# The impact of sanctions for young welfare recipients on transitions to work and wages and on dropping out

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## Abstract

The reintegration of young welfare recipients into the labor market is a major policy goal in many European countries. In this context monitoring and sanctions are important policy tools. In this paper, we analyze the impact of strict sanctions on job search outcomes for young welfare recipients in Germany. The German benefit system is characterized by harsh sanctions for this group. Strict sanctions effectively take away the benefits for three months if young welfare recipients do not comply with their job search requirements. We jointly analyze the impact of these sanctions on job search outcomes and on dropping out of the labor force based on administrative data on a large inflow sample of young male jobseekers into welfare. We estimate multivariate duration models taking selection based on unobservables into account. Our results indicate that there is a trade off between an increased job entry rate and an increased withdrawal from the labor force as well as lower entry wages. Sanctions increase the probability of finding a job, but these jobs go along with lower earnings. Moreover, sanctions significantly increase the probability of dropping out.

**Keywords:** social assistance, unemployment, sanctions, post unemployment outcomes, youth unemployment.
**JEL codes:** J64, J65, C41, C21.

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# 1 Introduction

In Europe, the lack of employment among the young is regarded to be the most pressing labor market problem. Many young welfare recipients face very small reemployment rates. It is important to know how strong they react to sanctions and therefore we examine this in a setting with a quite harsh sanction regime: in Germany. The German welfare system is characterized by especially strong sanctions for young welfare recipients: If they do not comply with their obligations like the requirement to search for a job, they risk not receiving any welfare payments for up to three months. If they are sanctioned a second time within a specific time period, even the payments for rent and heating can be cut.

Economic job search models that incorporate sanctions explicitly predict a faster transition to work once a sanction is imposed since we expect the reservation wage to fall and the search intensity to rise, which is in line with the findings of a large number of empirical studies.[1] However, once the welfare recipients have been sanctioned, not only the current utility of searching for a job might increase but also the expected utility from future welfare receipt might drop. This, and the considerable reduction of their welfare benefit, might increase the probability of dropping out of the regular labor market. In the context of job search monitoring, McVicar (2008) provides evidence from the UK that unemployed job seekers leave unemployment more often for 'other states' than employment and education if the monitoring intensity increases. This implies that sanctions might have negative long-run effects by pushing sanctioned individuals into black market activities or by lowering the probability that these individuals benefit from the counseling by the caseworkers, have access to job search and training programs and receive vacancy referrals. Therefore, it is important to consider the impact of strong sanctions on the probability of dropping out of the labor force without continuing welfare benefit receipt.

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[1]For corresponding evidence in the context of unemployment insurance systems, see e.g. Abbring, van den Berg and van Ours (2005), Lalive, van Ours and Zweimüller (2005), Svarer (2011), Røed and Weslie (2012) and van den Berg and Vikström (2014). van den Berg, van der Klaauw and van Ours (2004) and van der Klaauw and van Ours (2014) provide evidence about the positive effects of sanctions on the transition rate to work in the context of welfare benefit receipt.

Most of the previous studies focus on the impact of a sanction on the transition rate to work. One exception is Busk (2016), who analyzes the impact of sanctions based on a sample of unemployed job seekers aged older than 25 years in Finland. She finds that sanctions increase the transition rate from unemployment to work especially for job seekers who receive means-tested benefits. In separate models she additionally investigates the impact of sanctions on transitions out of the labor force. Her results suggest that sanctions increase the exit rate to outside the labor force. She does not consider the quality of the jobs found after a sanction. The main contribution of our study is to analyze jointly the impact of imposed sanctions on the probability of taking up a regular job, the wage of this job, and the transition rate out of labour force.

There exists evidence from qualitative studies indicating that strong sanctions might bring about negative effects for the sanctioned individuals that are usually not captured in studies focusing on job finding rates. Götz, Ludwig-Mayerhofer and Schreyer (2010) report that caseworkers are rather skeptical about strong sanctions. While caseworkers state that strong sanctions can change the behavior in a desirable way, they additionally stress potential adverse consequences for the sanctioned individuals. For example they might accept jobs that are low paid, unstable and that provide very little training. Schreyer, Zahradnik and Götz (2012) conducted a survey in one job center among young sanctioned welfare recipients. The respondents' statements suggest that sanctions lead to restricted nutrition. Some respondents reported that they lost their apartments and had to temporarily move into a hostel for the homeless. Many respondents reported increased debt problems. Moreover, the responses provided some indication that due to the sanction, welfare recipients took up jobs without declaring them to the welfare agency or engaged in criminal activities in order to earn some money. Results on the United Kingdom by Machin and Marie (2006) also point to a positive relationship between crime and benefit cuts or sanctions.

In contrast to the previous literature, we consider the quality of the job matches and analyze whether sanctions push the young welfare recipients into lower paid contributory

jobs.[2] Moreover, by measuring the effect of sanctions on the probability of dropping out the labor force and the benefit system, we extend the analysis of sanctions by an important dimension. If young people leave the labor force for some time due to benefit sanctions, it might for some of them imply stronger scarring effects and due to a lack of resources that they become more likely to engage in the shadow economy.

Our study is based on a large inflow sample of people aged 18 to 24 years into welfare without employment during the period of January 2007 to March 2008 that is drawn from administrative records. We restricted the sample to young men living in West Germany. We take into account the dynamic selection of young welfare recipients into the treatment by applying the "timing of events approach" following Abbring and van den Berg (2003). This approach allows to control for selection into treatment based on observed and unobserved characteristics. We are interested in the impact of imposed sanctions on two outcomes: leaving unemployment for a job and dropping out of the labor force. Therefore, we estimate a competing risks model for the two destination states, and additionally evaluate the treatment effect on the job match quality by estimating a wage equation for the initial daily wage. Moreover, we allow for different effects of the first and the second (cumulative) sanction.

Our results suggest that the first and the second strong sanction increase the probability of finding a job. However, these employment spells come along with a significantly lower daily wage. Moreover, sanctions significantly increase the transition rate out of the labor force. This holds for male jobseekers living in single households. In contrast to this, individuals living in multi-person households are not pushed out of the labor force after the imposition of a strong sanction. The reason for this might be that multi-person households still receive additional benefits which are not affected by the sanction, like benefit payments for children. Overall our results indicate that there exist – next to the strong positive effects on employment probabilities – additional effects on the job quality and on the exit rate out of the labor market which have to be taken into account when evaluating strong sanctions

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[2]For corresponding studies in the context of sanctions in UI systems, see e.g. van den Berg and Vikström (2014) and van den Berg, Hofmann and Uhlendorff (2015). Their results suggest that jobs found after the imposition of a sanction go along with lower wages and are less stable.

for young benefit recipients.

The paper is organized as follows: Section 2 describes the institutional background. Section 3 presents the administrative data and descriptive statistics. Section 4 describes the econometric approach. The results of the empirical analysis are presented in Section 5, and Section 6 concludes.

# 2    Welfare benefit sanctions in Germany

This section describes the means-tested benefit system of the Social Code II in place during our observations window and highlights its sanction rules. In our period under review of the years 2007 to 2009 the average stock of welfare recipients aged at least 15 years who were capable of working ranged from 4.9 to 5.3 million persons (Source: Statistics Department of the German Federal Employment Agency); about one fifth of them were aged younger than 25 years.

In the German benefit system welfare recipients are required to take actions that help to reduce the dependence on the welfare benefit. All members of welfare recipient households who are capable of working are supposed to take such actions. Hence, they must cooperate with their job centers, e.g., by participating in suitable active labor market programs and by actively searching for jobs or suitable training opportunities.

A system of benefit sanctions is intended to help to enforce the benefit rules. To understand how sanctions operate, it is necessary to describe the different components of the welfare benefit UB II. One component, the basic cash benefit, is supposed to cover regular expenditures. In 2007 this component amounted to 345 € a month for singles, single parents or welfare recipients whose partner was younger than 18 years. It is 80 percent of this level for additional household members aged 15 years or more. Household members aged below 15 years received 60 percent of the benefit level for singles. In our observation period the basic cash benefit was raised annually in July and was set to 359 € in July 2009. A second important component of the UB II is the benefit that covers costs for accommodation and

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heating.

If welfare benefit recipients do not comply with specific obligations they can be sanctioned for a duration of three months. Relatively low sanctions apply if welfare recipients miss an appointment with the job center or a necessary medical examination. In this case the sanction was 10 percent of their basic cash benefit. For any further non-compliance in the form of missing an appointment within a period of one year the sanction was at the level of the last sanction augmented by additional 10 percent of the full basic cash benefit.

Much harsher sanctions exist for infringements against other obligations. These sanctions apply for instance for insufficient efforts to search for work and to improve job finding perspectives, refusal of job offers, non-compliance with an individual action plan, and refusal of participation in active labor market programs. For welfare recipients aged younger than 25 years the sanctions are particularly severe. A first non-compliance of the types mentioned above implies that they are not paid their basic cash benefit. For this broad group of infringements any further non-compliance within one year leads to a full loss of the welfare benefit for three months.[3] In this paper, we focus on the impact of strict sanctions. van den Berg, Uhlendorff and Wolff (2014) analyse the impact of first strict and mild sanctions on the transition rate to work. Their results suggest that strict sanctions have a significantly larger effect on the transition rate to work.

Imposing a benefit sanction is a process involving several steps. The job centers must first of all inform welfare recipients about their obligations and the consequences of non-compliance as soon as they register. If an infringement against benefit rules then takes place and is observed by the job center, the case-worker has to document the infringement. The job center sends a written notification to the welfare recipients that contains the details

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[3]For individuals aged at least 25 years, a first non-compliance reduces the welfare benefit by 30 percent of the full cash benefit. It is 60 percent of the full cash benefit for a second infringement of the same category within one year and the sanction is a full temporary benefit loss for any further non-compliance within one year. For welfare recipients that are aged younger than 25 years caseworkers may reduce the duration of the sanction period to six weeks under certain circumstances, e.g., if a welfare recipient is very young and not fully aware of the consequences of his/her non-compliance. For any sanction that exceeds 30 percent of the basic cash benefit the job centers can provide the sanctioned welfare recipient with non-cash benefits like food stamps. No data are available that show how frequently sanction periods are reduced and how frequently non-cash benefits are received by sanctioned welfare recipients.

of the infringement and of the related sanction. The letter includes an answer form with which the welfare benefit recipient can explain a good cause for the non-compliance. It also includes a date until which the response has to be provided to the job center. How much time the welfare recipient has to provide a response is not specified by the benefit rules, but information available in our administrative microdata on the time between the non-compliance and the start of the sanction suggest that the time interval for such a response is one to three weeks. If a welfare recipient does not provide a good cause for non-compliance, the sanction comes into force the first day of the calendar month following the month in which the answer form to the notification letter had to be handed in.

