spells	1.43

Notes: n=128,377; individuals might receive more than one VR in a specific month. This is not taken into account. Repeated sanctions during one unemployment spell are ignored.

Table 2: Descriptive Statistics

Sample		
Full Sample		
Age	38.3 (8.6)	
German (%)	85.0	
Married (%)	56.3	
Child (%)	46.6	
Medium secondary school (%)	16.8	
Upper secondary school (%)	16.9	
Vocational training (%)	54.9	
University degree (%)	7.4	
Health restrictions (%)	15.2	
Local unemployment rate (%)	8.6 (3.5)	
Local vacancy rate (%)	15.0 (9.8)	
Sanctioned	*Yes*	*No*
Age	35.0 (7.5)	38.3 (8.6)
German (%)	75.2	85.2
Married (%)	43.4	56.5
Child(%)	41.7	46.7
Medium secondary school (%)	15.4	16.8
Upper secondary school (%)	7.7	17.1
Vocational training (%)	48.5	55.0
University degree (%)	1.8	7.5
Health restrictions (%)	11.0	15.3
Local unemployment rate (%)	7.8 (2.8)	8.6 (3.5)
Local vacancy rate (%)	16.8 (10.2)	14.9 (9.8)
Sick	*Yes*	*No*
Age	40.2 (8.8)	38.0 (8.5)
German (%)	82.5	85.3
Married (%)	58.0	56.1
Child (%)	47.8	46.4
Medium secondary school (%)	12.9	17.2
Upper secondary school (%)	7.4	18.1
Vocational training (%)	53.8	55.0
University degree (%)	2.1	8.0
Health restrictions (%)	25.6	14.0
Local unemployment rate (%)	8.5 (3.5)	8.6 (3.5)
Local vacancy rate (%)	15.0 (9.9)	15.0 (9.8)
VR received	*Yes*	*No*
Age	37.8 (8.3)	39.4 (9.2)
German (%)	84.3	86.7
Married (%)	55.2	59.3
Child (%)	47.1	45.1
Medium secondary school (%)	17.1	15.9
Upper secondary school (%)	15.4	21.0
Vocational training (%)	55.4	53.5
University degree (%)	6.1	10.7
Health restrictions (%)	14.3	17.6
Local unemployment rate (%)	8.5 (3.4)	8.7 (3.8)
Local vacancy rate (%)	15.1 (9.8)	14.7 (9.9)

Notes: Characteristics are measured in first month of first unemployment spell. Standard deviations in parentheses. The vacancy rate is defined as the number of vacancies divided by the number of job seekers. The different subsamples refer to whether the job seekers have been sanctioned, have been sick and have received a VR, respectively, at least once during their first unemployment spell. Health restrictions refer to health related restrictions that are relevant in relation to job search and are usually certified by a medical doctor.

Table 3: Estimated Effects of Sanctions in Model with Time-Varying Effects

	Exit to employment	Exit from employment	Log(wage)
Sanction ($t_u - t_s < 3$)	0.3398***	0.1305**	-0.1472***
	(0.0407)	(0.0575)	(0.0095)
Sanction ($t_u - t_s \geq 3$)	0.0384	-0.0097	-0.0448***
	(0.0596)	(0.1016)	(0.0159)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377; M=6. t_u: month of unemployment; t_s: month of the imposition of a sanction. LogLikelihood=-1,431,175.10.

Table 4: Estimated Effects of Vacancy Referrals in Model with Time-Varying Effects

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR months 1-3	2.1201***	0.6606***	0.5707***	0.0699***	-0.0382***
	(0.1275)	(0.0187)	(0.0096)	(0.0135)	(0.0027)
VR months 4-6	1.7560***	0.4753***	0.4477***	0.0155	-0.0173***
	(0.1170)	(0.0209)	(0.0130)	(0.0184)	(0.0038)
VR months 7-9	1.7970***	0.3711***	0.6020***	0.1887***	-0.0082
	(0.1473)	(0.0257)	(0.0200)	(0.0291)	(0.0055)
VR months 10-12	2.0723***	0.3350***	0.6574***	0.1557***	0.0072
	(0.1926)	(0.0312)	(0.0282)	(0.0426)	(0.0072)
VR months 13-18	1.7450***	0.3381***	0.6964***	0.2281***	0.0025
	(0.1546)	(0.0290)	(0.0342)	(0.0530)	(0.0093)
VR months 19-36	1.9671***	0.1157***	0.5549***	0.2202**	0.0144
	(0.1837)	(0.0304)	(0.0499)	(0.0875)	(0.0136)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377. M=6. LogLikelihood=-1,431,175.10.

Table 5: Simulated Durations and Initial Daily Wages

	(i) Standard Treatment	(ii) No Vancancy Referral	(iii) No Vancancy Referral in months 1-3
Unemployment Duration	13.73	15.99	14.41
	(0.04)	(0.06)	(0.06)
Wages	56.39	57.11	57.40
	(0.13)	(0.23)	(0.15)
Employment Duration	28.11	28.36	28.13
	(0.05)	(0.06)	(0.06)

Note: The simulations are based on the model with time-varying effects. All simulations are performed for the average individual in our sample in terms of observed and unobserved characteristics. Standard treatment implies that individuals have a positive probability of receiving a VR at every point in time. In scenario (ii) this probability is set to zero in all periods. In scenario (iii) this probability is zero in the first three months of unemployment. Standard errors are computed using parametric bootstrap based on 250 draws from the covariance matrix of the estimated parameters.

A Appendix: Supplementary Tables with Estimation Results

Table A.1: Estimated Effects of Vacancy Referrals and Sanctions in Baseline Model Specification

	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
Vacancy referral	1.9033***	0.4509***	0.5528***	0.0754***	-0.0260***
	(0.0627)	(0.0105)	(0.0075)	(0.0107)	(0.0021)
Sanctioned	-	-	0.2383***	0.0962*	-0.1146***
			(0.0327)	(0.0500)	(0.0082)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377; M=6. LogLikelihood= -1,431,406.33.

