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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 objective in many European countries. In this context, monitoring and sanctions are commonly used policy tools. We analyze the impact of strict sanctions for young welfare recipients in Germany. The German benefit system is characterized by harsh sanctions for this group, effectively cancelling benefits for three months after detection of non-compliance with job search requirements. We analyze the impacts of these sanctions on job search outcomes and on dropping out of the labor force, using administrative data on a large inflow sample. We estimate multivariate duration models taking selection on unobservables into account. Our results indicate an increased job entry rate at the expense of an increased withdrawal from the labor force and lower entry wages. Combining quantitative with qualitative evidence reveals that the latter side-effects of sanctions can have dramatic consequences for the quality of life of the youths involved.

JEL Classification: J64, J65, C41

Keywords: Social Assistance, unemployment, sanctions, post unemployment outcomes, youth unemployment

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

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December 20, 2019

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 job seekers 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. Combining quantitative with qualitative evidence reveals that these perhaps unintended side-effects of sanctions can have dramatic consequences for the quality of life of the youths involved.

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1 Introduction

In Europe, the lack of employment among the young is regarded to be one of the most pressing labor market problems (see for example OECD, 2016 and 2017, and European Commission, 2018). In many countries, young welfare recipients face very low reemployment rates. For policy reasons, it is important to know how strongly they react to financial incentives. In this context, punitive monetary sanctions provide an interesting tool. In this paper we examine effects of such sanctions for young welfare recipients in a country where the regime is relatively harsh: Germany.

The German welfare system is characterized by severe sanctions for young welfare recipients. If they do not comply with their obligations, such as the requirement to search for a job, they risk not receiving any welfare payments for up to three months. This does not include the payment for rent and heating as basic means of life support. However, if they are sanctioned a second time within a specific time period, those payments are cut as well. We argue that the German institutions provide a useful setting to study effects and side-effects of welfare benefits reductions that are harsh and prolonged, among disadvantaged youth with rather bleak labor market prospects.

Economic job search models that incorporate sanctions 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. This effect on the transition rate to work in line with the findings of a large number of empirical studies.[1] However, once the welfare recipients have been sanctioned, it becomes more likely that the expected present value of being on welfare drops below the expected present value of being out of the labor market, both because of the considerable reduction of their welfare benefit and the prospect of increased monitoring

[1] In the context of unemployment insurance (UI) systems, see Abbring, van den Berg and van Ours (2005) and a range of subsequent studies such as Lalive, van Ours and Zweimüller (2005), Svarer (2011), Røed and Weslie (2012), van den Berg and Vikström (2014) and Arni, Lalive and van Ours (2013). Furthermore, van den Berg, van der Klaauw and van Ours (2004) and van der Klaauw and van Ours (2014) provide evidence for positive sanction effects in general on the transition rate to work among unemployed welfare recipients.

and higher punishments while on welfare in the future. 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.[2] This suggests that sanctions may 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.

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.

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

[2]Busk (2016) analyzes the impact of sanctions based on a sample of unemployed job seekers aged above 25 in Finland and finds that they increase transition rates out of the labor force. Arni *et al.* (2013) also consider exits from UI to unregistered non-employment and find a positive impact of sanctions.

of labor force for welfare benefit recipients. We consider the quality of the job matches and analyze whether sanctions push the young welfare recipients into lower paid contributory jobs.[3] 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 job seekers 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

[3]For estimates of sanction effects on employment earnings in the context of UI systems, see e.g. van den Berg and Vikström (2014), Arni *et al.* (2013) and van den Berg, Hofmann and Uhlendorff (2019). In general, jobs found after the imposition of a UI sanction go along with lower wages.

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 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. A more detailed description of the benefit system is provided in the Online Appendix A. 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 Euro per 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 or above. 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 Euro in July 2009. A second important component of the UB II is the benefit that covers costs for accommodation and heating. The UB II is a household benefit. Nevertheless, job centers compute a benefit level for each individual, which is the amount targeted by possible benefit sanctions. The benefit that covers costs for accommodation and heating is divided by the number of household members and then added to the individually defined basic cash benefit. Details on how the individual benefit level is reduced if a household member obtains earnings is described in the Online Appendix A.

If welfare benefit recipients do not comply with specific obligations they can be sanctioned for a duration of three months. These sanctions only affect the individual benefit of the person who did not comply with the rules. 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-time infringement against other obligations than missing appointments results in their basic cash benefit being withheld. 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.[4,5]

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 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 can take to provide a response is not specified in the law. If the welfare recipient does not provide a satisfactory justification for the non-compliance, the next step follows. The job center sends a notification to the welfare recipient about the date when the sanction will be imposed. This notification is usually sent in the month prior to the sanction date. The welfare recipient can then formally object or even bring the case to court. However, this does not automatically stop the sanction process. Reasons for an objection can refer again to reasons for non-compliance or to formal mistakes made by the job center. Information on the date of registering a sanction and the actual start of the sanction in our data suggest that the time interval for such a response before the start of the sanction is usually one to three weeks. Formally, one can issue a complaint up to one month after receiving the notification about the sanction start, thus even after the sanction is already in place. If a welfare recipient does not provide a good reason for the non-compliance, the sanction comes into force the first day of the calendar month following

[4]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.

[5]In this paper, we focus on the impact of strict sanctions. van den Berg, Uhlendorff and Wolff (2014) analyze 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.

the formal notification about the sanction date.

The benefit rules envisage that a welfare recipient is sanctioned after non-compliance of the obligations. For various reasons, though, a sanction may 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 lawsuit. 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.

The institutional features and imperfections in the process of implementing sanctions are important for our methodological approach (see Section 4). The evaluation of sanction effects requires a "no anticipation" assumption capturing that individuals do not know in advance whether a specific 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). After all, if individuals leave welfare in response to a future sanction that they know will be imposed then that sanction as a treatment will not be observed. Such a sequence of events cannot be ruled out, but as we argue in subsequent sections, our results are robust to this because of data evidence on the short time interval between notification and imposition and because of evidence from sensitivity analyses.[6]

[6]It is important to distinguish anticipation from knowledge of the monitoring scheme. The latter leads to ex ante responses. These do not depend on the exact moment of a specific future sanction. As we shall see in Section 4, ex ante responses may implicitly affect values of certain model parameters but this does not invalidate inference on sanction effects. Ex ante 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

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. One potential reason for this higher sanction rate might be that 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. However, there might be other reasons contributing to this difference in sanction rates, including differences in "risky" behaviour due to intrinsic characteristics or different family responsibilities.

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 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

Vikström (2014) for a more detailed exposition.