The benefit rules envisage that a welfare recipient is sanctioned after a non-compliance with his obligations. For various reasons though a benefit sanction might not actually be imposed: First, not all infringements are fully observed and in turn cannot be well documented by the caseworkers, so that by imposing a sanction they would risk a law suit. Next, job center staff with a huge workload might not have enough time to monitor all welfare recipients with the same intensity or to provide them with job offers and active labor market program participation offers. Moreover, there is some scope for discretion of caseworkers when it comes to imposing a benefit sanction, as the benefit rules do not fully define what is a good cause for non-compliance. The literature on welfare benefit sanctions suggests that benefit sanctions are not universally imposed. Boockmann, Thomsen and Walter (2014) emphasize the substantial variation in job center sanction rates. Götz *et al.* (2010) report that benefit sanctions are not universally imposed. In another qualitative analysis, Karl, Müller and Wolff (2011) report that even in situations in which a sanction against a welfare recipient would be possible, some caseworkers attempt to find ways for avoiding a benefit sanction. All this is important for our methodological approach (see Section 4). The evaluation of sanction effects requires a "no anticipation" assumption, implying that individuals do not know in advance if a violation will be detected and whether a sanction will be imposed, and, if a sanction is imposed, at exactly which moment it will be imposed (see Abbring and Van

den Berg, 2003).[4]

Aggregate data from the Statistics Department of the German Federal Employment Agency show that from 2007 to 2009 the annual number of new welfare benefit sanctions ranged from 727 to 785 thousand (across all ages). Among these sanctions the share of relatively severe sanctions due to infringements other than missing appointments ranged from 42.1 to 46.4 percent. The overall sanction rates – defined as the share of welfare recipients with at least one sanction – range from 2.4 to 2.5 percent from 2007 to 2009. They are somewhat higher in West than in East Germany. Moreover, the sanction rates for men at 3.4 to 3.6 percent are more than twice as high as those for women. Finally, the sanction rates for welfare recipients aged less than 25 years at 3.8 to four percent are far higher than the overall sanction rate and the sanction rates for 25-49 year-olds or those aged at least 50 years. This is to be expected as young welfare recipients, as a special target group according the Social Code II, are granted more attention by their job centers than people in other age-groups.

# 3 Data

Our analysis is based on administrative data. We combine information from two databases: the Integrated Employment Biographies (IEB) and the Unemployment Benefit II History Records. The IEB contain spell data of daily precision on contributory and minor employment as well as registered unemployment and job search, and active labor market program participation. It also includes spells of unemployment benefit receipt. The Unemployment Benefit II History Records provide spell data on unemployment benefit II receipt together with a household identifier. These data also provide information about the day when a

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[4]It is important to distinguish this from knowledge of the monitoring scheme. The latter leads to ex ante responses. These do not depend on the exact moment of a future sanction. As we shall see in Section 4, ex ante responses may implicitly affect the values of the model parameters. However, these responses are generally not identified unless the data contain a policy change in the monitoring regime, which is not the case in our data. See Van den Berg and Vikström (2014) for a more detailed exposition. In this context it is useful to refer to the important literature on effects of activation requirements for welfare participation on welfare and labor market outcomes. Besley and Coate (1992) provide a theoretical discussion. Evaluations of the large 1996 welfare reform in the US are summarized in Blank (2002). Grogger (2003) examines effects of financial incentives and time limits.

sanction started and ended and the sanction type. Taken together we have information on the duration of welfare receipt while not being employed, the calendar day of the start of a welfare benefit sanction and the sanction type as well as on a number of different destination states and the (daily) post-unemployment wage.

Our sample was drawn from the population of young men aged 18 up to 24 years who started a period of welfare receipt while not being employed in the period of January 2007 up to March 2008. We restricted the sample to people who at their spell start were registered as jobseekers, since these individuals are facing a risk of being sanctioned if they do not comply with the job search requirements. These individuals might participate in active labor market programs or work in minor employment.

We focus on West German men. A considerably lower unemployment rate in West Germany implies much more scope for job centers to place welfare recipients to work. In turn, benefit sanctions in response to refusing job offers are far more likely for young welfare recipients living in the West than in the East and it is easier for sanctioned people to take up employment in West as opposed to East Germany. We did not study women, because they are the primary caretaker of children below the age of three implying much less strict job search requirements than for other welfare recipients. In turn they face a much lower risk of being sanctioned. Moreover, for women in the age-group 18 to 24 years it would have been quite important to model the (endogenous) fertility decisions together with the other dependent variables in our model, which is beyond the scope of this paper.

Note that we excluded welfare recipients in 50 job centers for which micro data on sanctions were not available. These job centers are entirely run by municipalities and not jointly with local labor agencies. In the years 2007 to 2009 about 13 percent of (the stock of) unemployed welfare recipients were registered in these job centers in West Germany (Source: Statistics Department of the Federal Employment Agency). Next, we excluded observations with sanctions during the first seven days after entry into welfare, as these sanctions are very likely a result of an infringement that took place prior to the studied welfare spell. Moreover,

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we excluded some observations due to missings in key variables. Finally, we discarded a few observations of disabled people who are rarely sanctioned and people with a university or technical university degree as extremely few of the young welfare recipients in our data (about 0.6 percent) are characterised by such a degree.

Though in the available data we could track employment transitions until December 2009, we modeled the duration of welfare spells at longest until the end of August 2009. The reason for this is that we regarded an exit state "out of the labor force for at least four months". This transition is defined by leaving welfare for at least four months without being observed as employed, as a jobseeker, as an active labor market program participant or as a recipient of UI benefit (or the welfare benefit) during the four month period after a spell end. We right-censored spells at the time when a welfare recipient reaches the age of 25 years, since the sanction rules change upon reaching this age threshold.

[**Table 1 about here**]

Table 1 displays the share of transitions into strong sanctions and into the two exit states that we consider. The Table displays these numbers for two sub-samples that we study separately: people living in single and in multi-person households (at the start of the spell). The sample consists of about 31,900 spells of people in single-person households and 38,500 spells of people in multi-person households. At 14.4 percent the share of young men in single households who were sanctioned at least once is about two percentage points higher than for young men in multi-person households. The share of transitions into unsubsidized contributory jobs is 33 percent for young men in single households and 38 percent for young men in multi-person households. 6.1 percent of young men in single households and 5.1 percent of young men in multi-person households drop out of the labor force. These statistics show that young men in multi-person households are hence more successful in the labor market than men in single households. As the former are also less frequently sanctioned, the different shares of employment exit might be a result of a difference in the compliance with job search requirements and other benefit rules. The median post-

unemployment daily wages of singles are with 35.8 € about 30 cents lower than for young men living in multi-person households.

Table 1 also displays the just mentioned statistics for three sub-samples of people who have (i) never been sanctioned, (ii) have been sanctioned at least once or (iii) have been sanctioned at least twice. The share of employment exits is highest for those who were not sanctioned. The differences in the share of employment exit among these groups might be due to a selection effect in the sense that people with relatively low job finding perspectives are sanctioned more often than people with better job finding perspectives. The differences might reflect that people with relatively high job hazards are more likely than others to exit into jobs prior to facing a situation in which the job center could sanction them. The median post-unemployment daily wages tend to be considerably lower for those with at least one or at least two sanctions compared with those individuals with no sanction at all. This may though be due to a selection issue and not due to the benefit sanction.

[**Table 2 about here**]

An important issue is what happens to welfare recipients who leave the labor force for at least four months. To shed light on this, we display in Table 2 for all exits of this type that took place prior to January 2009 the share of welfare recipients that are again not observed in one of the states available in our data six, nine or twelve months after their exit. For more than 85.5 percent of these welfare recipients without a strong sanction in single households we cannot find any information in our data six months after their exit date. This share gradually declines between the 6th and the 9th to more than 69 percent and the 9th and the 12th month after the exit out of the labor force to about 60 percent. The pattern is similar if we only regard the sanctioned men in single-person households, even though the shares are somewhat but not remarkably lower. There are no substantial differences between men in single households and men in multi-person households, when we compare the development of the share of individuals who are still out of the labor force at the three points in time.

To be sure that dropping out of the labor force for at least four months is not mainly a phenomenon of entering vocational training in classrooms starting in September, we studied the distribution of the out of labor force outflow over the calendar months from January 2007 until August 2009. The results show that the share of outflow in September of the total outflow into this state is only higher by a few percentage points than in the months just before or after September (the results are available on request). Therefore, exiting into vocational training in schools/classrooms is not a key reason for our out of the labor force exits.

Figure 1 plots the empirical transition rates (Kaplan-Meier estimates) of men living in single households and men living in multi-person households into a first strong sanction against the duration of welfare receipt without employment. The pattern of an initial peak and then a gradual decline is not surprising. One reason is that job centers were supposed to place young welfare recipients into jobs, training or work opportunities immediately after they enter welfare receipt, so that very quickly after entering welfare receipt much scope for non-compliance emerges. Moreover, the pattern may reflect heterogeneity among the welfare recipients, e.g., if they consist of different groups of people with different attitudes towards risking a sanction for non-compliance. Overall, the differences between the sanction rates of the two groups are not very large.