Table A.2: Full Estimates of the Model with Time-Varying Effects

	Vacancy Referral		Sickness Absence		Sanction		Exit to employment		Exit from employment		Log(wage)	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
VR months 1-3	-		0.6573	(0.0187)	2.1074	(0.1275)	0.5707	(0.0096)	0.0698	(0.0135)	-0.0381	(0.0027)
VR months 4-6	-		0.4724	(0.0209)	1.7575	(0.1170)	0.4480	(0.0130)	0.0150	(0.0184)	-0.0172	(0.0038)
VR months 7-9	-		0.3700	(0.0257)	1.7996	(0.1473)	0.6022	(0.0200)	0.1882	(0.0291)	-0.0081	(0.0055)
VR months 10-12	-		0.3369	(0.0312)	2.0742	(0.1926)	0.6579	(0.0282)	0.1549	(0.0426)	0.0072	(0.0072)
VR months 13-18	-		0.3446	(0.0290)	1.7455	(0.1546)	0.6968	(0.0342)	0.2270	(0.0531)	0.0020	(0.0093)
VR months 19-36	-		0.1267	(0.0304)	1.9647	(0.1837)	0.5554	(0.0499)	0.2199	(0.0875)	0.0137	(0.0136)
Sick	-		-		-		-0.9950	(0.0335)	-		-	
Sanction ($t_u - t_s < 3$)	-		-		-		0.3393	(0.0407)	0.1294	(0.0575)	-0.1462	(0.0095)
Sanction ($t_u - t_s \geq 3$)	-		-		-		0.0383	(0.0596)	-0.0101	(0.1016)	-0.0452	(0.0159)
Local unemp. rate	-0.0533	(0.0010)	-0.0029	(0.0016)	-0.1021	(0.0099)	-0.0368	(0.0011)	0.0026	(0.0016)	-0.0095	(0.0003)
Local vacancy rate	0.0125	(0.0003)	0.0041	(0.0006)	0.0064	(0.0028)	0.0090	(0.0004)	-0.0171	(0.0006)	0.0011	(0.0001)
Month Jan-Mar	0.0129	(0.0069)	0.0040	(0.0132)	-0.1965	(0.0662)	0.4663	(0.0100)	-0.3652	(0.0129)	0.0690	(0.0028)
Month Apr-Jun	0.1625	(0.0069)	-0.0338	(0.0125)	-0.2352	(0.0654)	0.5944	(0.0102)	-0.8210	(0.0145)	0.0389	(0.0028)
Month Jul-Sep	0.1716	(0.0069)	-0.0271	(0.0136)	0.0187	(0.0609)	0.4755	(0.0102)	-0.6851	(0.0133)	0.0326	(0.0027)
Inflow Jan-Mar	0.0792	(0.0065)	-0.0713	(0.0106)	0.0284	(0.0613)	0.1413	(0.0088)	-0.5463	(0.0116)	0.0114	(0.0024)
Inflow Apr-Jun	0.1122	(0.0071)	-0.0120	(0.0115)	-0.0095	(0.0668)	-0.1225	(0.0106)	-0.3934	(0.0135)	0.0001	(0.0028)
Inflow Jul-Sep	0.1068	(0.0068)	-0.0194	(0.0113)	0.0228	(0.0630)	-0.0484	(0.0101)	-0.2376	(0.0130)	0.0039	(0.0027)
Log(age)	-1.1291	(0.0127)	0.5592	(0.0226)	-1.8819	(0.1131)	-0.9910	(0.0147)	0.2598	(0.0220)	0.1587	(0.0044)
German	0.1322	(0.0073)	-0.0611	(0.0126)	-0.1803	(0.0563)	0.3081	(0.0093)	-0.1384	(0.0129)	0.0966	(0.0025)
Married	-0.0195	(0.0060)	0.0348	(0.0108)	-0.3341	(0.0563)	0.1916	(0.0073)	-0.1169	(0.0105)	0.0517	(0.0021)
Children in hh	0.0885	(0.0057)	0.0156	(0.0099)	-0.0678	(0.0550)	0.0008	(0.0070)	0.0224	(0.0099)	0.0212	(0.0020)
Med. lev. school	0.1144	(0.0071)	-0.1310	(0.0135)	-0.1443	(0.0627)	-0.0840	(0.0086)	-0.2721	(0.0124)	0.0908	(0.0024)
Higher lev. school	0.0661	(0.0088)	-0.3549	(0.0201)	-0.5747	(0.0943)	-0.1453	(0.0111)	-0.4783	(0.0171)	0.2840	(0.0028)
Vocational Training	0.2947	(0.0056)	-0.0219	(0.0100)	-0.1231	(0.0486)	0.2230	(0.0068)	-0.1069	(0.0095)	0.0834	(0.0019)
University Degree	-0.1477	(0.0134)	-0.5325	(0.0321)	-0.7083	(0.1883)	0.1633	(0.0165)	-0.2944	(0.0261)	0.2840	(0.0047)
Sector manufactoring	0.0899	(0.0081)	-0.0413	(0.0146)	-0.0827	(0.0720)	-0.1191	(0.0101)	-0.1672	(0.0148)	0.0754	(0.0029)
Sector construction	-0.0829	(0.0088)	0.0143	(0.0151)	-0.1664	(0.0786)	0.2081	(0.0100)	0.2616	(0.0139)	0.1020	(0.0030)
Sector trade	0.1670	(0.0089)	-0.0390	(0.0160)	-0.2644	(0.0833)	-0.1537	(0.0113)	-0.1924	(0.0167)	0.0429	(0.0032)
Sector services	0.0275	(0.0079)	-0.0055	(0.0143)	0.0264	(0.0700)	-0.0874	(0.0099)	0.0700	(0.0142)	-0.0173	(0.0027)
Health restrictions	-0.5500	(0.0061)	0.2005	(0.0093)	-0.4537	(0.0645)	-0.5444	(0.0090)	0.1629	(0.0124)	-0.0635	(0.0025)
Months in unemp 4-6	-		-		-		-		0.0982	(0.0145)	-0.0340	(0.0030)
Months in unemp 7-9	-		-		-		-		-0.0182	(0.0220)	-0.0724	(0.0041)
Months in unemp 10-12	-		-		-		-		-0.1096	(0.0306)	-0.1251	(0.0052)
Months in unemp 13-18	-		-		-		-		-0.1314	(0.0363)	-0.1341	(0.0061)
Months in unemp 19-36	-		-		-		-		-0.0707	(0.0526)	-0.1820	(0.0076)
Log(σ)	-		-		-		-		-		-1.2426	(0.0013)
Constant	-1.6484	(0.0142)	-7.0634	(0.0987)	-7.2032	(0.1629)	-3.5849	(0.0217)	-1.4668	(0.0284)	3.3632	(0.0057)
Months 4-6	-0.1122	(0.0062)	0.5576	(0.0174)	0.8795	(0.1567)	-0.0181	(0.0100)	-0.0458	(0.0133)	-	
Months 7-9	-0.1330	(0.0074)	0.6934	(0.0180)	0.9423	(0.1760)	-0.5339	(0.0148)	-0.0152	(0.0141)	-	
Months 10-12	-0.2381	(0.0092)	0.7276	(0.0200)	0.7799	(0.2097)	-0.7306	(0.0196)	0.1658	(0.0144)	-	
Months 13-18	-0.3257	(0.0092)	0.7518	(0.0170)	1.2545	(0.1765)	-1.1649	(0.0225)	-0.8005	(0.0171)	-	
Months 19-36	-0.4486	(0.0103)	0.9711	(0.0159)	1.1146	(0.1947)	-1.5049	(0.0287)	-0.9120	(0.0167)	-	

To be continued.

Table A.2 continued.

	Vacancy Referral		Sickness Absence		Sanction		Exit to employment		Exit from employment		Log(wage)	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Unobserved heterogeneity:												
$U^{(2)}$	1.0712	(0.0096)	-2.2201	(0.3224)	-0.0392	(0.0839)	0.6756	(0.0176)	-1.1002	(0.0230)	0.5912	(0.0042)
$U^{(3)}$	0.8006	(0.0114)	4.3631	(0.0964)	-0.1404	(0.0987)	0.2962	(0.0228)	-0.2093	(0.0299)	0.4993	(0.0052)
$U^{(4)}$	-0.8963	(0.0102)	-1.0639	(0.1389)	-1.2127	(0.1142)	0.6026	(0.0172)	-0.7392	(0.0212)	0.6137	(0.0039)
$U^{(5)}$	-0.7978	(0.0149)	3.8456	(0.0972)	-0.6177	(0.1545)	-1.4903	(0.0405)	-0.4165	(0.0575)	-1.0543	(0.0063)
$U^{(6)}$	-0.6383	(0.0140)	5.4789	(0.0966)	-1.0717	(0.1709)	0.5788	(0.0232)	-0.4519	(0.0292)	0.5504	(0.0140)
ω_2	1.0889	(0.0225)										
ω_3	-0.3591	(0.0282)										
ω_4	1.7436	(0.0217)										
ω_5	-0.7303	(0.0292)										
ω_6	-0.4396	(0.0261)										

n = 128,377; M=6. LogLikelihood=-1,431,175.10.