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 job seekers, 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, 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 modelled 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 job seeker, 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. Unsubsidized employment excludes any job with subsidies. Once an individual enters a subsidized job, we right-censor the corresponding unemployment spell. Similarly, employment spells with a monthly wage below 500 Euro are not considered, and we right-censor the corresponding unemployment spell. We choose this threshold with the aim to exclude transitions to jobs with a rather small number of working hours per week and an income that is insufficient to leave welfare. This affects around 13 percent of the transitions to employment for individuals in single households and around 10 percent in multi-person households. About 6 percent of young men in single households and 5 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 Euro 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. For single households, we observe 4,583 individuals with at least one strong sanction and 930 individuals with two strong sanctions. Among those with two sanctions, 850 experience the second sanction within 12 months after the first sanction. For multiple-person households, 4,765 men experience at least one strong sanction, while we observe 1,132 men with two sanctions. Of those with two sanctions, 1,031 individuals experience this second sanction within one year after the start of the first sanction.[7] 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. For those with at least one sanction we observe 1,167 individuals in single households and 1,327 in multiple-person households entering employment, while for individuals experiencing two sanctions we only observe 186

[7]In our empirical analysis, we distinguish between skilled and unskilled workers. Among the 4,583 (4,765) sanctioned individuals in single (multiple-) households, 566 (413) are skilled workers. For individuals in multi-person households, we additionally investigate effect heterogeneity with respect to the presence of children in the household. 995 out of 4,765 sanctioned individuals have children living in their household.

exits to employment in single households and 226 in multiple-person households. Due to these rather small numbers of observed wages for individuals with two sanctions, we only consider the effect of having at least one sanction on wages in our empirical analysis.

[Table 2 about here]

To shed light on what happens to welfare recipients who leave the labor force for at least four months, 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 not observed in one of the states available in our data at 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 the data do not provide any information at 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. We have conducted additional sensitivity analysis in which we interact a time-varying September dummy with the treatment. It turns out that the results are very stable (see Section 5.3).

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. 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 a remarkable decline in the subsequent month. During our observation period an application to prolong UB II receipt had to be carried out in intervals of six months. This institutional feature might explain the increase in the transition rate around 180 days. Some people who for the first time would have to apply for the prolongation of their UB II receipt might leave this system because they prefer to rely on the support of their family network or activities in the shadow economy. After six months, the transition rates 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 B.

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 simple extensions of the key identification results to settings 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 rate 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 rate of dropping out of the labor force can be characterized by the 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:

$$\begin{aligned}
\theta_e(t) &= \exp(\sum_{j=2}^{J} I_j(t)\lambda_{je} + x_t'\beta_e + I_s(t > t_{s1})\alpha_{e1} + I_s(0 < t_{s2} - t_{s1} < 365 | t > t_{s2})\alpha_{e2} \\
&\quad + I_s(t_{s2} - t_{s1} \geq 365 | t > t_{s2})\alpha_{e3} + V_e) \\
\theta_o(t) &= \exp(\sum_{j=2}^{J} I_j(t)\lambda_{jo} + x_t'\beta_o + I_s(t > t_{s1})\alpha_{o1} + I_s(0 < t_{s2} - t_{s1} < 365 | t > t_{s2})\alpha_{o2} \\
&\quad + I_s(t_{s2} - t_{s1} \geq 365 | t > t_{s2})\alpha_{o3} + V_o) \\
\theta_s(t) &= \exp(\sum_{j=2}^{J} I_j(t)\lambda_{js} + x_t'\beta_s + I_s(t > t_{s1}) \sum_{j=1}^{J} I_{sj}(t_{s1})\gamma_j + V_s) \qquad (1)
\end{aligned}$$

$I_j(\cdot)$ takes on the value one if t is in the interval j. λ_{je}, λ_{jo} and λ_{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. α_{e1} (α_{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 (α_{e2} and α_{o2}) and of sanctions which are imposed later (α_{e3} and α_{o3}). For a person who has been sanctioned two times, the estimated overall effect after the start of the second sanction (t_2) corresponds to the sum of the two sanction effects. For the hazard rate to work, the overall effect of the two sanctions corresponds to $\alpha_{e1} + \alpha_{e2}$ if the infringement takes place within one year after the first sanction and $\alpha_{e1} + \alpha_{e3}$ otherwise. 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.

The above equations specify that a sanction does not affect transition rates before the

moment of the sanction. This reflects the no-anticipation assumption briefly discussed in Section 2. It is based on the idea that if individuals do not know in advance exactly when a sanction is imposed on them then the current transition rate does not depend on the moment at which that future event takes place. A number of comments are in order. First, notice that individuals do not know in advance if a current violation leads to a future punishment since that would trivially enable them to avoid the punishment. Obviously, this reduces the scope for anticipation. We know from Section 2 that anticipation is nevertheless theoretically possible because of notifications just before imposition of the sanction. However, we believe that this plays a minor role in our context. The median duration between the registration of the sanction, which corresponds to the date of notification, and the start of the sanction is 17 days for singles and 18 days for individuals living in multiple-person households. We argue that this period is too short for the vast majority of individuals to leave unemployment for a job after the notification and before the start of the sanction. Sensitivity analyses below confirm this. If the no-anticipation assumption is nonetheless violated in our setting, this would probably lead to an underestimation of the true effect. Those notified people who leave unemployment before the sanction starts are part of the "control group". Therefore, we attribute their positive outcomes to the control group, which should underestimate the treatment effects.

As noted in Section 2, it is important to distinguish anticipation of the future moment at which a sanction is realized from knowledge of the monitoring process. The latter shapes the environment of the welfare recipients and thus may affect all parameters in the model. For example, if all individuals know that monitoring becomes stricter after the first three months of welfare and sanctions may become more likely after the first three months then, because of that, their transition rates out of welfare may increase after three months. This is an effect of the system and not an effect of an individual sanction. It does not depend on the exact moment of a future sanction and may even occur if the individual ultimately does not get any sanction at all. To put this differently, the sanction effects that we estimate are relative

to the behavior of the same individual in the same world with the same institutions but without an imposed sanction. The no-anticipation assumption therefore does not rule out that forward-looking agents' behavior depends on knowledge of the probability distribution of the moment of getting a sanction.

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.[8] 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:

$$\ln w \quad = \quad x_t'\beta_w + I_s(t > t_{s1})\alpha_{w1} + \sum_{j=2}^{J} I_j(t_e)\eta_w + V_w + \varepsilon_w \tag{2}$$

The sanction effect is given by α_{w1}. As described in Section 3, the number of observations for whom we observe a wage and who have experienced two sanctions is relatively small. Because of the corresponding lack of power, we do not estimate the impact a second sanction on wages. V_w is the unobserved heterogeneity, and ε_w is assumed to be normally distributed with mean zero and unknown variance σ_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

[8] 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).

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 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 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 modelling selection based on unobservables by way of 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:

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

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 δ_e is one if an exit into employment is observed and zero otherwise. δ_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 δ_{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 δ_{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 β_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.[9]

It turns out that it is difficult to find support for more than two values for the unobserved heterogeneity term in 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.[10] 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

[9] Recall that we focus on strong sanctions and ignore mild sanctions.

[10] The effects of imposed sanctions for these models are reported in the Tables C3 and C4 in the Online Appendix C. They are very similar to the ones of the full model including the wage equation.

