How do Unemployment Benefit Sanctions affect the Quality of Post-Unemployment Jobs?

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March 21, 2009

Work in Progress, please do not quote; comments welcome.

Abstract: This is the first empirical paper that aims for a comprehensive evaluation of sanction effects. Beyond effects on duration, we evaluate the individual's post-unemployment position in the wage hierarchy, up to two years after exit, as well as the issue of employment stability. A rich set of register data allows constructing appropriate measures to tackle these questions and assessing their empirical relevance for Switzerland. Preliminary evidence, based on a multiple-stage mixed proportional hazard model, indicates that the unemployment exit hazard shifts upward immediately by 7.2% after warning an individual that a sanction investigation has started. Actually enforcing the sanction increases the hazard by additional 2.8%, though statistically not significant. Do sanctions favour the exit to a certain quality of job? Our results suggest that they foster more mobility to worse paid jobs. In a competing risks model, the total sanction effect on downward mobility amounts to 28% whereas the likelihood to exit to a better (or equally) paid job increases by 7.5% due to a sanction. Moreover, sanctions reduce earnings levels in the first month after unemployment exit by 4.1%.
Remark: Further results from an extended model (described below) which assess long-term earnings effects (up to two years after exit) and effects on employment stability will follow in April.

JEL Classification: J64, J65, J68
Keywords: Benefit sanctions, earnings effects, unemployment duration, difference-in-differences.

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1 Introduction

Recent literature in the domain of unemployment insurance and welfare program research presents new evidence on the importance of sanctions and monitoring in shaping job search incentives. But an important set of questions in this context remains still unanswered: How do job seekers react to such incentivising measures? How sustainable are the effects of such measures? Research evidence on these questions of high policy relevance is broadly missing. This paper wants to make an empirical contribution to fill this gap.

The focus of this paper lies on post-unemployment effects of benefit sanctions – and the strictness of enforcing them – which are imposed during the unemployment spell. Previous research in the domain has mainly focused on theoretical models and on short-term effects of benefit sanctions. Several recent studies for different countries located such effects observing an increase in the exit rate from unemployment of sanctioned job seekers, compared to the non-sanctioned. But what happens afterwards? This paper, which focuses on the Swiss unemployment insurance system, goes a step further tackling the above-mentioned questions.

It is of essential policy relevance to evaluate the effects of unemployment insurance benefit sanctions from a *general welfare perspective* – and not to stop with the observation that sanctions reduce unemployment duration and therefore the UI budget. It is crucial to know what happens to the *quality of a job match once a sanction is imposed. Does the sanction harm the quality of the accepted jobs after unemployment – in terms of reduced earnings and job stability?* A worsening of post-UE job quality, with respective negative welfare effects, would of course not be in the intention of sanction policy makers.

In particular, this paper is the first paper in the field of policy evaluation of UI benefit sanction effects that can provide empirical contributions to the following two issues: *(i)* How do benefit sanction effects translate into the post-unemployment history of individuals? Specifically, *which effects can be observed on the post-unemployment development of earnings and job stability?* Thus, is there a – positive or negative – effect beyond the lowering of UE duration? *(ii)* This paper can make statements about the issue of *sustainability of the sanctions effects*. Of what empirical duration are earnings effects of sanctions in the subsequent job history of sanctioned job seekers after the accelerated exit from unemployment? *How* does the post-UE effect develop – in terms of permanence an tendencies to convergence (catch up with the non-sanctioned) or divergence?

Thanks to a vast and rich set of Swiss register data, this paper is able to produce evidence on these questions, based on long-term (un)employment histories of job seekers. – Histories on the pre- and post-unemployment earnings/job period as well as on sanctions and other events during the unemployment spell.

The remainder of this paper are structured in the following way: Some remarks on the theoretical context and measurement will be made in the next section. A review of the related empirical literature follows. Then, the relevant institutions in the Swiss unemployment insurance system will be highlighted – mainly the sanction procedure. Thereafter, data sources and the

structuring of the data are discussed. Descriptive statistics follow, highlighting in particular cantonal policy variation and group differences between the non-sanctioned and the sanctioned. Then, indicative results on the analysis of the procedures and mainly their income effects are presented; they offer already interesting insights in the potential results and lead therefore directly into the discussion about expected results. Finally, two possible strains of empirical strategy will be sketched – thus, the paper finishes with a discussion of possible econometric models which are suitable for the identification and evaluation of the causal effect of sanctions on post-unemployment job quality.