[**Figure 1 about here**]

The left panel in Figure 2 shows that both for men in single households and in multi-person households the daily transition rates into unsubsidized contributory employment tend to decline with duration of welfare receipt. In the first interval of 30 days they are quite high at more than 0.2 percent for men in single households and about 0.32 percent for men in multi-person households. Up to a duration of 150 days they are significantly higher for the latter than for the former group, but the differences, at less than 0.04 percentage points, are far lower than in the first interval. In the longer run the employment exit rates decline considerably to below 0.04 percent, when we regard a duration of more than 1.5 years.

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Compared with the employment transition rates, the transition rates into our out-of-labor-force status are much smaller (right panel in Figure 2). They show a peak in the interval of more than 180 up to 210 days, and decline remarkably in the next month. Thereafter, they still tend to decline somewhat. The out-of-labor-force transition rates of men in single households often exceed those of men in multi-person households. But only in one interval (more than 60 up to 90 days) do the confidence bands imply a significant difference.

[**Figure 2 about here**]

More detailed descriptive statistics are provided in the Online Appendix A.

# 4 Empirical Model

We are interested in the causal impact of the imposition of a sanction on two duration outcomes, the duration of welfare receipt until taking up employment and the duration until dropping out of the labor force. We apply the "timing of events" approach (Abbring and van den Berg, 2003) – which is the standard approach in the literature on sanction effects – to a setting with competing risks; we estimate a mixed proportional hazard rate model with one dynamic treatment and two competing risks (see Drepper and Effraimidis (2016) for identification results for timing of events models with competing risks). Some individuals are sanctioned more than once during the observed welfare spell. We extend the model by taking into account transition rates to the first and to the second strong sanction. In addition to that, we evaluate the impact on the job match quality, measured by the initial daily wage of the employment spell.

We observe an inflow sample into welfare receipt. We assume that all individual differences in the probability of finding a job at time $t$ can be characterized by observed characteristics $x_t$, unobserved characteristics $V_e$, and a sanction effect if a sanction has been imposed before $t$. Similarly, we assume that all individual differences in the probability of dropping out of the labor force can be characterized by the same observed characteristics $x_t$, unobserved characteristics $V_o$, and a sanction effect if a sanction has been imposed before $t$.

Also the duration until a sanction depends on observable characteristics $x_t$, whether or not the individual has been sanctioned before, and unobserved characteristics $V_s$.

We specify the transitions rate from welfare receipt to a job $\theta_e(t)$, the transition rate out of the labor force $\theta_o(t)$, and the transition rate into the first and the second sanction $\theta_s(t)$ as exponential transition rates with piecewise constant terms allowing for a flexible durations dependence:

$$\theta_e(t) = \exp(\sum_{j=2}^{J} I_j(t)\lambda_{je} + x_t'\beta_e + I_s(t > t_{s1})\alpha_{e1} + I_s(0 < t_{s2} - t_{s1} < 365)\alpha_{e2} + I_s(t_{s2} - t_{s1} \geq 365)\alpha_{e3} + V_e)$$

$$\theta_o(t) = \exp(\sum_{j=2}^{J} I_j(t)\lambda_{jo} + x_t'\beta_o + I_s(t > t_{s1})\alpha_{o1} + I_s(0 < t_{s2} - t_{s1} < 365)\alpha_{o2} + I_s(t_{s2} - t_{s1} \geq 365)\alpha_{o3} + V_o)$$

$$\theta_s(t) = \exp(\sum_{j=2}^{J} I_j(t)\lambda_{js} + x_t'\beta_s + I_s(t > t_{s1}) \sum_{j=1}^{J} I_{sj}(t_{s1})\gamma_j + V_s) \tag{1}$$

$I_j(\cdot)$ takes on the value one if $t$ is in the interval $j$. $\lambda_{je}$, $\lambda_{jo}$ and $\lambda_{js}$ describe the interval specific baseline hazard rates for $J$ intervals. $I_s(\cdot)$ takes on the value one if $t > t_{s1}$, $0 < t_{s2} - t_{s1} < 365$ and $t_{s2} - t_{s1} \geq 365$, respectively. $t_{s1}$ is the day of the first sanction, while $t_{s2}$ is the day of the second sanction. $\alpha_{e1}$ ($\alpha_{o1}$) is the effect of the first sanction on the transition rate into jobs (out of the labor force). The second sanction is more severe if the infringement takes place within one year after the first sanction, see Section 2. Therefore, we allow for different effects of sanctions which are imposed within one year after the first sanction ($\alpha_{e2}$ and $\alpha_{o2}$) and of sanctions which are imposed later ($\alpha_{e3}$ and $\alpha_{o3}$). The hazard rate of the imposition of a sanction might change after the first sanction and might depend on the timing of the first sanction. We control for this by including a series of dummies indicating the timing of the first sanction. The interval specific indicator $I_{sj}(\cdot)$ takes on the value one if the first sanction has been imposed in interval $j$. This indicator $I_{sj}(\cdot)$ is one from $t_{s1}$ onwards.

We assume that a sanction does not affect the two transition rates before the moment of the sanction. This assumption is referred to as the no-anticipation assumption. In the case of sanctions this assumption is very likely to hold, since whether a violation will be detected and whether and when a sanction will be imposed cannot be anticipated by the

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welfare recipient; see Section 2. Moreover, we assume that the unobserved heterogeneity $V_e$, $V_o$ and $V_s$ is constant over time, and that $V_e$, $V_o$ and $V_s$ are uncorrelated with observed characteristics $x$.[5]

In order to identify the causal impact of sanctions on realized wages, we assume that the unobserved heterogeneity and the causal effect have an additive impact on the mean log wage. We specify the following equation for the wage at the beginning of the new employment spell:

$$\ln w = x_t'\beta_w + I_s(t > t_{s1})\alpha_{w1} + I_s(0 < t_{s2}-t_{s1} < 365)\alpha_{w2} + I_s(t_{s2}-t_{s1} \geq 365)\alpha_{w3} + \sum_{j=2}^{J} I_j(t_e)\eta_w + V_w + \varepsilon_w$$

(2)

The sanction effects are given by $\alpha_{w1}$, $\alpha_{w2}$ and $\alpha_{w3}$, $V_w$ is the unobserved heterogeneity, and $\varepsilon_w$ is assumed to be normally distributed with mean zero and unknown variance $\sigma_w^2$. We allow the log wage to vary with respect to the previous duration of welfare receipt $t_e$ by including indicator $I_j(\cdot)$, which takes on the value one if $t_e$ is in the interval $j$.

## Distribution of unobserved heterogeneity

We specify the distribution of unobserved heterogeneity $G$ to have a discrete support with $M$ support points. In order to force the corresponding probabilities to be between zero and one and to sum to one 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 equation specific components of the unobserved heterogeneity $V$ takes on a specific value at support point $m$. This implies that for a model with $M = 2$ $G$ would be described by 5 parameters, for $M = 3$ we estimate 10 parameters, etc. This approach allows

[5]This warrants some discussion. If the monitoring regime changes over calendar time then this could affect the composition of the unobserved characteristics in the inflow into welfare. For example, stricter monitoring may lead to a stronger threat effect among potential recipients; see e.g. Black et al. (2003). In our setting, there was no regime change in the observation interval for the inflow. More in general, relaxing the "random effects" assumption requires richer data than those at our disposal. Notably, multiple welfare spells per individual would be helpful. Our observation window is too small for that. The subset for whom multiple spells are observed is highly selective as it requires very short welfare spells as well as employment spells that are sufficiently short to observe a return into welfare (and not a transition into a lenghty UI spell).

for a flexible covariance matrix for the unobserved components. For a similar model for unobserved heterogeneity in the context of timing of events models see Crepon, Ferracci, Jolivet and van den Berg (2014) 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 based on unobservables by a flexible discrete distribution 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. This model selection is based on the estimation of the multivariate duration model, i.e. the joint estimation of the parameters of the three hazard rates $\theta_e(t)$, $\theta_o(t)$ and $\theta_s(t)$. In a second step we estimate the full model including the wage equation using the "optimal" number of support points $M$ for each equation determined in the first step.

## Likelihood function

Given this setup, the likelihood contribution of an individual $i$ with an observed welfare spell duration $t$ for given unobserved and observed characteristics $V$ and $x$ is given by:

$$
\begin{aligned}
L_i(x, V) = {} & \theta_e(t|x_i)^{\delta_e} \theta_o(t|x_i)^{\delta_o} S(t|x_i, \theta_e, \theta_o) \\
& \left[ \underbrace{\theta_{s1}(t_{s1}|x_i) \exp[-\int_0^{t_{s1}} \theta_{s1}(\tau|x_i)d\tau]}_{S_{s1}(t_{s1}|x_i, \theta_{s1})} \right]^{\delta_{s1}} \left[ \underbrace{\exp[-\int_0^t \theta_{s1}(\tau|x_i)d\tau]}_{S_{s1}(t|x_i, \theta_{s1})} \right]^{1-\delta_{s1}} \\
& \left[ \theta_{s2}(t_{s2}|x_i) \exp[-\int_{t_{s1}}^{t_{s2}} \theta_{s2}(\tau|x_i)d\tau] \right]^{\delta_{s2}} \left[ \exp[-\int_{t_{s1}}^t \theta_{s2}(\tau|x_i)d\tau] \right]^{(1-\delta_{s2})\delta_{s1}} \\
& \left( \frac{1}{\sqrt{2\pi\sigma_w^2}} \exp\left( -\frac{(\ln w_i - \widehat{\ln w_i})^2}{2\sigma_w^2} \right) \right)^{\delta_e}
\end{aligned}
\tag{3}
$$

$t_{s1}$ and $t_{s2}$ are the duration until a first sanction and the duration until a second sanction, respectively. Both have to be lower than $t$ and the duration until the first sanction has to be shorter than the duration until the second sanction. The indicator $\delta_e$ is one if an exit into employment is observed and zero otherwise. $\delta_o$ is the corresponding indicator variable

for an out of the labor force exit. $S(t|x_i, \theta_e, \theta_o)$ is the survivor function representing the probability of no exit into employment nor out of the labor force until duration $t$. $\theta_{s1}(t_{s1}|x_i)$ is the transition rate into the first sanction and $\delta_{s1}$ is an indicator that is one if an exit to a first sanction occurred and zero otherwise. $\theta_{s2}(t_{s2}|x_i)$ represents the transition rate into the second sanction and $\delta_{s2}$ indicates an occurrence of such a sanction by one and no such occurrence by zero. $S_{s1}$ represents the survival probability with respect to first sanctions. If we observe a second sanction, the left part of the third row enters the likelihood. If no second sanction occurs but we observe a first sanction, the second part of the third row of equation (3) $\exp[-\int_{t_{s1}}^{t} \theta_{s2}(\tau|x_i)d\tau]$ enters the likelihood. This corresponds to the probability of receiving no second sanction until $t$ given an imposed first sanction at $t_{s1}$. If the individual is not sanctioned at all, the third row does not enter the likelihood. Finally, in the last row of equation (3) $\ln w_i$ is the logarithm of the observed wage in our data – in case we observe a transition from welfare to a regular job – and $\widehat{\ln w_i}$ corresponds to the predicted value based on the coefficients $\beta_w$.