Table A.3: Full Estimates of the Model with Time-Varying Effects without Unobserved Heterogeneity

	Vacancy Referral		Sickness Absence		Sanction		Exit to employment		Exit from employment		Log(wage)	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
VR months 1-3	-		0.4589	(0.0159)	2.4983	(0.1246)	0.5886	(0.0085)	-0.0313	(0.0118)	-0.0484	(0.0029)
VR months 4-6	-		0.3064	(0.0178)	2.1613	(0.1147)	0.4754	(0.0122)	-0.0665	(0.0171)	-0.0227	(0.0041)
VR months 7-9	-		0.2278	(0.0221)	2.1855	(0.1453)	0.6321	(0.0196)	0.1109	(0.0284)	-0.0036	(0.0061)
VR months 10-12	-		0.2513	(0.0272)	2.4614	(0.1914)	0.6854	(0.0279)	0.1012	(0.0424)	0.0155	(0.0079)
VR months 13-18	-		0.2774	(0.0243)	2.1090	(0.1540)	0.7215	(0.0340)	0.1979	(0.0530)	-0.0001	(0.0100)
VR months 19-36	-		0.1363	(0.0252)	2.3082	(0.1831)	0.5683	(0.0499)	0.2088	(0.0874)	-0.0192	(0.0134)
Sick	-		-		-		-1.1184	(0.0312)	-		-	
Sanction ($t_u - t_s < 3$)	-		-		-		0.3013	(0.0399)	0.1316	(0.0558)	-0.2200	(0.0089)
Sanction ($t_u - t_s \geq 3$)	-		-		-		-0.0034	(0.0588)	0.0059	(0.0998)	-0.1114	(0.0168)
Local unemp. rate	-0.0521	(0.0005)	-0.0212	(0.0008)	-0.1028	(0.0099)	-0.0362	(0.0011)	0.0004	(0.0015)	-0.0093	(0.0003)
Local vacancy rate	0.0094	(0.0002)	0.0034	(0.0004)	0.0053	(0.0028)	0.0084	(0.0004)	-0.0166	(0.0006)	0.0008	(0.0001)
Month Jan-Mar	-0.0087	(0.0063)	-0.0363	(0.0124)	-0.1974	(0.0659)	0.4776	(0.0100)	-0.3763	(0.0129)	0.0783	(0.0032)
Month Apr-Jun	0.1585	(0.0063)	-0.0536	(0.0117)	-0.2436	(0.0652)	0.5943	(0.0102)	-0.8311	(0.0145)	0.0460	(0.0031)
Month Jul-Sep	0.1624	(0.0065)	-0.0288	(0.0131)	0.0142	(0.0607)	0.4714	(0.0101)	-0.6868	(0.0133)	0.0330	(0.0031)
Inflow Jan-Mar	0.0596	(0.0040)	0.0135	(0.0064)	0.0287	(0.0613)	0.1313	(0.0086)	-0.5280	(0.0115)	0.0068	(0.0028)
Inflow Apr-Jun	0.0916	(0.0043)	0.0616	(0.0068)	-0.0194	(0.0667)	-0.1298	(0.0104)	-0.3799	(0.0134)	-0.0058	(0.0032)
Inflow Jul-Sep	0.0879	(0.0041)	0.0221	(0.0067)	0.0144	(0.0629)	-0.0566	(0.0100)	-0.2310	(0.0128)	-0.0017	(0.0031)
Log(age)	-0.9046	(0.0064)	0.4956	(0.0115)	-1.7146	(0.1124)	-0.9985	(0.0142)	0.2477	(0.0213)	0.1424	(0.0046)
German	0.1201	(0.0037)	0.0045	(0.0062)	-0.1812	(0.0563)	0.2975	(0.0091)	-0.1454	(0.0127)	0.1056	(0.0026)
Married	-0.0317	(0.0032)	0.0217	(0.0055)	-0.3351	(0.0562)	0.1960	(0.0071)	-0.1185	(0.0103)	0.0590	(0.0023)
Children in hh	0.0883	(0.0030)	0.0410	(0.0052)	-0.0727	(0.0549)	0.0007	(0.0068)	0.0233	(0.0097)	0.0235	(0.0022)
Med. lev. school	0.1068	(0.0036)	-0.2336	(0.0071)	-0.1432	(0.0626)	-0.0844	(0.0083)	-0.2633	(0.0122)	0.0861	(0.0026)
Higher lev. school	0.0577	(0.0045)	-0.5659	(0.0110)	-0.5591	(0.0939)	-0.1521	(0.0109)	-0.4482	(0.0166)	0.2529	(0.0029)
Vocational Training	0.2770	(0.0029)	0.0002	(0.0050)	-0.1285	(0.0484)	0.2302	(0.0066)	-0.1060	(0.0093)	0.0929	(0.0021)
University Degree	-0.1352	(0.0072)	-0.5821	(0.0202)	-0.7112	(0.1882)	0.1833	(0.0163)	-0.3077	(0.0258)	0.3057	(0.0046)
Sector manufactoring	0.0817	(0.0044)	0.0695	(0.0074)	-0.1086	(0.0719)	-0.1093	(0.0098)	-0.1784	(0.0146)	0.0936	(0.0033)
Sector construction	-0.0848	(0.0048)	0.1682	(0.0078)	-0.1922	(0.0786)	0.2336	(0.0097)	0.2423	(0.0137)	0.1296	(0.0034)
Sector trade	0.1612	(0.0049)	-0.0144	(0.0084)	-0.2768	(0.0833)	-0.1526	(0.0110)	-0.1931	(0.0165)	0.0468	(0.0036)
Sector services	0.0445	(0.0044)	-0.0231	(0.0074)	0.0359	(0.0699)	-0.0979	(0.0096)	0.0933	(0.0140)	-0.0412	(0.0029)
Health restrictions	-0.4812	(0.0034)	0.4779	(0.0050)	-0.4205	(0.0643)	-0.5532	(0.0087)	0.1229	(0.0144)	-0.0522	(0.0034)
Months in unemp 4-6	-		-		-		-		0.0191	(0.0218)	-0.1084	(0.0045)
Months in unemp 7-9	-		-		-		-		-0.0498	(0.0304)	-0.1738	(0.0055)
Months in unemp 10-12	-		-		-		-		-0.0620	(0.0362)	-0.1855	(0.0066)
Months in unemp 13-18	-		-		-		-		0.0303	(0.0526)	-0.2437	(0.0079)
Months in unemp 19-36	-		-		-		-		0.1588	(0.0121)	-0.0727	(0.0027)
Log(σ)	-		-		-		-		-		-1.0329	(0.0010)
Constant	-1.6197	(0.0072)	-4.1612	(0.0156)	-7.8750	(0.1488)	-3.0773	(0.0156)	-2.1386	(0.0216)		
Months 4-6	-0.1409	(0.0057)	0.5578	(0.0164)	0.8665	(0.1567)	-0.0530	(0.0099)	-0.0676	(0.0132)	-	
Months 7-9	-0.2043	(0.0067)	0.7019	(0.0166)	0.9329	(0.1755)	-0.5931	(0.0147)	-0.0536	(0.0140)	-	
Months 10-12	-0.3487	(0.0082)	0.7208	(0.0183)	0.7633	(0.2096)	-0.8062	(0.0195)	0.1104	(0.0142)	-	
Months 13-18	-0.4906	(0.0078)	0.7326	(0.0151)	1.2388	(0.1762)	-1.2634	(0.0223)	-0.8674	(0.0169)	-	
Months 19-36	-0.6949	(0.0081)	0.8804	(0.0131)	1.0820	(0.1945)	-1.6265	(0.0284)	-0.9993	(0.0164)	-	

n = 128,377; LogLikelihood=-1,512,593.93.

Comparison of sickness rates with a sample of employed workers

The share of days in sickness absence for employed workers is based on the SOEP. The SOEP is a yearly household panel. Every year the interviewees are asked about the number of days they have not been able to work in the previous year due to sickness. For the SOEP, we select individuals who have been employed throughout at least one year between 2000 and 2002 and who answered the question about the days with sickness absence in the following year. For each individual in our estimation sample and in the SOEP, we create the share of days in sickness absence during unemployment and employment, respectively. For our estimation sample, we use all unemployment spells in the years 2000 to 2002. Within these unemployment spells, we select the months which follow a vacancy referral. In line with that, for the SOEP we use all years between 2000 and 2002 spent in employment. We apply the same age restrictions for both samples (25-57).