2 Theoretical Background and Measurement

The questions in (i) above can be situated in the context of *job search theory*. Recent models which introduced sanctions and monitoring into a classical job search framework with endogenous search intensity are proposed by Abbring et al. (2005) and Boone et al. (2007) or, in a more descriptive version, Van den Berg et al. (2004). The first paper uses a partial equilibrium, the second a general equilibrium model. In general, they stress *two behavioral reactions that individuals can show in the situation of being sanctioned during job search*. On one hand, they can react (immediately) on a sanction warning or enforcement – which entails a benefit/utility loss for the concerned individual – by adapting their *search effort*. So, sanctions would lead directly to an increase in search intensity – one possible reason for the reduction of unemployment duration of sanctioned individuals. On the other hand, facing alternative opportunities, job seekers build an idea on the *reservation wage* they are willing to accept. Sanctions could make them lower their demands concerning post-employment jobs, i.e. reduce reservation wage.

Both of these behavioral predictions can be inferred from the theoretical models. It is quite intuitive that they are interrelated. The increase of search effort is linked with the growth of search costs and of the arrival rate of job offers – which can have impact on the setting of the reservation wage. The observation of post-unemployment earnings history can give us indication whether individuals only increase search (or compliance) effort in the situation of being sanctioned or whether they merely lower as well their demands on the quality of the post-unemployment job(s).

Both behavioral reactions that theory suggests result, in a first step, in the reduction of unemployment duration. But is this good or bad for post-UE earnings (and job stability)? From a theoretical point of view, you can argument in two ways: (i) Increased search effort leads to a job/wage match that is at least as good as without sanction. (ii) The reduction of the reservation wage drives you to accept lower quality jobs, linked with wage losses. Thus, theoretical predictions are *contradictive* concerning post-UE sanction effects. – Therefore, it is up to an *empirical evaluation* which lines of effects dominate in practice. See more on this in the expected results section.

A *second strain of theoretical reasoning* that is relevant in our context asks for the effects of being shorter or longer *in* unemployment on the labour market chances of the concerned individ-

ual. A first argument, as brought forward e.g. by Pissarides (1992), stresses *skill depreciation* or human capital loss during unemployment. Thus, as a sanction leads to a reduced UE duration, less skill depreciation takes place which leads to (relatively) better earnings perspectives after UE. A second argument is known in the literature by the notion of the *scarring effect* (see e.g. Arulampalam 2001 for a more recent example): It suggests that unemployment history leaves a bad stigma, a 'scar', on the foreheads of job seekers. Thus, being unemployed for a longer time is used as a signal by employers when sorting for good/bad workers. So, the sanction-induced reduction of unemployment duration could have a less strong scarring effect, leading to better earning perspectives after UE. Finally, one can argument from a *job matching* perspective that UE duration has to be seen as time to invest in search for a good job match. The shorter UE duration due to a sanction would thus lead to a worse job match – with negative effects on earning perspectives. As well from this theoretical strain, we have to conclude that predictions can be contradictive.

The mechanics of reaction on sanctions as described in the last paragraphs can have different time dimensions in term of how quick they materialise or of which permanence they are. Consequently, it is crucial to look at *different time spans* of the post-unemployment income and job history of the concerned individuals, i.e. to look at mid- as well as long-term effects of sanctions. In other words, it is advantageous to evaluate questions on the existence and qualitative direction of sanction effects and on their *sustainability*, (ii), together.

To assess post-unemployment job(s) quality, different income and earnings *indicators*, job tenure/job change probability and reentry rates into unemployment are proposed in this paper. These indicators allow a broader view on situation of post-unemployment conditions than just the level of the reservation wage (which is of course not directly observable). They will provide more general insights on the effects of reactions of sanctioned people, reflected in their labour market position some time after the end of their unemployment spell.