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 C, Tables C5 and C6. The results for singles imply a considerable positive effect of the first sanction on the transition rate to employment. It is raised by about 106 percent (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 coefficient of the second sanction captures the difference between the overall effect of the first sanction and the overall (cumulative) effect of both sanctions after the second sanction. 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.[11]

[11]The results of a model without unobserved heterogeneity are reported in Table C1 for single households and in Table C2 for individuals living in multi-person households. While we also find significant positive effects of the first sanctions on exit to work and exit out the labor market and significantly negative effects on wages, the estimated effects on the hazard rates are clearly larger in a model with unobserved heterogeneity. Moreover, in the model with unobserved heterogeneity, some of the coefficients of the second sanctions are statistically significant. The difference in these estimated effects can be explained by a dynamic selection with respect to unobserved heterogeneity over time and by a negative correlation of unobservables affecting the exit to work / the exit out of the labor market and the probability of being sanctioned. This (dynamic)

[**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 effect on daily earnings in the post-unemployment job are compatible with such an interpretation. A sanction lowers them by more than five percent and the effect is well determined.

The first sanction leads to a 205 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 a year amplifies the effect of the first and raises the transition rate out of the labor market by about 99 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

selection is not take into account in a model without unobserved heterogeneity, which leads to biased results.

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 67 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 a sanction at about -3.7 percent is in absolute terms smaller than for the group living in single households. 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. Moreover, conducting Wald tests suggests that we cannot reject the null hypothesis of equal coefficients for any of the sanction effects on exit to work and on wages.

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. While the differences in the estimated effects are not significantly different from zero for the second sanctions, the impact of the first sanction is significantly lower for persons living in multi-person households compared to

the effect 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]

Our results suggest a negative impact of sanctions on entry wages. To shed some light on the question whether these negative effects are long lasting, we have investigated changes of wages over time. Our data allow us to observe changes in wages within the same job only over calendar years. Therefore we have regressed the change in post-unemployment wages between two calendar years on a dummy which indicates whether the individuals have been sanctioned during their previous welfare spell. Note that this allows for an individual-specific fixed effect in the wage level, and hence this approach effectively deals with endogeneity issues arising out of the imposition of a sanction and the requirement that a job lasts sufficiently long to observe two consecutive wages. The findings show that previously sanctioned individuals experience a slower wage growth. For non-sanctioned individuals living in a single-household, we observe on average a 9.7 percent increase in wages. For sanctioned individuals, this is reduced by 4.4 percentage points (significant at a 5 percent level). For non-sanctioned individuals in multi-person households, we observe an average wage increase of around 10 percent. This wage increase is reduced for sanctioned individuals by around 3.3 percentage points (significant at a 10 percent level). These results suggest that the negative impact of sanctions on wages does not disappear over time.

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.[12]

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 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 more than 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

[12] 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.

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 observe a small negative 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 interaction effect with having children, which is not statistically significant.

[Table 6 about here]

5.3 Sensitivity Analyses

As discussed in Sections 2 and 4, the empirical approach requires that welfare recipients do not anticipate the exact timing of future sanctions. To anticipate future sanctions, individuals need to have detailed knowledge about the implementation of the rules of sanctioning and, in particular, the behavior of their caseworkers. Recall that this can never be fully predictable since otherwise sanctions would be easy to avoid. Still it seems plausible that any knowledge on the determinants of sanctions is correlated with the time spent on welfare in the past. This suggests that any potential anticipatory effects are smaller among individuals who have spent relatively little time on welfare in the past. Therefore, we conduct a sensitivity analysis based on individuals who have not received any unemployment benefit II within the 12 months before the start of their welfare spell. This sample restriction leads to a reduction in the sample size by more than 50 percent. The results are reported in Tables 7 and 8. While we lose some statistical power, the general pattern of the results is robust. For individuals living in single households, the point estimates for the exits to work are very similar. For the exit out of the labor market the estimate of the first sanction is smaller, but still statistically significant. The impact of the second sanction is smaller and

we lose statistical significance. The estimated effect of the sanction on wages lies within the confidence bands of the main effects, but it is not statistically significant. For individuals living in multiple-person households, we get stronger effects for the exit to work and for the wages. In line with the main specification, we do not find evidence for an impact on exit out of the labor market for multiple-person households.

Individuals who get a sanction are informed about this some days before the sanction period starts. As we have seen in Section 4, the median duration between the notification date and the start of a sanction is relatively short (17 days for individuals in single households and 18 days for individuals in multi-person household) but some individuals experience longer notification periods. For those individuals, it is more likely that the notification itself has an impact on their behavior and their outcomes. We run an additional sensitivity analysis in which we exclude individuals who have notification periods which are longer than 31 days. This leads to an exclusion of 580 individuals living in single household and 621 individuals living in multi-person households. The limitation of this sensitivity analysis is that we can only exclude those individuals for whom we observe a sanction in the data, and not those who leave unemployment before the sanction starts. The results are very robust to the exclusion of individuals with long notification periods, see Tables 9 and 10.

Exits to employment are defined as exits to jobs which go along with a wage of at least 500 Euro. In a sensitivity analysis, we change this threshold to 400 Euro. This leads to an increase in the number of transitions to employment by 482 for single households and by 417 for multi-person households. The results are reported in Tables 11 and 12. While we get a stronger impact on wages for single households, the estimated effect for non-single households is close to zero. All the other treatment effects are very robust. This suggests that the results of sanctions on wages are sensitive to the chosen wage threshold for the sample of individuals living in multiple-person households, and we have to interpret the wage effects for this sample with caution.

In our main specification, an exit out of the labor market is defined by leaving welfare

for at least four months without being observed as employed, as a job seeker, as an active labor market program participant or as a recipient of UI or welfare benefits. To investigate the importance of the choice of this threshold for our results, we estimate our model based on a minimum length of 5 months without being observed in one of these states in the administrative data. The results are reported in Tables 13 and 14. Our main findings are stable.

For the estimation of our model, we use a flexible baseline hazard rate based on 8 time intervals. To investigate the robustness of our result with respect to the degree of flexibility of the baseline hazard rate, we estimate a more restrictive model with 6 time intervals. It turns out that a model with 6 intervals gives very similar results (see Tables 15 and 16). The only exception are the effects on exit out of the labor market for single households. While the point estimates are within the confidence bands of the main specification, the first sanction effect is significant only at the 10 percent level, and the second sanction effect loses significance. For these effects, it seems to be more important to adequately model the duration dependence. Overall, our results are not sensitive with respect to the flexibility of the baseline hazard rate.

Part of the individuals who are dropping out of the labor force for at least four months might enter vocational training in classrooms starting in September. To investigate whether these transitions drive our results, we estimate a model with an interaction effect between a time-varying dummy for being in September and the treatment status. The results are reported in Tables 17 and 18. These interaction effects are usually not statistically significant (with the exception of a significant effect at the 10 percent level for the duration until finding a job for non-single households). More important, the main effects are very stable.

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 job seeker 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 job seekers focuses 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. Some of these considerations were also reflected in a court decision of the German Federal Constitutional Court in November 2019 on the benefit sanctions under the Social Code II (German Federal Constitutional Court, 2019). Until a new law is passed that takes into account the court decision, strong benefit

sanctions will be limited to 30 percent of the full basic cash benefit for all welfare recipients.