The log-likelihood contribution of individual $i$ equals to the weighted sum of the $M$ log-likelihood contributions corresponding to the different points of support. The log-Likelihood function for the $M$ points of support with $N$ individuals is given by:

$$\ln L = \sum_{i=1}^{N} \ln \sum_{m=1}^{M} \pi_m L_i(x, V(m)) \tag{4}$$

# 5  Results

We estimate two separate models for young men living alone and young men living in multi-person households. For both samples, we start by selecting the preferred specification for the discrete distribution of unobserved heterogeneity based on the competing risks models without the wage equation. In our empirical specifications, we control for observed characteristics including education, age and household composition and allow for flexible duration dependencies. For the estimation of the hazard rate into strong sanctions we additionally control in a flexible way for the timing of the first sanction by a set of dummy variables.

It turns out that it is difficult to find support for more than two unobserved groups with respect to the hazard rate from welfare to out of the labor force. Therefore, we only increase the number of mass points for the unobserved heterogeneity with respect to the hazard rate to work and the hazard rate for the risk of being sanctioned. The maximum number of support points for the hazard rate to out of the labor force is set to two. An evaluation of the model fit based on the Akaike Criterion suggests a specification with three support points for the sample of individuals living in single households. For the sample of individuals living in multi-person households four support points is the preferred specification. This implies that for the competing risks model the unobserved heterogeneity distribution is estimated with seven and ten additional parameters, respectively, compared to the model without unobserved heterogeneity.[6] The results are qualitatively the same when we slightly increase or decrease the number of support points, which suggests that our findings are robust with respect to small changes in the specification of the unobserved heterogeneity. Moreover, neither the estimated effects for imposed sanctions nor the parameter estimates of the unobserved heterogeneity change qualitatively when we estimate the full model including the wage equation.

In the following, we will first present results of a baseline model which allows for homogeneous treatment effects. In a second step, we introduce effect heterogeneity by allowing the impact of a sanction to be different for skilled welfare recipients and for welfare recipients living in households with children.

## 5.1   Baseline results

Tables 3 and 4 display estimation results of our baseline models for people living in single households and people living in multi-person households. We focus on the sanction effects on our three outcomes. The parameter estimates for the full model are presented in the Online Appendix B, Tables B5 and B6. The results for singles imply a considerable positive effect of the first sanction on the transition rate to employment. It is raised by about 109 percent

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[6]The effects of imposed sanctions for these models are reported in the Tables B3 and B4 in the Online Appendix B. They are very similar to the ones of the full model including the wage equation.

(permanently). The order of magnitude of the effect of the first sanction on the employment hazard is lower than in a comparable study for welfare recipients in Rotterdam by van den Berg *et al.* (2004). Their results imply an effect of more than 140 percent, though applying to men and women in all age-groups and households and not only to young men in single households. The second sanction within 12 months, which implies a complete temporary benefit loss, increases the employment transition rate further by more than 150 percent. The second sanction after more than 12 months also implies such considerable increases of the hazards, this latter result however is based on few sanctions in our sample.

[**Table 3 about here**]

The results on the exit rate to employment are in line with implications of a standard job search model that would suggest that sanctions will lead to lower reservation rates and/or more search effort, in turn leading to a faster take up of jobs. The effects on daily earnings in the post-unemployment job are compatible with such an interpretation. The first sanction lowers them by more than five percent and the effect is well determined. The two types of second sanctions though do not exhibit any significant additional effect on daily wages, even though they further increase the exit rate into employment. This would be in line with an explanation that repeated sanctions do not affect reservation wages, but only raise job search effort. And this is plausible, as we regard a population that from the start is characterised by a rather low earnings potential and low reservation wages. Their scope for raising their exit rate into employment by lowering reservation wages may be very limited.

The first sanction leads to a 286 percent higher exit rate out of the (regular) labor market, which is considerably higher than the corresponding effect on employment. When comparing these magnitudes one should keep in mind that the employment transition rates as displayed in the left panel of Figure 2 are usually at least more than twice up to more than 20 times higher than the transition rate out of the labor market, shown in the right panel of Figure 2. The employment effect contributes more to an increased (overall) exit rate out of welfare receipt than the effect on the out of the labor market hazard. The second sanction within

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a year amplifies the effect of the first and raises the transition rate out of the labor market by more than 105 percent. The point estimate of the parameter of the second sanction after more than one year would even imply a greater effect, but is not well determined. These results suggest that sanctions make alternatives to job search on welfare more attractive, including work in the shadow economy or continued job search while receiving support by friends or the family. That the effect of the first sanction is stronger than the effect of a second one might be due to various reasons. A plausible explanation is that after the first, but not after the second sanction monitoring is increased considerably. That could for instance be a key issue for people working in the shadow economy. Some of them could fear that their activity might be detected due to increased monitoring, which might lead to more severe consequences than only a second benefit sanction. Increased monitoring that is accompanied by more active labor market program offers might make it difficult to continue with an activity in the shadow economy. Therefore, for some people an exit from welfare could become the best option. A similar argument might hold for welfare recipients who on top of their welfare benefit rely on some other undeclared source of income like financial support from their family and friends.

Let us now turn to Table 4 with the baseline results for young men living in multi-person households. Once more we first regard the effects on the employment hazard and on daily wages. Again the implications are that the first sanction affects the hazard and the second sanction within one year amplifies the effect of the first sanction and raises the hazard even further. The positive effect of a second sanction after more than one year is insignificant. With rises of about 70 percent due to the first and an additional 124 (22.5) percent for the second sanction within (after more than) one year, the effects are considerably smaller than for men living in single households. Also, the significant wage effect of the first sanction at about -3.3 percent is in absolute terms smaller than for the group living in single households. The wage effects for the second sanction are again statistically insignificant. These differences just described are quite plausible. In contrast to men in single households

men in multi-person households can rely on the remaining welfare benefit of other household members, provided that they at least partly pool their welfare benefit. Hence, the financial consequences of the welfare benefit sanction are absorbed as additional financial resources are available and the need to reduce reservation wages and to raise search effort is lower than for singles. However, we have to be cautious with the interpretation of the differences in the point estimates. The 95 percent confidence intervals of the point estimates for the two samples are overlapping, which suggests that the differences do not differ significantly from zero.

The different sanction effects on the transition rate out of the labor market are all statistically insignificant for men in multi-person households. Moreover, the point estimates are considerably lower than for men in single households. That unlike men in single households men in multi-person households do not react by increasing their transition rate out of the labor force is again a plausible result. They do not have much reason either to let their household leave welfare, because in this case they could no longer rely on welfare benefits of other household members. Moreover, in contrast to single males, men in multi-person households could only react this way if among the members of the welfare recipient household an agreement on ending benefit receipt is reached, which is not very likely.

[**Table 4 about here**]

## 5.2 Effect Heterogeneity

Our second set of specifications includes interactions of the first sanction with being skilled (possessing a formal occupational qualification) and for people in multi-person households additionally with having children. We do not include interaction effects for the second sanctions as the number of second sanctions is rather small.[7]

The results for singles are reported in Table 5. The point estimates for the interaction effects with the skill level imply a stronger rise of the exit rates into work and out of labor

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[7]Allowing for heterogeneous treatment effects adds considerably to the computational burden of estimating the model. We therefore restrict attention to heterogeneity in covariates that are thought to play a crucial role in the institutional setting of the monitoring.

force for skilled than for unskilled welfare benefit recipients. In addition, we find a stronger reduction of the post-unemployment wage for skilled individuals due to a first sanction. As the number of job opportunities but also reservation wages tend to be higher for skilled than for unskilled workers, they might reduce their reservation wages further. However, all interaction terms are statistically insignificant and rather small.

[**Table 5 about here**]

For welfare recipients living with other household members, Table 6 shows no significant effects of the first sanction's interaction with being skilled on the transition rate into work. It also has no well-determined effect on the logarithmic wage, even though the size of the interaction effect would imply a negative wage effect that is twice as high as for unskilled workers. We find a considerable and significant effect of this interaction on the transition rate out of the labor force. The rise of this exit rate due to a first sanction is about 80 percent higher for skilled people than for unskilled people in multi-person households. One reason for this may be that skilled people might more easily have access to support from relatives with (relatively) high income living in other households or might even move back into such a household not qualifying for welfare.

For the interaction of first sanctions with having at least one child we find a significant impact on the transition rate to employment. The estimate implies that the effect of a first sanction on the employment rate is still positive for people with children, but it is roughly 20 percent lower than for childless people. The coefficient for the interaction effect between first sanction and own children in the wage equation is not statistically significant. However, the point estimate suggests that for parents we do not observe an impact on the wages. These results are plausible. The job search behavior of parents as opposed to childless people is affected to a lower extent by a benefit sanction, as parents have to deal with more restrictions in order to balance future work and time with their family. Hence, after a benefit sanction they remain more particular with respect to acceptable jobs than childless people. For the impact on the transition rate out of labor force we find a negative point estimate for the

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interaction effect with having children, which is not statistically significant.