3 Literature Review

The small literature on benefit sanctions – temporary reductions in unemployment benefits due to noncompliance with eligibility requirements – evokes the interesting conclusion that it may be more efficient to enhance compliance with the eligibility requirements of unemployment insurance via a strict sanction policy than to lower the overall benefit replacement rate in order to achieve a reduction in unemployment.[1]

The small body of recent empirical literature mainly supports the positive short-term effect of sanctions and monitoring on the exit rate from unemployment. For Switzerland, one study

[1]See Becker (1968) for the first economic analysis of an optimal system of criminal justice. 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.

was conducted on this issue – the paper of Lalive, van Ours and Zweimüller (2005) [LvOZ in the following] which uses the same sanctions data as this paper here. The LvOZ paper stresses that benefit sanctions may affect unemployment duration through two channels. Benefit sanctions will increase the search intensity of the sanctioned due to the reduction in the value of being unemployed. This first effect is the*ex-post effect*, the effect that an *actually imposed benefit reduction* stimulates a worker in his or her search effort. Furthermore, also the non-sanctioned may increase search intensity due to more strict enforcement of job-search requirements. This second effect is the *ex-ante effect*, the effect that the *risk of getting a benefit sanction* influences the search behavior of the unemployed worker. LvOZ provide the first empirical study to investigate jointly the magnitude of the ex-ante effect and the importance of the ex-post effect of a system of benefit sanctions.

LvOZ find that not only the enforcement of a sanction has a positive effect on the exit rate from unemployment. Already the warning that a sanction is announced has a quantitative effect of similar importance. Unemployment duration is shown to be reduced by roughly three weeks for the sanctioned. Also the ex-ante effect is proven to be important: An increase in the strictness of the sanction policy by one standard deviation reduces the duration of unemployment by about a week.

The other existing empirical literature deals almost exclusively with estimating the ex-post effect of benefit sanctions. One exception is the paper of Svarer (2007) about the Danish UI benefit sanctions. He shows that men react ex ante to the risk of being sanctioned and exhibit a higher exit rate from unemployment. He finds that, ex-post, for both males and females the exit rate increases by more than 50% following enforcement of a sanction. Further, the strength of the sanction influences the size of the effect. Another Danish study (Jensen et al., 2003) used a grouped duration model to find a small effect of the sanctions that are part of a youth unemployment program. One study on unemployment benefit II sanctions in Germany deals explicitly with the effect on reservation wages. It is based on a cross-section survey conducted in winter 2005/6 by IAB that included a question about reservation wages. Using propensity score matching, Schneider (2008) finds no significant effect of sanctions on the measured reservation wages. Analysing administrative data on UI entries between April 2000 and March 2001 in West Germany, Hofmann (2008) reports, based on a matching approach, positive effects of benefit sanctions on the employment probability for both women and men.

Two Dutch papers find that a reduction of unemployment benefits may have a substantial effect on the outflow from unemployment to a job. Abbring et al. (2005) study the effect of financial incentives by comparing the unemployment duration of individuals that have faced a benefit reduction with similar individuals that have not been penalized. They find that benefit sanctions have a positive effect on individual transition rates from unemployment to a job. The job finding rate doubles after a sanction has been imposed. Van den Berg et al. (2004) perform a similar study for welfare recipients in the city of Rotterdam. Although this group of unemployed has a labor market position that is often considered to be very weak they too find that benefit sanctions stimulate the transitions from welfare to work. Again, the job finding rate doubles

when a sanction gets imposed. From this study it appears that – in opposition to the evidence from Denmark – the size of the benefit sanction is not very relevant. It is the shock of getting a benefit sanction imposed that activates the job seeker, not the size of that sanction.

For Belgium, Cockx et al. (2002, 2004) do not deal with administrative benefit sanctions in their two papers, but rather with a special category of benefit exhaustion which is enforced like a sanction. These exhaustions principally apply to women who have partners with labour income and are long-term unemployed (two years or more). The first paper concludes that a significant effect is only observed for one group of women who have still relatively recent labour market experience and less duties in child care. The second paper uses different propensity score matching approaches to show that benefit expiration exerts an effect from the moment at which the individual is notified and that it gradually increases the employment rate up to 25 percentage points 14 months after benefit withdrawal.