We apply inverse probability weighting (IPW) to make the two samples comparable. For the estimation of the propensity score, we include – next to age, education, family status, children, marital status, nationality and dummies for the state of residency – the share of days spent in employment in 1998 and 1999. We end up with 83,261 observations from our estimation sample and 2,193 observations from the SOEP. We repeat this analysis for a selective sample consisting of individuals in the SOEP who have been unemployed at least once between 2000 and 2002. The number of observations in the SOEP drops to 177.

Table A.4: IPW estimates for the difference in shares of days spent in sickness

	Share of days with sickness absence	Standard Error
Employed (observed in the SOEP)	-0.0018**	0.0007
Constant (unemployed in period after VR)	0.0199***	0.0005
Employed (observed in the SOEP, at least once unemployed in 2000-2002)	-0.0020	0.0026
Constant (unemployed in period after VR)	0.0225***	0.0012

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 83,261 (2,193 from the SOEP) for the first analysis, n = 81,245 (177 from the SOEP) for the second analysis.

Interaction effects for lagged sickness absence and VRs

Table A.5: Estimated Effects of Vacancy Referrals in Model with Time-Varying Effects

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR x Sickness t-4			0.0310		
			(0.0694)		
VR x Sickness t-5			0.1002		
			(0.0788)		
VR x Sickness t-6			0.1412*		
			(0.0808)		
VR months 1-3	2.1232***	0.6625***	0.5528***	0.0777***	-0.0387***
	(0.1275)	(0.0187)	(0.0096)	(0.0135)	(0.0027)
VR months 4-6	1.7584***	0.4758***	0.4373***	0.0220	-0.0177***
	(0.1170)	(0.0209)	(0.0130)	(0.0184)	(0.0038)
VR months 7-9	1.7997***	0.3704***	0.6146***	0.1943***	-0.0084
	(0.1473)	(0.0257)	(0.0201)	(0.0291)	(0.0055)
VR months 10-12	2.0730***	0.3341***	0.6730***	0.1608***	0.0072
	(0.1927)	(0.0312)	(0.0283)	(0.0426)	(0.0072)
VR months 13-18	1.7454***	0.3362***	0.7099***	0.2327***	0.0021
	(0.1546)	(0.0290)	(0.0342)	(0.0531)	(0.0093)
VR months 19-36	1.9702***	0.1146***	0.5640***	0.2234***	0.0146
	(0.1837)	(0.0304)	(0.0499)	(0.0875)	(0.0136)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377. M=6. LogLikelihood=-1,431,091.48.

Seasonal pattern of sickness reporting

Table A.6: Logit model for seasonal patterns of reporting sick

	Coefficient	Standard error
Constant	-4.196***	0.031
February	0.084***	.030
March	0.097***	.030
April	0.031	.031
May	0.088***	.031
June	0.055*	.031
July	0.073**	.031
August	0.058*	.031
September	0.092***	.031
October	0.131***	.030
November	0.138***	.030
December	0.001	.030
VR	0.352***	0.040
VR × February	-0.0147	.054
VR × March	-.011**	.054
VR × April	-.120**	.054
VR × May	-.080	.054
VR × June	-.001	.054
VR × July	-.053	.054
VR × August	-.063	.054
VR × September	-.025	.053
VR × October	-.000	.053
VR × November	-.012	.052
VR × December	-.019	.054

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377. We control for duration dependence and for the same observed characteristics as in Table A.2.

Separate estimations for skill groups

Table A.7: Time-Varying Effects of Vacancy Referrals for Skilled Job Seekers

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR months 1-3	2.1616***	0.5474***	0.5579***	0.0738***	-0.0344***
	(0.1835)	(0.0240)	(0.0116)	(0.0164)	(0.0031)
VR months 4-6	1.8882***	0.3701***	0.4067***	0.0101	-0.0123***
	(0.1772)	(0.0276)	(0.0163)	(0.0229)	(0.0043)
VR months 7-9	2.0196***	0.3241***	0.6113***	0.2085***	-0.0170**
	(0.2328)	(0.0343)	(0.0261)	(0.0382)	(0.0067)
VR months 10-12	2.5768***	0.3103***	0.6254***	0.1647***	0.0047
	(0.3408)	(0.0427)	(0.0379)	(0.0588)	(0.0090)
VR months 13-18	1.8038***	0.2734***	0.6948***	0.2636***	-0.0009
	(0.2380)	(0.0415)	(0.0484)	(0.0776)	(0.0125)
VR months 19-36	2.1043***	0.1184***	0.4784***	0.2437*	0.0304
	(0.3112)	(0.0434)	(0.0736)	(0.1354)	(0.0197)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 82,178. M=6. LogLikelihood= -875,750.42. Skilled workers are those with a vocational training or a university degree.

Table A.8: Time-Varying Effects of Vacancy Referrals for Unskilled Job Seekers

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR months 1-3	2.0644***	0.7677***	0.5921***	0.0582**	-0.0436***
	(0.1818)	(0.0303)	(0.0170)	(0.0244)	(0.0054)
VR months 4-6	1.6230***	0.5407***	0.5172***	0.0119	-0.0250***
	(0.1600)	(0.0326)	(0.0217)	(0.0315)	(0.0072)
VR months 7-9	1.5807***	0.3608***	0.6067***	0.1482***	0.0010
	(0.1963)	(0.0394)	(0.0316)	(0.0463)	(0.0098)
VR months 10-12	1.7115***	0.3061***	0.7232***	0.1305**	0.0025
	(0.2448)	(0.0468)	(0.0426)	(0.0635)	(0.0126)
VR months 13-18	1.6659***	0.3729***	0.7290***	0.1733**	-0.0008
	(0.2065)	(0.0417)	(0.0487)	(0.0747)	(0.0145)
VR months 19-36	1.8663***	0.1137***	0.6561***	0.1952*	0.0030
	(0.2327)	(0.0441)	(0.0684)	(0.1166)	(0.0207)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 46,199. M=6. LogLikelihood= -552,910.84. Unskilled workers are those without a vocational training or a university degree.

Estimation Results using 10 days as the minimum length for a sickness absence

Table A.9: Estimated Effects of Sanctions in Model with Time-Varying Effects

	Exit to employment	Exit from employment	Log(wage)
Sanction ($t_u - t_s < 3$)	0.3453***	0.1173**	-0.1455***
	(0.0406)	(0.0575)	(0.0095)
Sanction ($t_u - t_s \geq 3$)	0.0464	-0.0075	-0.0461***
	(0.0596)	(0.1022)	(0.0158)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377; M=6. t_u: month of unemployment; t_s: month of the imposition of a sanction. LogLikelihood=-1,440,886.01.

Table A.10: Estimated Effects of Vacancy Referrals in Model with Time-Varying Effects

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR months 1-3	2.1271***	0.6657***	0.5698***	0.0707***	-0.0386***
	(0.1273)	(0.0180)	(0.0096)	(0.0135)	(0.0027)
VR months 4-6	1.7605***	0.4919***	0.4460***	0.0150	-0.0178***
	(0.1170)	(0.0204)	(0.0130)	(0.0184)	(0.0038)
VR months 7-9	1.8027***	0.3659***	0.5995***	0.1875***	-0.0078
	(0.1473)	(0.0250)	(0.0200)	(0.0291)	(0.0055)
VR months 10-12	2.0802***	0.3500***	0.6545***	0.1557***	0.0061
	(0.1927)	(0.0304)	(0.0282)	(0.0426)	(0.0072)
VR months 13-18	1.7501***	0.3469***	0.6919***	0.2315***	0.0031
	(0.1545)	(0.0283)	(0.0342)	(0.0531)	(0.0093)
VR months 19-36	1.9728***	0.1311***	0.5511***	0.2163**	0.0145
	(0.1835)	(0.0297)	(0.0499)	(0.0875)	(0.0137)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377. M=6. LogLikelihood=-1,440,886.01.

