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IZA DP No. 4509

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October 2009

Forschungsinstitut
zur Zukunft der Arbeit
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Discussion Paper No. 4509
October 2009

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IZA Discussion Paper No. 4509
October 2009

ABSTRACT

How Effective Are Unemployment Benefit Sanctions? Looking Beyond Unemployment Exit*

This paper provides a comprehensive evaluation of benefit sanctions, i.e. temporary reductions in unemployment benefits as punishment for noncompliance with eligibility requirements. In addition to the effects on unemployment durations, we evaluate the effects on post-unemployment employment stability, on exits from the labor market and on earnings. In our analysis we use a rich set of Swiss register data which allow us to distinguish between ex ante effects, the effects of warnings and the effects of enforcement of benefit sanctions. Adopting a multivariate mixed proportional hazard approach to address selectivity, we find that both warnings and enforcement increase the job finding rate and the exit rate out of the labor force. Warnings do not affect subsequent employment stability but do reduce post-unemployment earnings. Actual benefit reductions lower the quality of post-unemployment jobs both in terms of job duration as well as in terms of earnings. The net effect of a benefit sanction on post-unemployment income is negative. Over a period of two years after leaving unemployment workers who got a benefit sanction imposed face a net income loss equivalent to 30 days of full pay due to the ex post effect. In addition to that, stricter monitoring may reduce net earnings by up to 4 days of pay for every unemployed worker due to the ex ante effect.

JEL Classification: J64, J65, J68

Keywords: benefit sanctions, earnings effects, unemployment duration, competing-risk duration models

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* We thank Jaap Abbring, Jan Boone, Bart Cockx, Bo Honoré, Bruno van der Linden, Blaise Melly, Arthur van Soest, and seminar participants at Copenhagen, IAB Nuernberg, IZA, Louvain-la-Neuve, Tilburg University, EALE 2009 for comments on previous versions of the paper, and Jonathan Gast for support in interpreting the data. Financial support from the Swiss National Science Foundation projects No. 100014-120356/1 and PBLAP1-127652/1 is gratefully acknowledged.

1 Introduction

When unemployed workers receive unemployment benefits they have a disincentive to search for a job. To restore search incentives often activation measures are introduced. Unemployed are required to attend intensive interviews with employment counselors, to apply for job vacancies as directed by employment counselors, to independently search for job vacancies and to apply for jobs, to accept offers of suitable work, and to attend training programs. If unemployed workers are unwilling to participate in such activities, search insufficiently for a job or reject job offers they may face a reduction of their unemployment benefits, i.e. they may get a benefit sanction imposed. Such a benefit sanction may be permanent or temporary and may involve a partial reduction or a complete removal of unemployment benefits.

Interest in the use benefit sanctions is motivated by the observation that, on one hand, the frequently used policy of active labor market programs is often not successful in getting the unemployed immediately back to work. On the other hand, the potentially successful policy of close monitoring and benefit sanctions is not used very often. The overview by Grubb (2000) shows a wide range of experiences in terms of sanction policies. For instance, sanctions enforced on unemployed job seekers are frequently applied in Switzerland and the Czech Republic, while in Denmark they are hardly used. Furthermore, an interesting result in the recent evaluation literature is that, among the broad range of active labor market policies, programs with intensive counseling and job search assistance did much better than other programs, in particular when combined with close monitoring and enforcement of the work test. Typically these programs do not involve risks that participants are locked into programs with reduced search activity as a consequence.[1]

This paper presents one of the first empirical studies that looks beyond unemployment exits and provides a comprehensive evaluation of the effects of benefit sanctions. In addition to the effects on unemployment durations, we evaluate the effects on post-unemployment employment stability, exits from the labor market and earnings. Assessing the overall impact of a system of benefit sanctions is a non-trivial exercise. Consider for example the case in which benefit sanctions induce to accept jobs that do not last that long. Then it may be that the reduced employment duration and reduced unemployment duration cancel out, i.e. equilibrium unemployment is not affected.[2] Or, even worse, the average duration of employment goes down so much that equilibrium unemployment goes up despite the fact that the average duration of unemployment goes down. Also at the level of the individual worker a reduction in employment duration could imply that overall the worker is worse off in terms of earnings, i.e. the earlier em-

[1] In their survey on the success of active labor market policy programs in OECD countries Martin and Grubb (2001) conclude that governments should rely as much as possible on in-depth counseling, job-finding incentives and job-search assistance programs as other more intense programs are not very effective. In Lalive et al. (2008) and Gerfin and Lechner (2002) similar pessimistic conclusions are drawn with respect to the effectiveness of Swiss active labor market programs.

[2] It is easy to show that in a steady state labor market the unemployment rate is equal to $\frac{T_u}{T_u+T_e}$, where T_u is the average duration of unemployment and T_e is the average duration of employment.

ployment re-entry is insufficient to compensate for the reduction in earnings due to the shorter employment duration. Even if job stability is not an issue, individual workers could still face a reduction in their life time income if they are forced to accept jobs with lower wages. Again the reduction in unemployment duration could be insufficient in income terms to cover the lower income while employed. In other words, in income terms benefit sanctions only represent a net gain to individual workers if their post-unemployment job stability and earnings do not go down too much.

We use rich, administrative data on Swiss job seekers with four distinguishing features. First, we merge detailed and comprehensive histories on the timing of benefit sanctions with medium-run information on the post-unemployment labor market success. This allows us to assess the effects of benefit sanctions on post-unemployment earnings. Second, exhaustive information on pre-unemployment earnings and employment allow us to control for a key source of heterogeneity between job seekers. Third, a unique feature of this data is that the available information also allows us to distinguish between the effect of a warning that a sanction may be imposed and the actual benefit reduction. Fourth, we distinguish between exits to paid employment and (possibly temporary) unregistered unemployment. This is important because benefit sanctions may affect both transitions to employment and transitions to non-employment. Taken together, this database allows us to provide comprehensive information on how benefit sanctions affect job seekers.

Our empirical analysis provides estimates of the key parameters that are essential in a comprehensive analysis of the effects of benefit sanctions. Specifically, we contrast the effects of sanctions on the time spent in unemployment with the effects of benefit sanctions on employment durations and earnings for job seekers who experience a sanction. This allows us to speak about the net effect of actually experiencing a benefit sanction on post unemployment earnings – i.e. the ex post effect of benefit sanctions. Moreover, we use regional variation in the probability of being warned of future benefit reductions to provide key evidence on the ex ante effects of benefit sanctions on the time spent unemployed and on post unemployment earnings. This allows us to provide evidence on the net effects of benefit sanctions on all job seekers regardless of whether they are actually sanctioned or not.

The small body of recent empirical literature on benefit sanctions is mainly of European origin and supports the positive short-term effects on the exit rate from unemployment.[3] Lalive et al. (2005) use similar unemployment data as we do finding that not only the enforcement of a benefit sanction has a positive effect on the exit rate from unemployment. A warning that a sanction may be imposed has a similar effect. Lalive et al. (2005) is also the first empirical investigation on the magnitude of the so called ex-ante effect, the effect that in the presence of a

[3] In the U.S. sanctions have been a central feature of the welfare reforms of the 1990s (Bloom and Winstead, 2002). Nevertheless, little is known about the effects of such sanctions. Ashenfelter et al. (2005) for example do not find a significant impact of sanctions on unemployment insurance claims and benefits, which may be related to the small size of the sanctions.

sanction system the job finding rate goes up because unemployed want to avoid being punished.[4] The ex ante effect also reduces unemployment duration. Two Dutch papers find that benefit sanctions double the outflow from unemployment to a job (Abbring et al. (2005) and Van den Berg et al. (2004)). Using Danish data Svarer (2007) finds that the unemployment exit rate increases by more than 50% following enforcement of a sanction. Jensen et al. (2003) find a small effect of the sanctions that are part of Danish youth unemployment program. Schneider (2008) studying benefit sanctions in Germany finds no significant effect of sanctions on reported reservation wages. Hofmann (2008) on the other hand reports positive effects of benefit sanctions on the employment probability of West-German unemployed.

A common element in these benefit sanction studies is that they are restricted to the analysis of the effects on the duration of unemployment. This is not surprising as suitable data to perform an analysis of post-unemployment jobs are often not available. Even in the context of much more frequently investigated effects of changes in level or duration of unemployment benefits effects on post-unemployment outcomes are rarely considered.[5] Our paper contributes to the existing literature in at least three respects. First, we provide evidence on the effects of benefit sanctions on employment stability and income after leaving unemployment. This information is crucial in assessing the net effects of benefit sanctions on earnings. Second, we provide a detailed analysis of the effects of benefit sanctions on exits to regular jobs, and of the effects of benefit sanctions on temporary exits to unregistered unemployment. This distinction is essential in thinking about policies that activate job seekers to take regular jobs as opposed to policies that discourage labor force participation altogether. Third, we provide an exhaustive set of simulations of the net effects of benefit sanctions on work income.

The remainder of this paper are structured as follows. Section 2 discusses institutional procedures in the Swiss UI system, both concerning unemployment benefits and sanction procedures. Section 3 presents our data and a descriptive analysis. In section 4 we provide the set-up of the econometric analysis while in section 5 we provide our parameter estimates. Section 6 concludes.

2 Institutional Procedures in the Swiss UI System

Job seekers are entitled to unemployment benefits if they meet two requirements. First, they must have paid unemployment insurance taxes for at least six months in the two years prior to registering at the public employment service (PES). The contribution period is extended to

[4] Other existing empirical literature deal almost exclusively with the ex-post effect of benefit sanctions. One exception is the paper of Svarer (2007) on Danish benefit sanctions. Boone et al. (2009) present results on experiment on benefit sanctions in which the relevance of the ex ante effect is investigated.

[5] Three recent studies which do look at the post-unemployment effects are Card et al. (2007), Van Ours and Vodopivec (2008), and Lalive (2007). These studies assess the effects of a change of potential duration of UE benefits in Austria and Slovenia. Both find no or little effect on job match quality or wages. Only very recently we became aware of Van den Berg and Vikström (2009), who also investigate post-unemployment effects of unemployment benefit sanctions. Using Swedish data on post-unemployment jobs - wage rates, hours of work and occupational level - they find that sanctions lower wages and hours of work and lead to a lower occupational level.

12 months for those individuals who have been registered at least once in the three previous years. Job seekers entering the labor market are exempted from the contribution requirement if they have been in school, in prison, employed outside of Switzerland or have been taking care of children. Second, job seekers must possess the capability to fulfill the requirements of a regular job - they must be 'employable'. If a job seeker is found not to be employable there is the possibility to collect social assistance. Social assistance is means tested and relatively generous. For instance, social assistance is roughly 76% of unemployment benefits for a single job seeker with no other sources of income (OECD, 1999).

The potential duration of unemployment benefits is 2 years for individuals who meet the contribution and employability requirements. After this period of two years unemployed have to rely on social assistance. The marginal replacement ratio is 80% for previous income up to Sfr 4030; 70 % for income between Sfr 4030 and 8100; and 0 % for income beyond Sfr 8100. For job seekers with children, the marginal replacement ratio is 80 % for income up to Sfr 8100; and 0 % thereafter. Job seekers have to pay all income and social insurance taxes except for the unemployment insurance contribution.

The entitlement criteria during the unemployment spell concern job search requirements and participation in active labor market programs. Job seekers are obliged to make a minimum number of applications to 'suitable' jobs each month.[6] And, they are obliged to participate in active labor market programs during the unemployment spell.[7]

Compliance with the job search and program participation requirements is monitored by roughly 2500 caseworkers at 150 PES offices. When individuals register at the PES office they are assigned to a caseworker on the basis of either previous industry, previous occupation, place of residence, alphabetically or the caseworker's availability. Job seekers have to meet at least once a month with the caseworker. Compliance with the job search requirements is enforced by way of communication with the human resources department of the potential employer. Participation in a labor market program is monitored by the caseworker as well as the program staff.

In this paper we focus on benefit sanctions because of noncompliance with eligibility requirements. Sanctions are private information and neither caseworkers nor job seekers share information on benefit sanctions with potential employers.[8] The process until a sanction is im-

[6] A suitable job has to meet four criteria: (i) the travel time from home to job must not exceed two hours, (ii) the new job contract can not specify longer hours of availability than are actually paid, (iii) the new job must not be in a firm which lays off and re-hires for lower wages, and (iv) the new job must pay at least 68% of previous monthly earnings. Potential job offers are supplied by the public vacancy information system of the PES, from private temporary help firms or from the job seeker's own pool of potential jobs. Setting the minimum number of job applications is largely at the discretion of the caseworker at the PES.

[7] The exact nature and scope of the participation requirement is determined at the beginning of the unemployment spell and in monthly meetings with the caseworker. Gerfin and Lechner (2002) and Lalive et al. (2001) contain background information on and an evaluation of the active labor market programs.

[8] We ignore a second type of benefit sanctions which refer to 'unnecessary' job loss and are inflicted upon workers at the start of the unemployment spell. The legal bases for the sanction procedure are mainly given by Art. 30 of the Swiss UI Law (AVIG), Art. 44 and Art. 45 of the corresponding UI Ordinance (AVIV) and part D ("Sanctions") of the Decree about Unemployment Benefits (Kreisschreiben) issued by the Swiss State Secretariat

posed can be divided into two stages. The first stage of the sanction process starts when some type of misbehavior by the unemployed is detected and reported to the cantonal ministry of economic affairs (CMEA) either by the caseworker, by a prospective employer or by the active labor market program staff. In this case the job seeker must be notified of the possible sanction and be given the opportunity to clarify why he or she was not able to fulfil the eligibility requirements (Article 4 of Federal Social Insurance Law). Notification is in written form and contains the reason for the sanction and the date until which the clarification is to be sent back. The average duration between the date job-seekers are informed and the date until which the clarification is to be received is about two weeks.

The second stage of the sanction process starts as soon as the clarification period ends. Depending on the nature of the clarification provided by the job seeker the CMEA decides whether or not the sanction will be enforced. If there is sufficient ground for an excuse the sanction process will be stopped. If the excuse is deemed not valid, the sanction is enforced. A benefit sanction entails a 100% reduction of benefits for a maximum duration of 60 work days.[9]

Once the CMEA has decided on legitimacy and duration of the sanction, benefit payments are stopped for time specified in the warning letter. The CMEA has to take this decision within an enforcement period of six months. The enforcement period for the benefit cut starts at the first day of the committed noncompliance[10]. Due to administrative delay at the CMEA, there is no strict one-to-one relationship between receiving a warning letter and the day when benefits are stopped. Once the sanction has been imposed, the unemployed can appeal to a cantonal court within 30 days of the start of the benefit sanction. The court then decides whether the sanction conforms to current legal practice. However, it takes at least one year until the court reaches a decision. Appeal to the court does not keep the CMEA from imposing the sanction. Job seekers who leave unemployment to a job after receiving the warning do not have to pay the benefit payments due to a benefit sanction.

3 Data and Descriptive Analysis

3.1 Data Sources and Data Structure

Our study is based on data from the Swiss unemployment register. Our main sample is drawn from the unemployment insurance register database (UIR) covering the time period 1998-2003. It contains information on all individuals registering with the public employment service (PES) – which can be job seekers who are eligible for unemployment benefits but also other individuals asking the PES for assistance. The database also contains information on unemployment benefit

for Economic Affairs seco. The right of job seekers under suspicion of noncompliance to get the opportunity to justify themselves is based on Art. 42 of the Federal Social Insurance Law (ATSG) and the paragraphs D8 and D9 of the above-mentioned decree.

[9] Depending on the nature of the infringement, there are four levels of sanction strengths; in workdays: 1 to 15, 16 to 30, 31 to 60, several months up to more than a year.

[10] Exception: The enforcement of the sanction can take place after this period of six months if benefits in the size of the sanction have been withheld within the period.

payments, as well as on benefit sanctions. Information on sanctions is particularly rich containing dates of issue of sanction warnings and sanction impositions as well as on the reasons for imposing a sanction and its severity. This database records the timing of events at daily precision.

We merge to the UIR information on income provided from the social security administration (SSA) covering the period 1993 to 2002. This database contains income information on individuals which are eligible for the public retirement pension system. The data provide information on earnings but also on non-labor income sources such as unemployment benefits, disability benefits, military benefits, etc. Earnings and non-labor income information is available in monthly precision. The SSA does not record information on hours worked.

From the merged UIR-SSA database, we draw an inflow sample covering individuals entering the UIR between August 1998 and July 1999. From these, we selected UI eligible job seekers aged 30 to 55 entering unemployment from a job with positive earnings in the year prior to entering unemployment. Moreover, we restrict the sample to individuals who are entering unemployment in cantons with reliable information on warnings. Cantons differ in terms of the number of actual benefit reductions that are preceded by a warning letter. We interpret this as missing information on warning letters because job seekers must be informed before actual benefit reductions take place. The analysis focuses on cantons where almost all warnings preceding actual benefit reductions are present[11]. While this sample is not representative for Switzerland[12], this sample restriction allows understanding both the effects of a warning and the effect of enforcing the benefit sanction. The resulting sample covers 23,961 spells. The median duration of unemployment is 153 days, 80.0% of the unemployed found a job, 19.8% of the unemployed received a sanctions warning, while 8.4% actually got a benefit sanction imposed (see for more details Appendix D).

3.2 Descriptive Analysis

This section provides a descriptive analysis of the dynamics in the Swiss labor market, the sanction process, post-unemployment earnings and the duration of post-unemployment spells. Figure 1 shows the empirical Kaplan-Meier estimates of the transition rate from unemployment to employment or non-employment and the sanction warnings rate. The exit rate to employment starts at a rather low level of 5 % per month, peaks at 14 % per month after 5 months of job search have elapsed, and tapers off gradually to a level of about 7% per month after 10 months of elapsed unemployment duration. The transition rate to non-employment, on the other hand, doesn't show a peak in the early months of unemployment: It slightly increases in the first 6

[11] These cantons are Vaud, Valais and Fribourg in the West, Solothurn and Uri in the center, and Appenzell-Innerrhoden and Graubünden in the East. On average, 5% of the warnings are missing. Cantons with at least 87.5% warnings present were chosen for the sample. We predict warning times for the remaining 5% of sanctioned job seekers using a tobit regression based on information on observed characteristics. Results are unaffected by disregarding these job seekers.