[Table 6 about here]

# 6    Conclusions

Sanctions are a key tool to provide incentives to unemployed benefit recipients to cooperate with their job center and to take actions that raise their chances of getting a job. The German welfare system is characterized by especially strong sanctions for welfare recipients younger than 25 years. Strong sanctions, which are imposed for instance if the jobseeker refuses a job offer, imply a loss of the basic cash benefit for three months. A second sanction for repeated non-compliance within one year leads to the loss of their entire welfare benefit for three months including the costs of their accommodation.

The existing literature on sanctions for unemployed jobseekers focusses mainly on the impact of a first benefit sanction on the exit rate from unemployment to work. Our study contributes to the literature by investigating jointly the effects of sanctions on the transition rate out of the labor force, on daily earnings and on the exit rate to work. As the sanction effects might be less pronounced if sanctioned individuals can rely on support from other household members, we estimate separate models for individuals living alone and individuals living in multi-person households.

We find positive effects of sanctions on the exit rate to work, which are accompanied by a wage reduction. A second sanction further raises the exit rates into employment, in particular for young men living alone. Moreover, we find large effects of sanctions on the exit rate out of the labor force for young males living alone, while there is no evidence for such an effect for young males living with other welfare recipients. Taken together these results indicate that sanction effects are (in absolute terms) lower in multi-person households than in single households. With more than one person in the household, a sanctioned person can often rely on support from other household members and hence on their welfare benefit. In turn, effects on exit rates into employment and wages are less pronounced. Moreover,

a multi-person as opposed to a single household has no considerable incentive to leave the welfare benefit system if one person is sanctioned.

The results from our analysis should be interpreted taking the results of qualitative studies on the sanction regime for young welfare recipients in Germany into account. The qualitative evidence suggests that sanctioning is accompanied by some effects that are not desirable. Young sanctioned recipients report harsh consequences like having their energy supply cut off or losing their accommodation. Some caseworkers seem to be reluctant to implement the very strong repeated sanctions within one year because they fear for instance that young welfare recipients can no longer pay their rent and end up homeless. Moreover, they fear that sanctioned individuals might terminate their registration at their job center and start activities in the shadow economy including petty crime.

While the presented evidence underlines the importance of a sanction system in providing incentives to search for jobs, it also indicates that there is a policy trade off between an increased job entry rate on the one side and an increased withdrawal from the labor force and lower entry wages on the other side. There are good reasons for policy-makers to take actions that avoid an increased exit rate out of the labor market that is induced by very strong benefit sanctions. Our results show that temporarily losing the entire benefit for a single household has more severe consequences in terms of increased exit rates out of the labor force than losing temporarily the benefit for a member of a multi-person household. A reform might therefore try to prevent particularly high sanctions so that singles have sufficient incentives to remain registered with their job center. An upper limit for the sanction could be defined in such a way that it would help to avoid extreme consequences for people who have to rely on welfare benefits to meet their basic needs. When designing such a policy, the policy-makers should be aware of the fact that some people are particularly vulnerable, because they cannot even rely on the support of other household or family members. Very high sanctions with severe short-term consequences for people without any sources of income other than welfare could also be avoided by reducing the monthly sanction

amount while prolonging the number of months the sanction is in force. This could allow welfare recipients to continue paying regular bills and in turn avoid having the energy supply to their apartments cut or losing their apartment.

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# Tables and Figures

FIGURE 1

EMPIRICAL TRANSITION RATES INTO THE FIRST STRONG PUNITIVE SANCTION
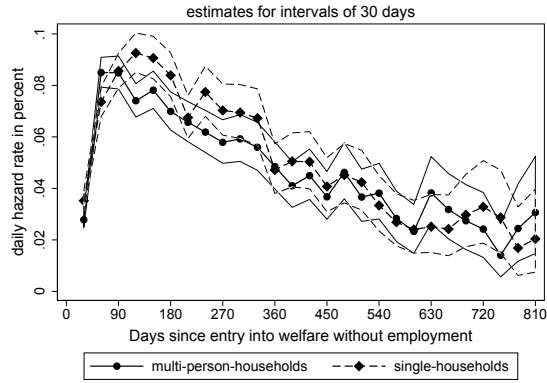


FIGURE 2

EMPIRICAL TRANSITION RATES INTO UNSUBSIDIZED CONTRIBUTORY EMPLOYMENT AND OUT OF THE LABOR FORCE
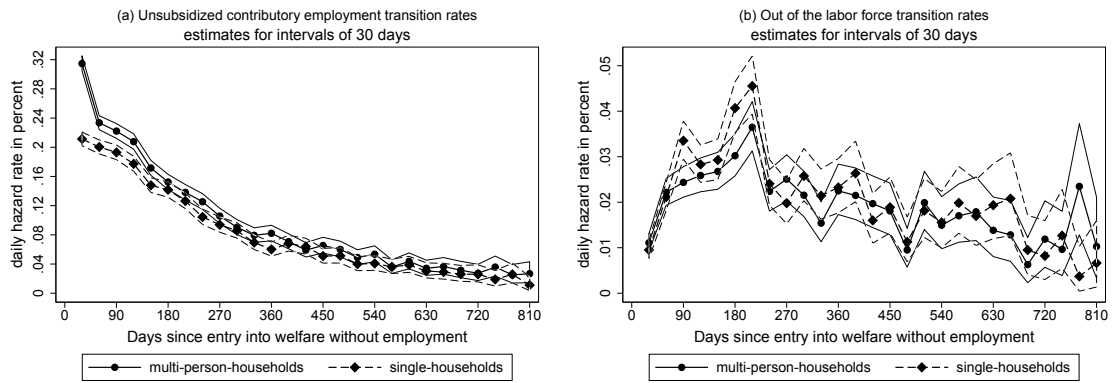
Table 1: Share of exit into strong sanctions and into different labor force states and median post-unemployment real daily wage (in €) [1,2]

| | All | No strong sanction | At least one strong sanctions | At least two strong sanctions |
|---|---|---|---|---|
| | | In single-person household | | |
| Share of exit into | | | | |
| - 1st sanction | 0.144 | 0.000 | 1.000 | 1.000 |
| - 2nd sanction | 0.029 | 0.000 | 0.203 | 1.000 |
| - 2nd sanction within one year after 1st sanction | 0.027 | 0.000 | 0.185 | 0.914 |
| - unsubsidized contributory job | 0.330 | 0.343 | 0.255 | 0.200 |
| - out of the labor force | 0.061 | 0.060 | 0.069 | 0.067 |
| - median daily wage | 35.8 | 36.2 | 32.9 | 31.0 |
| Number of spells | 31,890 | 27,307 | 4,583 | 930 |
| | | In multi-person household | | |
| Share of exit into | | | | |
| - 1st sanction | 0.124 | 0.000 | 1.000 | 1.000 |
| - 2nd sanction | 0.029 | 0.000 | 0.238 | 1.000 |
| - 2nd sanction within one year after 1st sanction | 0.027 | 0.000 | 0.216 | 0.909 |
| - unsubsidized contributory job | 0.380 | 0.394 | 0.278 | 0.199 |
| - out of the labor force | 0.051 | 0.051 | 0.055 | 0.057 |
| - median daily wage | 36.1 | 36.4 | 33.2 | 32.2 |
| Number of spells | 38,492 | 33,727 | 4,765 | 1,134 |

Source: Own calculations

1) Contributory jobs exclude vocational training.

2) Real daily wages in prices of the year 2005 for exits into unsubsidized contributory jobs.

Table 2: No status found 6, 9 and 12 months after an out of the labor force transition (shares)[1]

| Months after exit | 6 months | 9 months | 12 months | Number of spells |
|---|---|---|---|---|
| | In single-person household | | | |
| - No strong sanction | 0.855 | 0.691 | 0.600 | 1,531 |
| - At least one strong sanction | 0.802 | 0.645 | 0.542 | 273 |
| | In multi-person household | | | |
| - No strong sanction | 0.870 | 0.737 | 0.637 | 1,599 |
| - At least one strong sanction | 0.800 | 0.677 | 0.614 | 220 |

Source: Own calculations

1) Only for out of the labor force exits prior to January 2009.

Table 3: Model for male jobseekers living in single households

|  | Exit to work | Exit out of Labor Market | Log(Wage) |
|---|---|---|---|
|  | Coefficients and standard errors in brackets | | |
| First sanction | 0.7368 *** | 1.3496 *** | -0.0549 ** |
|  | (0.1087) | (0.3759) | (0.0272) |
| Second sanction within 12 months | 0.9215 *** | 0.7199 ** | 0.0100 |
|  | (0.1488) | (0.3222) | (0.0432) |
| Second sanction after 12 months | 0.7659 * | 0.9293 | 0.0704 |
|  | (0.4151) | (0.6100) | (0.2766) |

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -142,748.44. We control for observed characteristics and duration dependence. For the full set of coefficients see Table B5.

Table 4: Model for male jobseekers living in multi-person households

|  | Exit to work | Exit out of Labor Market | Log(Wage) |
|---|---|---|---|
|  | Coefficients and standard errors in brackets | | |
| First sanction | 0.5294 *** | 0.0649 | -0.0332 * |
|  | (0.1088) | (0.0949) | (0.0172) |
| Second sanction within 12 months | 0.8073 *** | 0.2940 | 0.0528 |
|  | (0.1502) | (0.1901) | (0.0456) |
| Second sanction after 12 months | 0.2033 | 0.3874 | -0.0592 |
|  | (0.4234) | (0.5156) | (0.1780) |

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood=-174,581.35. We control for observed characteristics and duration dependence. For the full set of coefficients see Table B6.

Table 5: Model for male jobseekers living in single households: Effect heterogeneity

|  | Exit to work | Exit out of Labor Market | Log(Wage) |
|---|---|---|---|
| First sanction | 0.7021*** | 1.3497 *** | -0.0527** |
|  | (0.1077) | (0.3822) | (0.0266) |
| First sanction x skilled worker | 0.1601 | 0.0421 | -0.0365 |
|  | (0.1135) | (0.2371) | (0.0341) |
| Second sanction within 12 months | 0.9981*** | 0.7003** | -0.0036 |
|  | (0.15668) | (0.3357) | (0.0503) |
| Second sanction after 12 months | 0.8314** | 0.9114 | 0.0573 |
|  | (0.4211) | (0.6059) | (0.2803) |

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. Skilled worker are individuals having a vocational training. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood=-142,746.69. We control for observed characteristics and duration dependence.