Estimation results with observations censored if sanctions arrived in the same month as a VR

Table A.11: Estimated Effects of Sanctions in Model with Time-Varying Effects

	Exit to employment	Exit from employment	Log(wage)
Sanction ($t_u - t_s < 3$)	0.4108***	0.1122	-0.1503***
	(0.0694)	(0.0916)	(0.0163)
Sanction ($t_u - t_s \geq 3$)	-0.0812	-0.1370	-0.0187
	(0.1031)	(0.1883)	(0.0251)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377; M=6. t_u: month of unemployment; t_s: month of the imposition of a sanction. LogLikelihood=-1,412,186.09.

Table A.12: Estimated Effects of Vacancy Referrals in Model with Time-Varying Effects

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR months 1-3	1.0011***	0.6637***	0.5694***	0.0713***	-0.0378***
	(0.1513)	(0.0187)	(0.0096)	(0.0136)	(0.0027)
VR months 4-6	0.7354***	0.4818***	0.4451***	0.0197	-0.0172***
	(0.1423)	(0.0211)	(0.0131)	(0.0184)	(0.0038)
VR months 7-9	1.0778***	0.3777***	0.6052***	0.1884***	-0.0068
	(0.1692)	(0.0258)	(0.0202)	(0.0292)	(0.0055)
VR months 10-12	1.1504***	0.3439***	0.6536***	0.1468***	0.0070
	(0.2286)	(0.0314)	(0.0285)	(0.0430)	(0.0073)
VR months 13-18	1.0521***	0.3322***	0.6876***	0.2216***	-0.0019
	(0.1839)	(0.0294)	(0.0346)	(0.0538)	(0.0093)
VR months 19-36	1.4628***	0.1280***	0.5513***	0.2222**	0.0124
	(0.2101)	(0.0306)	(0.0506)	(0.0886)	(0.0138)

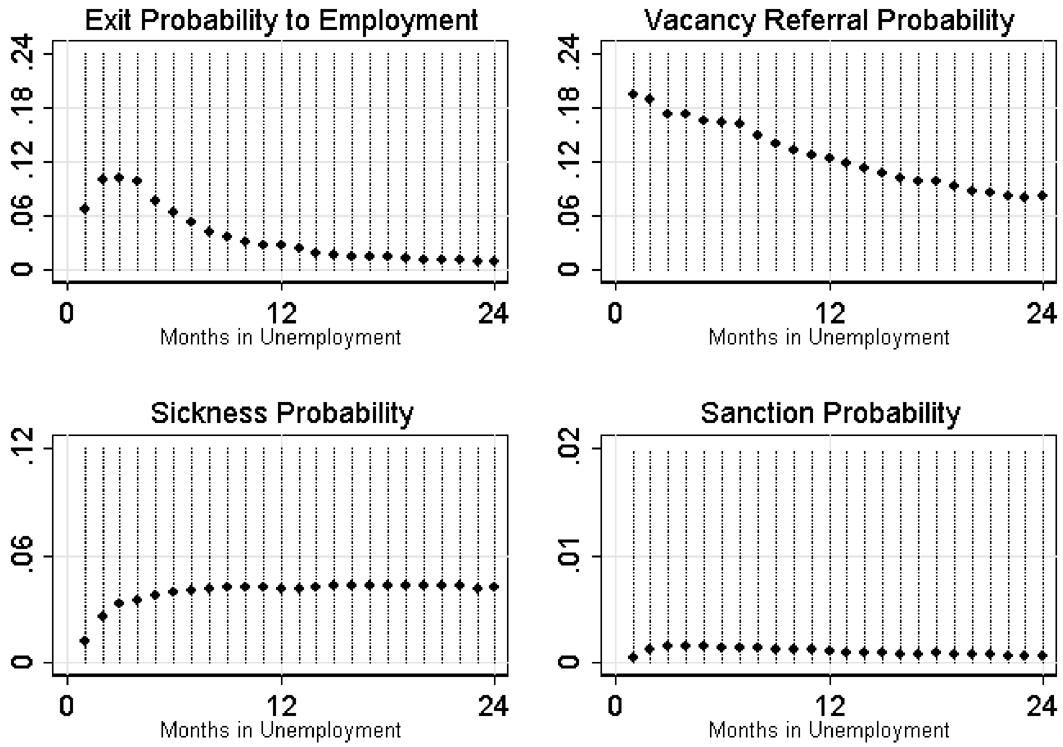
Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377. M=6. LogLikelihood=-1,412,186.09.

B Supplementary Appendix

This supplementary appendix contains additional information to made available online in case of publication.

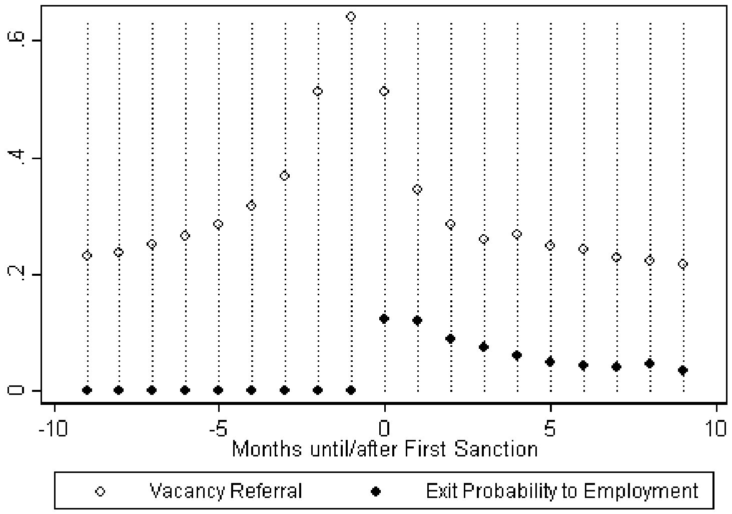
B.1 Descriptive statistics for the unrestricted sample

Figure B.1: Empirical Transition Rates and Treatment Probabilities (unrestricted sample)



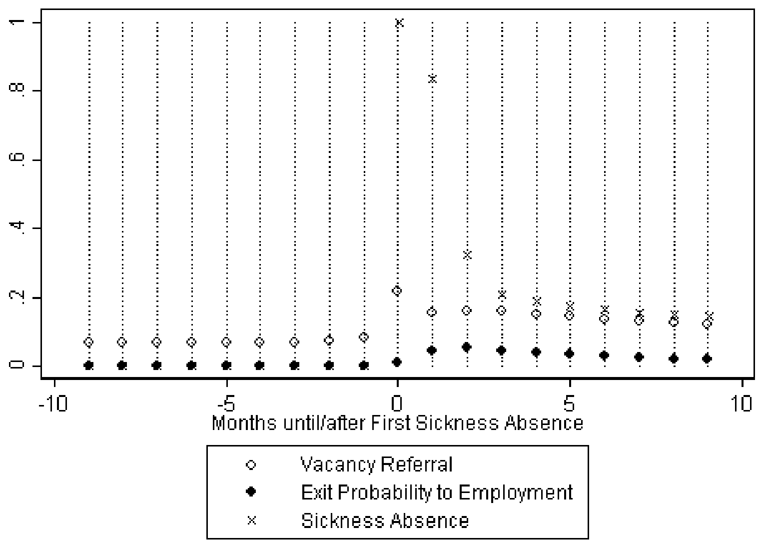
Notes: Graphs are based on 2,363,438 unemployment spells; number of individuals: 1,350,301.

Figure B.2: Timing of Vacancy Referrals, Sanctions, and Transitions to Employment (unrestricted sample)



Notes: Month 0 refers to the month a sanction is imposed. Figure is based on individuals who have been sanctioned during their unemployment spell (n=18,833). By construction, the job finding probability is zero before month 0.

Figure B.3: Timing of Vacancy Referrals, Sickness Absence, and Transitions to Employment (unrestricted sample)



Notes: Month 0 refers to the first time an individual reports sick. Figure is based on individuals who reported sick at least once during their unemployment spell (n=184,015). By construction, the job finding probability is zero before month 0.