[12] Using the mentioned sampling criteria but without the restriction to cantons with reliable information on warnings, an inflow sample of 90'897 spells would have resulted. Thus, our sample covers 26.4% of the inflow in the Swiss UIR during the respective year.

months from 1 to 2% of exits to non-employment. From then on, it remains on this level. In general, the distribution of the UE durations in the sample (not illustrated) shows the well-known shape with a peak in the first four months of unemployment and another peak, though smaller, at the end of the normal benefit entitlement period after two years.

The third hazard rate in Figure 1 is the sanction warning rate. The sanction warning rate measures the probability of a sanction warning in the next month for those who are still unemployed at the start of each month. The sanction warnings rate shows a peak of almost 5% in the second month of UE, gradually decreasing afterwards. The median duration until the first warning was 77 days.

The bottom graph of Figure 1 shows the enforcement hazard, i.e. the rate at which sanctions are enforced among those who have been warned. Clearly, there is a strong tendency to enforce a sanction in the first month after giving the warning. The enforcement hazard peaks at about 23 % in the first month, and decreases strongly to 7 % in month 2, and more gradually to levels below 5 % per month thereafter. This evidence suggests on one hand that at least one quarter of all warnings immediately lead to withdrawal of benefits. On the other hand, the fact that the enforcement hazard is substantially below 100 % in the first month after the warning also suggests that not all warnings are actually enforced.

Figure 2 gives insights into the stability of the individual's post-unemployment situation. Recall that job seekers leave unemployment either directly for a job or they leave unemployment for a period of temporary or permanent non-employment. The SSA allows constructing information on the duration of the first employment or non-employment spell after leaving registered unemployment (in months) between the calendar date a job seeker leaves unemployment and the end of the SSA observation period (December 2002). Employment and non-employment spells which are on-going in December 2002 are treated as right censored. Employment spells are terminated by a transition into non-employment whereas non-employment spells are terminated by a transition into employment. Consider first job seekers who leave unemployment directly for employment. The first employment spell after exit lasts 25 months in median (mean: 24). The employment exit hazard peaks after 8 months at 6% exit rate from the first employment period. People in employment spells beyond one year show a propensity to exit of about 1 to 2% per month – a sign of high stability of employment relations. Turning to job seekers who leave unemployment to temporary or permanent non-employment, we find that their first non-employment spell lasts for 11 months in median (mean: 18). There is an important group of short non-employment spells of 1 to 2 months that drives up the respective hazard. This group seems to be confronted with a very short unstable transition period until reemployment is established. Later, the hazard gradually decreases, and after 15 months of duration, the non-employment exit hazard stabilizes on approximately the same level as the employment exit hazard.

The econometric analysis will provide evidence on the causal impact on earnings in the first (complete) month after unemployment, and earnings on the entire 24 month period after leaving unemployment for job seekers who leave unemployment directly for a paid job. We analyze earnings using hazards because this brings a number of methodological advantages – mainly in terms of a more flexible (and less parametric) econometric design. We discuss these (and some issues on the interpretation of earnings hazards) in the corresponding econometrics section 4.2. The middle graph of Figure 2 displays the hazard of leaving the earnings distribution for the first post-unemployment month grouped in intervals of 500 CHF (about 330 €). The one month earnings hazard – i.e. the (instantaneous) probability of earning an amount y conditional on earning at least y – is steadily increasing over the support of the earnings distribution; at a level of 5000 CHF it reaches about 30% per 500 CHF. This means that individuals who earn at least 5000 CHF have a probability of earning between 5000 CHF and 5499 CHF of 30%. The observation that the proportion of people ”leaving” the earnings distribution markedly increases until this peak reflects the fact that earnings levels around 5000 CHF are the most frequent ones for individuals in their first employment month after unemployment exit. The high ”exit rate” from the earnings distribution of about 25% per 500 CHF thereafter shows that earnings higher 5000 CHF are the less and less frequent in the e group.

A similar shape of the earnings hazard can be found when analyzing the *sum* of earnings over 24 months after unemployment exit for job seekers who start working immediately after leaving unemployment, see the bottom graph of Figure 2. This hazard peaks at 15% towards 125,000 CHF, reflecting the fact that cumulative earnings of a bit more than 100,000 CHF over two years are the most common ones for the e group of our sample. Then, the hazard gradually decreases down to a level of about 10%. Earnings sums beyond 200,000 CHF are very rare extreme cases (which will be censored for estimation). Extending the analyzed subsample to *all* individuals who realized positive earnings during these two years (corresponds to estimated Model IV later on), we observe almost exactly the same shape of the hazard. This is not surprising since the two considered groups do not differ tremendously in their composition (see discussions on that in the econometrics section 4.2 and the results section 5.3).

The final piece of descriptive evidence concerns earnings *histories* of individuals who never experience a sanction, individuals who receive a warning but this warning does not lead to an actual reduction in benefits, and individuals who receive a warning and the benefit cut is also realized. Recall that our earnings data span the time period 1993 to 2002. This allows constructing average (deflated) earnings in the 5 years prior to entering unemployment and in the 2 years after leaving unemployment by sanction status (top graph of Figure 3). Results indicate that non-sanctioned and sanctioned differ tremendously with respect to earnings levels. Whereas non-sanctioned earn almost 3500 CHF per month[13], individuals with either a warning or an actual benefit reduction earned on the order of 2750 CHF per month.

[13] When interpreting the absolute earnings levels in this and the previous figures, one has to consider that: (i) individuals may be partly employed, partly non-employed in their earnings history; (ii) also part-time workers are in the sample; (iii) the sample contains all the individuals who gained at least once employment earnings in the last 12 months before inflow into unemployment (with no restrictions on being in the labor force or not in the years before). This explains the low level of average employment earnings reported in the graph.

Interestingly, while the earnings gap between individuals who were warned only and those who are warned and enforced is visible 5 years before entering unemployment, the gap disappears around the time when individuals enter unemployment. This suggests that while selectivity is important in comparing the non-sanctioned to either warned or warned plus enforced individuals, direct comparisons within the latter two groups are more informative. Moreover, enforcing the sanction appears to lower post-unemployment monthly earnings for the group with a sanction by about 200 CHF in comparison with the warned group. This is a first descriptive hint that benefit sanctions may reduce post-unemployment earnings. But this picture could be misleading since the descriptive effect may be confounded by unobserved characteristics and endogenous selectivity. These will be taken into account in the estimated models. The bottom graph of Figure 3 distinguishes the earnings paths with respect to the exit destination – into employment or nonemployment. This figure supports the previous one, pointing to an increased earnings difference between the sanctioned and non-sanctioned after unemployment exit for both, the exit to employment and to non-employment group.[14]

4 Econometric Analysis

Our dataset allows the use of detailed duration analysis methods. In particular, we use a multi-state duration model that combines information on the timing of benefit sanctions with information on unemployment dynamics and the quality of post-unemployment jobs. As we explain in more detail below we estimate four models. Model I is the baseline model in which we jointly estimate transition rates from unemployment to employment and out of the labor force, transition rates to the warning state and transition rates to enforcement. Model II adds to this estimates of post-unemployment outcomes, i.e. employment stability and durations of out of the labor force spells. In Models III and IV we also include various measures of post-unemployment earnings.

4.1 Modeling Individual's Event Histories

As a base for the evaluation of sanction effects on post-unemployment outcomes, we model the event history of an individual during and after unemployment. As depicted in Figure 4, the individual experiences *multiple stages*, starting at t_0, the entry into unemployment. The first selection is the treatment assignment: to be sanctioned or not. Since we dispose of non-experimental data, this *assignment is non-random and endogenous.* It comprises two stages, the warning (subscript w) that a sanction investigation has started, and later the possible sanction enforcement (s). Thus, at the point of exit from unemployment (T), the individual can be

[14] Note that the upward-tendency of the earnings paths in the last year before unemployment entry in the two graphs in Figure 3 is generated by the sampling: The fact that having at least once positive earnings in the year before unemployment entry is one of the conditions of being sampled and leads to a higher proportion of individuals in employment in this year. Consequently, average earnings are higher. This causes no problems for estimation later on because we will control for the full past earnings and employment history.

potentially in three different states (s, w or not sanctioned). In addition, unemployment spells can be censored if they last longer than 720 days.

By T, the third selection takes place, individuals exit to employment (e) or non-employment (ne). Employment is defined in our data by a positive value of employment earnings in a specific month[15]. Beyond T, we observe the post-unemployment outcome – in the form of subsequent (non-)employment (t_m/t_{nm}) or of earnings (y) over a certain period. Due to the fact that our post-unemployment observation period ends by 31 December 2002, we analyze outcomes up to two years after unemployment exit. There is a very small group that may be censored in these outcomes: Those who enter at the end of the inflow period and exploit (almost) fully the two year's benefit availability can only be observed for 1.5 years.

We implement the event histories of individuals by using a competing risk mixed proportional hazard (MPH) framework with dynamic treatment effects. Work of Abbring and van den Berg (2003b) shows that identification of such models is given under an MPH structure and weak regularity conditions. To avoid parametric assumptions as far as possible, we model the MPH using a flexible, piecewise-constant duration dependence function and specify a discrete mass points distribution for the unobserved heterogeneity.

There are two central assumptions for the nonparametric identification of causal effects of dynamic treatments (Abbring and van den Berg 2003a). The first assumption states that job seekers do not anticipate a warning or the actual reduction of a benefit sanction. This assumption is crucial to rule out changes in behavior before the actual treatment takes place. No anticipation is clearly justified in the present context. While job seekers may have some information regarding the monitoring technology used by caseworkers, the can not anticipate the actual date of receiving the warning letter. This is because issuing the warning letter takes several steps. First, caseworkers, firms, or program staff need to detect non-compliance and decide to report it. Second, the official at the CMEA will look into the case and decide whether non-compliance is present. Third, job seekers can not anticipate the actual day of receiving the letter because administrative delays are introducing a strong degree of uncertainty. Moreover, job seekers also can not anticipate the day when benefits are reduced. Justification introduces uncertainty in the with regard to whether the warning leads to a benefit reduction. Moreover, even if justification is not valid, the CMEA can take up to 6 months until the benefit sanction is actually enforced.

The second key identifying assumption is that the hazards of leaving unemployment have a mixed proportional hazard structure (MPH). This assumption states that selectivity can be modeled assuming time invariant unobserved heterogeneity that is independent of observed characteristics. The assumption of time invariance appears warranted (referring to individual specific characteristics such as motivation for job search, etc.). In contrast, the assumption of independence between observed and unobserved characteristics appears to be more questionable.

[15] In addition, employment earnings must be higher than the amount of additional social transfer (if the individual gets some). Thus, individuals mainly relying on social transfer are considered as non-employed.

However, note that while correlation between observed characteristics and unobserved characteristics is likely to bias parameter estimates attached to control variables, the bias to the treatment effects are likely to be less severe since selectivity is explicitly taken into account. Assuming an MPH structure also means that observed covariates shift the hazard rate proportionately. Proportionality is one of the most common assumptions in duration studies and earlier work on Switzerland suggests that it is not driving results on the effects of dynamic treatments (Lalive, van Ours and Zweimüller 2008).

To expose the model structure, t_e denotes the duration of unemployment until a paid exit from unemployment, t_{ne} denotes the time from entering unemployment until leaving paid unemployment to an unpaid exit state, t_w denotes the time from entering unemployment until a sanction warning takes place, and t_s denotes the time from a sanction warning until an actual benefit reduction takes place. The treatment indicators can then be defined as follows. $D_w \equiv I(t_w < min(t_e, t_{ne}))$ identifies job seekers who experience benefit reduction before leaving unemployment. $D_s \equiv I(t_w + t_s < min(t_e, t_{ne}))$ identifies job seekers who experience a benefit reduction before leaving unemployment. The starting point to set up the duration model is a specification where the treatment variables D_w and D_s indicate warning and sanction enforcement. The unemployment exit hazard to destination $l \in \{e, ne\}$ is then:

$$\theta_l(t_l|x, r, p, D_{wl}, D_{sl}, v_l) = \lambda_l(t_l)\exp(x'\beta_l + r'\alpha_l + p'\gamma_l + \delta_{wl}D_{wl} + \delta_{sl}D_{sl} + v_l) \quad (1)$$

$\lambda_l(t)$ stands for individual duration dependence in our proportional hazard model, x represents a vector of observable individual characteristics, r is a vector of public employment service dummy variables, p is a vector of controls for state dependence[16] and v_l represents the unobserved heterogeneity that accounts for possible selectivity in the exit process (see subsection 4.3 for the empirical specification of unobserved heterogeneity and Appendix D for a detailed description of the observables). The parameters δ_{wl} and δ_{sl} measure the effect that a warning and an enforcement have on the exit rate from UE. Note that δ_{sl} measures the additional effect of enforcement relative to the effect of a warning. A common approach to modeling flexible duration dependence is the use of a step function (piecewise-constant duration model)

$$\lambda_l(t_l) = \exp(\sum_k(\lambda_{l,k} \cdot I_k(t_l)) \quad (2)$$

where $k = 0, .., 3$ is a subscript for time-intervals and $I_k(t)$ are time-varying dummy variables that are one in subsequent time-intervals. Taking into account the shape of the descriptive hazards (see section 3.2) and the fact that for our Swiss data we observe median UE durations of a bit less/more than half a year for the exit to e/ne groups, we fix the four time intervals as follows: 1-40/1-90 days, 40-210/90-270 days, 210-360/270-480 days and 360/480 and more

[16] We control for the individual's labor market history over the past five years: past earnings, past employment. For details, see Appendix D.

days. Because estimation includes as well a constant term, normalization is necessary which is achieved by setting $\lambda_{l,0} = 0$ (i.e. the constant measures the baseline exit rate in interval 0).

In a similar way we can model the rate by which individuals are warned about a possible sanction and the rate by which a sanction is enforced at time t conditional on x, r, p and v as

$$\theta_h(t_h|x, r, p, v_h) = \lambda_h(t_h) \exp(x'\beta_h + r'\alpha_h + p'\gamma_h + v_h) \tag{3}$$

where for $h = \{w, s\}$, $\lambda_h(t_h) = \exp(\sum_k(\lambda_{h,k} \cdot I_k(t_h))$ with normalization $\lambda_{h,0} = 0$ and v_h representing the respective unobserved heterogeneity.[17]

Using the elements outlined above, this leads us to the following likelihood function (replacing the conditioning on x, r, v, p by an index i and suppressing notation on the treatments):

$$\mathcal{L} = \prod_{i=1}^{I} \int_v \theta_{w,i}^{c_w}(t_w) S_{w,i}(t_w) \theta_{s,i}^{c_s}(t_s) S_{s,i}(t_s) \theta_{e,i}^{c_e}(t_e) S_{e,i}(t_e) \theta_{ne,i}^{c_{ne}}(t_{ne}) S_{ne,i}(t_{ne}) \mathcal{L}_{p,i} \, dG(v) \tag{4}$$

where c_m ($m \in \{e, ne, w, s\}$) designates a censoring indicator, being 1 if the respective duration is not censored, and zero otherwise, and $S_{m,i}(t_m) \equiv exp(-\int_0^{t_m} \theta_{m,i}(z)dz)$ is a time-to-event specific "survivor" function, v is a vector of unobserved heterogeneity components (further discussed in section 4.3), and $G(v)$ is the corresponding cumulative joint distribution. Note that 4 accounts for both right-censoring and the competing risks nature of unemployment exits.

The most important element in (4) is $\mathcal{L}_{p,i}$ containing information on the individual likelihood contribution of the post-unemployment period. This element of our model varies, depending on which post-unemployment outcome we evaluate. In our baseline Model I, we set $\mathcal{L}_{p,i} = 1$ thus disregarding information on post-unemployment outcomes. In the following, we describe the Models II to IV that incorporate different measures of post-unemployment outcomes.

4.2 Modeling the post-unemployment outcome measures

4.2.1 Employment stability

Our *Model II* is designed to evaluate the effects of benefit sanctions on the *employment stability* in the post-unemployment period. We analyze the impact of being sanctioned or not on the duration of the first employment or nonemployment spell starting right after unemployment exit.

Following Figure 4, (non-censored) individuals enter into a spell of subsequent employment, described by the duration t_m, or into subsequent nonemployment, t_{nm}. Due to the fact that the SSR data we use are of monthly precision, we model the respective hazards in a discrete manner. The discrete hazards for t_o (with $o = \{m, nm\}$) can be represented as the difference

[17] Based on descriptive analysis of the duration distributions and hazards, duration splits to implement the piecewise-constant design are set to 30/90/240 days for the warnings hazard and 10/30/150 days. Note that enforcements usually take place already 10 to 20 days after the warning, therefore the early splits (see section 3.2 for descriptive details).

between two survivor functions of two consecutive months, be it $t_o - 1$ and t_o, divided by the survivor of the earlier month.[18] Thus, the discrete-time hazard is the probability of failure in the interval between two consecutive months, conditioned on the probability of surviving to at least the earlier month.

The corresponding likelihood contribution consists therefore in

$$S_o(t_o - 1|x, r, p, D_{wo}, D_{so}, t_u, v_o) - S_o(t_o|x, r, p, D_{wo}, D_{so}, t_u, v_o) \tag{5}$$

if the observation is not censored and in $S_o(t_o|x, r, p, D_{wo}, D_{so}, t_u, v_o)$ if censored. The survivors[19] are modeled in the same way as described in the last subsection. In the post-unemployment period, the treatment effect results in a constant upward or downward shift of the respective hazard.