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Tables and Figures

Figure 1
Empirical transition rate into the first strong punitive sanction

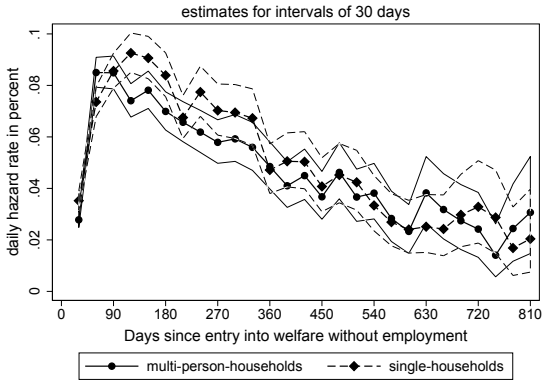

Figure 2
Empirical transition rates into unsubsidized contributory employment and out of the labor force

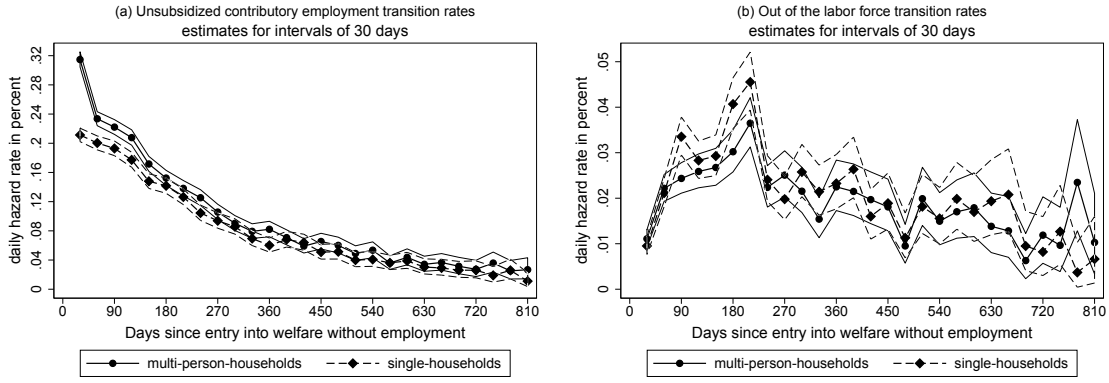

Table 1: Share of exit into strong sanctions and into different labor force states and median post-unemployment real daily wage (in Euro)

	(1)	(2)	(3)	(4)
	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

Column (1) reports the share of spells experiencing different transitions and the average wage observed in the first employment spell for all individuals. Columns (2) contains the corresponding shares and wages for individuals who are not sanctioned during their spell, column (3) for those who experience at least one strong sanction, and column (4) for those who experience at least two strong sanctions. The upper panel reports these statistics for the single-person households, the lower panel reports the corresponding statistics for individuals living in multi-person households. Contributory jobs exclude vocational training. 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)

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

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

Table 3: Model for male job seekers living in single households

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.7247***	1.1139***	-0.0552**
	[0.5106 ; 0.9387]	[0.4243 ; 1.8036]	[-0.1041 ; -0.0063]
Second sanction within 12 months	0.9407***	0.6872**	-
	[0.6469 ; 1.2346]	[0.0570 1.3174]	-
Second sanction after 12 months	0.7763*	0.8896	-
	[-0.0397 ; 1.5922]	[-0.3134 2.0925]	-

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -142,715.67. We control for observed characteristics and duration dependence. For the full set of coefficients see Table C5.

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

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.5127***	0.0589	-0.0375**
	[0.2987 0.7267]	[-0.1228 0.2407]	[-0.0703 -0.0048]
Second sanction within 12 months	0.8065***	0.2958	-
	[0.5078 1.1052]	[-0.0746 0.6662]	
Second sanction after 12 months	0.2027	0.3869	-
	[-0.6250 1.0304]	[-0.6224 1.3963]	

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood=-174,545.64. We control for observed characteristics and duration dependence. For the full set of coefficients see Table C6.

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

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.6872***	1.0559***	-0.0519**
	[0.4754 0.8990]	[0.3651 1.7466]	[-0.0999 -0.0038]
First sanction x skilled worker	0.1692	0.0421	-0.0339
	[-0.0541 0.3925]	[-0.4167 0.4930]	[-0.0301 0.0026]
Second sanction within 12 months	1.0280***	0.6443*	-
	[0.7173 1.3388]	[-0.0100 1.2988]	
Second sanction after 12 months	0.8503**	0.8460	-
	[0.0227 1.6778]	[-0.3490 2.0409]	

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers 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=-142,724.85. We control for observed characteristics and duration dependence.

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

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.5253***	0.0490	-0.0461***
	[0.2964 0.7541]	[-0.1399 0.2379]	[-0.0795 -0.0127]
First sanction x skilled worker	0.0467	0.6002***	-0.0635*
	[-0.1947 0.2882]	[0.1643 1.0361]	[-0.1272 0.0002]
First sanction x children	-0.2221**	-0.3692	0.0294
	[-0.3935 -0.0508]	[-0.8879 0.1496]	[-0.0255 0.0027]
Second sanction within 12 months	0.7926***	0.3638*	-
	[0.4914 1.0940]	[-0.0292 0.7567]	
Second sanction after 12 months	0.1915	0.4713	-
	[-0.6264 1.0094]	[-0.5438 1.4864]	

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers 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=-174,544.07. We control for observed characteristics and duration dependence.

Table 7: Model for male job seekers living in single households, sample of individuals without any welfare receipt in the previous 12 months

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.8376***	0.5451***	-0.0372
	[0.4960 1.1792]	[0.1865 0.9038]	[-0.0944 0.0200]
Second sanction within 12 months	0.9153***	0.3135	-
	[0.5085 1.3222]	[-0.2467 0.8736]	
Second sanction after 12 months	1.1734*	0.1189	-
	[-0.0525 2.3993]	[-2.1579 2.3958]	

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=14,164. LogLikelihood= -59,208.10. We control for observed characteristics and duration dependence. The sample is restricted to individuals who have not been received any welfare payments during the 12 months before the start of the new welfare spell.

Table 8: Model for male job seekers living in multi-person households, sample of individuals without any welfare receipt in the previous 12 months

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.7279***	-0.1961	-0.0949**
	[0.3824 1.0734]	[-0.6441 0.2519]	[-0.1798 -0.0099]
Second sanction within 12 months	1.6962***	-0.0426	-
	[0.9315 2.4609]	[-0.6454 0.5601]	
Second sanction after 12 months	0.2051	-	-
	[-2.4831 2.8932]		

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=12,365. LogLikelihood=-52,292.38. We control for observed characteristics and duration dependence. The sample is restricted to individuals who have not been received any welfare payments during the 12 months before the start of the new welfare spell.

Table 9: Model for male job seekers living in single households, excluding individuals with long notification periods.

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.6194***	1.2233***	-0.0530*
	[0.3894 0.8495]	[0.3552 2.0914]	[-0.1109 0.0049]
Second sanction within 12 months	0.8987***	0.7470**	-
	[0.5734 1.2239]	[0.0844 1.4096]	
Second sanction after 12 months	0.7040	0.6222	-
	[-0.1891 1.5971]	[-0.7312 1.9756]	

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,310. LogLikelihood= -135,825.73. We control for observed characteristics and duration dependence. In this specification we exclude sanctioned individuals who have been informed about the sanction more than 31 days before the actual start of the sanction.