Table B.1: Number of observations and transitions (unrestricted sample)

Unemployment	
No. months	17,353,168
No. spells	2,363,438
% exits to employment	43.7
% 1 spell	51.04
% 2 spells	29.92
% 3 spells	13.91
% > 3 spells	5.13
Vacancy Referrals (VR)	
No. VR arrivals	2,654,949
% individuals received VR	68.0
% 1 VR	35.04
% 2 VRs	22.71
% 3 VRs	14.68
% > 3 VRs	27.57
Sanctions (S)	
No.	19,160
% unemployment spells with S	0.8
% sanctioned individuals	1.4
Sickness absence (SA)	
No. months in SA	584,317
% individuals in SA	13.6
% 1 month SA	15.82
% 2 months SA	44.35
% 3 months SA	14.06
% > 3 months SA	25.77
Employment	
No. spells	1,028,717
% exits to unemployment	51.4
% 1 spell	65.13
% 2 spells	24.04
% 3 spells	8.62
% > 3 spells	2.22

Notes: n=1,350,301; individuals might receive more than one VR in a specific month. This is not taken into account. Repeated sanctions during one unemployment spell are ignored.

Table B.2: Descriptive statistics (unrestricted sample)

Sample		
Full Sample		
Female	0.41	
East	0.36	
Age	37.66 (12.00)	
German	0.92	
Married	0.49	
Child	0.38	
Medium secondary school	0.35	
Upper secondary school	0.12	
Vocational training	0.63	
University degree	0.05	
Health restrictions	0.18	
Local unemployment rate	11.86 (5.58)	
Local vacancy rate	11.09 (9.85)	
Sanctioned	*Yes*	*No*
Female	0.29	0.41
East	0.18	0.36
Age	30.47 (9.50)	37.76 (12.00)
German	0.84	0.92
Married	0.29	0.50
Child	0.29	0.38
Medium secondary school	0.25	0.35
Upper secondary school	0.05	0.13
Vocational training	0.53	0.63
University degree	0.01	0.05
Health restrictions	0.11	0.18
Local unemployment rate	9.69 (4.79)	11.89 (5.58)
Local vacancy rate	14.21 (10.46)	11.05 (9.83)
Sick	*Yes*	*No*
Female	0.43	0.41
East	0.46	0.34
Age	39.83 (11.27)	37.31 (12.07)
German	0.92	0.92
Married	0.53	0.49
Child	0.42	0.37
Medium secondary school	0.35	0.35
Upper secondary school	0.06	0.13
Vocational training	0.63	0.63
University degree	0.02	0.05
Health restrictions	0.27	0.17
Local unemployment rate	12.84 (5.71)	11.70 (5.54)
Local vacancy rate	9.96 (9.55)	11.27 (9.88)
VR received	*Yes*	*No*
Female	0.41	0.42
East	0.39	0.30
Age	36.10 (10.91)	41.19 (13.50)
German	0.92	0.92
Married	0.46	0.56
Child	0.41	0.32
Medium secondary school	0.38	0.29
Upper secondary school	0.11	0.15
Vocational training	0.64	0.60
University degree	0.04	0.06
Health restrictions	0.15	0.26
Local unemployment rate	12.10 (5.60)	11.31 (5.48)
Local vacancy rate	10.80 (9.73)	11.76 (10.06)

Notes: Characteristics are measured in first month of first unemployment spell. Standard deviations in parentheses. The vacancy rate is defined as the number of vacancies divided by the number of job seekers. The different subsamples refer to whether the job seekers have been sanctioned, have been sick and have received a VR, respectively, at least once during their first unemployment spell.

B.2 Main estimation results based on a model without post-unemployment outcomes

Table B.3: Estimated Effects of Sanctions in Model with Time-Varying Effects

	Exit to employment	Exit from employment	Log(wage)
Sanction ($t_u - t_s < 3$)	0.2843***	-	-
	(0.0403)	-	-
Sanction ($t_u - t_s \geq 3$)	-0.0064	-	-
	(0.0591)	-	-

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377; M=6. t_u: month of unemployment; t_s: month of the imposition of a sanction. LogLikelihood=-1,166,481.57.

Table B.4: Estimated Effects of Vacancy Referrals in Model with Time-Varying Effects

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR months 1-3	2.0267***	0.5253***	0.5794***	-	-
	(0.1278)	(0.0190)	(0.0097)	-	-
VR months 4-6	1.6837***	0.3336***	0.4698***	-	-
	(0.1170)	(0.0213)	(0.0130)	-	-
VR months 7-9	1.7293***	0.2643***	0.6313***	-	-
	(0.1473)	(0.0261)	(0.0200)	-	-
VR months 10-12	2.0055***	0.2403***	0.6876***	-	-
	(0.1927)	(0.0319)	(0.0281)	-	-
VR months 13-18	1.6848***	0.2817***	0.7288***	-	-
	(0.1544)	(0.0300)	(0.0341)	-	-
VR months 19-36	1.9132***	0.1274***	0.5851***	-	-
	(0.1836)	(0.0311)	(0.0498)	-	-

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377. M=6. LogLikelihood=-1,166,481.57.

B.3 Main estimation results based on single-spell data

Table B.5: Estimated Effects of Sanctions in Model with Time-Varying Effects

	Exit to employment	Exit from employment	Log(wage)
Sanction ($t_u - t_s < 3$)	0.2721***	0.0390	-0.1355***
	(0.0554)	(0.1058)	(0.0136)
Sanction ($t_u - t_s \geq 3$)	0.1333*	0.0001	-0.0623***
	(0.0736)	(0.1591)	(0.0172)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377; M=6. t_u: month of unemployment; t_s: month of the imposition of a sanction. LogLikelihood=-867,374.98.

Table B.6: Estimated Effects of Vacancy Referrals in Model with Time-Varying Effects

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR months 1-3	2.0433***	0.7799***	0.6224***	0.0349	-0.0465***
	(0.1780)	(0.0277)	(0.0133)	(0.0253)	(0.0036)
VR months 4-6	1.7015***	0.5540***	0.5385***	-0.0150	-0.0326***
	(0.1578)	(0.0291)	(0.0173)	(0.0311)	(0.0046)
VR months 7-9	1.9850***	0.4255***	0.6741***	0.1395***	-0.0190***
	(0.2083)	(0.0340)	(0.0255)	(0.0474)	(0.0066)
VR months 10-12	2.1358***	0.3660***	0.7401***	0.0471	0.0062
	(0.2418)	(0.0395)	(0.0344)	(0.0673)	(0.0086)
VR months 13-18	1.7179***	0.2998***	0.7645***	0.1744**	-0.0088
	(0.1829)	(0.0355)	(0.0396)	(0.0803)	(0.0101)
VR months 19-36	1.8771***	0.1226***	0.5975***	0.1835	-0.0109
	(0.1958)	(0.0336)	(0.0535)	(0.1286)	(0.0131)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377. M=6. LogLikelihood=-867,374.98.

B.4 Main estimation results based on a model allowing for state dependence in sickness absence

Table B.7: Estimated Effects of Sanctions in Model with Time-Varying Effects

	Exit to employment	Exit from employment	Log(wage)
Sanction ($t_u - t_s < 3$)	0.3347***	0.14803**	-0.1451***
	(0.0414)	(0.0618)	(0.0096)
Sanction ($t_u - t_s \geq 3$)	0.0678	0.0249	-0.0579***
	(0.0609)	(0.1045)	(0.0155)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377; M=6. t_u: month of unemployment; t_s: month of the imposition of a sanction. LogLikelihood=-1,394,744.08.