Table 6: Model for male jobseekers living in multi-person households: Effect heterogeneity

|  | Exit to work | Exit out of Labor Market | Log(Wage) |
|---|---|---|---|
| First sanction | 0.5777*** | 0.0634 | -0.0345* |
|  | (0.1179) | (0.0996) | (0.0185) |
| First sanction x skilled worker | 0.0194 | 0.5863 *** | -0.0502 |
|  | (0.1218) | (0.2209) | (0.0341) |
| First sanction x children | -0.2228** | -0.3701 | 0.0380 |
|  | (0.0870) | (0.2645) | (0.0247) |
| Second sanction within 12 months | 0.7831*** | 0.3441* | 0.0441 |
|  | (0.1528) | (0.1966) | (0.0495) |
| Second sanction after 12 months | 0.1954 | 0.4599 | -0.0688 |
|  | (0.4181) | (0.5177) | (0.1922) |

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. Skilled worker are individuals having a vocational training. The variable children is an indicator being one if there are children living in the household, zero otherwise. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood=-174,571.73. We control for observed characteristics and duration dependence.

# Appendix A Detailed descriptive statistics intended to be made available online

Tables A1 to A3 display averages of the covariates for the non-sanctioned and sanctioned welfare recipients with at least one and at least two sanctions for the single household and multi-person household sample. The statistics refer to characteristics at the start of the welfare spells in our sample. The sanctioned welfare recipients tend to be younger, more frequently of German nationality and unskilled than those that did not face a sanction.

Our analysis is concerned with sanction effects on welfare recipients' post-unemployment (real daily) wage in an unsubsidized contributory job. Table A4 displays selected percentiles of the wages separately for welfare recipient who were not sanctioned, who were sanctioned at least once or at least twice. Note that the jobs considered include part-time jobs. Hence, daily wages can be quite low. Our statistics therefore only take exits into employment into account provided that the daily wage exceeds 16.44 €, which corresponds to a monthly wage of 500 €. In all our analyses spells that are characterized by an exit into a contributory job that pays a lower wage were regarded as right-censored.

It becomes clear from Table A4 that the daily wages of sanctioned people are lower than those of welfare recipients who were not sanctioned. They tend to be lowest for people who were sanctioned at least twice. The differences between the non-sanctioned group and the group with at least one sanction are quite low at the 10th percentile of the post-unemployment wage distribution. However, there are already some considerable differences between the wages of these two groups at the 25th percentile of close to three Euros. This holds both for the sample of men in single and in multi-person households. These differences reach more than six Euros when we regard the 75th and eight to nine Euros for the 90th percentile. The differences in post-unemployment wages between the group of welfare recipients that were sanctioned at least once and the group of welfare recipients that were sanctioned at least twice are relatively small for men living in multi-person households (1.4 € or less). For men in single households this difference is very small for the 10th and 25th percentile, but ranges from about two up to more than three Euros when we consider the other percentiles. These descriptive results fit well to an expectation that sanctions lower reservations wages and hence sanctioned welfare recipients more frequently accept low-paid jobs than non-sanctioned welfare recipients. This hypothesis, however, needs to be tested in our main analysis. The descriptive statistics may just imply that the sanctioned welfare recipients are more frequently characterized by placement impediments like low skills or low talents than the non-sanctioned ones.

1

Table A1: In single-person household sample: Averages of selected characteristics of the welfare recipients[1]

| | No strong sanction | At least one strong sanction | At least two strong sanctions |
|---|---|---|---|
| Number of observations | 27,307 | 4,583 | 930 |
| Entry quarter | | | |
| - Quarter 1, year 2007 | 0.279 | 0.260 | 0.281 |
| - Quarter 2, year 2007 | 0.201 | 0.220 | 0.228 |
| - Quarter 3, year 2007 | 0.206 | 0.203 | 0.212 |
| - Quarter 4, year 2007 | 0.160 | 0.174 | 0.156 |
| - Quarter 1, year 2008 | 0.155 | 0.143 | 0.124 |
| Age-distribution: | | | |
| - 18 to 19 years | 0.161 | 0.242 | 0.283 |
| - 20 years | 0.121 | 0.151 | 0.165 |
| - 21 to 22 years | 0.304 | 0.328 | 0.342 |
| - 23 to 24 years | 0.415 | 0.280 | 0.211 |
| Nationality | | | |
| - German | 0.873 | 0.913 | 0.931 |
| - Turkish | 0.037 | 0.029 | 0.028 |
| - other foreign nationality | 0.090 | 0.058 | 0.041 |
| Education: | | | |
| - no occupational degree, no schooling degree | 0.141 | 0.215 | 0.249 |
| - no occupational degree, low schooling degree | 0.407 | 0.484 | 0.482 |
| - no occupational degree, high schooling degree | 0.029 | 0.011 | 0.013 |
| - voc. training, no high schooling degree | 0.229 | 0.119 | 0.090 |
| - voc. training, high schooling degree | 0.023 | 0.005 | 0.004 |
| - education missing | 0.170 | 0.167 | 0.161 |
| Federal States | | | |
| - Schleswig-Holstein | 0.071 | 0.079 | 0.098 |
| - Hamburg | 0.047 | 0.047 | 0.053 |
| - Lower Saxony | 0.145 | 0.152 | 0.157 |
| - Bremen | 0.025 | 0.024 | 0.024 |
| - North Rhine-Westphalia | 0.335 | 0.341 | 0.337 |
| - Hesse | 0.063 | 0.054 | 0.053 |
| - Rhineland-Palatinate | 0.053 | 0.063 | 0.061 |
| - Baden-Württemberg | 0.097 | 0.075 | 0.057 |
| - Bavaria | 0.142 | 0.138 | 0.137 |
| - Saarland | 0.022 | 0.026 | 0.025 |
| Other regional controls | | | |
| - District unemployment rate in % | 9.150 | 9.267 | 9.532 |
| - District long-term unemployment rate in % | 4.347 | 4.388 | 4.523 |
| - District vacancy-unemployment ratio | 0.134 | 0.129 | 0.126 |

Source: Own calculations

1) Measured at the start of their spell.

Table A2: In multi-person household sample: Averages of selected characteristics of the welfare recipients[1] (part A)

| | No strong sanction | At least one strong sanction | At least two strong sanctions |
|---|---|---|---|
| Number of observations | 33,727 | 4,765 | 1,134 |
| Entry quarter | | | |
| - Quarter 1, year 2007 | 0.279 | 0.259 | 0.272 |
| - Quarter 2, year 2007 | 0.202 | 0.208 | 0.205 |
| - Quarter 3, year 2007 | 0.226 | 0.202 | 0.201 |
| - Quarter 4, year 2007 | 0.151 | 0.180 | 0.184 |
| - Quarter 1, year 2008 | 0.142 | 0.152 | 0.137 |
| Age-distribution: | | | |
| - 18 to 19 years | 0.274 | 0.373 | 0.411 |
| - 20 years | 0.144 | 0.150 | 0.151 |
| - 21 to 22 years | 0.271 | 0.280 | 0.287 |
| - 23 to 24 years | 0.311 | 0.197 | 0.151 |
| Family status: | | | |
| - not living with partner | 0.640 | 0.710 | 0.719 |
| - married | 0.144 | 0.075 | 0.061 |
| - not married but living with partner | 0.216 | 0.215 | 0.220 |
| Nationality | | | |
| - German | 0.746 | 0.764 | 0.747 |
| - Turkish | 0.110 | 0.108 | 0.122 |
| - other foreign nationality | 0.144 | 0.127 | 0.131 |
| Education: | | | |
| - no occupational degree, no schooling degree | 0.170 | 0.258 | 0.284 |
| - no occupational degree, low schooling degree | 0.425 | 0.472 | 0.462 |
| - no occupational degree, high schooling degree | 0.027 | 0.009 | 0.009 |
| - voc. training, no high schooling degree | 0.164 | 0.084 | 0.076 |
| - voc. training, high schooling degree | 0.011 | 0.003 | 0.002 |
| - education missing | 0.203 | 0.174 | 0.168 |
| Number of own children: | | | |
| - aged less than 3 years | 0.163 | 0.150 | 0.154 |
| - aged 3 to 5 years | 0.049 | 0.040 | 0.036 |
| - aged 6 to17 years | 0.031 | 0.038 | 0.036 |

Source: Own calculations

  1) Measured at the start of their spell.

Table A3: In multi-person household sample: Averages of selected characteristics of the welfare recipients[1] (part B)

| | No strong sanction | At least one strong sanction | At least two strong sanctions |
|---|---|---|---|
| Partner information | | | |
| - aged at least 26 years | 0.069 | 0.046 | 0.045 |
| - foreign nationality | 0.069 | 0.049 | 0.043 |
| - no occupational degree, no schooling degree | 0.043 | 0.047 | 0.053 |
| - no occupational degree, schooling degree | 0.133 | 0.113 | 0.110 |
| - education missing | 0.095 | 0.082 | 0.078 |
| Federal States | | | |
| - Schleswig-Holstein | 0.060 | 0.056 | 0.049 |
| - Hamburg | 0.038 | 0.041 | 0.047 |
| - Lower Saxony | 0.141 | 0.140 | 0.131 |
| - Bremen | 0.023 | 0.021 | 0.019 |
| - North Rhine-Westphalia | 0.341 | 0.345 | 0.336 |
| - Hesse | 0.064 | 0.064 | 0.073 |
| - Rhineland-Palatinate | 0.073 | 0.084 | 0.087 |
| - Baden-Württemberg | 0.101 | 0.082 | 0.084 |
| - Bavaria | 0.140 | 0.145 | 0.148 |
| - Saarland | 0.019 | 0.022 | 0.026 |
| Other regional controls | | | |
| - District unemployment rate in % | 8.874 | 8.844 | 8.846 |
| - District long-term unemployment rate in % | 4.181 | 4.145 | 4.130 |
| - District vacancy-unemployment ratio | 0.131 | 0.128 | 0.128 |

Source: Own calculations
  1) Measured at the start of their spell.