Note that we control here as well for the realized duration of unemployment, t_u ($= \min(t_e, t_{ne})$). To allow for nonlinear unemployment duration dependence we add a polynomial function $g(ln\ t_u)$[20] to the controls. This implies for the complete likelihood functions – which describe the joint distribution of t_w, t_s, t_e, t_{ne}, t_m and t_{nm} – that we claim independence between the distributions of these durations *conditional on* x, r, p, D_w, D_s, the respective unobserved heterogeneity v and duration t_u in the case of the two post-unemployment processes.

Taking the two options of employment (m) or non-employment (nm) together, the individual likelihood contribution of the post-unemployment period (suppressing again the conditioning) is

$$\begin{aligned} \mathcal{L}_{p,i} &= \left[[S_m(t_m - 1) - S_m(t_m)]^{c_m} S_m(t_m)^{1-c_m}\right]^{c_e} \cdot \\ &\quad \left[[S_{nm}(t_{nm} - 1) - S_{nm}(t_{nm})]^{c_{nm}} S_{nm}(t_{nm})^{1-c_{nm}}\right]^{c_{ne}} \end{aligned} \tag{6}$$

Since these contributions are at the third stage of the selection (see Figure 4), double-censoring occurs. First, censored employment or non-employment durations (with c_m or c_{nm} equal zero) may occur since the post-unemployment observation window is restricted to the end of 2002. Second, uncensored unemployment spells with c_e or c_{ne} equal 1 are censored in the other exit destination and therefore as well in the respective post-unemployment process. Finally, in the case of a censored unemployment spell, c_e *and* c_{ne} are zero and $\mathcal{L}_{p,i}$ equals 1.[21]

[18] Note that we again assume that the hazard of leaving employment and the hazard of leaving non-employment have an MPH structure. This assumption is crucial for identification.

[19] Based on descriptive analysis of the duration distributions and hazards, duration splits to implement the piecewise-constant design are set to 5/10/24 months for the employment process and to 2/6/16 months for the non-employment process.

[20] We add polynomial terms of $ln\, t_u$ up to the sixth power.

[21] 19,149 of total 23,961 spells (i.e. 79.9%) exit from unemployment to employment ($c_e = 1$), 2985 (12.5%) exit to non-employment ($c_{ne} = 1$); 1827 (7.6%) exhibit censored unemployment durations. After exit, 42.5% and 34.9% of the respective populations are censored in their first employment/non-employment spell (i.e. $c_m = 0$ or $c_{nm} = 0$). These high censoring rates point to the fact that remarkable parts of the sample show stable labor force participation statuses after unemployment exit.

4.2.2 Post-unemployment earnings

Our *Models III and IV* feature *earnings* as an outcome measure in the post-unemployment period. We evaluate the effects of benefit sanctions on the earnings in the first (complete) month after unemployment exit and on the sum of earnings over the first 24 months after unemployment exit (y_1 and y_{24}, respectively). Thus, we generate measures that *incorporate* endogenous changes of the labor market status during the respective periods (see Klepinger et al. 2002 for a similar design). These outcome measures are global in the sense that they capture the effects of sanction warnings and enforcement on the duration of employment, on the level of wages, and on hours worked for individuals leaving unemployment.

We use an MPH structure to model the post-unemployment earnings distribution for at least two reasons. First, the MPH model structure is more flexible than assuming a specific parametric distribution – e.g., log-normality – by applying the same flexible hazard function design as for the durations above. Second, results from the duration literature show that the earnings hazard model is identified.[22] We extend this approach additionally in two respects: First, we use this multiple states hazard framework with earnings to evaluate a specific treatment. Accordingly, we introduce dynamic treatment effects in this context. Second, we handle the double selectivity problem that is implied by our framework: Selection at the *entry* into the two sanction states and at the *exit* from those states into (non-)employment.

The earnings hazard describes the (instantaneous) probability of earning y conditional on earning at least y. Thus, like the unemployment exit hazard, the earnings hazard has an upward-directed interpretation: the probability of generating an earnings level of exactly y conditional on earning at least y. What are the implications of assuming that the earnings hazard follow an MPH structure? In case earnings are exactly exponentially distributed, the MPH structure implies that both observed and unobserved characteristics change log expected earnings in an additive fashion – quite similar to modeling log earnings using linear models.[23] In case earnings are not exponential, assuming an MPH structure generally implies modeling proportionate shifts on the integrated earnings hazards. Moreover, it can be shown that assuming an MPH structure implies that the effect of benefit sanctions on mean earnings as well as on all the quantiles of earnings are of opposite sign as the effect on the hazard.[24]

[22] The idea to model wages, earnings or income in a hazard framework first appeared in Donald et al. (2000); Cockx and Picchio (2008) extended it by introducing competing risks, unobserved heterogeneity and state dependence.

[23] To see this, note that $E(T|x,v) = \lambda_0^{-1} exp(-x'\beta - v)$ where λ_0 is the baseline hazard.

[24] To see this, suppose that earnings without sanction are Y_0 with hazard $\theta_0(y|x) = \lambda(y)exp(x'\beta)$ and Y_1 follow a distribution with hazard $\theta_1(y|x) = \theta_0(y|x)exp(\delta)$ where δ is the effect of a benefit sanction on the earnings hazard. Since $E(T_1|x) = \int_0^\infty exp(-\int_0^y \theta_1(z|x)dz)dy$, it follows $E(T_1|x) < E(T_0|x) \iff \delta > 0$. Moreover, note that the α quantile treatment effect is $y_1^\alpha - y_0^\alpha = \Lambda_0^{-1}(-log(1-\alpha)exp(-\delta)) - \Lambda_0^{-1}(-log(1-\alpha))$ where $\Lambda_0^{-1}()$ is the inverse of the integrated hazard of the counterfactual earnings distribution. This means that $y_1^\alpha - y_0^\alpha < 0 \iff \delta > 0$ since $\Lambda_0^{-1}()$ is a monotonically increasing function. Finally, consider the log likelihood ratio of earnings with sanction and counterfactual earnings without sanction, i.e. $ln f_1(y|x)/f_0(y|x) = \delta - (exp(\delta) - 1)\Lambda_0(y)$. This shows that the likelihood ratio satisfies the monotone likelihood ratio property, and benefit sanctions shift the earnings distribution in the sense of first order stochastic dominance.

For the earnings data, we implement the estimation of sanction effects on earnings in the same way as in Model II one above – we just replace t_o by y_j, i.e. by one of the mentioned earnings measures (whereby $j = \{1, 24\}$). Since the earnings data are considered as being continuous we use continuous hazards. Depending on the descriptive hazards and medians of the respective measures, we define suitable splits of the earnings values to design the respective piecewise-constant earnings-level-dependence functions $\lambda_{y_j}(y_j)$[25].

The Model III results in an individual post-unemployment likelihood contribution (suppressing conditioning) of

$$\mathcal{L}_{p,i} = \left[\theta_{y_j}^{c_{yj}}(y_j) S_{y_j}(y_j)\right]^{c_e} \tag{7}$$

Model IV is very similar in the design – except that it uses different exit destinations. Going back to Figure 4, this means that at time T individuals are not separated by exiting to e or to ne as described in Model III, but the exit destinations are now $y_{24} > 0$ and $y_{24} = 0$. So, we separate individuals with a sum of earnings over 24 months which is positive from those with zero sum of earnings. The second group represents the part of the sample that permanently exits labor force over 24 months. The comparison of the Models III and IV allows interesting statements about the effect of sanctions on individuals who *temporarily* exit to nonemployment, thus who reenter labor force during the 24 months (i.e. the subgroup which has different exit destinations in the two models). See more on that comparison in the respective results subsection 5.3. Consequently, the likelihood contribution for Model IV has the same structure as the one for Model III:

$$\mathcal{L}_{p,i} = \left[\theta_{y_{24t}}^{c_{y24t}}(y_{24t}) S_{y_{24t}}(y_{24})\right]^{c_y} \tag{8}$$

where c_y represent the non-censoring indicator, being one if $y_{24} > 0$. Note that in the Models III and IV we estimate five processes. There is no sixth process here (like in Model II) since individuals exiting to nonemployment do not dispose of an earnings distribution[26].

As described for Model II, the post-unemployment process is again confronted with double censoring. First, c_{yj}/c_{y24t} can be zero for two reasons: earnings can't be observed over 24 months[27] after unemployment exit (since this was late in the observation window); in addition, earnings are right-censored at 10,000/200,000 CHF over 1/24 months due to the top coding of

[25] The earnings measure for the first month after unemployment (y_1) exhibits a median of 3,871 CHF for the group which exited from unemployment to employment (e). The earnings splits for y_1 are set to 1500/3000/4500 CHF. For earnings over 24 months – i.e. y_{24} – we find a median of 87,698 CHF for the e group. The median of y_{24} for all individuals with positive earnings sums over 24 months (Model IV, the $y_{24} > 0$ group) is 83,542 CHF. Since the descriptive earnings (y_{24}) hazards for the e and the $y_{24} > 0$ group in the Models III and IV are of a very similar shape, we apply the same earnings splits for these two models: They amount to 50000/100000/150000 CHF.

[26] In Model IV, this is true in general since we defined the exit destinations by distinguishing $y_{24} > 0$ vs. $y_{24} = 0$. In Model III, some individuals in the ne group have a positive earnings sum, those who only temporarily exited labor force – but not all.

[27] In the 1-month-case, there is no such censoring for y_1.

social security earnings. In our data, very small proportions had to be censored due to these reasons[28]. The second hierarchy of censoring (c_e/c_y) is the same as for Model II.

Note that we divide all the earnings measures by 1000, in order to avoid extreme value levels in estimation. Again, we condition on the unemployment duration by adding the polynomial $g(ln\, t_u)$[29] to the controls.

4.3 Dealing with multiple selectivity

Our evaluation setup implies that we have to deal with the issue of multiple selectivity. First, the sorting into the treatment is endogenous – the assignment of sanction warnings and enforcements is obviously non-random. Second, the exit from (treated or non-treated) unemployment into a state of employment or nonemployment (or $y_{24} > 0$ vs. $y_{24} = 0$ for Model IV) is driven as well by individual characteristics, thus by a non-random process. In both cases, we end up with a post-selection population that potentially differs from the original one: First, in terms of relative composition of individual characteristics; second, by observing only a non-random subpopulation in the subsequent stages (e.g., only those who found indeed a job). For observed characteristics, these composition and selection effects are controlled by the inclusion of covariates.

To take into account this multiple selectivity on the level of unobserved characteristics, we follow the approach of Gritz (1993) and Ham and LaLonde (1996). They point out that addressing the selection problem consists in *simultaneously* modeling the selection processes into the treatment and later into (non-)employment and in allowing for *correlation* between the different stages of the individual's history. The first point is met by the model presented above. The second is handled by allowing for correlation between the unobserved heterogeneity components of the different processes. For example, an individual who leaves unemployment for employment may have above average unobserved characteristics. This positive composition and selection effect (linked to the fact of having indeed found a job) may mask the potentially negative effect of a sanction on subsequent employment duration – if we don't control for the correlation in unobservables between the unemployment exit process and the subsequent employment process. Such arguments may be made for all our proposed models.

Combining such a design and our precise data, the effect of interest – the *causal* effect of benefit sanctions – can be separated from the discussed selectivity effects due to availability of information on the exact timing of the sanction process and the exit process. Causal effects of

[28] In Model III with y_1 earnings, 235 cases (of the 19,149 spells in the e group, i.e. 1.23%) are censored at 10,000 CHF. In Model III with y_{24}, 255 cases (1.33%) are censored due to non-observability and additional 468 cases (2.47%) are censored at 200,000 CHF. In Model IV, 278 cases (of the 20,012 spells in the $y_{24} > 0$ group, i.e. 1.32%) are censored due to non-observability and additional 478 cases (2.27%) are censored at 200,000 CHF.

[29] For Model III with y_1 estimation shows that none of the included log duration terms (up to 6th power) gets significant, whereas for the Models III and IV with y_{24} as outcome we find that all the included log duration terms get significant (at the 1 or 2% level). This interesting observation suggests that individuals with longer unemployment duration have a higher propensity to fall back into un- or nonemployment and therefore to realize a lower y_{24}, compared to people with shorter unemployment spell.

sanction warnings and enforcements on unemployment exit and the post-unemployment process create a conditional dependence between the five or six processes: i.e., the outcome measure changes *only* in the case a warning has been issued or a sanction has been enforced. On the other hand, selectivity creates a global dependence between the outcome and the sanction processes, captured by the correlation of the unobserved heterogeneity components.

In estimation we handle unobserved heterogeneity in the standard way by integrating it out over the joint density function $G(v)$, as shown in equation (4) above. The vector $v \in \mathbb{R}^6_+$ or $v \in \mathbb{R}^5_+$ comprises all the unobserved heterogeneity components of the respective model: In the Model II, $v = (v_w, v_s, v_e, v_{ne}, v_m, v_{nm})$, in the Models III and IV we replace the last two elements by v_{y1}, v_{y24} or v_{y24t}.

We model $G(v)$ to be a multivariate discrete distribution of unobserved heterogeneity. Work by Heckman and Singer (1984) suggests that discrete distributions can approximate any arbitrary distribution function. We assume that each heterogeneity component has two points of support (subscripts a and b). Given the six sources of unobserved heterogeneity in Model II and the five in the Models III and IV, this implies that the joint distribution has in maximum 64 or 32 mass points, respectively. The associated probabilities are of the form

$$Pr(v_w = v_{wg}, v_s = v_{sg}, v_e = v_{eg}, v_{ne} = v_{neg}, v_m = v_{mg}, v_{nm} = v_{nmg}) = p_i \quad (9)$$

$$Pr(v_w = v_{wg}, v_s = v_{sg}, v_e = v_{eg}, v_{ne} = v_{neg}, v_r = v_{rg}) = p_i \quad (10)$$

whereby expression (9) applies to Model II and expression (10) to the Models III and IV. In the latter case, we distinguish $r = \{y1, y24, y24t\}$. All unobserved heterogeneity level combinations with $g = \{a, b\}$ for each process are possible. This generates probabilities p_i for $i = 1, \ldots, I$ in Model II and for $i = 1, \ldots, 32$ in the Models III and IV. To ensure that the probabilities p_i are between zero and one, and sum to one, we model $p_i = exp(a_i)/\sum_i \exp(a_i)$ and normalize the last a as being $a_I = 0$. Note that we specify the correlated unobserved heterogeneity in a more flexible way than in Ham and LaLonde (1996), who rely on a one-factor structure, and most of the applications (e.g. Van den Berg and Vikström 2009 or Bonnal et al. 1997).

5 Estimation Results

We report in the following the results of the parameter estimates of the *Models I to IV* as described in the econometrics section 4. Then, we proceed to the analysis of the ex-ante effects. Thereafter, we discuss how we explain our findings from a theoretical point of view. The section ends with simulation exercises based on the reported estimation results, which allow to quantify the different treatment effects.

Tab. 1: The effect of benefit sanctions on exit behavior and subsequent non-/employment duration

	Model I			Model II		
(*Coeff./Transf.*)	Coeff.	z-value	Transf.	Coeff.	z-value	Transf.
Effect on exit from employment (M)						
warning (δ_{wm}/in %)				0.018	0.34	0.019
enforcement (δ_{sm}/in %)				0.140	2.35	0.150
Effect on exit from non-empl. (NM)						
warning (δ_{wnm}/in %)				0.146	1.14	0.157
enforcement (δ_{snm}/in %)				0.267	1.97	0.307
Effect on exit UE → E						
warning (δ_{we}/in %)	0.158	3.48	0.171	0.147	3.39	0.159
enforcement (δ_{se}/in %)	0.149	2.98	0.161	0.148	3.07	0.160
Effect on exit UE → NE						
warning (δ_{wne}/in %)	0.637	4.69	0.890	0.689	5.05	0.992
enforcement (δ_{sne}/in %)	0.515	4.10	0.674	0.513	4.05	0.670
Unobserved heterogeneity		Yes			Yes	
Control variables		Yes			Yes	
Control for state dependence		Yes			Yes	
PES dummies		Yes			Yes	
-Log-Likelihood		198309			255064	
N		23961			23961	

Notes: We report coefficients and their transformations: Transformed treatment effects are changes in %. Asymptotic z-values.
Source: Own estimations based on merged UIR-SSA database.

5.1 Unemployment Exit Behavior and Subsequent (Non-)Employment Stability

Table 1 provides information on the econometric estimates of Models I and II. *Model I* focuses on the effects of benefit sanctions on the exit behavior of concerned individuals, assuming correlated unobserved heterogeneity. Results of the point estimates of the treatment effects indicate that the log hazard rate of exits into employment (E) goes up by 0.158 once individuals get warned that they are under suspicion of having committed a non-compliance. Once the sanction is enforced, the exit to E rate increases by additional 0.149. Both effects are substantial and highly significant. Expressed in percentage changes (i.e. $exp(\delta) - 1$), results indicate that a sanction warning caused a 17.1 % increase in the exit to E rate, whereas actually imposing the sanction adds a further increase of the rate by 16.1 %.

But sanctions and warnings do not only foster a quicker take-up of a regular job, they also cause an increase in labor force exit. An announcement of a sanction leads to a remarkable rise in the exit to non-employment (NE) rate by 89.0 %. Enforcing the sanction results in an additional increment of the exit to NE rate by 67.4 %. This insight, that *the present and future disutility of a sanction (warning) influences the labor supply decision*, is new in the literature, to our knowledge. The (highly significant) effect is non-trivial: adding up the warning and enforcement effects amounts to more than doubling the exit to NE rate (+116 %). But one has to put this result in the right context of interpretation: First, by taking into account that "only" 12.5% of the sample exits to non-employment. Second, as shown below, exit to NE is often temporary and can partly be read as an unpaid prolongation of unemployment.