Table B.8: Estimated Effects of Vacancy Referrals in Model with Time-Varying Effects

Month of Vacancy Referral	Sanction	Sickness absence	Exit to employment	Exit from employment	Log(wage)
VR months 1-3	2.0946***	0.4701***	0.5607***	0.1205***	-0.0418***
	(0.1277)	(0.0245)	(0.0098)	(0.0140)	(0.0029)
VR months 4-6	1.7113***	0.3800***	0.4458***	0.0189	-0.0195***
	(0.1172)	(0.0275)	(0.0131)	(0.0187)	(0.0038)
VR months 7-9	1.7654***	0.3529***	0.6091***	0.1716***	-0.0090*
	(0.1477)	(0.0348)	(0.0202)	(0.0295)	(0.0055)
VR months 10-12	2.0601***	0.2972***	0.6685***	0.1386***	0.0031
	(0.1928)	(0.0431)	(0.0284)	(0.0431)	(0.0071)
VR months 13-18	1.7173***	0.3807***	0.6994***	0.1995***	0.0004
	(0.1545)	(0.0433)	(0.0343)	(0.0536)	(0.0093)
VR months 19-36	1.9924***	0.2258***	0.5664***	0.2220**	0.0261*
	(0.1836)	(0.0484)	(0.0500)	(0.0876)	(0.0138)

Notes: Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n = 128,377. M=6. LogLikelihood=-1,394,744.08.

B.5 Theoretical framework

We develop a job search model that allows for vacancy referrals, sanctions and sickness absence, taking into account the relevant institutional setting and the opportunities for moral hazard behavior.[21] By analogy to the empirical setting, this model is in discrete time with a month as the time unit. We should point out that

[21]For the sake of readability, this appendix includes statements that are also in its summary in Subsection 2.5 in the paper.

the model is stylized and does not include each and every institutional detail. For example we assume that a punitive benefits reduction always lasts exactly 1 time period and that the rate at which VR arrive does not change over time. Such simplifications keep the model manageable without, hopefully, compromising the use of the model as a benchmark for the empirical specification and as a tool for the interpretation of the results.

Consider first the process leading to employment, in *absence* of VR, sanctions, and sickness. For an unemployed individual, job offers arrive at random moments in time. Every period, there is a probability λ that an offer arrives, while with probability $1-\lambda$ no offer arrives. Job offers are random drawings from a wage offer distribution F. Individuals do not know in advance when job offers arrive. Every time an offer arrives, the decision has to be made whether to accept it or to reject it and search further. We assume that once a job is accepted it will be kept forever at the same wage w. During unemployment, per-period unemployment benefits b are received.

Unemployed individuals aim at maximization of their own expected present value of utility over an infinite horizon. We assume that utility is intertemporally separable and equals the instantaneous income flow w in case one works at a wage w and b in case one is out of work. Clearly, in the absence of wage dispersion, any sufficiently high fixed value of w leads to acceptance at the first possible moment.

Let r be the discount factor (meaning that utility one period ahead has weight $1/(1+r)$ in the current present value), and let R denote the expected present value when following the optimal strategy. We assume that the model is stationary (see e.g. Eckstein and Van den Berg, 2007). In this standard model,

$$R = b + \frac{\lambda}{1+r}\mathbb{E}_w \max\left\{\frac{(1+r)w}{r}, R\right\} + \frac{1-\lambda}{1+r}R \tag{7}$$

where the expectation is taken over the distribution F. The optimal job acceptance strategy has the reservation wage property: an offer w is accepted iff $w > \phi$ with $\phi = rR/(1+r)$.

We now introduce VR into the model. These arrive according to the per-period probability μ. Specifically, there is a probability μ of a VR, a probability λ of a regular job offer, and a probability $1-\lambda-\mu$ of no event. At the moment they arrive, VR do not reveal the full set of job characteristics. Instead, they reveal some characteristics x (notably, the occupation) but not yet the wage.[22] The features x are a random drawing from a distribution G. Let $R_{VR}(x)$ denote the expected present value of having obtained a VR with features x. We can replace equation (7) by

$$R = b + \frac{\lambda}{1+r}\mathbb{E}_w \max\{\frac{(1+r)w}{r}, R\} + \frac{\mu}{1+r}\mathbb{E}_x(R_{VR}(x)) + \frac{1-\lambda-\mu}{1+r}R \tag{8}$$

where the expectation over x is taken with respect to the distribution G.

Upon arrival of a VR, the individual has three choices: to apply, to try to obtain a sick note to report sickness, and to do nothing and run the risk of a sanction as

[22]Subsection 2.5 in the paper uses notation z for x.

a punishment for not applying to the VR. We denote the corresponding expected present values by $R_{VRa}(x)$, R_{ill} and R_s, respectively. Notice that R_{ill} and R_s do not depend on x because the decision not to apply implies that x is irrelevant thereafter. However, that R_{ill} and R_s do not depend on time or on earlier events still needs to be justified. With this in mind, $R_{VR}(x)$ can now be expressed as

$$R_{VR}(x) = \max\{R_{VRa}(x), R_{ill}, R_s\} \tag{9}$$

We will now model the three expected present values in the right-hand side of (9). First, consider R_{ill}. We assume that in every period in which an attempt is made to obtain a sick note, the outcome is a random drawing from a Bernoulli distribution with p_{ill} being the probability of obtaining the sick note. The outcome depends on the availability of a cooperating physician, and we assume that a success in one period does not affect the success probability in another period. To some extent this is justified by the conjecture that multiple requests to the same physician may result in a decreasing willingness of the physician to meet the request. As a result,

$$R_{ill} = p_{ill}\left(b + \frac{1}{1+r}R\right) + (1-p_{ill})R_s \tag{10}$$

If the individual does nothing after a VR then there is a probability p_s of being detected of not having applied. In that case a sanction is imposed, meaning that for one period no benefits are received. By analogy to p_{ill}, we assume that p_s does not depend on earlier violations. Hence,

$$R_s = p_s\frac{1}{1+r}R + (1-p_s)\left(b + \frac{1}{1+r}R\right) \tag{11}$$

Clearly, equations (10) and (11) imply that $R_{ill} > R_s$. This means that an individual always tries to obtain a sick note. (If, in reality, this is costly, then it depends on the cost magnitude what is more attractive.)

If the individual applies to the VR then with a probability p_r he is rejected by the employer. If he is not rejected then he is offered a wage w. This is a random drawing from a wage offer distribution H that depends on x, so we may write the corresponding distribution function as $H(w|x)$. The dependence on x reflects that x is informative on the type of job. The decision to apply is made after observing x and before observing w, but the fact that x is informative on w means that x is an important determinant of the decision whether to apply. After the revelation of w, the decision has to be made whether to accept it or not.

If the individual is rejected by the employer then he need not fear a sanction. However, if he is not rejected by the employer but rejects the offer himself then he runs the risk of a sanction. The expected present value $R_{VRa}(x)$ can now be expressed as follows,

$$R_{VRa}(x) = p_r\left(b + \frac{1}{1+r}R\right) + (1-p_r)\mathbb{E}_{w|x}\max\left\{\frac{(1+r)w}{r}, R_s\right\} \tag{12}$$

where the expectation is taken over the distribution H of $w|x$. For convenience we assume here that the risk of a sanction upon rejection of offers generated by a VR is the same as the risk of a sanction upon failure to apply to a VR if no sick note is obtained.[23] The optimal strategy concerning the acceptance of offers generated by a VR again has the reservation wage property: an offer w is accepted iff $w > \xi$ with $\xi = rR_s/(1+r)$. Notice that the VR reservation wage ξ does not depend on x because x is irrelevant after w is revealed. Also note that $\xi < \phi$ because rejection of a regular job offer does not incur the risk of a sanction whereas the rejection of a VR-generated job offer does.

In (12), the "$\mathbb{E}\max$" term can be written as $R_s + \frac{1+r}{r}\int_\xi^\infty \overline{H}(w|x)dw$ with $\overline{H}(w|x) = 1 - H(w|x)$. If wage offers associated with x tend to be low then the integral will have a small value. Together, equations (9)-(12) can be shown to imply that

$$R_{VR}(x) = \frac{1}{1+r}R + (1-(1-p_{ill})p_s)b + \max\left\{(p_r - p_{ill})p_s b + (1-p_r)\frac{1+r}{r}\int_\xi^\infty \overline{H}(w|x)dw, 0\right\} \tag{13}$$

In (13), the first term in the "max" corresponds to the decision to apply to the VR while the second term corresponds to the decision not to apply but to try to obtain a sick note instead. Notice that if $p_r \geq p_{ill}$ then the first term is positive so then the individual always prefers to apply, whatever the value of x. Alternatively, if $p_{ill} = 1$ while p_r is small and p_s is large and the wage offers associated with x tend to be low then the first term may be negative. More in general, the probability that the individual applies to a VR, unconditional on x, is the probability

$$\Pr_x\left\{\int_\xi^\infty \overline{H}(w|x)dw > \frac{(p_{ill} - p_r)p_s}{1-p_r}\frac{r}{1+r}b\right\} \tag{14}$$

where x has the distribution G.