In a more general perspective, interest in 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 frequently used. 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.[2]

Further interest in benefit sanctions comes from recent U.S. welfare reform programs (for a recent survey, and its relevance for Europe, see Blank, 2003). Sanctions have been a central feature of the welfare reforms of the 1990s (Bloom and Winstead, 2002). There is huge variation in sanctions policies across programs and states. For instance Pavetti and Bloom (2001) mention that 25 states follow rather strict sanction policies and, in some states, non-compliance with benefit rules results in permanent full benefit losses. While it is of high interest to policy makers how such sanctions might affect the compliance of eligible workers with benefit rules and their labor market outcomes, little is known about the effects of such sanctions.[3]

[2]Martin and Grubb (2001) in their survey on the success of ALMPs in OECD countries conclude that governments should rely as much as possible on in-depth counseling, job-finding incentives and job-search assistance programs. The prototypical country that relied heavily on active labor market policies is Sweden. Recent evidence by Calmfors et al. (2001) suggests that Swedish programs were not very effective in maintaining regular employment. Furthermore, Swedish labor market training had no or negative employment effects, whereas a lot of other programs had a locking-in effect. Participants are not willing to exit from the programs before they are completed. In an earlier study Calmfors (1994) concludes that intensified counseling and job search assistance raise re-employment probabilities substantially. 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.

[3]See also Meyer (1995) who reviews empirical evidence on compliance with unemployment insurance rules in the U.S. Using data from a randomized experiment Ashenfelter et al. (2005) do not find a significant impact of stricter sanctions on unemployment insurance claims and benefits. Ashenfelter et al. (2005) focus on the effect

Finally, there are two recent studies which look at the post-unemployment job matches and wages – but not in the context of sanction effects. Card et al. (2007) as well as Van Ours and Vodopivec (2008) assess the effects of a change of potential duration of UE benefits. The first looks at an extension in Austria, the second at a reduction in Slovenia. Both find no or little effect on job match quality or wages. It is important to note that *general* extensions or reductions of potential benefit durations show other reaction mechanics than *individual* benefit penalties which are explicitly linked to a *noncompliance behaviour* as it is the case in the paper here.

Thus, this paper differs from the small previous literature in two important respects. First, it is the first paper that empirically evaluates effects of UI benefit sanctions on the post-unemployment phase – mainly earnings and job tenure effects. Second, the detailed and precise data available for the earnings/job histories as well as for the timing of sanction procedures allow an analysis of earnings/job effects of high detail precision – with correspondingly interesting perspectives in empirical design.

4 Institutional Procedures in the Swiss UI System

4.1 Unemployment Benefits

Job seekers are entitled to unemployment benefits if they meet two requirements. First, the unemployed 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 12 months for those individuals who have been registered at least once in the three previous years. Individuals entering from non-employment who are looking for work 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 requirement. 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

of stricter review of benefit claims and information regarding job search obligations during the first 6-9 weeks of the unemployment spell. The treatment results in a rather modest 2 percentage point reduction in the initial qualification rate and does not affect the total claim duration. Our findings are different in two important respects. First, our study focuses on the effects of very strong financial sanctions that can be imposed on individuals who have passed the initial review during the entire unemployment spell. Second, the ex-ante effect we report is based on very salient differences in the likelihood of detecting a failure of adequate job search behavior. These two key differences explain why our study finds strong evidence for the effectiveness of benefit sanctions in shortening unemployment spell.

4030; 70 % for income between Sfr 4030 and Sfr 8100; and 0 % for income beyond 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. 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.

The second on-going obligation concerns participation in active labor market programs during the unemployment spell.[4] 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.

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.

4.2 Sanction Procedures

In the legal regulations[5], basically two motives for a sanction by benefit cut are brought out: First, to participate in an adequate amount at the loss to the UI that was caused by the noncompliance of the job seeker. However, the size of the benefit cut does finally not depend on the amount of the loss incurred by the UI but on the extent of the noncompliance. Secondly, these sanctions are aimed at exerting pressure on the job seekers to fulfil their obligations. In order to support a learning effect, sanction strength is increased in the case of repeated noncompliance for the same reason. It is useful to distinguish two types of sanctions. First, benefits can be

[4]Gerfin and Lechner (2002) and Lalive et al. (2001) contain background information on and an evaluation of the active labor market programs.

[5]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 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.

withheld for quitting the previous job, i.e. for causing 'unnecessary' job loss. Second, job-seekers can be punished for lack of compliance with eligibility requirements during the spell. The first type of sanctions are inflicted upon workers at the start of the unemployment spell. The second type of sanctions are imposed during the spell of unemployment. In this paper we focus on this second type of sanctions. Sanctions are private information and potential employers do not know whether a job applicant got sanctioned or not.