Estimates differ from the earlier studies by Abbring et al. (2005), van den Berg (2004), and Svarer (2007). The two Dutch studies report increases in the exit rate due to sanctions on the order of 100 %. Yet both Dutch studies do not have access to information on sanction warnings. As Lalive et al. (2005) show, this may lead to considerable upward bias in the estimate of the enforcement effect in a system like the Swiss where job seekers are informed of the sanction process starting. Svarer (2007) finds for Denmark an increase in the unemployment exit rate of yet more than 50% following enforcement. Our results are near to Lalive et al. (2005) who use a similar dataset. They find that warnings increase the hazard rate by 25 % and a further increase by 20 % is estimated to take place after benefits have been reduced for Swiss job seekers entering unemployment in late 1997. Some differences between the studies have to be taken into account: First, Lalive et al. (2005) do not have access to information on previous earnings. Arguably, previous earnings capture labor market success quite tightly leaving little room for unobserved heterogeneity. Second, the current study is using information on benefit sanctions covering a broader range of cantons in Switzerland than Lalive et al. (2005). To the extent that warnings and enforcement effects vary across Swiss regions, this also gives rise to differences in estimates. Third, the distribution of unobserved heterogeneity is more comprehensively estimated in this paper than in Lalive et al. (2005). Finally, endogenous selection of the exits into E and NE is explicitly taken into account in this study by modeling the exit to NE process, thereby allowing for correlated unobserved heterogeneity in this destination as well.

In the Appendix B, Table 7, we report additionally the baseline transition rates for all processes of Model I and Model II as well as the estimated mass point probabilities. Besides the estimated constant of the first piece of the baseline hazard (λ_1), we indicate the transition rate of an "average" individual (see notes of Table 7 for details) for the same first split period. Our estimates allow for two levels of unobserved heterogeneity in all four hazard rates. Starting from a restrictive specification with only a small number of mass points, we add more of them as long as they increase the log likelihood. As recommended by Gaure et al. (2007), we select the model that provides the best fit according to the log likelihood.

Finally, we take a look on the role of the unobserved heterogeneity in Model I. Estimating a version of the model without unobserved heterogeneity (not reported) reveals treatment effects of $\delta_{we} = -0.052/\delta_{se} = 0.082$ for the exit to E and $\delta_{wne} = 0.281/\delta_{sne} = 0.295$ for the exit to NE (significant on the 5% and the 1% level, respectively). The clear difference to the effects reported above points to the presence of unobserved heterogeneity. Moreover, the comparison of the models with and without (correlated) unobserved heterogeneity reveals a certain amount of selectivity[30].

[30] This can be explained as follows. Estimates indicate that there are 12 different groups. Is there selectivity with respect to exit to E and warnings? To see this, consider the average baseline exit rate of the groups that have a high warnings rate (probabilities p_1 up to p_8). The high warnings group has an average exit rate of about .15 % per day. The low warnings group (probabilities p_9, p_{11}, p_{13} and p_{16}) has an average exit rate of .18 %. This means

Now, we turn to *Model II* and the discussion of the quality of post-unemployment jobs: How do benefit sanctions affect the non-/employment stability? To answer this question, the duration of the first spell of employment (M) for the exit to E group and the duration of the first spell of non-employment (NM) for the exit to NE group is analyzed. Individuals of the E group who face a sanction warning are confronted with an immediate increase of the exit rate from the employment spell M by 1.9%. This change is not significant. In contrast, the additional treatment effect coming from imposing the sanction is highly significant and amounts to 15.0% for the M spells. The point estimate of the warning effect for the NE group on the NM spell is markedly higher, 15.7%, but not significant either. Again, the additional enforcement effect is significant; it results in a considerable increase of the NE hazard by 30.7%.

Thus, Model II reveals three important messages: First, and most importantly, we find *clear evidence that sanctions cause highly relevant effects on the individuals' outcomes after unemployment exit.* Second, estimates show that the sanction-driven reduction of unemployment duration for the exit to E group is paralleled by an also important reduction of the duration of the first employment period thereafter. I.e., sanctions reduce subsequent employment stability. Third, sanctions foster labor force exit of NE individuals, but also considerably reduce the subsequent stay in non-employment. Thus, these individuals have tendency to leave paid unemployment for unregistered unemployment in order to avoid pressures exerted by the sanction system and to "gain" more (unpaid) time for job search. The substantial NM treatment effect shows that this situation of subsequent non-employment is often of transitory nature. This is supported by the descriptive evidence that – whereas the median M spell counts 25 months – the median NM spell only amounts to 11 months.

In the post-unemployment period, unobserved heterogeneity plays a relevant role in shaping the treatment effects on the duration of the non-/employment spells. The corresponding version of Model II without unobserved heterogeneity (not reported) exerts sanction effects of $\delta_{wm} = 0.053/\delta_{sm} = 0.035$ for the E group and of $\delta_{wnm} = -0.094/\delta_{snm} = 0.141$ for the NE group. Except for the warning effect on the M spell (which falls from weak to no significance), all the effects go up once unobserved heterogeneity is taken into account. A certain amount of selectivity into the post-unemployment spells is present, too – mainly with respect to the enforcement of a sanction[31]. Finally, we may note that in Model II the exit to E and to NE treatment effects as well as the four transitions in the unemployment period are very similar to the corresponding estimates of Model I. This is a comfortable and sensible result since there is no obvious argument

that there is relatively small negative selection into warnings of individuals with lower exit rates. This explains why the warnings effect increases when accounting for unobserved heterogeneity. In the same manner, the group with high enforcement rates shows a bit lower exit rates (0.16 %) than those with a low/zero enforcement rate (0.18 %). This shows that there is a certain negative selection into enforcement and correspondingly we observe an increase in the treatment effect.

[31] Proceeding as in footnote 30 we find, when analyzing the M spells, that there is virtually no selectivity with respect to warnings: The group with high warnings propensity exerts an exit rate of 3.21% per month; the low warnings rate people transit out of M by 3.20% per month. In contrast, selectivity between enforcement and M exit is clearly negative: High enforcement rate individuals exit from M with 2.89% per month whereas no-enforcement people have an exit rate of 3.78%.

that adding post-unemployment information should crucially alter the estimation results for the unemployment processes.

To wrap up the results from analyzing the sanction effects on exit behavior and subsequent non-/employment stability, we may state that, besides the intended effect of reducing the time until unemployed take up a regular job, we find as well effects that were presumably not intended by the sanction system designers. The enforcement of a sanction causes a relevant reduction of subsequent employment. In addition, sanctions may as well foster labor force exit of concerned individuals. At least, there is a group among these for whom the exit to non-employment is only transitory, acting as a non-paid extension of the job search duration.

5.2 The Effects on Earnings and their Persistence

Tab. 2: The effect of benefit sanctions on earnings: over 1 vs. 24 months after unemployment exit; E (exit to employment) group

	Model III: earn 1 mt				*Model III: earn 24 mt*		
(*Coeff./Transf.*)	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Effect on earnings over 1/24 mt							
warning (δ_{wy1}/in %)	0.077	2.40	0.080	$\delta_{wy24}/\%$	0.102	3.27	0.107
enforcement (δ_{sy1}/in %)	0.050	1.18	0.051	$\delta_{sy24}/\%$	0.076	1.78	0.079
Effect on exit UE → E							
warning (δ_{we}/in %)	0.154	3.41	0.167		0.154	3.39	0.167
enforcement (δ_{se}/in %)	0.152	3.02	0.165		0.147	2.93	0.159
Effect on exit UE → NE							
warning (δ_{wne}/in %)	0.612	4.66	0.843		0.625	4.66	0.869
enforcement (δ_{sne}/in %)	0.522	4.16	0.686		0.518	4.12	0.679
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-Likelihood		231704				289436	
N		23961				23961	

Notes: We report coefficients and their transformations: Transformed treatment effects are changes in %. Asymptotic z-values.
Source: Own estimations based on merged UIR-SSA database.

The impact of sanction effects on the sustainability of post-unemployment jobs is one important aspect of an analysis of UI sanction systems that looks beyond unemployment exit. But in order to gain an even more comprehensive view on how a sanction system may influence post-unemployment job quality, the analysis of earnings is essential. A glimpse on the duration-dependent earnings histories of Figure 3 in the descriptive analysis may lead to the hypothesis that sanctions reduce subsequent earnings. But as mentioned as well, this analysis could be misleading since it doesn't incorporate the issue of selectivity. This problem is addressed in the *Models III* which feature simultaneous estimation of the sanctioning and unemployment processes together with the earnings process of the exit to E group, allowing for correlated unobserved heterogeneity in all the 5 processes.

Table 2 reports two versions of Model III: First, we analyze as outcome the earnings in the first (complete) month after exit to employment, i.e. for the E group. Second, we build the sum of realized earnings over 24 months as outcome in the fifth process (for the same E group). The comparison of the two sub-models of Model III allow statements on the *persistence* of the sanction effects in the development of the earnings flow. Whereas the first analysis gives insights on how the individual's reaction on a sanction (warning) is reflected in the take-up of the first job after unemployment, the second analysis aims for a comprehensive view on the total effect of sanctions on earnings generation in mid-terms for the E group. Thereby, the latter allows for and incorporates the effects of switches between employment and non-employment over the two years, directly or indirectly driven by previous sanctions.

How do sanctions affect earnings in the first month after leaving unemployment? Results from Table 2 clearly suggest a *negative effect*. Already the act of warning a job seeker that a sanction procedure has been started increases the earnings hazard by 8.0 % for job seekers who leave unemployment after having been warned that a benefit reduction may take place in the future. The earnings hazard increases somewhat more, albeit statistically insignificantly, for job seekers who experience an actual benefit reduction. Both effects translate into lower average earnings for sanctioned job seekers. We defer a discussion of the magnitude of the effects of benefit sanctions on average earnings to section 5.5.

Do these negative earnings effects persist over two years? Indeed, they do – they even accentuate. When looking at the treatment effect of a sanction warning on the level of the sum of earnings over 24 months, we clearly observe a negative effect. Warnings increase the 24 month earnings hazard by 10.7 %, and subsequent actual benefit reduction increases the earnings hazard by an additional 7.9% – significant at the 10% level. Therefore, we can clearly state that the Models III provide *evidence that sanction warnings and enforcements exert immediate as well as persistent negative effects on post-unemployment earnings.*

Estimations of the earnings Models III are affected much less by the inclusion of unobserved heterogeneity than Model II. Comparison with corresponding models without unobserved heterogeneity (not reported) reveals that unobserved heterogeneity only plays a (rather small) role in shaping the enforcement effect[32]. Selectivity into earnings is not relevant[33]. The small role of unobserved heterogeneity in this model is presumably due to the inclusion of extensive controls for state dependence into the model. Controlling for earnings and employment paths in the last

[32] The treatment effects estimates without unobserved heterogeneity for the earnings models over 1 and 24 months are the following: $\delta_{wy1} = 0.086/\delta_{sy1} = -0.036$ and $\delta_{wy24} = 0.106/\delta_{sy24} = 0.033$

[33] Analyzing the hazards of earnings over 24 months, we find that there is virtually no selectivity with respect to warnings which is of non-relevant size: The group with high warnings propensity has an earnings realization rate of 0.348% per 1000 CHF; the low warnings rate people leave earnings distribution by 0.350% per 1000 CHF. The same is true concerning selectivity with respect to enforcement: High enforcement rate individuals realize earnings with 0.349% per 1000 CHF whereas no-enforcement people have exactly the same rate of 0.349% per 1000 CHF. The non-existence of a selectivity issue here is supported by the observation that only 0.6% of the sample belongs to the b level of the earnings hazard. Thus, there is indeed almost no unobserved heterogeneity in earnings.

five years before unemployment seems to capture pretty well the heterogeneity in future earnings development as well. This is consistent with the long-term stability of earnings paths that we observed in the descriptive Figure 3.

Summing up, we can clearly state that sanctions not only negatively affect stability and duration of employment (of the E group), but as well the level of earnings that is generated from this employment after unemployment exit. This suggests that sanctions not only affect the search behavior by favoring more temporary jobs, but that *they cause the concerned individuals as well to reduce their demands towards future jobs in terms of earnings.*

5.3 The Effects on Earnings: Temporary vs Permanent Labor Force Exits

Tab. 3: The effect of benefit sanctions on earnings over 24 months: E group (excluding temporary and permanent labor force exits) vs. total population with positive earnings (excluding only permanent labor force exits)

	Model III: earn 24 mt				*Model IV: earn 24 mt*		
(*Coeff./Transf.*)	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Effect on earnings over 24 mt							
warning (δ_{wy24}/in %)	0.102	3.27	0.107	$\delta_{wy24t}/\%$	0.117	4.02	0.124
enforcement (δ_{sy24}/in %)	0.076	1.78	0.079	$\delta_{sy24t}/\%$	0.104	2.66	0.109
Effect on exit UE → E/Y							
warning (δ_{we}/in %)	0.154	3.39	0.167	$\delta_{wy}/\%$	0.181	4.33	0.198
enforcement (δ_{se}/in %)	0.147	2.93	0.159	$\delta_{sy}/\%$	0.211	4.55	0.235
Effect on exit UE → NE/0							
warning (δ_{wne}/in %)	0.625	4.66	0.869	$\delta_{w0}/\%$	0.830	2.59	1.294
enforcement (δ_{sne}/in %)	0.518	4.12	0.679	$\delta_{s0}/\%$	0.294	1.73	0.342
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-Likelihood		231704				294752	
N		23961				23961	

Notes: We report coefficients and their transformations: Transformed treatment effects are changes in %. Asymptotic z-values.
Source: Own estimations based on merged UIR-SSA database.

In a final step, we analyze *Model IV* – by comparing it to *Model III* – which features as well earnings over 24 months as outcome. But whereas Model III only focuses on earnings for job seekers who start earning immediately after leaving unemployment, Model IV adds those job seekers who temporarily leave the labor force. Thus, the key difference between the two models lies in the feature that individuals exiting first to non-employment and taking up a job later on are part of the analyzed earnings group in Model IV, whereas they are not in Model III. Table 3 reports the treatment effects on this total population with positive earnings and opposes them to the results of Model III with earnings over 24 months, which is reproduced here for convenience. The effects of announcing to an individual the start of a sanction investigation and of effectively imposing a temporary benefit reduction both are stronger in Model IV than in the corresponding Model III. A warning increases the earnings hazard by 12.4% whereas imposing the sanction leads in addition to an increase in the earnings hazard by 10.9%. What does the fact that warnings and sanctions exert a *higher* reductive effect on earnings in Model IV mean? This suggests that *individuals coming back from a transitory non-employment period after unemployment are faced with a stronger sanction effect in total over 24 months.* Thus, the additional non-paid time for job search doesn't allow them to get a job that is so much better that it would compensate the incurred additional earnings loss during the non-employment period. Exiting labor force to avoid sanction pressure is truly costly.

Note that the estimation of Model IV implies different competing risks destinations with respect to unemployment exit than the Models I to III did[34]. Here, we distinguish the exits to positive earnings over the 24 subsequent months versus the exit to *permanent* labor force exit over 24 months. Accordingly, the exit treatment effects and the four respective transition rates estimates may be different from the ones of the previous models. Indeed, they are – albeit not to large amount. The warning and enforcement effects on the two exit destinations are stronger (in the case of the permanent labor force exit group only when looking at the total effect). The higher increases in the respective hazard rates are sensible: The temporary labor force exit individuals who are now in the Y group contribute with their tendency to exit labor force (which is quantitatively higher as the exit to E effect, as we know from the previous models) to the now higher treatment effects.

The individuals in the permanent exit from labor force (0) group – a small group of 1122 people or 4.7% of the sample – seem to show an increased propensity to immediately leave registered unemployment once a sanction investigation is announced. Their expected value of finding a job in the future must have been very near to the value of leaving the formal labor market already before a sanction event occurred. Thus, once the disutility of being warned (with an increased expectation of being enforced in the future) materializes, the decision of these individuals tends to change towards an increased willingness to leave formal labor market.

5.4 Ex-ante Effects

Previous theory and evidence in the small UI sanctions literature pointed to the importance of *ex-ante effects* of benefit sanctions (see section 1). The mere "threat" of the presence of a sanction system may induce job seekers to behave more according to the search, job acceptance and

[34] But with respect to the presence of unobserved heterogeneity and of selectivity, the conclusion is broadly the same as for the Models III: Unobserved heterogeneity is virtually non-relevant. Only the enforcement effect increases a bit when taking it into account. The treatment effects for a model without unobserved heterogeneity (table again not reported) are $\delta_{wy24t} = 0.119/\delta_{sy24} = 0.065$. Selectivity into earnings is non-existent: High warnings rate people have an earnings realization rate of 0.413% per 1000 CHF whereas it amounts for those with low warnings rates to 0.416%. Individuals with high enforcement propensity exert an earnings realization of 0.414% per 1000 CHF, never-enforced individuals one of 0.412%. Again, the b level of unobserved heterogeneity in the earnings process covers as less as 1% of the sample, indicating virtually no heterogeneity (once controlled for state dependence).

Tab. 4: Ex-ante effects: Regression of PES-specific outcomes on monitoring/warning policy and unemployment rates by PES

	Model I		*Model II*		*Models III*		*Model IV*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	exit to E	exit to NE	empl	non-empl	earn 1 mt	earn 24 mt	earn 24 mt
	α_e	α_{ne}	α_m	α_{nm}	α_{e1}	α_{e24}	α_{e24y}
α_w	0.107*	0.030	0.137	0.148	0.031**	0.056*	0.054**
	(0.061)	(0.042)	(0.084)	(0.101)	(0.014)	(0.028)	(0.025)
UER	-0.254***	-0.004	0.021	-0.726***	-0.001	-0.021	-0.022
	(0.092)	(0.102)	(0.082)	(0.178)	(0.033)	(0.043)	(0.040)
Const	-2.246***	-1.882***	-0.022	-3.237***	-0.147	-0.186	-0.223
	(0.317)	(0.335)	(0.281)	(0.586)	(0.115)	(0.147)	(0.135)
N	52	52	52	52	52	52	52
R^2	0.323	0.009	0.228	0.403	0.096	0.155	0.163

Notes: OLS regressions, weighted by the population of the PES (registered unemployed during inflow period). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. α_w is averaged over the five estimated models in order to reduce measurement error. The alphas and the unemployment rates are in logs.