Table 10: Model for male job seekers living in multi-person households, excluding individuals with long notification periods.

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.4336***	-0.0335	-0.0493***
	[0.2085 0.6587]	[-0.2179 0.1510]	[-0.0826 -0.0160]
Second sanction within 12 months	0.8681***	0.3966**	-
	[0.5466 1.1895]	[0.0017 0.7915]	
Second sanction after 12 months	-0.2814	0.2576	-
	[-1.3697 0.8068]	[-0.8969 1.4120]	

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=37,871. LogLikelihood=-180,294.87. We control for observed characteristics and duration dependence. In this specification we exclude sanctioned individuals who have been informed about the sanction more than 31 days before the actual start of the sanction.

Table 11: Model for male job seekers living in single households, alternative definition of employment spells

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.8197***	0.9755***	-0.0721***
	[0.6433 0.9961]	[0.4199 1.5311]	[-0.1163 -0.0279]
Second sanction within 12 months	1.0340***	0.5239**	-
	[0.7532 1.3147]	[0.0363 1.0116]	-
Second sanction after 12 months	1.0975***	0.6981	-
	[0.3331 1.8618]	[-0.4293 1.8254]	-

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -147,038.06. We control for observed characteristics and duration dependence. In this specification we define a transition into work as taking up a job which goes along with a monthly wage of at least 400 Euro.

Table 12: Model for male job seekers living in multi-person households, alternative definition of employment spells

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.5869***	0.0680	-0.0006
	[0.4465 0.7272]	[-0.1384 0.2745]	[-0.0412 0.0401]
Second sanction within 12 months	0.7572***	0.2428	-
	[0.5043 1.0100]	[-0.1000 0.5856]	
Second sanction after 12 months	0.2578	0.3510	-
	[-0.5015 1.0171]	[-0.6552 1.3572]	

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood=-180,294.87. We control for observed characteristics and duration dependence. In this specification we define a transition into work as taking up a job which goes along with a monthly wage of at least 400 Euro.

Table 13: Model for male job seekers living in single households, alternative definition for the exit out of labor markets

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.7337***	1.2271***	-0.0544**
	[0.5196 0.9477]	[0.4321 2.0221]	[-0.1046 -0.0042]
Second sanction within 12 months	0.9290***	0.7360**	-
	[0.6377 1.2203]	[0.0519 1.4200]	-
Second sanction after 12 months	0.7653*	1.0668*	-
	[-0.0499 1.5805]	[-0.1582 2.2917]	-

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -141,205.70. We control for observed characteristics and duration dependence. In this specification we define an exit out of labor market as not being observed in our administrative data for at least 5 months.

Table 14: Model for male job seekers living in multi-person households, alternative definition for the exit out of labor markets

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.5054***	0.0242	-0.0382 **
	[0.2926 0.7182]	[-0.1683 0.2166]	[-0.0709 -0.0056]
Second sanction within 12 months	0.7850***	0.2795	-
	[0.5071 1.1037]	[-0.0980 0.6933]	
Second sanction after 12 months	0.2046	0.5149	-
	[-0.6268 1.0295]	[-0.5115 1.5143]	

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood=-173,118.05. We control for observed characteristics and duration dependence. In this specification we define an exit out of labor market as not being observed in our administrative data for at least 5 months.

Table 15: Model for male job seekers living in single households, alternative specification of the duration dependence

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.6920***	0.6239*	-0.0524**
	[0.4766 0.9074]	[-0.0061 1.2539]	[-0.0962 -0.0085]
Second sanction within 12 months	0.9881***	0.4127	-
	[0.6944 1.2817]	[-0.1268 0.9521]	-
Second sanction after 12 months	0.7885*	0.6077	-
	[-0.0285 1.6056]	[-0.5566 1.7720]	-

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -142,725.09. We control for observed characteristics and duration dependence. In this specification we use 6 instead of 8 intervals for capturing the duration dependence (1-3 months, 4-6 months, 7-9 months, 10-15 months, 16-21 months, 22 and more months).

Table 16: Model for male job seekers living in multi-person households, alternative specification of the duration dependence

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.5054***	0.0242	-0.0382**
	[0.2926 0.7182]	[-0.1683 0.2166]	[-0.0709 -0.0056]
Second sanction within 12 months	0.8054***	0.2976	-
	[0.5071 1.1037]	[-0.0980 0.6933]	
Second sanction after 12 months	0.2013	0.5014	-
	[-0.6268 1.0295]	[-0.5115 1.5143]	

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood=-174548.81. We control for observed characteristics and duration dependence. In this specification we use 6 instead of 8 intervals for capturing the duration dependence (1-3 months, 4-6 months, 7-9 months, 10-15 months, 16-21 months, 22 and more months).

Table 17: Model for male job seekers living in single households, allowing for different treatment effects in September

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.7163***	1.0546***	-0.0488*
	0.5004 0.9323	0.3742 1.7352	-0.0982 0.0007
Second sanction within 12 months	0.9533***	0.6471**	-
	0.6550 1.2517	0.0223 1.2719	-
Second sanction after 12 months	0.7897*	0.8373	-
	-0.0278 1.6072	-0.3629 2.0375	-
Sanction x September	0.1099	-0.3722	-0.0597
	[-0.1078 0.3276]	[-0.8193 0.0750]	[-0.1355 0.0162]

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. Log-Likelihood= -142,721.05. We control for observed characteristics and duration dependence. This specification includes interaction effects between the treatment dummy and a time-varying dummy for September.

Table 18: Model for male job seekers living in multi-person households, allowing for different treatment effects in September

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.5266***	0.0592	-0.0416**
	[0.3121 0.7412]	[-0.1274 0.2459]	[-0.0748 -0.0084]
Second sanction within 12 months	0.8047***	0.2952	-
	[0.5064 1.1032]	[-0.0767 0.6672]	
Second sanction after 12 months	0.1931	0.3858	-
	[-0.6362 1.0225]	[-0.6248 1.3965]	
Sanction x September	-0.1923*	-0.0153	0.0536
	[-0.4122 0.0277]	[-0.4644 0.4337]	[-0.0209 0.1282]

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood=-174,532.33. We control for observed characteristics and duration dependence. This specification includes interaction effects between the treatment dummy and a time-varying dummy for September.

Appendix A The welfare benefit system in Germany

In the year 2005, the Social Code II came into force, introducing a new minimum-income benefit in Germany. The rules described here apply to our observation window of 2007 to 2009. For people between 15 years of age and the standard retirement age[13], this law replaced the social assistance and unemployment assistance benefit by the unemployment benefit II (UB II). UB II is available to people who are capable of working for at least three hours a day. Essentially, it is a household benefit for households with at least one household member who is capable of working.[14] Household members (such as under age children) who are not capable of working receive a social benefit, which is also regulated in the Social Code II. The benefit is means-tested: Only people living in households with no other income than UB II or with a household income that does not exceed a legally defined subsistence level can receive UB II. The subsistence level is not fixed but depends on some conditions like household size and composition, which becomes clear when we explain the different benefit components in the next paragraph. To pass the means test a household's assets have to be lower than a threshold in order for the household to be eligible for this benefit. The threshold for a household depends on the number of minor children and of adults as well as the age of each adult and partly on the asset type. Some assets, such as an owner-occupied apartment or house or a household's car, are not relevant for the means test, given that they are of moderate value and adequate for meeting a family's basic needs. UB II is not conditional on being unemployed or on exhausting unemployment insurance (UI) benefits. The household members' income from UI receipt and/or from work may be lower than the subsistence level so that they might still pass the means test for UB II.