The process until a sanction is imposed 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. The UI law distinguishes four levels of sanction strengths: (i) Noncompliance of small degree leads to a benefit cut of 1 to 15 workdays. These short sanction durations are typically imposed if an unemployed person fails to apply to the minimum number of jobs (which is fixed by the caseworker at the PES) or doesn't show up at a meeting at the PES office. (ii) Benefit cut sanctions of medium duration (16 to 30 days), which can be imposed suite to an unappropriate rejection of a temporary job offer or suite to a second small sanctions noncompliance. (iii) Benefit cut sanctions of long duration (31 to 60 days). They are applied mainly as a consequence to a rejection of a 'suitable' longer temporary (half a year) or permanent job offer. (iv) There exists finally an "ultima ratio" sanction type which comes to application in cases of repeated noncompliance with demanded obligations in the UI system. For those people benefit entitlement will be reconsidered and potentially cancelled for a certain period. These cancellations can last some months or even more than a year. If the reluctant job seeker shows willingness to participate at the obligations again, the case will be reconsidered. In the dataset used for the empirical analysis, 88 % of the sanctions imposed were of short duration, 8% of all benefit reduction were of medium duration, and 9 % of long duration. Benefit entitlement cancellations are a rare phenomenon, a group of only some hundred people is observed in the data sample.

Benefits are immediately stopped after the CMEA has decided on legitimacy and duration of the sanction. 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.

Sanctions have to be executed within an enforcement period of six months. The enforcement period for the benefit cut starts at the first day of the committed noncompliance[6].

The actual application of these rules is delegated to the CMEA of the 26 cantons of Switzerland. All cantons have delegated the first phase of the sanction process to the public employment service. Some cantons have also delegated the second phase of the sanction process to the public employment service. Thus, the actual application of the sanction policy may differ both across cantons and within cantons. For instance, in December 1998 the average sanction rate was 10.8 sanctions enforced per 100 unemployment spells. The variation of the sanction rate across cantons is big, the minimum being 4.5 (Jura, Zug), the maximum being 25.3 (Obwalden) sanctions per 100 spells.

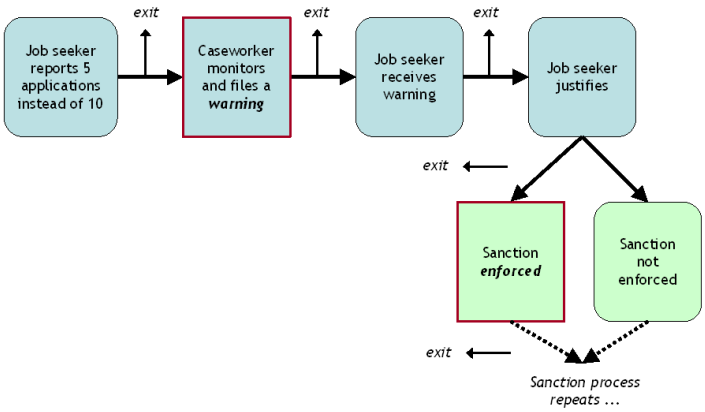
Figure 1 provides an overview over the subsequent steps in the sanction process, the timing of whom is really crucial for the identification of the respective steps' effects. The figure presents the possible action alternatives dependent on the actual stage of the process. At every stage, the option to exit from registered UE is available. This paper relies on information on the warning step as well as the enforcement step in evaluating the effect of sanctions on the outcome measures. Note that the results may be biased when individuals anticipate the exact date when a sanction is imposed. No such bias arises if individuals know the parameters of the sanction system, i.e. they may *anticipate* that they have a higher sanction probability when they do not comply with the benefit rules. The fact that *warnings* are issued to the concerned job seekers and that the available data exactly record the timing of these warnings is therefore a double advantage: First, the mentioned bias isn't present since the individuals get to know the warning; second, the precise data on timing allows us to explicitly model the warning as a separate state and to estimate its effect.

5 Data Sources and Data Structure

The present study is based on data from the Swiss unemployment register. Two characteristics of the data are particularly useful in studying the ex-post effect of benefit sanctions. In Switzerland, it is mandatory that an unemployed job-seeker be informed in advance that he or she is being monitored for non-compliance with benefit eligibility requirements. The unemployment register data contains the exact date when a job seeker is informed that a benefit sanction may be enforced. Such data is critical for the identification of the ex-post effect because it is necessary to know the date at which the job-seeker learns about the possibility that a sanction may be imposed. The reason is that all estimates of the ex-post effect will entail a comparison of a job-seeker with a sanction imposed to a suitable control group. Without data on announcements one may classify a job-seeker as a control that has already been notified of a pending

[6]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.