Source: Own estimations based on merged UIR-SSA database.

ALMP participation obligations imposed by unemployment insurance. The estimated Models I to IV allow us to investigate this kind of policy effect for the Swiss sanction system. In all the models, we estimated PES fixed effects for all the respective processes. The PES effects in the warning process, α_w, represent, presumably, a measure of *how strictly a certain PES office monitors and consequently warns.* Being the result of the very federalist way of policy implementation in Switzerland, these PES fixed effects – and PES-specific warning rates in general (as descriptive analyses show) – vary considerably. We exploit this variation to estimate the effect of monitoring strictness on the PES-specific level of the different outcomes. Since the regional labor market conditions could influence PES-specific sanction policy, we control in addition for the regional unemployment rates by PES (averaged over 1998 and 1999).[35]

Table 4, featuring the respective (population-weighted) OLS regressions, shows that ex-ante effects are in most of our estimated models a relevant issue. In the case of exit to employment, we find a significant ex-ante effect: When increasing monitoring intensity (measured as the PES-specific log warnings rate) by one standard deviation (0.887), the PES-specific log exit to E rate increases by 0.095 or a quarter of a standard deviation. Moreover, for the ex-ante effect we find a tradeoff that is very similar to the ex post effect. While higher warnings rates increase the probability of leaving unemployment for employment, they tend to reduce post unemployment

[35] Note that accounting for regional unemployment rate is important for transitions from paid and unregistered unemployment to employment suggesting that this rate captures key differences in labor demand across Swiss PES.

earnings. A one standard deviation increase in warnings, increases the earnings hazard by 2.7 % in the first month after leaving unemployment suggesting that non-sanctioned job seekers leave unemployment for jobs that are paid worse or that offer shorter hours. Moreover, a one standard deviation increase in warnings increases the earnings hazard in the first two years after leaving unemployment by 4.9 % – even though sanction warnings do not appear to reduce employment stability to a significant extent.[36] This suggests that job seekers are locked into jobs of worse quality. Interestingly, the sanction policy is not relevant for those leaving unemployment for non-employment suggesting that those who have tendency to extend unemployment duration by leaving for temporary non-employment do not yet react on the mere "threat" of a stricter sanction policy.

5.5 Quantifying the effects of benefit sanctions

The key result of the empirical analysis is that sanction warning and enforcement speed up exit from registered unemployment thereby increasing post unemployment earnings due to earlier start on the job. However, sanction warnings and enforcements also reduce the level of post-unemployment earnings. How do these two effects on post unemployment earnings add up?[37] We provide two sets of simulations on the effects of sanctions on earnings in a two year period after leaving unemployment. Note that we focus on post unemployment earnings rather than post unemployment income.

The first set of simulations provides information on the *ex post effects* of benefit sanctions. The simulation compares the actual pattern of leaving unemployment and post unemployment earnings with counterfactual unemployment exit and post unemployment trajectories. Actual and counterfactual trajectories only differ with respect to the post warning unemployment experience. Whereas the actual trajectory imposes our estimates of the warning and enforcement treatment effects from Model IV, the counterfactual scenario sets these treatment effects to zero (see appendix section B for further details).[38] Note that all simulations fully take the competing risks nature (exits to paid post unemployment vs exits to unpaid post unemployment) of the exit destination into account.

Table 5, panel A provides the results. Actual time in unemployment until an exit with at least some earnings in the two year period after leaving unemployment lasts for 244 days. Counterfactual time to leaving unemployment is 277 days. Thus, sanction warning and enforcement

[36] Note, however, that the effect of warnings on the rate of leaving employment is quantitatively importance and it is very near to significant at the 10 % level. Using a model that population-weights inside each canton - i.e. stressing the inter-cantonal variation – this effect becomes significant too.

[37] Note that we discuss effects on *earnings* rather than on income to isolate the mechanical effects of sanctions (i.e. unemployment benefit reduction) on income from the behavioral effects of sanctions on income. Moreover, we completely abstract from discounting of future pay reductions which tends to bias our results in the negative direction. Finally, we do not address general equilibrium effects of sanctions, as discussed in Boone et al. (2007).

[38] Note that we take both the warnings effect and the enforcement effect into account because warning without enforcing is not a policy option. We simulate an enforcement date for those job seekers who leave unemployment before the enforcement date by assuming their benefits are reduced at the median time from warning to enforcement.

Tab. 5: Simulations: Effects of sanctions on expected earnings and unemployment durations

		Expected earnings/ duration (CHF/days)
A: Ex-post effects (on the sanctioned)		
… *on post-unemployment earnings (Y group)*		$E(Y24)$
with sanction		71943.58
without sanction		78113.38
$ATET_{Y24}$: $E(Y24^1 - Y24^0 \mid D=1)$		**-6169.80**
… *on duration until leaving unemployment for Y*		$E(T)$
with sanction		243.80
without sanction		277.23
$ATET_{T_y}$: $E(T^1 - T^0 \mid D=1)$		**-33.43**
Trade-off: in days of lost earnings (with sanction)		$E(T)$
$E(ATET_{Y24,i})$		**-62.83**
$E(Tradeoff_i)$	**net loss**	**-29.40**
… *on duration until leaving unemployment for 0*		$E(T)$
with sanction		309.09
without sanction		343.37
$ATET_{T_0}$: $E(T^1 - T^0 \mid D=1)$		**-34.28**
B: Ex-ante effects (on everyone, non-sanctioned)		
… *on post-unemployment earnings (Y group)*		$E(Y24)$
under intensified warning policy		83200.79
under actual warning policy		84683.60
$ATET_{Y24}$: $E(Y24^1 - Y24^0 \mid D=1)$		**-1482.81**
… *on duration until leaving unemployment for Y*		$E(T)$
under intensified warning policy		193.34
under actual warning policy		202.84
$ATET_{T_y}$: $E(T^1 - T^0 \mid D=1)$		**-9.49**
Trade-off: in days of lost earnings (under intensified warning policy)		$E(T)$
$E(ATET_{Y24,i})$		**-13.47**
$E(Tradeoff_i)$	**net loss**	**-3.98**
… *on duration until leaving unemployment for* 0		$E(T)$
under intensified warning policy		269.69
under actual warning policy		280.62
$ATET_{T_0}$: $E(T^1 - T^0 \mid D=1)$		**-10.93**

Notes: Simulation is based on actual sanction histories; see Appendix B for details. Treated group = at least one warning. Tradeoff: Mean of individual tradeoffs which represent the difference between $ATET_{T_y,i}$ and $ATET_{Y24,i}$ in days of lost earnings with sanction; note that the earnings loss, $ATET_{Y24,i}$, is reduced by $ATET_{T_y,i}$ days since the comparison period for the non-sanctioned/actual warning regime is $ATET_{T_y,i}$ days longer than for the sanctioned/intensified warning regime. $Y/0$=positive/zero earnings over 24 months after unemployment.
Source: Own calculations from merged UIR-SSR database.

reduce job search duration by 33 days or a bit more than 1 month. Clearly, reduced unemployment duration implies earlier exit to paid post unemployment. But is one month of earlier exit enough to undo the reductions in post unemployment earnings? Earnings simulations indicate that individuals who are sanctioned have, on average, post unemployment earnings of 71,944 CHF in the two years after unemployment. In contrast, had they not been sanctioned, they would have earned 78,113 CHF in a period of two years. This means that post unemployment earnings have been reduced by 6,170 CHF or by 8.6 % compared to earnings with a sanction or about 63 days of pay with a sanction. On net, this means that while sanctioned individuals gain about one month of pay, they lose the equivalent of two months of earnings with sanction. How about individuals who leave unemployment to non-employment? Actual time to leaving unemployment is 309 days, whereas the counterfactual duration is 343 days, or 34 days shorter (reduction of 10 %).[39] Yet since the labor earnings of individuals who leave to non-employment are zero, earlier exit to unpaid post unemployment does not affect post unemployment earnings.

The second set of simulations provides information on the *ex ante effect*. Here, we first simulate actual time to paid and unpaid post unemployment, as well as subsequent earnings in the former case, for *all* job seekers using *actual* estimates of the PES dummies in the respective exit and earnings processes. We then ask, how much earlier job seekers would leave unemployment if PES were asked to *increase their warning intensity to a minimum standard*, and what effect that would have on the earnings thereafter. We set this minimum standard equal to the mean estimated warnings intensity plus one standard deviation of the estimated PES dummies. This means that PES with estimated warnings intensities below that level are required to increase warnings intensity while PES which already fulfil that minimum standard will face no adjustment. How does this affect the hazards of leaving unemployment and generating earnings thereafter? We use estimates of the ex ante effects in Table 4 to assess how changes in warning rates translate into changes in exit rates and earnings hazards.

Results indicate that job search until leaving for paid post unemployment lasts for about 203 days (Table 5 panel B). With increased warnings intensity, job search would last for 193 days. Thus, job search is reduced by about 10 days due to the ex ante effect. In contrast, leaving unemployment earlier due to more strict warning also leads to earnings reductions. Whereas job seekers earn 84,684 CHF in the two years after leaving unemployment in the actual situation, their incomes would be reduced to 83,201 CHF or 1,483 CHF (1.8 % of actual earnings) in the counterfactual situation with more intense warning. This means that, in the intensified warning regime, leaving unemployment earlier by 10 days is associated with an earnings loss that is equivalent 13 days of full pay. Interestingly, in contrast to our finding for the ex post effects, the ex ante effects on leaving unemployment and post unemployment earnings roughly balance for those individuals who leave unemployment for paid post unemployment situation. But one has

[39] Interestingly, whereas the treatment effects on the hazard of leaving unemployment for unpaid post unemployment are much larger than the treatment effects of leaving unemployment for paid post unemployment, the treatment effects on expected duration are very similar. This is due to the fact that the (log) hazard of leaving unemployment for unpaid post unemployment is much lower than the hazard of leaving unemployment for paid post unemployment. Thus, while the relative effect on the hazard is indeed much larger for exits to unpaid post unemployment, the changes in the hazard rates and durations are much more similar.

Tab. 6: Simulations: Proportions by unemployment exit destinations

A: Ex-post effects (on the sanctioned)

	Exit to Y	Exit to 0
With sanction	0.8929	0.1085
Without sanction	0.8774	0.0676

B: Ex-ante effects (on everyone, non-sanctioned)

	Exit to Y	Exit to 0
Under intensified warning policy	0.8964	0.0612
Under actual warning policy	0.8758	0.0720

Notes: Simulation is based on actual sanction histories. Calculation of proportions is based on integrated densities; for details, see Appendix B. Treated group = at least one warning. Y/0=positive/zero earnings over 24 months after unemployment.
Source: Own calculations from merged UIR-SSR database.

to take into account that this rather small net ex ante effect of 4 days of loss concerns *everyone* of the leavers to paid post unemployment, i.e. 89.3% of the Y group (see Table 6, panel A).

How about leaving unemployment for non-employment? Average duration until exiting for unpaid post unemployment is about 280.6 days. With increased warnings intensity two things happen. On one hand, the propensity of leaving unemployment for paid post unemployment increases, whereas the rate of leaving unemployment for unpaid post unemployment decreases. The net effect of these two countervailing effects turns out to be negative, i.e. with increased warnings intensity time to exit from unemployment decreases by 10.9 days to 269.7 days. Again, the earnings situation of individuals leaving for unpaid post unemployment does not change since there are no post unemployment earnings.

Based on the simulations, we can calculate the proportions of individuals leaving for the two possible exit destinations (Y and 0). These proportions, shown in Table 6, support the observation from above about countervailing effects in the 0 group. Under actual warning, 7.2% of the job seekers exit to unpaid post unemployment (panel B), whereas under the intensified warning policy only 6.1% exit to 0. The opposite is the case for exiting to Y. This highlights the mechanism of reaction on the policy change in the 0 group: Due to intensified warnings, some job seekers now rather exit to paid post unemployment instead of entering the unpaid as they would in the status quo. Thus, an intensified warning policy brings some individuals back to reentering labor market. This is, over the whole, not the case for the ex post effects (panel A): Being sanctioned leads to some more entries into Y, but the proportion of exits to 0 increases even more[40].

[40] Thus, what appears less often in the sanctioned case, are the long, censored durations.

5.6 What explains our findings?

Job search theory provides a convenient framework for understanding our results on the effects of benefit sanctions.[41] There are two behavioral responses of unemployed workers to benefit sanctions. First, they might increase *search intensity*. Second, sanctions could make them lower their demands concerning post-unemployment jobs, i.e. reduce their *reservation wage*. Benefit sanctions affect behavior because they reduce the value of being unemployed. Two effects may be distinguished. The first effect is the *ex-post effect*, the effect that a benefit reduction increases costs of being unemployed thereby changing the behavior of the unemployed. However, unemployed may already change their behavior in anticipation of a benefit sanction, to avoid getting one imposed. This second effect is the *ex-ante effect*, the effect that the *risk of getting a benefit sanction* influences behavior as well.

Both increased search intensity and lower reservation wages lead to a reduction of unemployment duration. But how will benefit sanctions affect post unemployment earnings and job stability? From a theoretical point of view, increased search intensity could lead to a post-unemployment job that is at least as good as the job that would have been found without a sanction. However, to the extent that a reduction of the reservation wage leads to acceptance of lower quality jobs, wage loss and reduced job duration may be expected. Thus, theoretical predictions are *inconclusive* concerning post-unemployment sanction effects. It is up to an *empirical evaluation* to establish which effects dominate in practice.

Moreover, the effects of warnings and of enforcing the benefit sanction may differ if job seekers search for jobs of different quality. Job seekers who receive a warning letter know that the probability of a benefit reduction has substantially increased but they continue to receive the same benefits. In contrast, job seekers who receive the information that their benefits are cut experience a strong, temporary reduction in the stream of benefits received. Differences in the effects of a warning and the effects of an actual benefit reduction may be related to the quality of jobs workers are looking for. Suppose there are two types of jobs; "good" jobs referring to full-time permanent positions and "bad" jobs referring to part-time and/or temporary positions. Job seekers entering unemployment will be searching for good jobs while disregarding bad jobs. Receiving the warning letter decreases the value of remaining unemployed. This will increase intensity of searching for good jobs while leaving unaffected intensity of searching for bad jobs. Seeing the benefits actually reduced decreases the value of staying unemployed more substantially leading job seekers to search for bad jobs as well as for good jobs. So, warnings may have different effects from actual benefit reductions with respect to the quality of jobs accepted. The fact that warnings do, ex post, not reduce employment stability but enforcements do, could

[41] See Boone and Van Ours (2006) and Boone et al. (2007) for recent analyses of this issue in the labor market context. It is shown that from a welfare point of view it may be optimal to introduce monitoring and sanctions into the system of unemployment insurance. In Becker's (1968) theory with risk neutral agents the social loss from offenses would be minimized by setting fines high enough to eliminate all offenses. If unemployed workers are risk averse this result may not hold for the labor market and a combination of intensive monitoring and small fines may be the optimal outcome.

theoretically be explained *distinguishing between search for a temporary vs a permanent job.* The key idea is that job seekers may not search for temporary jobs until they experience actual benefit reductions[42]. This would explain why sanction warnings have no effect on employment stability whereas benefit reductions clearly shorten post employment spells. In Appendix A, we outline this theoretical explanation more in detail.

Our findings for the ex post effects of benefit sanctions suggest that, consistent with job search theory, benefit warnings and reductions increase the rate of leaving unemployment. Yet, there is also a significant reduction in post unemployment earnings possibly because of lower reservation wages. On net, the positive effects of leaving unemployment more quickly do not outweigh the negative effects of benefit sanctions. In terms of ex ante effects, we find that job seekers who are confronted with higher warning probabilities leave unemployment more quickly. Yet again, faster exit from unemployment is accompanied by lower earnings leading to a net reduction in post unemployment earnings. Regarding warning and enforcement effects, we find that while mere warnings increase the rate of leaving unemployment, they do not affect employment and non-employment durations. In contrast, actual benefit reductions do not only lead to a faster exit from unemployment but they also tend to reduce the duration of employment.

The clear *persistence* of negative sanction effects on earnings up to two years after unemployment exit may be explained by *lock-in into the accepted job or by faster return to unemployment.* Once the individual has accepted a lower-quality-job, it may be difficult for him/her to catch up with the non-sanctioned people by quickly changing to a better job. Moreover, individuals who accept a worse paid job are more likely to leave this job and return to unemployment. Both lines of reasoning explain why sanctions lead to a reduction in post unemployment earnings.

6 Conclusions

Activating unemployed workers through the introduction of a system of benefit sanctions may be relatively cheap and effective in bringing unemployed back to work more quickly. However, a comprehensive policy evaluation of a system of benefit sanctions should not only consider direct effects in terms of reduced unemployment durations and reductions in benefit payments, but also consider indirect effects in terms of employment stability, earnings and attachment to the labor market. This is what we do in our study using a rich set of Swiss register data. We present one of the first empirical studies that looks beyond unemployment exits providing a comprehensive evaluation of benefit sanctions.

In terms of ex post effects, we find that both warnings and actual enforcement of benefit sanctions increase the unemployment exit rate. Whereas warnings do not affect the duration of subsequent employment they have a persistent negative impact on post-unemployment earnings. Enforcement of benefit sanctions reduces the quality of post-unemployment jobs both in terms of

[42] Our theoretical explanation in Appendix A comprises as well an alternative set-up where the unemployed search for a bad job with low(er) intensity already before the enforcement of a sanction, but increase search for these jobs relatively more thereafter. See footnote 47 for details.

job duration as well as in terms of earnings. We also find evidence of benefit sanctions increasing exits out of the labor market. In terms of ex ante effects, we find that stricter monitoring of job search leads to faster exit from unemployment but also reduces post unemployment earnings while leaving employment durations unchanged.