The welfare benefit UB II consists of different components. One component, the basic cash benefit, is meant to cover regular expenditures (in particular for food, clothing, personal hygiene, furniture, and cultural life). From January to June 2007, this component amounted to 345 Euro per month for singles, single parents or welfare recipients whose partner was younger than 18 years. The basic cash benefit was 80 percent of this level for additional household members aged 15 years or more. For each of two adult partners it was 90 percent of the level for singles. Household members aged under 15 years received as social benefit 60 percent of the benefit level for singles who are capable of working. In our observation period, the basic cash benefit was raised annually in July and was set to 359 Euro in July 2009. A second important component of UB II covers costs for accommodation and heating. It is determined by job centers under consideration of key factors such as size and composition of

[13]For birth cohorts who could retire in our observation window the retirement age was 65 years.

[14]For people living in households, in which no person is capable of working, means-tested social assistance is available. Social assistance is regulated in Social Code XII.

the household, local rent levels, etc.[15] Other parts of the welfare benefit cover contributions to old-age pension or specific temporary expenditures/special needs (e.g. costs related to pregnancy). Until the start of the year 2011, an additional benefit was available to people who had exhausted their unemployment insurance (UI) benefit within the last two years.

With regard to the means test, it is important to note that each additional Euro earned is not completely offset against UB II. In our observation window the first 100 Euro earned did not alter the benefit. Earnings of more than 100 up to 800 Euro per month reduced the benefit by 0.8 Euro per additional Euro earned. For earnings of more than 800 up to 1,200 Euro (1,500 Euro if children aged younger than 18 years live in the household), it was reduced by 0.9 Euro per additional Euro earned. The benefits are calculated for every person in the household and the household members' shares of the total benefit of the household can differ. If the earnings of one household member increase and lead to a reduction of the total benefit of the household, the reduction is distributed over the household members according to their share of the total benefit.

The Social Code II is intended to guarantee a minimum standard of living. But it also aims at raising the employability of the benefit recipients so that they can take up work to reduce their dependence on the welfare benefit. UB II 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 required 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.

If welfare benefit recipients do not comply with specific obligations, they can be sanctioned for a duration of three months. These sanctions only affect the benefit of the person who did not comply with the rules. Relatively low sanctions apply if welfare recipients miss an appointment with the job center or a necessary medical examination. In this case, the sanction is 10 percent of their basic cash benefit. In our observation period, 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 an additional 10 percent of the full basic cash benefit. In April 2011 this latter rule was abolished and a sanction for missing an appointment is always 10 percent of their 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 younger than 25

[15] As job centers compute an individual benefit level, the household benefit for accommodation and heating divided by the number of household members is assigned to each household member.

years of age the sanctions are particularly severe. A first-time infringement against other obligations than missing appointments results in their basic cash benefit being withheld. 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.[16]

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 takes place and is observed by the job center, the caseworker has to document the infringement. The job center sends a letter to the welfare recipients that contains the details 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 can take to provide a response is not specified in the law. If the welfare recipient does not provide a satisfactory justification for the non-compliance, the next step follows. The job center sends a notification to the welfare recipient about the date when the sanction will be imposed. This notification is usually sent in the month prior to the sanction date. The welfare recipient can then formally object or even bring the case to court. However, this does not automatically stop the sanction process. Reasons for an objection can refer again to reasons for non-compliance or to formal mistakes made by the job center. Information on the date of registering a sanction and the actual start of the sanction in our data suggest that the time interval for such a response before the start of the sanction is usually one to three weeks. Formally, one can issue a complaint up to one month after receiving the notification about the sanction start, thus even after the sanction is already in place. If a welfare recipient does not provide a good reason for the non-compliance, the sanction comes into force the first day of the calendar month following the formal notification about the sanction date.

The benefit rules envisage that a welfare recipient is sanctioned after non-compliance of the obligations. For various reasons, though, a sanction may 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 lawsuit. Next, job center

[16]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.

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 *et al.* (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 *et al.* (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.

Appendix B Detailed descriptive statistics

Tables B1 to B3 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 B4 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 Euro, which corresponds to a monthly wage of 500 Euro. 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 B4 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 Euro 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.

Table B5 displays the labor force status in the month prior to the spell begin. The three most important states are working in a job subject to social security contributions, being

already registered as unemployed or as a job seeker without unemployment benefit receipt or none of the available states in our data.

Table B1: In single-person household sample: Averages of selected characteristics of the welfare recipients[1]

	(1) No strong sanction	(2) At least one strong sanction	(3) 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

Strong sanctions effectively take away the benefits for three months if young welfare recipients do not comply with their job search requirements. Column (1) reports the average observed characteristics for individuals who do not experience any strong sanction during their welfare spell. The corresponding statistics for individuals who have experienced at least one and least two strong sanctions during their welfare spell are reported in columns (2) and (3), respectively. All characteristics are measured at the start of their spell.

Table B2: In multi-person household sample: Averages of selected characteristics of the welfare recipients[1] (part A)

	(1) No strong sanction	(2) At least one strong sanction	(3) 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

Strong sanctions effectively take away the benefits for three months if young welfare recipients do not comply with their job search requirements. Column (1) reports the average observed characteristics for individuals who do not experience any strong sanction during their welfare spell. The corresponding statistics for individuals who have experienced at least one and least two strong sanctions during their welfare spell are reported in columns (2) and (3), respectively. All characteristics are measured at the start of their spell.

Table B3: In multi-person household sample: Averages of selected characteristics of the welfare recipients[1] (part B)

	(1) No strong sanction	(2) At least one strong sanction	(3) 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

Strong sanctions effectively take away the benefits for three months if young welfare recipients do not comply with their job search requirements. Column (1) reports the average observed characteristics for individuals who do not experience any strong sanction during their welfare spell. The corresponding statistics for individuals who have experienced at least one and least two strong sanctions during their welfare spell are reported in columns (2) and (3), respectively. All characteristics are measured at the start of their spell.

Table B4: Distribution of the post-unemployment real daily wage of non-sanctioned and sanctioned welfare recipients

	(1) No strong sanction	(2) At least one strong sanction	(3) 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

Strong sanctions effectively take away the benefits for three months if young welfare recipients do not comply with their job search requirements. Column (1) reports the distribution of wages for individuals who do not experience any strong sanction during their welfare spell. The corresponding statistics for individuals who have experienced at least one and least two strong sanctions during their welfare spell are reported in columns (2) and (3), respectively. Wages are reported in prices of the year 2005.