Table A4: Distribution of the post-unemployment real daily wage of non-sanctioned and sanctioned welfare recipients[1]

| | No strong sanction | At least one strong sanction | At least two strong sanctions |
|---|---|---|---|
| | In single-person household | | |
| Number of observations | 9,356 | 1,167 | 186 |
| 10th percentile | 22.3 | 20.8 | 20.7 |
| 25th percentile | 28.6 | 25.9 | 25.7 |
| median | 36.2 | 32.9 | 31.0 |
| 75th percentile | 46.9 | 40.5 | 38.0 |
| 90th percentile | 58.7 | 50.9 | 47.6 |
| | In multi-person household | | |
| Number of observations | 13,286 | 1,327 | 226 |
| 10th percentile | 22.5 | 20.5 | 20.8 |
| 25th percentile | 29.2 | 26.3 | 26.0 |
| median | 36.4 | 33.2 | 32.2 |
| 75th percentile | 47.2 | 40.8 | 39.9 |
| 90th percentile | 59.6 | 50.7 | 49.3 |

Source: Own calculations
  1) In prices of the year 2005.

# Appendix B   Additional estimation results intended to be made available online

## Models without unobservables

Table B1: Model for male jobseekers living in **single** households

|  | Exit to work | Exit out of Labor Market | Log(Wage) |
|---|---|---|---|
| First sanction | 0.2614*** | 0.2267*** | -0.04771*** |
|  | (0.0337) | (0.0705) | (0.0129) |
| Second sanction within 12 months | 0.18430** | 0.1662 | -0.0213 |
|  | (0.0793) | (0.1492) | (0.0301) |
| Second sanction after 12 months | 0.0440 | 0.3034 | 0.0445 |
|  | (0.3793) | (0.5354) | (0.2707) |

Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -142,810.77. We control for observed characteristics and duration dependence.

Table B2: Model for male jobseekers living in **multi-person** households

|  | Exit to work | Exit out of Labor Market | Log(Wage) |
|---|---|---|---|
| First sanction | 0.1201*** | -0.0002 | -0.0490*** |
|  | (0.0320) | (0.0784) | (0.0115) |
| Second sanction within 12 months | 0.0165 | 0.1629 | -0.0008 |
|  | (0.0735) | (0.1500) | (0.0281) |
| Second sanction after 12 months | -0.3523 | 0.2885 | -0.0858 |
|  | (0.3737) | (0.5102) | (0.1692) |

Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood= -174,773.41. We control for observed characteristics and duration dependence.

**Competing risk model for exit to work and out of labor market**

Table B3: Model for male jobseekers living in **single** households. Estimations based on joint ToE models for exit to work and exit out of the Sample

|  | Exit to work | Exit out of Labor Market |
|---|---|---|
| First sanction | 0.7653*** | 1.2595*** |
|  | (0.1099) | (0.3820) |
| Second sanction within 12 months | 0.9652*** | 0.6785** |
|  | (0.1490) | (0.3235) |
| Second sanction after 12 months | 0.8024* | 0.8821 |
|  | (0.4160) | (0.6118) |

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -138,912.91. We control for observed characteristics and duration dependence.

Table B4: Model for male jobseekers living in **multi-person** households. Estimations based on joint ToE models for exit to work and exit out of the Sample

|  | Exit to work | Exit out of Labor Market |
|---|---|---|
| First sanction | 0.5479*** | 0.0742 |
|  | (0.1156) | (0.1049) |
| Second sanction within 12 months | 0.8679*** | 0.3156 |
|  | (0.1551) | (0.2211) |
| Second sanction after 12 months | 0.2459 | 0.4141 |
|  | (0.4178) | (0.5224) |

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood= -169,394.30. We control for observed characteristics and duration dependence.

## Table B5: Full model for male jobseekers living in **single** households

| | Exit to work | | Exit out of LF | | Sanction | | Log(Wage) | |
|---|---|---|---|---|---|---|---|---|
| | Coeff. | Std. err. | Coeff. | Std. err. | Coeff. | Std. err. | Coeff. | Std. err. |
| Constant | -7.046 | 0.190 | -10.181 | 0.484 | -6.224 | 0.142 | 3.652 | 0.074 |
| Months (4-6) | -0.101 | 0.030 | 0.381 | 0.064 | 0.353 | 0.038 | -0.010 | 0.009 |
| Months (7-9) | -0.330 | 0.046 | 0.356 | 0.075 | 0.224 | 0.051 | -0.022 | 0.014 |
| Months (10-12) | -0.682 | 0.062 | 0.062 | 0.094 | 0.145 | 0.064 | -0.033 | 0.018 |
| Months (13-15) | -0.736 | 0.076 | -0.090 | 0.112 | -0.067 | 0.082 | -0.019 | 0.022 |
| Months (16-18) | -1.006 | 0.093 | -0.373 | 0.139 | -0.125 | 0.096 | -0.041 | 0.028 |
| Months (19-21) | -1.095 | 0.113 | -0.153 | 0.147 | -0.401 | 0.123 | -0.061 | 0.036 |
| Months (22+) | -1.424 | 0.121 | -0.627 | 0.169 | -0.388 | 0.126 | 0.038 | 0.035 |
| First sanction | 0.736 | 0.108 | 1.349 | 0.375 | - | - | -0.054 | 0.027 |
| Second sanction within 12 months | 0.921 | 0.148 | 0.719 | 0.322 | - | - | 0.010 | 0.043 |
| Second sanction after 12 months | 0.765 | 0.415 | 0.929 | 0.610 | - | - | 0.070 | 0.276 |
| District unempl. rate | -0.024 | 0.016 | 0.087 | 0.035 | 0.057 | 0.021 | 0.001 | 0.005 |
| District long-term unempl. rate | -0.050 | 0.026 | -0.202 | 0.059 | -0.149 | 0.036 | -0.013 | 0.008 |
| District vacancy-unempl. ratio | 0.827 | 0.178 | 1.339 | 0.397 | -0.203 | 0.248 | -0.168 | 0.054 |
| 20 years | 0.262 | 0.046 | 0.252 | 0.085 | -0.231 | 0.054 | 0.042 | 0.014 |
| 21 to 22 years | 0.394 | 0.039 | 0.173 | 0.075 | -0.301 | 0.049 | 0.080 | 0.012 |
| 23 to 24 years | 0.416 | 0.040 | -0.010 | 0.080 | -0.337 | 0.051 | 0.121 | 0.013 |
| No occ. degree, no schooling degree | -0.988 | 0.042 | -0.067 | 0.099 | 0.614 | 0.060 | -0.204 | 0.012 |
| No occ. degree, low schooling degree | -0.830 | 0.032 | -0.141 | 0.083 | 0.526 | 0.051 | -0.165 | 0.009 |
| No occ. degree, high schooling degree | -0.848 | 0.078 | 0.902 | 0.145 | -0.274 | 0.124 | -0.145 | 0.024 |
| Voc. training, high schooling degree | 0.068 | 0.074 | 1.185 | 0.161 | -0.587 | 0.186 | 0.089 | 0.021 |
| Education missing | -0.947 | 0.046 | 0.402 | 0.099 | 0.092 | 0.066 | -0.134 | 0.014 |
| Schleswig-Holstein | -0.150 | 0.054 | -0.077 | 0.112 | -0.055 | 0.069 | -0.027 | 0.017 |
| Hamburg | 0.072 | 0.063 | -0.589 | 0.153 | -0.036 | 0.089 | -0.035 | 0.019 |
| Lower Saxony | -0.125 | 0.039 | -0.217 | 0.087 | 0.036 | 0.052 | 0.007 | 0.011 |
| Bremen | 0.069 | 0.076 | 0.086 | 0.156 | -0.110 | 0.106 | 0.000 | 0.025 |
| Hesse | -0.178 | 0.053 | -0.014 | 0.110 | -0.156 | 0.073 | 0.002 | 0.016 |
| Rhineland-Palatinate | -0.158 | 0.058 | -0.029 | 0.125 | 0.079 | 0.077 | 0.002 | 0.017 |
| Baden-Württemberg | -0.067 | 0.053 | -0.115 | 0.117 | -0.149 | 0.075 | -0.018 | 0.016 |
| Bavaria | 0.190 | 0.047 | 0.122 | 0.103 | 0.143 | 0.065 | 0.026 | 0.014 |
| Saarland | 0.207 | 0.075 | -0.452 | 0.200 | 0.060 | 0.109 | -0.073 | 0.023 |
| Turkish nationality | 0.333 | 0.060 | -0.228 | 0.151 | -0.188 | 0.086 | 0.035 | 0.017 |
| Other nationality | 0.455 | 0.039 | -0.069 | 0.093 | -0.426 | 0.065 | 0.004 | 0.011 |
| Quarter 2 | -0.189 | 0.032 | -0.056 | 0.074 | 0.128 | 0.044 | -0.027 | 0.009 |
| Quarter 3 | -0.331 | 0.034 | 0.170 | 0.072 | -0.005 | 0.046 | -0.044 | 0.010 |
| Quarter 4 | -0.431 | 0.037 | -0.086 | 0.081 | 0.003 | 0.050 | -0.047 | 0.011 |
| Year 2008 | -0.284 | 0.037 | -0.107 | 0.086 | -0.059 | 0.053 | -0.032 | 0.011 |
| First sanction in month 1-3 | - | - | - | - | -0.436 | 0.114 | - | - |
| First sanction in month 4-6 | - | - | - | - | -0.424 | 0.124 | - | - |
| First sanction in month 7-9 | - | - | - | - | -0.239 | 0.139 | - | - |
| First sanction in month 10-12 | - | - | - | - | -0.076 | 0.166 | - | - |
| First sanction in month 13-15 | - | - | - | - | -0.150 | 0.205 | - | - |
| First sanction in month 16-18 | - | - | - | - | 0.233 | 0.235 | - | - |
| First sanction in month 19-21 | - | - | - | - | -0.917 | 0.525 | - | - |
| First sanction in month 22+ | - | - | - | - | 0.143 | 0.372 | - | |
| Log($\sigma_w$) | - | - | - | - | - | - | -1.065 | 0.016 |
| V$_1$ | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| V$_2$ | 1.267 | 0.246 | 2.431 | 0.518 | -2.517 | 0.508 | -0.014 | 0.109 |
| V$_3$ | 2.239 | 0.168 | 0 | 0 | -1.196 | 0.232 | 0.100 | 0.072 |

Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. n=31,890. LogLikelihood= -142,748.44. The estimated probabilities of the discrete distribution of the unobserved heterogeneity are $\pi_1 = 0.225$, $\pi_2 = 0.349$ and $\pi_3 = 0.425$. Correlations of unobservables: -0.42 (job-sanction), -0.05 (OLF-sanction), 0.44 (job-OLF), 0.92 (job-wage), 0.44 (OLF-wage), -0.05 (wage-sanction).