Benefit sanctions not only reduce unemployment durations but also reduce post-unemployment employment duration and earnings. As for the financial consequences there is a tradeoff between the positive effect of finding a job sooner rather than collecting unemployment benefits for a longer period of time, and the negative effect of finding a less well-paid job with a shorter duration. Using our estimation results we are able to quantify this tradeoff. We show that over a period of two years following the exit from unemployment, the net effect of benefit sanctions is negative. For sanctioned workers, the loss in earnings is in the order of two months whereas the gain from shorter unemployment duration is about one month. We also find substantial ex ante effects: Increasing monitoring and thus the warning intensity to a minimum standard, which lies one standard deviation above the mean, reduces unemployment duration with 10 days and also reduces post-unemployment earnings. The net income effect amounts to a loss of 4 days of earnings, a small effect compared to the ex post effect of benefit sanctions. A further, interesting observation is that an intensified warning policy may reduce labor force exits. Taken together, these results indicate that increased monitoring harms post-unemployment earnings substantially less than actually imposing benefit sanctions.

Turning to policy options, recall that benefit sanctions in the Swiss system entail full reduction of unemployment benefits. We show that these full reductions in unemployment benefits lead to substantially lower post unemployment earnings. Moreover, we show that increased monitoring is effective in generating incentives to leave unemployment without inflicting a large post unemployment penalty on job seekers. Taken together, these results suggests that an alternative policy could be constructed that preserves search incentives but moderates the post unemployment consequences of benefits sanctions: a system with increased monitoring of search behavior but decreased penalties in case of non-compliance.

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Appendices

A. Benefit sanctions and the quality of post-unemployment jobs – theoretical notions

The Swiss data allow us to make a distinction between warnings and enforcement of benefit sanctions. Furthermore, the data contains information about the quality of post-unemployment jobs. To illustrate how benefit sanctions may affect the quality of post-unemployment jobs we extend the benefit sanctions part of the search-matching model of Boone and Van Ours (2006) accordingly.[43] Workers are assumed to be risk-neutral and cannot save; hence they consume all their income each period. This assumption rules out the possibility that agents save to insure themselves against the loss of income due to unemployment. Once a worker becomes unemployed, he receives an unemployment benefit that is constant over the unemployment spell unless a benefit sanction is imposed in which case the benefits are canceled. Workers have only one instrument of search, their search intensity.[44] Different from Boone and Van Ours (2006) we introduce two sanction "states": the warning state and the enforcement state. Thus there are three types of unemployment: unemployment without benefit sanctions ($u1$), unemployment with a warning ($u2$) and unemployment with sanctions imposed ($u3$). Also different from Boone and Van Ours (2006), to investigate the relationship between benefit sanction and the quality of post-unemployment jobs we introduce two types of jobs: temporary and permanent jobs. So there are two types of employment, permanent ($e1$) and temporary ($e2$). The jobs pay the same wage w and differ only in the job destruction rate $\delta_1 < \delta_2$.[45]

Unemployed workers receive unemployment benefits b, with $b \leq w$ being the replacement rate. Unemployed workers are looking for job offers and as soon as they get one they will accept it. Thus the unemployed have only one instrument of search, their search intensity. An unemployed worker is assumed to search for both types of jobs with search intensities $s_1 \geq 0$ and $s_2 \geq 0$. The disutility of searching at intensity s equals $\gamma(s)$, such that $\gamma(s_1) = \frac{1}{2}\gamma s_1^2$ and $\gamma(s_2) = \frac{1}{2}\gamma s_2^2$, with $\gamma > 0$. So the disutility of search increases with the search intensity with an increasing marginal disutility.

The search for the jobs generates flows of job offers, which follow a Poisson process with arrival rate $\mu_1 s_1$ and $\mu_2 s_2$. The arrival rates of job offers consist of two parts, one part (μ_1 and μ_2) is determined by the state of the labor market i.e. the number of vacancies and unemployed and the other part (s_1 and s_2) is determined by the optimizing behavior of the unemployed worker.

[43] We ignore wage bargaining, vacancy creation, matching of unemployed and vacancies and payment of benefits/taxes. Thus we focus on the behavior of unemployed workers and how this is affected by benefit sanctions.

[44] Note that we could introduce two margins of search, search intensity and replacement rate. This would complicate matters a lot with no obvious advantages. One could even argue that reservation wages are already at the lower end of the wage distribution.

[45] Note that the introduction of two wages would be straightforward, for example $w_1 > w_2$. This would not change the results very much except for allowing for the possibility that some post-unemployment jobs pay less than others. Now the main difference between the two jobs is that one doesn't last as long as the other. Therefore, in expectation the earnings – taking into account that the wage is paid over a shorter time period – are lower.

Unemployed without a benefit sanction are monitored and they face the risk of receiving a warning if they search less than required. The monitoring intensity is ϕ_1, and the required intensity of search equals λ. Workers will never search more than required: $s_1 + s_2 \leq \lambda$.

Now the following Bellman equation can be derived for the unemployed workers without a benefit sanction, with V_{u1} denoting the expected discounted vale of being unemployed without a benefit sanction:

$$\rho V_{u1} = \max_s\{b - \gamma(s) + \mu_1 s_1(V_{e1} - V_{u1}) + \mu_2 s_2(V_{e2} - V_{u1}) + \phi_1(\lambda - s_1 - s_2)(V_{u2} - V_{u1})\} \quad (11)$$

where V_{e1} is the value of being employed with a permanent job, V_{e2} is the value of being employed with a temporary job, V_{u2} is the value of being unemployment with a sanction warning and ρ is the discount rate. The flow value of unemployment without benefit sanctions consists of two parts: the flow of utility during unemployment (utility of benefits minus search costs) and the expected flow of additional income after the job is found. The optimal search intensities follow directly from differentiating equation (11):

$$s^*_{11} = [\mu_1(V_{e1} - V_{u1}) + \phi_1(V_{u1} - V_{u2})]/\gamma$$
$$s^*_{12} = [\mu_2(V_{e2} - V_{u1}) + \phi_1(V_{u1} - V_{u2})]/\gamma$$

with s^*_{11} (s^*_{12}) representing the optimal search intensity for type 1 (type 2) jobs in unemployment state 1. So, the optimal search intensity increases with the difference between the values of employment and unemployment without benefit sanctions, the monitoring intensity and the difference between the value of unemployment without benefit sanctions and unemployment with a sanction warning. Furthermore, optimal search intensities are higher when search costs are lower and more job offers arrive. Also note that if there was no system of benefit sanctions the optimal search intensities would be lower with for example $s^{**}_{11} = \mu_1(V_{e1} - V_{u1})/\gamma \leq s^*_{11}$. The differences $s^*_{11} - s^{**}_{11}$ and $s^*_{12} - s^{**}_{12}$ represent the ex ante effect of benefit sanctions.

The Bellman equation for the unemployed workers with a sanction warning:[46]

$$\rho V_{u2} = \max_s\{b - \gamma(s) + \mu_1 s_1(V_{e1} - V_{u2}) + \mu_2 s_2(V_{e2} - V_{u2}) + \phi_2(\lambda - s_1 - s_2)(V_{u3} - V_{u2})\} \quad (12)$$

where ϕ_2 is the monitoring intensity in unemployment state 2 ($\phi_2 \leq \phi_1$) and V_{u3} is the value of unemployment in the sanction state. The optimal search intensities can again be found by differentiating equation (12):

$$s^*_{21} = [\mu_1(V_{e1} - V_{u2}) + \phi_2(V_{u2} - V_{u3})]/\gamma$$
$$s^*_{22} = [\mu_2(V_{e2} - V_{u2}) + \phi_2(V_{u2} - V_{u3})]/\gamma$$

Note that the differences $s^*_{21} - s^*_{11}$ and $s^*_{22} - s^*_{21}$ represent the ex post effect of a warning. Finally,

[46] Now, we don't introduce a perceived penalty of receiving a warning. we could introduce psychological costs or disutility but I think it is nicer to have just the increased monitoring intensity "doing the job".

the Bellman equation for the unemployed workers with a sanction enforced:

$$\rho V_{u3} = \max_s\{-\gamma(s) + \mu_1 s_1(V_{e1} - V_{u3}) + \mu_2 s_2(V_{e2} - V_{u3})\} \tag{13}$$

where the penalty imposed is equal to the benefits. We assume that unemployed with a benefit sanction are no longer monitored because their benefits are equal to zero. Once again, the optimal search intensities can be found by differentiating equation (13):

$$s^*_{31} = \mu_1(V_{e1} - V_{u3})/\gamma$$
$$s^*_{32} = \mu_2(V_{e2} - V_{u3})/\gamma$$

Note that the differences $s^*_{31} - s^*_{11}$ and $s^*_{32} - s^*_{12}$ represent the ex post effect of the imposition of a benefit sanction. For the employed workers the following Bellman equations hold:

$$\rho V_{e1} = w + \delta_1(V_{u1} - V_{e1}) \tag{14}$$
$$\rho V_{e2} = w + \delta_2(V_{u1} - V_{e2}) \tag{15}$$

These equations says that the flow value of being employed for a worker equals the utility from the wage he receives each period plus the rate in which the match is dissolved, in which case he becomes unemployed and receives V_u instead of V_{e1} or V_{e2}. Now, if the following inequality holds:

$$V_{e1} > V_{u1} > V_{u2} > V_{e2} > V_{u3} \tag{16}$$

workers will initially only search for jobs of type 1. Receiving a warning will induce them to search with a higher intensity for jobs of type 1, but they will still not look for jobs of type 2. Only once they get a benefit sanction imposed will they start looking for jobs of type 2. Then, their average expected job duration will be lower because now they start accepting temporary jobs.[47]

B. Simulations

B1. Ex post effects

We simulate the ex post effect of a benefit sanction as follows. First, we look at earnings over 24 months after unemployment exit as outcome. Let $\theta^{D_w,D_s}_{y24}(t|x,v)$ denote the earnings hazard, depending on sanction warning status D_w and sanction enforcement status D_s. The density of earnings realizations (for the group of individuals with positive medium run earnings) is

$$f^{D_w,D_s}_{y24}(y|x,v) = \theta^{D_w,D_s}_{y24}(y|x,v)S^{D_w,D_s}_{y24}(y|x,v).$$

[47] Note that in this set-up only unemployed with a benefit sanction would search for a temporary job. Alternatively we could have: $V_{e1} > V_{e2} > V_{u1} > V_{u2} > V_{u3}$. Then, unemployed initially search with a lower intensity for jobs of type 2. Due to the convexity of the search costs function, at the points in time when they get a sanction warning and a benefit reduction, they will increase both search intensities, but relatively more for jobs of type 2.

Based on this density, we can compute the expected earnings as follows:

$$E(y|x, v, D_w, D_s) = \int_0^{199} y\, f_{y24}^{D_w,D_s}(y|x, v)dy + \left[1 - \int_0^{199} f_{y24}^{D_w,D_s}(y|x, v)dy\right] \cdot 200 \tag{17}$$

whereby y is earnings in 1000 CHF. The second term of the equation (17) above accounts for the high earnings censored at 200,000 CHF. In the treated case, i.e. with both sanction warning and enforcement imposed, we set $D_w = 1$ and $D_s = 1$. This amounts to increasing the earnings hazard in (17) by the estimated treatment effects δ_{wy24t} and δ_{sy24t} over the whole support. In the non-treated counterfactual, equation (17) is evaluated at $D_w = 0$ and $D_s = 0$. The difference between these two mean earnings results in the ex post effect. Note that we simulate first conditional on unobserved heterogeneity and then we integrate unobserved heterogeneity out.

Now, secondly, we describe the simulation of the unemployment durations, separated by the two exit destinations. Let $\theta_y^{D_w,D_s}(t|x, v)$ denote the transition rate from unemployment to positive income y, depending on sanction warning status D_w and sanction enforcement D_s status. Also, $\theta_0^{D_w,D_s}(t|x, v)$ is the transition rate from unemployment to no medium run earnings. The density of unemployment spells ending in a transition to y is

$$f_y^{D_w,D_s}(t|x, v) = \theta_y^{D_w,D_s}(t|x, v)S_y^{D_w,D_s}(t|x, v)S_0^{D_w,D_s}(t|x, v),$$

i.e. the proportion having survived without exit until t, making a transition to a job at time t. The density of unemployment spells ending in a transition to 0 is defined in an analogous manner.

We can now calculate the proportion of individuals making a transition to a paid job between time 0 and time c. This amounts to summing up transitions occurring at times between 0 and c, i.e.

$$F_y^{D_w,D_s}(c|x, v) = \int_0^c f_y^{D_w(t),D_s(t)}(t|x, v)dt$$

We take actual realizations of time to warning t_w and time to enforcement t_s as observed in the dataset. This means that we simulate the effect of sanctions on time remaining in unemployment after a sanction warning. This expected duration has to be constructed using a conditional version of density f_y where conditioning reflects (i) the fact that we only observe spells until day 720, and (ii) that – being interested in the average treatment effect on the treated (ATET) – we focus on individuals who have survived in unemployment until time t_w without a sanction warning. Duration to paid employment with both a sanction warning and a sanction enforcement is

$$E(t_y|x, v, D_w = 1, D_s(t), t_w < T_y < 720) = \int_{t_w}^{720} t \frac{f_y^{1,D_s(t)}(t|x, v)}{\int_{t_w}^{720} f_y^{1,D_s(t)}(t|x, v)dt} dt \tag{18}$$

the counterfactual duration is simulated setting both treatment effects in this expression to zero.

$$E(t_y|x,v,D_w=0,D_s=0,t_w<T_y<720)=\int_{t_w}^{720} t\frac{f_y^{0,0}(t|x,v)}{\int_{t_w}^{720} f_y^{0,0}(t|x,v)dt}dt \tag{19}$$

Substituting f_y by f_0 generates the corresponding mean duration from unemployment to non-paid post unemployment.

The ex post effect of benefit sanctions is the difference between actual mean duration (18) and counterfactual mean duration (19). Note again that we simulate first conditional on unobserved heterogeneity and then we integrate unobserved heterogeneity out.

B2. Simulating the ex ante effect

We simulate the ex ante effect on the post-unemployment outcome by focusing on everyone who generated positive earnings over 24 months after unemployment exit. We set their sanction statuses D_w and D_s to zero. Now, let $\theta_{y24}^{D_w,D_s,\alpha_{e24y}}(y|x,v)$ denote the earnings hazard, depending on sanction warning status D_w, sanction enforcement D_s status, and the vector of PES dummies in the outcome, α_{e24y}. The counterfactual of expected earnings under actual warning intensity and outcome dummies, implying $\alpha_{e24y}^0=\hat{\alpha}_{e24y}$, is described by equation (17) above, now evaluated for the whole $y24>0$ group.

The experiment we evaluate is an increase in the warning intensity by one standard deviation for all PES which are below the mean warning intensity plus one standard deviation. This leads to an increase in the PES dummy in the post-unemployment earnings process on the order of

$$\alpha_{e24y}^1=\hat{\alpha}_{e24y}+\hat{\delta}\max(\bar{\hat{\alpha}}_w+\sigma_{\hat{\alpha}_w}-\hat{\alpha}_w,0)$$

where δ is the regression coefficient from the respective ex ante effect regression. Expected earnings with the increased warning regime is

$$E(y|x,v,D_w=0,D_s=0,\alpha_{e24y}^1)=\int_0^{199} y\, f_{y24}^{0,0,\alpha_{e24y}^1}(y|x,v)dy+\left[1-\int_0^{199} f_{y24}^{0,0,\alpha_{e24y}^1}(y|x,v)dy\right]\cdot 200.$$

The difference between the expected earnings under the two regimes represents the ex ante ATET for the post-unemployment outcome.

The ex ante effect on unemployment duration is simulated by focusing on everyone's duration without a sanction. Let $\theta_y^{D_w,D_s,\alpha_{e24y}}(t|x,v)$ denote the transition rate from unemployment to positive income y. Expected duration to paid employment with actual warning intensity, implying $\alpha_y^0=\hat{\alpha}_y$, is

$$E(t_y|x,v,D_w=0,D_s=0,\alpha_y^0,T_y<720)=\int_0^{720} t\frac{f_y^{0,0,\alpha_y^0}(t|x,v)}{\int_0^{720} f_y^{0,0,\alpha_y^0}(t|x,v)dt} \tag{20}$$

Doing the same experiment by increasing the warning intensity as described above results in an increase in the PES dummy in the unemployment to paid employment process by

$$\alpha_y^1 = \hat{\alpha}_y + \hat{\delta} \max(\bar{\hat{\alpha}}_w + \sigma_{\hat{\alpha}_w} - \hat{\alpha}_w, 0).$$

Expected duration with the increased warning regime is

$$E(t_y|x, v, D_w = 0, D_s = 0, \alpha_y^1, T_y < 720) = \int_0^{720} t \frac{f_y^{0,0,\alpha_y^1}(t|x, v)}{\int_0^{720} f_y^{0,0,\alpha_y^1}(t|x, v)dt} \tag{21}$$

The ex ante effect on unemployment duration with exit in employment consists in the difference between the equations (21) and (20). The respective effect on unemployment duration that ends in medium run non-employment is calculated analogously, replacing f_y by f_0.