The model can be straightforwardly extended to include a probability of being sick for other reasons, e.g. with the per-period probability γ. For convenience we assume that the sickness occurs just before other events in the same period occur, and that it excludes such events and that it always goes along with a sick note. Analogously, we may include a per-period probability σ of getting a sanction for a different reason (e.g., refusal to participate in a training program). Such extensions are essential in the empirical analysis but they do not add important insights to the findings of the current section so far.[24]

From the above we may deduce expressions for the distributions of outcomes of interest. At every elapsed duration of unemployment, the transition probability to work equals

$$\lambda\overline{F}(\phi) + \mu\mathbb{E}_x\left(\mathrm{I}\left(\int_\xi^\infty \overline{H}(w|x)dw > \frac{(p_{ill} - p_r)p_s}{1-p_r}\frac{r}{1+r}b\right)\cdot(1-p_r)\overline{H}(\xi|x)\right) \tag{15}$$

[23]Recall from Section 2 that it is allowed to reject offers obtained through a VR if the wage is below a certain fraction of the benefits level. Therefore the probability p_s may over-estimate the probability of obtaining a sanction upon rejection of a VR-generated offer.

[24]The same applies to settings where individuals can get both a regular offer and a VR in the same time period.

where $\overline{F} = 1 - F$ and I(.) is the indicator function which equals 1 iff its argument is true. Jobs are obtained in the regular way or by way of a VR; in the latter case the value of x must be sufficiently promising to induce the individual to apply, and the applicant must not be rejected by the employer, and the wage offer must be sufficiently attractive to take the job.

Similarly, at every elapsed duration of unemployment, the sanction probability equals

$$\mu \mathbb{E}_x \left\{ \mathrm{I}(\int_{\xi}^{\infty} \overline{H}(w|x)dw < \frac{(p_{ill} - p_r)p_s}{1-p_r} \frac{r}{1+r} b) \ \cdot \ (1-p_{ill})p_s \right\}$$

$$+ \mu \mathbb{E}_x \left\{ \mathrm{I}(\int_{\xi}^{\infty} \overline{H}(w|x)dw > \frac{(p_{ill} - p_r)p_s}{1-p_r} \frac{r}{1+r} b) \ \cdot \ (1-p_r)H(\xi|x)p_s \right\} \ + \ \sigma$$

The three additive terms represent sanctions due to refusal to apply to a VR, sanctions due to rejecting a job offer generated by a VR, and sanctions due to other reasons, respectively.

The conditional probability to be on sick leave equals

$$\gamma + \mu \mathbb{E}_x \left\{ \mathrm{I} \left(\int_{\xi}^{\infty} \overline{H}(w|x)dw < \frac{(p_{ill} - p_r)p_s}{1-p_r} \frac{r}{1+r} b \right) \ \cdot \ p_{ill} \right\} \tag{16}$$

It can be shown that the distribution of accepted wages is a mixture of, on the one hand, $F(w)$ truncated from below at ϕ, and, on the other hand, for every x for which (14) is true, the distributions $H(w|x)$ truncated from below at ξ.[25] All these expressions can be used to analyze the comparative statics effects of parameters such as μ, p_{ill} and p_s on individual outcomes.

The model also provides expressions for causal effects of interest. For example, for individuals who were not sick in the beginning of the period, the probability that a VR leads to sick leave equals the term in accolades in expression (16), which has a value between 0 and 1.

In the model so far, sanctions have an ex ante threat effect on exit to work but no ex post effect on exit to work. That is, the punitive benefits reduction does not have an income effect on the reservation wages in periods beyond the period in which the benefits are cut. This is a mechanical artifact of the model, because the benefits reduction only lasts one period which is the period in which the violation is detected. Conceptually it is not difficult to extend the model by assuming that the reduction covers multiple periods. In that case, the reservation wages will be even lower in the periods after the violation is detected, and hence the transition rate to work will be even higher, and the mean post-unemployment wage even lower, than before the sanction was imposed. A practical problem is that the number of different reservation wages increases with the number of time periods during which benefits are reduced. Moreover, one would need to consider the modeling of multiple

[25]Specifically, apart from a multiplicative normalization factor, the probability density of accepted wages equals $\lambda f(w)\mathrm{I}(w > \phi) + \mathbb{E}_x\{\mu h(w|x)\mathrm{I}(w > \xi)\mathrm{I}(\mathrm{apply}(x))(1-p_r)\}$ where apply(x) is condition (14) and where f and h are the densities of F and H, respectively. If $\Pr(w = x) = 1$, so that individuals observe the VR wage before applying, then this expression and other expressions in this section simplify considerably.

consecutive sanctions. Yet an additional issue is that sanctions may have persistent effects because they may increase the subsequent level of monitoring. For these reasons we do not elaborate on ex post sanction effects in the theoretical setting. However, in the empirical analysis those effects are clearly of interest.

In the model, the ex ante threat effects of sanctions work as follows. If the monitoring detection intensity p_s is positive then ϕ is lower than if $p_s = 0$, because being unemployed is less attractive if there is a chance that the individual encounters a sanction in the future. The threat of a sanction also implies that the individual applies to a VR more often.[26] And if $p_s > 0$ then ξ is lower than otherwise, because the rejection of a VR entails the risk of an immediate sanction. Indeed, without sanctions, individuals would apply to all VR jobs and use the same reservation wage for VR-generated offers as for regular offers. Because of the lower ϕ and ξ and because of the higher probability of applying to a VR job, the transition rate to work is higher due to the threat of sanctions. For the same reasons, the mean post-unemployment wage is lower. These ex ante threat effects are reduced in absolute value if $p_{ill} > 0$, compared to if $p_{ill} = 0$, because having a sick note prevents a sanction.

Notice that the probability p_{ill} of obtaining a sick note after receipt of a VR is in itself a potentially important determinant of the unemployment duration. Since it reduces the threat of a sanction, it reduces the incentive to apply to a VR job, and it makes workers more selective with respect to job offers. All this increases unemployment durations.

Because one of the contributions of the paper is to provide the first evaluation of VR as an ALMP, it may be useful to examine the special model with VR but without sanctions in some more detail. We noted above that in that case the reservation wages for VR-generated offers and for regular offers are identical. The distributions of accepted wages need not be identical. Among regular jobs this is proportional to $f(w)$ on $w > \phi$ whereas among VR-generated jobs it is proportional to $\mathbb{E}_x(h(w|x))$ on $w > \phi$. Clearly, if $\Pr(w = x) = 1$ and if F first-order stochastically dominates H then regular jobs pay on average higher wages than VR-generated jobs.

We end this section by mentioning an additional implication of the theoretical model for the empirical model. The per-period conditional probabilities of an exit to work, a sanction, and a sick leave, and the mean post-unemployment wage, all depend on all underlying parameters. This is e.g. because they all depend on the reservation wage ξ which in turn depends on all parameters by way of the value function R_s. By contrast, the per-period conditional probability of receiving a VR μ is determined outside of the model. In reality, the case worker may fine-tune μ to individual characteristics that are partly unobserved to us, so that a stochastic association with (determinants of) the other conditional probabilities may occur.

[26] In a monitoring experiment among VR recipients, Engström, Hesselius and Holmlund (2012) find that the more intensive the monitoring, the higher the probability that the individual applies to the assigned VR.