Table B6: Full model for male jobseekers living in **multi-person** households

| | Exit to work | | Exit out of LF | | Sanction | | Log(Wage) | |
|---|---|---|---|---|---|---|---|---|
| | Coeff. | Std. err. | Coeff. | Std. err. | Coeff. | Std. err. | Coeff. | Std. err. |
| Constant | -7.551 | 0.239 | -8.662 | 0.271 | -5.909 | 0.161 | 3.541 | 0.088 |
| Months (4-6) | -0.008 | 0.031 | 0.375 | 0.064 | 0.147 | 0.039 | 0.025 | 0.010 |
| Months (7-9) | -0.147 | 0.046 | 0.490 | 0.073 | 0.089 | 0.052 | 0.024 | 0.014 |
| Months (10-12) | -0.421 | 0.059 | 0.151 | 0.097 | -0.020 | 0.066 | 0.023 | 0.018 |
| Months (13-15) | -0.548 | 0.072 | 0.191 | 0.110 | -0.176 | 0.081 | 0.027 | 0.021 |
| Months (16-18) | -0.664 | 0.084 | -0.069 | 0.136 | -0.182 | 0.095 | 0.032 | 0.026 |
| Months (19-21) | -0.892 | 0.104 | 0.113 | 0.150 | -0.403 | 0.119 | 0.065 | 0.030 |
| Months (22+) | -0.953 | 0.111 | -0.110 | 0.160 | -0.500 | 0.127 | 0.098 | 0.032 |
| First sanction | 0.529 | 0.108 | 0.064 | 0.094 | - | - | -0.033 | 0.017 |
| Second sanction within 12 months | 0.807 | 0.150 | 0.294 | 0.190 | - | - | 0.052 | 0.045 |
| Second sanction after 12 months | 0.203 | 0.423 | 0.387 | 0.515 | - | - | -0.059 | 0.178 |
| District unempl. rate | -0.023 | 0.016 | 0.064 | 0.033 | 0.043 | 0.020 | -0.001 | 0.004 |
| District long-term unempl. rate | -0.057 | 0.026 | -0.175 | 0.055 | -0.149 | 0.035 | -0.010 | 0.007 |
| District vacancy-unempl. ratio | 1.668 | 0.167 | 1.411 | 0.338 | 0.110 | 0.246 | -0.096 | 0.046 |
| 20 years | 0.223 | 0.039 | 0.242 | 0.067 | -0.289 | 0.048 | 0.040 | 0.010 |
| 21 to 22 years | 0.321 | 0.033 | 0.113 | 0.063 | -0.237 | 0.041 | 0.068 | 0.009 |
| 23 to 24 years | 0.305 | 0.036 | 0.132 | 0.073 | -0.319 | 0.047 | 0.112 | 0.009 |
| No occ. degree, no schooling degree | -0.962 | 0.041 | -0.213 | 0.104 | 0.480 | 0.063 | -0.214 | 0.011 |
| No occ. degree, low schooling degree | -0.821 | 0.034 | -0.198 | 0.092 | 0.376 | 0.057 | -0.182 | 0.008 |
| No occ. degree, high schooling degree | -0.994 | 0.084 | 0.744 | 0.130 | -0.497 | 0.133 | -0.160 | 0.022 |
| Voc. training, high schooling degree | -0.187 | 0.112 | 0.878 | 0.202 | -0.752 | 0.274 | 0.047 | 0.026 |
| Education missing | -1.118 | 0.044 | 0.207 | 0.099 | -0.031 | 0.067 | -0.188 | 0.011 |
| Schleswig-Holstein | -0.192 | 0.057 | -0.152 | 0.114 | -0.375 | 0.075 | -0.011 | 0.014 |
| Hamburg | -0.198 | 0.068 | -0.545 | 0.146 | -0.139 | 0.089 | -0.077 | 0.018 |
| Lower Saxony | -0.100 | 0.039 | -0.107 | 0.082 | -0.044 | 0.050 | 0.008 | 0.010 |
| Bremen | 0.033 | 0.078 | 0.153 | 0.152 | -0.144 | 0.108 | 0.028 | 0.019 |
| Hesse | -0.098 | 0.051 | -0.164 | 0.106 | -0.036 | 0.065 | -0.021 | 0.012 |
| Rhineland-Palatinate | -0.063 | 0.050 | 0.135 | 0.100 | -0.017 | 0.066 | 0.001 | 0.013 |
| Baden-Württemberg | 0.017 | 0.051 | 0.068 | 0.102 | -0.156 | 0.070 | 0.030 | 0.012 |
| Bavaria | 0.163 | 0.046 | 0.394 | 0.093 | 0.083 | 0.063 | 0.028 | 0.012 |
| Saarland | 0.149 | 0.085 | -0.448 | 0.231 | 0.170 | 0.109 | -0.001 | 0.023 |
| Married | 0.535 | 0.051 | -0.450 | 0.134 | -0.676 | 0.082 | 0.113 | 0.012 |
| Living with parter, not married | 0.324 | 0.043 | -0.358 | 0.113 | -0.170 | 0.067 | 0.038 | 0.011 |
| Turkish nationality | 0.279 | 0.038 | -0.203 | 0.083 | 0.117 | 0.048 | 0.019 | 0.009 |
| Other nationality | 0.132 | 0.033 | -0.093 | 0.069 | -0.062 | 0.045 | -0.021 | 0.008 |
| No. of children < 3 years | 0.054 | 0.028 | -0.361 | 0.092 | 0.186 | 0.042 | 0.034 | 0.007 |
| No. of children between 3-5 years | 0.000 | 0.043 | -0.108 | 0.156 | 0.108 | 0.061 | 0.014 | 0.011 |
| No. of children between 6-17 years | -0.139 | 0.054 | -0.168 | 0.179 | 0.083 | 0.072 | -0.001 | 0.014 |
| Partner below 25 years old | 0.000 | 0.048 | -0.087 | 0.150 | -0.060 | 0.079 | -0.024 | 0.011 |
| Partner foreigner | -0.144 | 0.049 | -0.171 | 0.139 | -0.073 | 0.077 | 0.005 | 0.011 |
| Partner no occ. degree, no schooling degree | -0.137 | 0.062 | -0.007 | 0.171 | 0.303 | 0.090 | -0.066 | 0.015 |
| Partner no occ. degree, schooling degree | 0.066 | 0.043 | -0.060 | 0.125 | 0.121 | 0.070 | -0.030 | 0.010 |
| Partner educ. missing | -0.194 | 0.050 | 0.065 | 0.129 | 0.184 | 0.077 | -0.040 | 0.012 |
| Quarter 2 | -0.034 | 0.032 | 0.002 | 0.067 | 0.095 | 0.044 | -0.038 | 0.008 |
| Quarter 3 | -0.246 | 0.033 | -0.048 | 0.066 | -0.056 | 0.044 | -0.062 | 0.008 |
| Quarter 4 | -0.306 | 0.037 | -0.214 | 0.075 | 0.120 | 0.047 | -0.059 | 0.009 |
| Year 2008 | -0.115 | 0.037 | -0.094 | 0.078 | 0.061 | 0.051 | -0.032 | 0.009 |
| First sanction in month 1-3 | - | - | - | | -0.206 | 0.118 | - | |
| First sanction in month 4-6 | - | - | - | | 0.024 | 0.122 | - | |
| First sanction in month 7-9 | - | - | - | | 0.268 | 0.135 | - | |
| First sanction in month 10-12 | - | - | - | | 0.202 | 0.156 | - | |
| First sanction in month 13-15 | - | - | - | | 0.376 | 0.185 | - | |
| First sanction in month 16-18 | - | - | - | | 0.540 | 0.225 | - | |
| First sanction in month 19-21 | - | - | - | | 0.602 | 0.323 | - | |
| First sanction in month 22+ | | | - | | 0.302 | 0.390 | - | |
| Log($\sigma_w$) | - | - | - | - | - | - | -1.109 | 0.012 |
| V$_1$ | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| V$_2$ | 2.391 | 0.227 | 0.516 | 0.392 | -1.442 | 0.168 | 0.194 | 0.084 |
| V$_3$ | 4.058 | 0.238 | 0 | 0 | -1.775 | 0.406 | 0.316 | 0.094 |
| V$_4$ | 0.754 | 0.349 | 0 | 0 | -1.980 | 0.288 | -0.090 | 0.105 |

Estimations are based on inflow samples of individuals who are registered as jobseekers at the beginning of the spell. n=38,492. LogLikelihood=-174,581.35. The estimated probabilities of the discrete distribution of the unobserved heterogeneity are $\pi_1 = 0.134$, $\pi_2 = 0.513$, $\pi_3 = 0.117$ and $\pi_4 = 0.237$. Correlations of unobservables: -0.38 (job-sanction), -0.87 (OLF-sanction), -0.11 (job-OLF), 0.78 (job-wage), -0.71 (OLF-wage), 0.27 (wage-sanction).