C. Tables

Tab. 7: The effect of benefit sanctions on exit behavior and subsequent non-/employment duration

⟶ *see next page*

(*Coeff./Transf.*)	Coeff.	*Model I* z-value	Transf.		Coeff.	*Model II* z-value	Transf.
Effect on exit from employment (M)							
warning (δ_{wm}/in %)					0.018	0.34	0.019
enforcement (δ_{sm}/in %)					0.140	2.35	0.150
Effect on exit from non-empl. (NM)							
warning (δ_{wnm}/in %)					0.146	1.14	0.157
enforcement (δ_{snm}/in %)					0.267	1.97	0.307
Effect on exit UE → E							
warning (δ_{we}/in %)	0.158	3.48	0.171		0.147	3.39	0.159
enforcement (δ_{se}/in %)	0.149	2.98	0.161		0.148	3.07	0.160
Effect on exit UE → NE							
warning (δ_{wne}/in %)	0.637	4.69	0.890		0.689	5.05	0.992
enforcement (δ_{sne}/in %)	0.515	4.10	0.674		0.513	4.05	0.670
Transition rate: exit from M							
$\lambda_{ma,1}/exp(u_{ma})$					-1.962	-3.56	3.832
$\lambda_{mb,1}/exp(u_{mb})$					-4.557	-5.27	0.286
Transition rate: exit from NM							
$\lambda_{nma,1}/exp(u_{nma})$					-0.367	-0.23	2.932
$\lambda_{nmb,1}/exp(u_{nmb})$					2.022	1.28	31.972
Transition rate: exit to E							
$\lambda_{ea,1}/exp(u_{ea})$	-5.309	-13.58	0.183		-5.321	-13.48	0.183
$\lambda_{eb,1}/exp(u_{eb})$	-6.446	-15.76	0.059		-6.478	-15.70	0.058
Transition rate: exit to NE							
$\lambda_{nea,1}/exp(u_{nea})$	-2.704	-2.67	0.052		-2.790	-2.69	0.052
$\lambda_{neb,1}/exp(u_{neb})$	-5.256	-5.08	0.004		-5.342	-5.08	0.004
Transition rate: warning							
$\lambda_{wa,1}/exp(u_{wa})$	-5.060	-4.81	0.181		-5.151	-4.77	0.181
$\lambda_{wb,1}/exp(u_{wb})$	-9.277	-8.66	0.003		-9.373	-8.54	0.003
Transition rate: enforcement							
$\lambda_{sa,1}/exp(u_{sa})$	-3.316	-2.13	0.441		-3.382	-2.07	0.447
$\lambda_{sb,1}/exp(u_{sb})$	-100	–	0		-100	–	0
Probabilities							
a_1/p_1	4.202	2.06	0.141	a_1/p_1	2.937	2.87	0.088
a_2/p_2	3.038	1.51	0.044	a_2/p_2	1.494	0.95	0.021
a_3/p_3	1.057	0.26	0.006	a_3/p_3	1.334	1.12	0.018
a_4/p_4	1.145	0.48	0.007	a_5/p_5	3.645	3.72	0.178
a_5/p_5	4.565	2.23	0.202	a_6/p_6	1.927	1.69	0.032
a_6/p_6	3.617	1.83	0.078	a_7/p_7	1.481	1.32	0.020
a_7/p_7	2.604	1.25	0.028	a_9/p_9	2.026	0.72	0.035
a_8/p_8	-1.066	-0.17	0.001	a_{11}/p_{11}	3.650	3.42	0.179
a_9/p_9	2.940	0.87	0.040	a_{13}/p_{13}	2.656	2.40	0.066
a_{11}/p_{11}	4.932	2.38	0.292	a_{17}/p_{17}	2.168	2.10	0.041
a_{13}/p_{13}	4.331	2.10	0.160	a_{18}/p_{18}	0.467	0.33	0.007
a_{16}/p_{16}	–	–	0.002	a_{22}/p_{22}	0.786	0.40	0.010
				a_{24}/p_{24}	-0.008	-0.01	0.005
				a_{27}/p_{27}	3.287	3.47	0.124
				a_{34}/p_{34}	1.218	0.63	0.016
				a_{37}/p_{37}	2.135	2.02	0.039
				a_{38}/p_{38}	1.983	2.06	0.034
				a_{45}/p_{45}	2.887	2.91	0.083
				a_{64}/p_{64}	–	–	0.005
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-Likelihood		198309				255064	
BIC		200982				259158	
N		23961				23961	

Notes: We report coefficients and their transformations: Transformed treatment effects are changes in %. Transition rates are in % per day (exception: M/NM in % per month), suitable for the first split period of the piecewise constant hazards (see respective footnotes); the transformations are calculated for an "average" individual: $u_{jg} = \lambda_{jg,1} + v_{jg} + \bar{x}'\beta_j + \bar{r}'\alpha_j + \bar{p}'\gamma_j$ where $j = \{m, nm, e, ne, w, s\}$, $g = \{a, b\}$ and the bars are means, except for the past earnings variables in the state dependence (p) where we use medians. Asymptotic z-values. Other probabilities are zero.
Source: Own estimations based on merged UIR-SSA database.

Tab. 8: The effect of benefit sanctions on earnings: over 1 vs. 24 months after unemployment exit; E (exit to employment) group

	Model III: earn 1 mt				Model III: earn 24 mt		
(Coeff./Transf.)	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Effect on earnings over 1/24 mt							
warning (δ_{wy1}/in %)	0.077	2.40	0.080	$\delta_{wy24}/\%$	0.102	3.27	0.107
enforcement (δ_{sy1}/in %)	0.050	1.18	0.051	$\delta_{sy24}/\%$	0.076	1.78	0.079
Effect on exit UE → E							
warning (δ_{we}/in %)	0.154	3.41	0.167		0.154	3.39	0.167
enforcement (δ_{se}/in %)	0.152	3.02	0.165		0.147	2.93	0.159
Effect on exit UE → NE							
warning (δ_{wne}/in %)	0.612	4.66	0.843		0.625	4.66	0.869
enforcement (δ_{sne}/in %)	0.522	4.16	0.686		0.518	4.12	0.679
Earnings realisation rate for Y1/24							
$\lambda_{y1a,1}/exp(u_{y1a})$	-3.008	-7.31	4.613	$\lambda/exp(u_{y24a})$	-5.094	-12.41	0.352
$\lambda_{y1b,1}/exp(u_{y1b})$	-4.785	-11.37	0.781	$\lambda/exp(u_{y24b})$	-7.311	-16.49	0.038
Transition rate: exit to E							
$\lambda_{ea,1}/exp(u_{ea})$	-5.302	-13.51	0.183		-5.312	-13.54	0.183
$\lambda_{eb,1}/exp(u_{eb})$	-6.442	-15.69	0.059		-6.430	-15.68	0.060
Transition rate: exit to NE							
$\lambda_{nea,1}/exp(u_{nea})$	-2.686	-2.66	0.051		-2.734	-2.70	0.052
$\lambda_{neb,1}/exp(u_{neb})$	-5.308	-5.11	0.004		-5.303	-5.12	0.004
Transition rate: warning							
$\lambda_{wa,1}/exp(u_{wa})$	-5.083	-4.81	0.181		-5.055	-4.79	0.180
$\lambda_{wb,1}/exp(u_{wb})$	-9.300	-8.66	0.003		-9.276	-8.64	0.003
Transition rate: enforcement							
$\lambda_{sa,1}/exp(u_{sa})$	-3.323	-2.12	0.448		-3.300	-2.11	0.443
$\lambda_{sb,1}/exp(u_{sb})$	-100	–	0		-100	–	0
Probabilities							
a_1/p_1	4.102	3.34	0.148	a_1/p_1	4.158	5.21	0.146
a_2/p_2	2.907	2.37	0.045	a_2/p_2	2.948	3.55	0.044
a_3/p_3	1.301	0.48	0.009	a_3/p_3	0.822	0.19	0.005
a_4/p_4	1.003	0.58	0.007	a_4/p_4	1.189	0.85	0.008
a_5/p_5	4.291	3.47	0.179	a_5/p_5	4.441	5.68	0.194
a_6/p_6	3.407	2.89	0.074	a_6/p_6	3.511	4.51	0.077
a_7/p_7	2.471	1.90	0.029	a_7/p_7	2.552	2.80	0.029
a_8/p_8	-1.562	-0.18	0.001	a_8/p_8	-1.852	-0.15	0.000
a_9/p_9	3.069	1.26	0.053	a_9/p_9	2.826	0.92	0.039
a_{11}/p_{11}	4.741	3.74	0.281	a_{11}/p_{11}	4.848	5.84	0.291
a_{13}/p_{13}	4.099	3.34	0.148	a_{13}/p_{13}	4.236	5.34	0.158
a_{21}/p_{21}	1.759	1.51	0.014	a_{21}/p_{21}	0.689	0.74	0.005
a_{22}/p_{22}	-0.218	-0.10	0.002	a_{22}/p_{22}	-0.127	-0.10	0.002
a_{29}/p_{29}	1.233	0.82	0.008	a_{32}/p_{32}	–	–	0.002
a_{32}/p_{32}	–	–	0.002				
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-Likelihood		231704				289436	
BIC		235077				292804	
N		23961				23961	

Notes: We report coefficients and their transformations: Transformed treatment effects are changes in %. Transition rates are in % per day (earnings Y1/24: in % per 1000 CHF), suitable for the first split period of the piecewise constant hazards (see respective footnotes); the transformations are calculated for an "average" individual: $u_{jg} = \lambda_{jg,1} + v_{jg} + \bar{x}'\beta_j + \bar{r}'\alpha_j + \bar{p}'\gamma_j$ where $j = \{y1, y24, e, ne, w, s\}$, $g = \{a, b\}$ and the bars are means, except for the past earnings in the state dependence (p) where we use medians. Asymptotic z-values. Other probabilities are zero.
Source: Own estimations based on merged UIR-SSA database.

Tab. 9: The effect of benefit sanctions on earnings over 24 months: E group (excluding temporary and permanent labor force exits) vs. total population with positive earnings (excluding only permanent labor force exits)

	Model III: earn 24 mt				*Model IV: earn 24 mt*		
(*Coeff./Transf.*)	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Effect on earnings over 24 mt							
warning (δ_{wy24}/in %)	0.102	3.27	0.107	$\delta_{wy24t}/\%$	0.117	4.02	0.124
enforcement (δ_{sy24}/in %)	0.076	1.78	0.079	$\delta_{sy24t}/\%$	0.104	2.66	0.109
Effect on exit UE → E/Y							
warning (δ_{we}/in %)	0.154	3.39	0.167	$\delta_{wy}/\%$	0.181	4.33	0.198
enforcement (δ_{se}/in %)	0.147	2.93	0.159	$\delta_{sy}/\%$	0.211	4.55	0.235
Effect on exit UE → NE/0							
warning (δ_{wne}/in %)	0.625	4.66	0.869	$\delta_{w0}/\%$	0.830	2.59	1.294
enforcement (δ_{sne}/in %)	0.518	4.12	0.679	$\delta_{s0}/\%$	0.294	1.73	0.342
Earnings realisation rate for Y24/24t							
$\lambda_{y24a,1}/exp(u_{y24a})$	-5.094	-12.41	0.352	$\lambda/exp(u_{y24ta})$	-4.696	-12.24	0.418
$\lambda_{y24b,1}/exp(u_{y24b})$	-7.311	-16.49	0.038	$\lambda/exp(u_{y24tb})$	-6.850	-16.09	0.048
Transition rate: exit to E/Y							
$\lambda_{ea,1}/exp(u_{ea})$	-5.312	-13.54	0.183	$\lambda/exp(u_{ya})$	-4.797	-12.70	0.211
$\lambda_{eb,1}/exp(u_{eb})$	-6.430	-15.68	0.060	$\lambda/exp(u_{yb})$	-5.887	-15.06	0.071
Transition rate: exit to NE/0							
$\lambda_{nea,1}/exp(u_{nea})$	-2.734	-2.70	0.052	$\lambda/exp(u_{0a})$	-4.785	– [1]	0.002
$\lambda_{neb,1}/exp(u_{neb})$	-5.303	-5.12	0.004	$\lambda/exp(u_{0b})$	-2.812	-6.29	0.011
Transition rate: warning							
$\lambda_{wa,1}/exp(u_{wa})$	-5.055	-4.79	0.180		-5.086	-4.85	0.181
$\lambda_{wb,1}/exp(u_{wb})$	-9.276	-8.64	0.003		-9.261	-8.68	0.003
Transition rate: enforcement							
$\lambda_{sa,1}/exp(u_{sa})$	-3.300	-2.11	0.443		-3.358	-2.17	0.446
$\lambda_{sb,1}/exp(u_{sb})$	-100	–	0		-100	–	0
Probabilities							
a_1/p_1	4.158	5.21	0.146	a_1/p_1	4.473	5.59	0.241
a_2/p_2	2.948	3.55	0.044	a_2/p_2	3.561	4.59	0.097
a_3/p_3	0.822	0.19	0.005	a_3/p_3	2.744	3.54	0.043
a_4/p_4	1.189	0.85	0.008	a_5/p_5	3.527	3.14	0.094
a_5/p_5	4.441	5.68	0.194	a_6/p_6	2.160	1.62	0.024
a_6/p_6	3.511	4.51	0.077	a_8/p_8	0.570	0.47	0.005
a_7/p_7	2.552	2.80	0.029	a_9/p_9	2.397	0.48	0.030
a_8/p_8	-1.852	-0.15	0.000	a_{11}/p_{11}	3.949	4.34	0.143
a_9/p_9	2.826	0.92	0.039	a_{13}/p_{13}	4.736	5.46	0.314
a_{11}/p_{11}	4.848	5.84	0.291	a_{17}/p_{17}	0.175	0.16	0.003
a_{13}/p_{13}	4.236	5.34	0.158	a_{18}/p_{18}	0.248	0.27	0.004
a_{21}/p_{21}	0.689	0.74	0.005	a_{32}/p_{32}	–	–	0.003
a_{22}/p_{22}	-0.127	-0.10	0.002				
a_{32}/p_{32}	–	–	0.002				
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-Likelihood		231704				294752	
BIC		235077				298110	
N		23961				23961	

Notes: We report coefficients and their transformations: Transformed treatment effects are changes in %. Transition rates are in % per day (earnings Y24/24t: in % per 1000 CHF), suitable for the first split period of the piecewise constant hazards (see respective footnotes); the transformations are calculated for an "average" individual: $u_{jg} = \lambda_{jg,1} + v_{jg} + \bar{x}'\beta_j + \bar{r}'\alpha_j + \bar{p}'\gamma_j$ where $j = \{y24, y24t, e, ne, w, s\}$, $g = \{a, b\}$ and the bars are means, except for the past earnings in the state dependence (p) where we use medians. Asymptotic z-values. Other probabilities are zero. [1] Constant could not be estimated in final model, value fixed. Its value was estimated from a version of the model with fixed probabilities.
Source: Own estimations based on merged UIR-SSA database.

D. Observables

In the following table we provide means (or medians in the case of durations) for all the variables used in the estimated Models I to IV (see section 4 for a description of the models). The means are given for the total sample as well as for the treatment subgroups: the non-sanctioned (non-sanc), those who were warned only (warn only), and those who were warned and got a benefit sanction imposed (warn&enf). The variables below, except the last two paragraphs, are the control variables which are present in all the Models I to IV. The post-unemployment outcome Models II to IV feature as well the endogenous state dependence variables as further controls. Finally, the last paragraph gives a descriptive insight in how outcome levels are different depending on in which treatment subgroup an individual is. The estimated coefficients for the control variables in Models I to IV are not reported in this paper due to space reasons. They are available from the authors upon request.