Table B5: Share of different entry states in month before spell begin

States	In single-person household	In multi-person household
- Contributory job	0.250	0.331
- Vocational training in firm	0.042	0.060
- No status in previous month	0.204	0.132
- Unemployment insurance benefit but no unemployment benefit II receipt	0.073	0.051
- Training program participation	0.049	0.091
- Minor employment with no unemployment benefit II receipt	0.058	0.055
- Registered as unemployed or as job seeker with no unemployment benefit receipt	0.296	0.259
- Other	0.029	0.022

People may at the same time be in more than one of the states displayed in this table. We simplified by defining a hierarchy so that each person was assigned to only one status: E.g., if a person worked for some time in a job subject to social security contributions in the month before the spell start, the person was assigned to"contributory job". If this is not the case and in the same month a person was for some time in vocational training in a firm, then the person was assigned to"vocational training in firm", etc.

Appendix C Additional estimation results intended to be made available online

Models without unobservables

Table C1: Model for male job seekers living in **single** households

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.2592***	0.2222***	-0.0504***
	[0.1932 ; 0.3252]	[0.0841 ; 0.3604]	[-0.0742 ; -0.0267]
Second sanction within 12 months	0.1859**	0.1679	-
	[0.0304 ; 0.3414]	[-0.1250 0.4608]	-
Second sanction after 12 months	0.0438*	0.3017	-
	[-0.6996 ; 0.7872]	[-0.7485 1.3519]	-

Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -142,779.30. We control for observed characteristics and duration dependence.

Table C2: Model for male job seekers living in **multi-person** households

	Exit to work	Exit out of Labor Market	Log(Wage)
	Coefficients and 95% confidence bands in brackets		
First sanction	0.1183***	-0.0029	-0.0498***
	[0.0555 ; 0.1810]	[-0.1566 ; 0.1508]	[-0.0713 ; -0.0284]
Second sanction within 12 months	0.0168	0.1624	-
	[-0.1272 ; 0.1609]	[-0.1320 0.4567]	-
Second sanction after 12 months	-0.3533	0.2877	-
	[-1.0862 ; 0.3795]	[-0.7123 1.2877]	-

Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood= -174,738.97. We control for observed characteristics and duration dependence.

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

Table C3: Model for male job seekers 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
	Coefficients and 95% confidence bands in brackets	
First sanction	0.7515***	1.1202***
	[0.5350 0.9681]	[0.3960 1.8444]
Second sanction within 12 months	0.9695***	0.6416**
	[0.6786 1.2604]	[0.0205 1.2627]
Second sanction after 12 months	0.8015*	0.8431
	[-0.0133 1.6163]	[-0.3523 2.0384]

Joint estimation with unobserved heterogeneity (M=3). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=31,890. LogLikelihood= -138,887.55. We control for observed characteristics and duration dependence.

Table C4: Model for male job seekers 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
	Coefficients and 95% confidence bands in brackets	
First sanction	0.5365***	0.0704
	[0.3105 0.7626]	[-0.1316 0.2725 []
Second sanction within 12 months	0.8672***	0.3167
	[0.5629 1.1715]	[-0.1151 0.7485]
Second sanction after 12 months	0.2404	0.4140
	[-0.5785 1.0593]	[-0.6091 1.4372]

Joint estimation with unobserved heterogeneity (M=4). Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. Standard errors in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. n=38,492. LogLikelihood= -169,370.69. We control for observed characteristics and duration dependence.

Table C5: Full model for male job seekers 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	-7.060	0.195	-9.922	0.549	-6.193	0.146	3.662	0.054
Months (4-6)	-0.094	0.031	0.386	0.064	0.359	0.038	-0.010	0.009
Months (7-9)	-0.326	0.048	0.335	0.075	0.224	0.052	-0.021	0.014
Months (10-12)	-0.665	0.064	0.060	0.092	0.172	0.065	-0.030	0.019
Months (13-15)	-0.720	0.077	-0.103	0.110	-0.039	0.084	-0.017	0.023
Months (16-18)	-0.990	0.094	-0.387	0.136	-0.093	0.098	-0.038	0.028
Months (19-21)	-1.081	0.114	-0.177	0.144	-0.368	0.125	-0.058	0.036
Months (22+)	-1.400	0.122	-0.629	0.165	-0.323	0.129	0.043	0.036
First sanction	0.725	0.109	1.114	0.352	-	-	-0.055	0.024
Second sanction within 12 months	0.941	0.150	0.687	0.322	-	-	-	
Second sanction after 12 months	0.776	0.416	0.890	0.613	-	-	-	
District unempl. rate	-0.023	0.016	0.090	0.034	0.059	0.021	0.001	0.005
District long-term unempl. rate	-0.051	0.026	-0.209	0.058	-0.154	0.036	-0.014	0.008
District vacancy-unempl. ratio	0.814	0.179	1.253	0.390	-0.280	0.250	-0.166	0.054
20 years	0.264	0.046	0.250	0.083	-0.232	0.055	0.043	0.014
21 to 22 years	0.396	0.040	0.169	0.073	-0.302	0.049	0.081	0.012
23 to 24 years	0.418	0.040	-0.011	0.078	-0.339	0.051	0.122	0.013
No occ. degree, no schooling degree	-0.993	0.042	-0.061	0.096	0.616	0.061	-0.204	0.012
No occ. degree, low schooling degree	-0.834	0.032	-0.134	0.081	0.530	0.052	-0.165	0.009
No occ. degree, high schooling degree	-0.852	0.079	0.876	0.141	-0.271	0.125	-0.145	0.024
Voc. training, high schooling degree	0.067	0.075	1.151	0.156	-0.585	0.187	0.088	0.021
Education missing	-0.952	0.046	0.393	0.096	0.092	0.067	-0.136	0.014
Schleswig-Holstein	-0.151	0.054	-0.084	0.110	-0.057	0.070	-0.028	0.017
Hamburg	0.074	0.064	-0.575	0.150	-0.028	0.090	-0.034	0.019
Lower Saxony	-0.126	0.039	-0.213	0.086	0.037	0.052	0.007	0.011
Bremen	0.070	0.076	0.092	0.152	-0.108	0.107	0.002	0.025
Hesse	-0.178	0.053	-0.013	0.108	-0.155	0.074	0.003	0.016
Rhineland-Palatinate	-0.158	0.058	-0.031	0.122	0.078	0.078	0.002	0.017
Baden-Württemberg	-0.066	0.053	-0.110	0.115	-0.142	0.076	-0.019	0.016
Bavaria	0.191	0.047	0.117	0.101	0.151	0.065	0.025	0.014
Saarland	0.208	0.076	-0.457	0.198	0.057	0.110	-0.073	0.023
Turkish nationality	0.334	0.060	-0.227	0.149	-0.190	0.087	0.033	0.017
Other nationality	0.456	0.040	-0.069	0.091	-0.427	0.065	0.003	0.011
Quarter 2	-0.196	0.032	-0.067	0.073	0.117	0.045	-0.027	0.009
Quarter 3	-0.348	0.034	0.146	0.070	-0.018	0.046	-0.045	0.010
Quarter 4	-0.431	0.038	-0.074	0.079	0.010	0.050	-0.048	0.011
Year 2008	-0.286	0.037	-0.108	0.084	-0.057	0.053	-0.032	0.011
September	0.116	0.038	0.325	0.076	0.284	0.048	0.001	0.013
First sanction in month 1-3	-	-	-	-	-0.465	0.115	-	-
First sanction in month 4-6	-	-	-	-	-0.461	0.125	-	-
First sanction in month 7-9	-	-	-	-	-0.271	0.139	-	-
First sanction in month 10-12	-	-	-	-	-0.110	0.167	-	-
First sanction in month 13-15	-	-	-	-	-0.183	0.206	-	-
First sanction in month 16-18	-	-	-	-	0.206	0.237	-	-
First sanction in month 19-21	-	-	-	-	-0.943	0.527	-	-
First sanction in month 22+	-	-	-	-	0.126	0.374	-	
Log(σ_w)	-	-	-	-	-	-	-1.066	0.015
V_1	0	0	0	0	0	0	0	0
V_2	1.256	0.252	2.040	0.618	-2.393	0.434	-0.022	0.089
V_3	2.293	0.189	0	0	-1.225	0.230	0.092	0.052

Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. n=31,890. LogLikelihood= -142,715.67. 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 C6: Full model for male job seekers 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	-7.592	0.243	-8.686	0.269	-5.909	0.160	3.607	0.060
Months (4-6)	-0.004	0.031	0.378	0.064	0.147	0.039	0.026	0.010
Months (7-9)	-0.149	0.046	0.477	0.074	0.077	0.052	0.025	0.014
Months (10-12)	-0.407	0.060	0.162	0.098	-0.017	0.066	0.023	0.018
Months (13-15)	-0.534	0.072	0.195	0.111	-0.175	0.081	0.027	0.021
Months (16-18)	-0.649	0.084	-0.063	0.138	-0.182	0.095	0.035	0.026
Months (19-21)	-0.879	0.105	0.114	0.152	-0.408	0.118	0.069	0.031
Months (22+)	-0.924	0.111	-0.084	0.162	-0.485	0.126	0.098	0.032
First sanction	0.513	0.109	0.059	0.092	-	-	-0.038	0.016
Second sanction within 12 months	0.807	0.152	0.296	0.188	-	-	-	
Second sanction after 12 months	0.203	0.422	0.387	0.514	-	-	-	
District unempl. rate	-0.021	0.016	0.069	0.033	0.046	0.020	-0.001	0.004
District long-term unempl. rate	-0.060	0.026	-0.184	0.055	-0.156	0.034	-0.010	0.007
District vacancy-unempl. ratio	1.637	0.168	1.349	0.339	0.039	0.246	-0.093	0.046
20 years	0.224	0.039	0.243	0.067	-0.288	0.048	0.039	0.010
21 to 22 years	0.321	0.034	0.113	0.063	-0.235	0.041	0.067	0.009
23 to 24 years	0.305	0.036	0.133	0.074	-0.317	0.047	0.111	0.009
No occ. degree, no schooling degree	-0.966	0.042	-0.217	0.104	0.476	0.063	-0.213	0.011
No occ. degree, low schooling degree	-0.824	0.035	-0.201	0.093	0.372	0.057	-0.182	0.009
No occ. degree, high schooling degree	-0.997	0.085	0.741	0.130	-0.500	0.133	-0.160	0.022
Voc. training, high schooling degree	-0.188	0.113	0.877	0.203	-0.753	0.27	0.048	0.026
Education missing	-1.122	0.045	0.203	0.099	-0.034	0.067	-0.188	0.011
Schleswig-Holstein	-0.196	0.057	-0.159	0.115	-0.380	0.075	-0.011	0.014
Hamburg	-0.195	0.068	-0.542	0.146	-0.134	0.089	-0.078	0.018
Lower Saxony	-0.102	0.039	-0.111	0.082	-0.047	0.050	0.009	0.010
Bremen	0.035	0.079	0.153	0.153	-0.142	0.107	0.030	0.020
Hesse	-0.098	0.051	-0.162	0.106	-0.034	0.065	-0.021	0.012
Rhineland-Palatinate	-0.065	0.050	0.130	0.100	-0.020	0.066	0.002	0.013
Baden-Württemberg	0.019	0.051	0.073	0.102	-0.149	0.070	0.030	0.012
Bavaria	0.163	0.046	0.393	0.093	0.083	0.063	0.028	0.012
Saarland	0.145	0.085	-0.455	0.231	0.164	0.109	0.002	0.024
Married	0.536	0.051	-0.449	0.134	-0.674	0.082	0.112	0.012
Living with parter, not married	0.325	0.044	-0.357	0.113	-0.168	0.067	0.037	0.011
Turkish nationality	0.280	0.038	-0.202	0.083	0.117	0.048	0.019	0.009
Other nationality	0.132	0.033	-0.092	0.069	-0.061	0.045	-0.021	0.008
No. of children < 3 years	0.055	0.028	-0.361	0.092	0.186	0.042	0.035	0.007
No. of children between 3-5 years	0.002	0.044	-0.107	0.156	0.107	0.061	0.014	0.011
No. of children between 6-17 years	-0.140	0.054	-0.169	0.179	0.083	0.072	-0.001	0.014
Partner below 25 years old	0.001	0.048	-0.087	0.150	-0.060	0.079	-0.024	0.011
Partner foreigner	-0.143	0.049	-0.170	0.139	-0.073	0.077	0.004	0.011
Partner no occ. degree, no schooling degree	-0.140	0.063	-0.009	0.171	0.302	0.090	-0.065	0.015
Partner no occ. degree, schooling degree	0.066	0.044	-0.060	0.125	0.121	0.070	-0.030	0.010
Partner educ. missing	-0.195	0.050	0.063	0.129	0.182	0.077	-0.040	0.012
Quarter 2	-0.039	0.033	-0.004	0.067	0.085	0.044	-0.037	0.008
Quarter 3	-0.269	0.034	-0.063	0.066	-0.075	0.044	-0.058	0.008
Quarter 4	-0.302	0.037	-0.203	0.075	0.125	0.047	-0.059	0.009
Year 2008	-0.114	0.037	-0.092	0.078	0.062	0.051	-0.032	0.009
Sept	0.164	0.032	0.306	0.077	0.263	0.047	-0.016	0.011
First sanction in month 1-3	-	-	-		-0.197	0.117	-	
First sanction in month 4-6	-	-	-		0.032	0.121	-	
First sanction in month 7-9	-	-	-		0.280	0.134	-	
First sanction in month 10-12	-	-	-		0.209	0.154	-	
First sanction in month 13-15	-	-	-		0.383	0.184	-	
First sanction in month 16-18	-	-	-		0.549	0.225	-	
First sanction in month 19-21	-	-	-		0.614	0.322	-	
First sanction in month 22+		-	-		0.311	0.389	-	
Log(σ_w)	-	-	-	-	-	-	-1.107	0.012
V_1	0	0	0	0	0	0	0	0
V_2	2.446	0.230	0.542 0.391	-1.424	0.167	0.130	0.058	
V_3	4.096	0.242	0	0	-1.758	0.402	0.247	0.067
V_4	0.820	0.338	0	0	-1.957	0.266	-0.152	0.076

Estimations are based on inflow samples of individuals who are registered as job seekers at the beginning of the spell. n=38,492. LogLikelihood=-174,545.64. 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).