Tab. 10: Observable characteristics: Means by sanction status group

	total	non-sanc	warn only	warn&enf
State dependence: past earnings & employment				
Sum of earnings mt -25 to -60	116809	120692	103443	97797
Sum of earnings mt -13 to -24	38928	40016	34562	34442
Sum of earnings mt -7 to -12	19300	19784	17302	17375
Sum of earnings mt -2 to -5	17450	17928	15802	15108
Sum of earnings mt -1	3474	3573	3129	2988
Sum of employed months mt -25 to -60	27.58	28.01	26.18	25.34
Sum of employed months mt -13 to -24	9.23	9.31	8.87	8.94
Sum of employed months mt -7 to -12	4.63	4.65	4.49	4.58
Sum of employed months mt -2 to -5	4.21	4.23	4.18	4.10
Sum of employed months mt -1	0.85	0.85	0.84	0.80
Sociodemographic characteristics				
Qualification: semi-skilled (or skilled w/o (recognised) certificate)	0.164	0.159	0.183	0.181
Qualification: non-skilled (base: skilled with certificate)	0.266	0.254	0.318	0.315
Age	39.9	40.0	39.4	39.3
Age squared	1641.9	1652.3	1603.1	1595.0
Civil status: Married/separated (base: unmarried)	0.647	0.653	0.647	0.585
Civil status: Widowed	0.010	0.010	0.010	0.006
Civil status: Divorced	0.128	0.124	0.129	0.161
Woman (base: man)	0.391	0.396	0.357	0.380
Not Swiss (base: Swiss)	0.444	0.433	0.506	0.469
Language region: French-speaking (base: German-speaking)	0.682	0.693	0.659	0.609
Language region: Italian-speaking	0.008	0.009	0.003	0.005
Mother tongue not the one of language region	0.444	0.435	0.503	0.455
Skilled*non-Swiss	0.140	0.142	0.138	0.125
Semi-skilled*non-Swiss	0.104	0.100	0.121	0.114
Non-skilled*non-Swiss	0.198	0.189	0.244	0.225
Parttime unemployed	0.116	0.118	0.089	0.127
Speaks at least 2 foreign languages	0.381	0.387	0.345	0.369
At least one registered UE spell in 2 years before observed spell	0.092	0.091	0.094	0.103

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	total	non-sanc	warn only	warn&enf
Placeability[1]: good (base: "without problems")	0.131	0.137	0.104	0.107
Placeability: medium	0.732	0.732	0.746	0.719
Placeability: bad	0.099	0.091	0.116	0.144
Placeability: special cases/hardly placeable	0.011	0.010	0.016	0.010
Residence status: foreigner w. yearly residence permit (base: Swiss)	0.143	0.135	0.185	0.157
Residence status: foreigner w. permanent residence permit	0.285	0.284	0.295	0.278
Residence status: asylum seekers (incl refugees)	0.017	0.014	0.025	0.032
Residence status: season workers, short stayers, rest	0.001	0.001	0.001	0.002
Last function: self-employed, incl home workers (base: professionals)	0.008	0.008	0.007	0.010
Last function: management	0.062	0.069	0.034	0.039
Last function: support function	0.375	0.356	0.458	0.445
Last function: students,incl apprenticeship	0.005	0.005	0.004	0.003
Household size: 2 people (incl job seeker; base: 1 person)	0.239	0.240	0.220	0.247
Household size: 3 people	0.199	0.200	0.204	0.180
Household size: 4 people	0.217	0.220	0.209	0.194
Household size: 5 people	0.070	0.068	0.083	0.070
Household size: 6 people	0.028	0.026	0.039	0.029
Household size 2 * woman	0.119	0.121	0.103	0.113
Household size 3 * woman	0.075	0.075	0.080	0.066
Household size 4 * woman	0.071	0.071	0.068	0.082
Household size 5 * woman	0.017	0.016	0.017	0.024
Household size 6 * woman	0.005	0.004	0.006	0.007
Occupations (base category: office, administration, accounting, police, military)				
Food & agriculture occupations	0.041	0.042	0.041	0.039
Blue-collar manufacturing (machines, watches, chemicals,...)	0.092	0.089	0.109	0.099
Transportation, travel, telecom, media, print	0.055	0.053	0.063	0.063
Construction, carpenters (wood preparation)	0.154	0.155	0.172	0.119
Engineers, technicians	0.056	0.059	0.046	0.038
Enterpreneurs, directors, chief civil servants, lawyers	0.019	0.021	0.010	0.018
Informatics	0.006	0.006	0.006	0.006
Sales	0.068	0.070	0.052	0.073
Marketing, PR, wealth management, insurance	0.012	0.012	0.012	0.010
Gastronomy, housekeeping, cleaning, personal service	0.203	0.192	0.244	0.257
Health occupations (incl social workers)	0.035	0.036	0.029	0.035
Science & arts	0.028	0.030	0.021	0.021
Education	0.026	0.027	0.021	0.024
Students (& people looking for apprenticeship)	0.005	0.005	0.004	0.004
Rest (mainly unskilled workers, helpers)	0.080	0.075	0.093	0.103
Benefits: Maximum duration of eligibility & replacement rate[2]				
Maximum of passive benefit days >= 250 (base: 150 days)	0.170	0.175	0.148	0.146
Maximum of passive benefit days = 75	0.020	0.019	0.023	0.027
Replacement rate category: 70% (base: 80%)	0.222	0.231	0.185	0.191
Replacement rate category: 72%	0.012	0.011	0.017	0.012
Replacement rate category: 74%	0.013	0.013	0.014	0.015
Replacement rate category: 76%	0.010	0.010	0.010	0.008
Replacement rate category: 78%	0.010	0.010	0.010	0.013
PES (regional public employment service) dummies (base: SOA1)[3]				
AIA2	0.002	0.003	0.000	0.003
FRB1	0.017	0.017	0.021	0.008
FRC1	0.008	0.008	0.006	0.008

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	total	non-sanc	warn only	warn&enf
FRD1	0.010	0.011	0.008	0.005
FRF1	0.011	0.013	0.005	0.004
FRK1	0.005	0.005	0.004	0.004
FRL1	0.031	0.032	0.027	0.021
FRM1	0.019	0.017	0.039	0.011
FRM4	0.002	0.002	0.004	0.005
FRN1	0.009	0.011	0.005	0.002
GRD1	0.042	0.039	0.023	0.093
GRE1	0.009	0.009	0.008	0.018
GRF1	0.009	0.008	0.003	0.024
GRG1	0.005	0.006	0.001	0.003
GRH1	0.010	0.010	0.005	0.012
GRI1	0.015	0.015	0.010	0.022
SOA2	0.016	0.015	0.020	0.024
SOA3	0.022	0.021	0.026	0.029
SOA4	0.009	0.010	0.006	0.006
SOA5	0.016	0.015	0.019	0.018
SOA6	0.009	0.011	0.002	0.007
SOA7	0.005	0.003	0.007	0.027
SOA8	0.003	0.003	0.002	0[4]
SOA9	0.006	0.005	0.006	0.007
SOAA	0.010	0.011	0.006	0.005
SOAB	0.018	0.019	0.011	0.020
URA2	0.008	0.007	0.011	0.008
VDB1	0.091	0.096	0.066	0.073
VDB2	0.007	0.007	0.005	0.003
VDC1	0.008	0.008	0.008	0.004
VDD1	0.030	0.028	0.034	0.038
VDD4	0.003	0.002	0.005	0.006
VDE1	0.013	0.015	0.001	0.011
VDH1	0.024	0.025	0.007	0.039
VDJ1	0.022	0.025	0.009	0.005
VDL1	0.040	0.040	0.039	0.050
VDM1	0.015	0.013	0.019	0.020
VDN1	0.005	0.006	0.001	0.002
VDP1	0.023	0.026	0.012	0.005
VDQ1	0.021	0.019	0.011	0.053
VDT1	0.009	0.009	0.009	0.007
VDU1	0.027	0.027	0.023	0.031
VDV1	0.033	0.034	0.035	0.020
VDW1	0.009	0.010	0.008	0.003
VDZ1	0.006	0.006	0.007	0.007
VSL1	0.026	0.020	0.050	0.050
VSM1	0.052	0.051	0.077	0.036
VSM2	0.004	0.004	0.003	0.000
VSN1	0.053	0.047	0.113	0.029
VSO1	0.021	0.024	0.004	0.017
VSO2	0.045	0.053	0.003	0.032
VSP1	0.080	0.071	0.164	0.055
Endogenous state dependence: duration of past stage (unemployment)[5]				
Log unemployment duration (median, days)	5.10	5.00	5.38	5.73
Log unemployment duration, squared (median, days)	26.01	24.97	28.99	32.87

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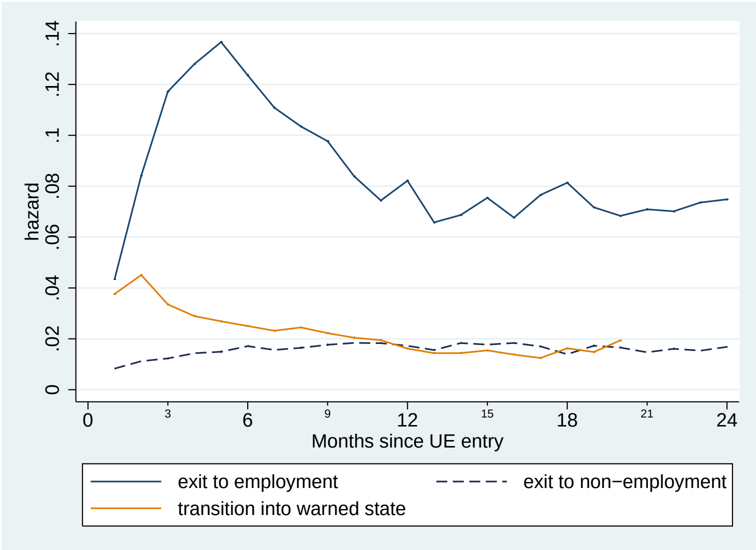
	total	non-sanc	warn only	warn&enf
Log unemployment duration, 3rd power (median, days)	132.6	124.8	156.1	188.5
Log unemployment duration, 4th power (median, days)	676.4	623.6	840.6	1080.5
Log unemployment duration, 5th power (median, days)	3449.8	3116.3	4526.1	6195.0
Log unemployment duration, 6th power (median, days)	17593.5	15572.8	24370.8	35517.9
Outcomes (dependent variables for Models I to IV)[6]				
Unemployment duration	164	148	218	309
Duration first spell after ue: employment (E: 19149 obs)	25	26	19	22
Duration first spell after ue: nonemployment (NE: 2985 obs)	11	10	16	12
Earnings in the first month after ue exit (E: 19149 obs)	89826.85	92364.93	79733.43	75292.16
Earnings over 24 months after ue exit (E: 19149 obs)	3992.41	4087.35	3611.41	3453.90
Earnings over 24 months after ue exit (Y: 21012 obs)	85954.90	88855.57	75708.11	69206.41
Observations	**23961**	**19228**	**2714**	**2019**

Notes: Means for each subgroup are reported, medians in the case of durations. For dummy variables proportions of individuals with = 1 are reported. [1] Placeability: judgement by caseworker how hard it will be to place the job seeker on the labour market. [2] Passive benefits (150 days normally) are that part of the total benefits that are paid without a compulsory obligation to participate at the active labor market programs. Normally, passive benefit days are reduced to half for individuals under 25 years and go to 250 or more if a job seeker is above 50 years old. Normal case for the replacement rate is 80%. Individuals without children and with higher earnings may only get 70%. The replacement rate reduction is not discrete but rather smoothed for earnings around the reduction limit (130 CHF per day). [3] PES cover parts of cantons; AI=Appenzell Innerrhoden (complete canton), FR=Fribourg, GR=Graubünden, SO=Solothurn, UR=Uri (complete canton), VD=Vaud, VS=Valais. [4] No cases which are warned & enforced in PES SOA8 in our sample. Coefficient of this dummy not estimated in enforcement process. [5] Not used as control variables in Model I. [6] For details on the modelling of these outcomes for the Models I to IV, see econometrics section 4. For the durations medians are reported, for the earnings means. Unemployment duration is in days, durations of the first post-unemployment spell are in months. Earnings are in CHF (deflated). Note that the post-unemployment outcomes are only measured for subgroups in which they were realised (E/NE/Y), see section 4 for details.
Source: Own estimations based on merged UIR-SSA database.

E. Figures

Fig. 1: Unemployment transition rates and sanction enforcement rates

a. Exit rates from unemployment & sanction warning rates



b. Sanction enforcement rates

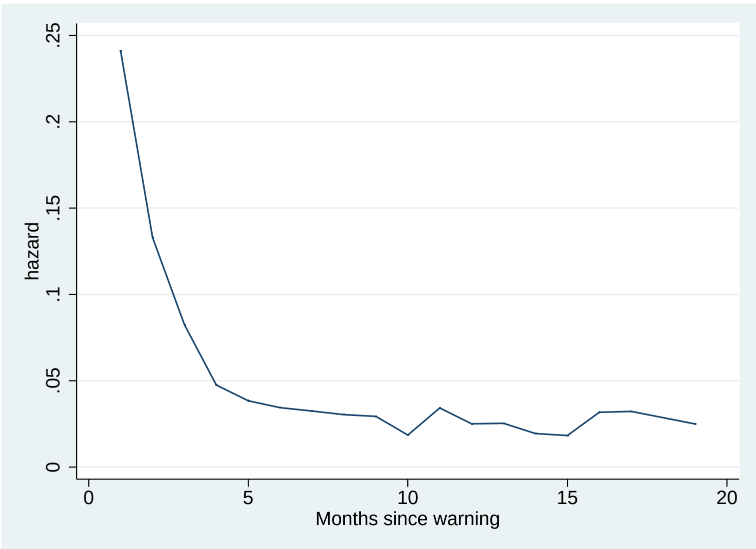
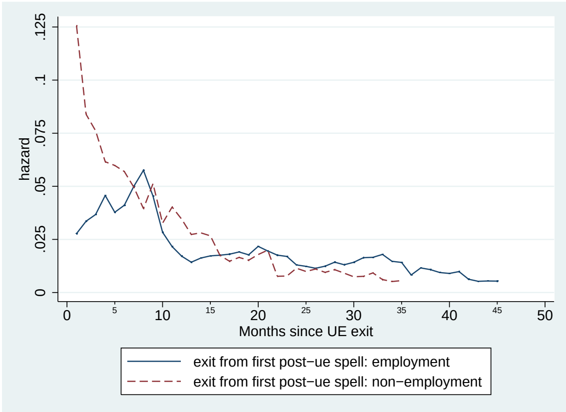


Fig. 2: Post-unemployment transition rates

a. Transition rates from first post-unemployment spell



b. Month 1 after unemployment exit, e group

c. Sum over 24 months after unemployment exit, e group (Model III) vs $y_{24} > 0$ group (Model IV)

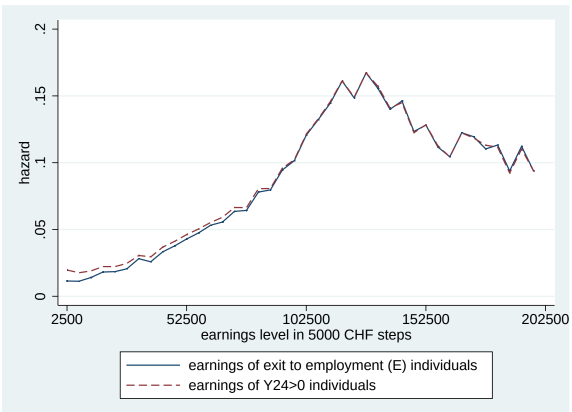
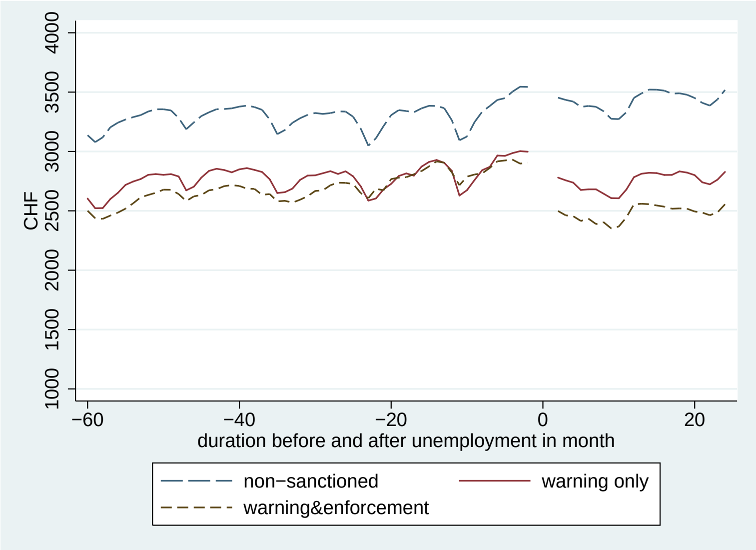
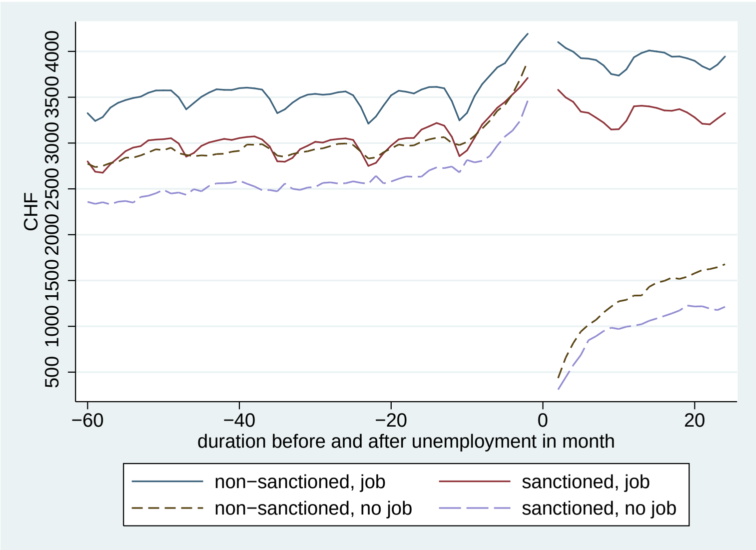


Fig. 3: Duration-dependent employment earnings histories: by sanction status.

a. By sanction status

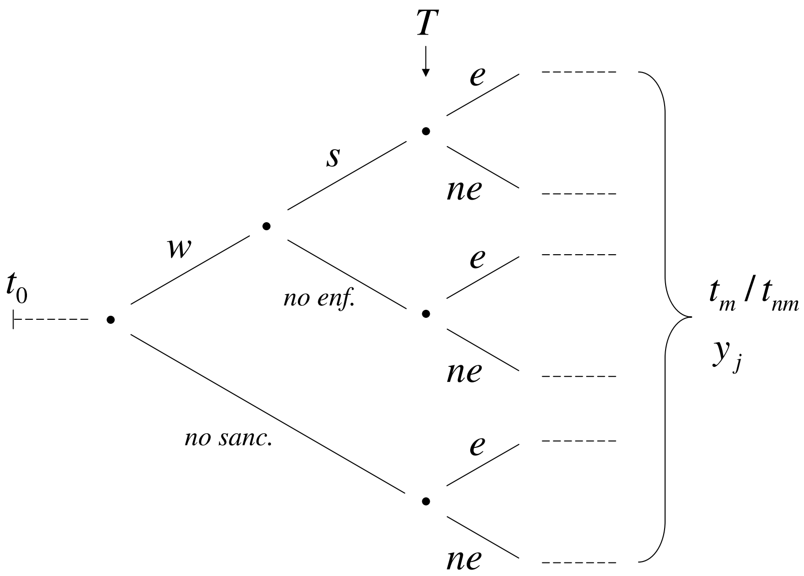


b. By employment status



Note: These lines average earnings histories dependent on the duration before entry in UE (negative values) or after exit from UE (positive) for all spells belonging to the inflow sample and to the respective subgroup.

Fig. 4: Multiple states of the individual's process history



Note: Abbreviations of states: w=warned, s=sanction enforced, e=exit to employment (i.e. positive labor earnings in the first month after unemployment exit), ne=exit to nonemployment (zero earnings in the first month). Note that for Model IV, the exit destinations e and ne are replaced by y=positive labor earnings over 24 months after unemployment exit and 0=zero earnings over that period. See the econometrics and results sections (4 and 5) for more explanations and discussion.