Figure 1: Steps and action alternatives in the sanction system of Swiss UI



sanction. Clearly, the estimated ex-post effect will be biased since job-seekers will respond to the information that a sanction may be imposed in the future.

Moreover, data on the Swiss labor market is ideal to study the ex-ante effect of benefit sanctions because Swiss public employment service offices have substantial leeway in choosing the monitoring intensity. Labor market policy is decentralized in Switzerland: cantons are the main authority concerned with the implementation of the federal labor market policy. Most cantons delegate aspects of the labor market policy to public employment service units. In terms of the actual sanction policy there is a tremendous variation in the probability of being sanctioned across cantons as well as across public employment service units within cantons. Based on such variation in the sanction rate it is possible to study the effect of increasing the probability of being punished on the unemployment exit rate of the non-sanctioned.

Our main analysis sample is drawn from the unemployment insurance register database (UIR). The purpose of this database is twofold. The first part of the data collects 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 second part of the database contains information on unemployment benefit 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). 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 219'499 individuals entering the UIR between August 1998 and July 1999. From these, we selected job seekers aged 30 to 55 entering unemployment from a job where they earned at least 500 CHF in the month prior to entering unemployment. These two sample restrictions reduce the sample to 83'952 individuals. The final sample restriction is based on the canton where individuals register for unemployment benefits. Recall that information on warnings is crucial from an identification point of view. Because such information is not available for all cantons, we select individuals registering at the PES of six major cantons that record information on warnings for most enforced sanctions (these cantons are Vaud, Valais and Fribourg in the West, Solothurn and Uri in the center, and Appenzell-Innerrhoden and Graubünden in the East). The resulting sample therefore covers 22'383 spells starting between August 1998 and July 1999 covering individuals aged 30 to 55 years in regions with full information on warnings and enforcement of benefit sanctions.

6 Descriptive Analysis

This section provides information on the sanction process, evolution of earnings as well as on the characteristics of individuals who are sanctioned and not sanctioned.

Table 1 provides descriptive statistics on the background characteristics of sanctioned and non-sanctioned individuals as well as the corresponding unemployment duration and share exiting to jobs. Clearly, there are strong differences with respect to the median duration of unemployment. Median search for a regular job lasts 211 days for sanctioned individuals who were warned that the sanction would be enforced, and even 311 days for individuals who were warned of a sanction and that sanction had also been imposed, whereas the median non-sanctioned individual searches for a job only for 138 days. These differences in duration are also reflected in the differences in terms of the proportion leaving unemployment for a regular job. Non-sanctioned individuals end up finding a regular job in 82 % of all cases, whereas warned individuals and individuals whose benefits were actually reduced leave unemployment for a regular job in 74 % or 67 % of all cases, respectively. Note that this difference has a mechanical and a behavioral component. Because sanction status is only realized during the on-going unemployment spell, jobless duration will be longer for job seekers with a sanction than for job seekers without a sanction. Isolating the mechanical from the behavioral component will require setting up an appropriate duration model.

The other characteristics in Table 1 illustrate that there are strong differences in terms of background characteristics between sanctioned and non-sanctioned spells. Sanctioned spells belong to people who are in comparison rather male, younger, without professional degree, full-time unemployed, unmarried and not Swiss. Sanctioning is less frequently practiced in French-speaking regions (see also section **??**). People who are good in foreign languages (understand at least two foreign languages) have a lower tendency to be sanctioned. Communication problems

Table 1: Comparison of the groups of non-sanctioned and sanctioned people in individual characteristics

	total	0 non-sanc	1 warn	2 w & enf	0 vs 1	0 vs 2	1 vs 2
UE duration,days (median)	153	138	211	311	***	***	***
Exit from UE to job, %	80.0	82.1	73.7	66.7	***	***	***
Women, %	38.5	39.3	34.2	37.1	***	*	*
Age	40.2	40.3	39.7	39.6	***	***	
W/o professional degree, %	26.6	25.4	31.7	32.1	***	***	
Part-time UE, %	9.1	9.5	7.1	8.3	***	*	
Unmarried, %	20.8	20.4	21.3	24.4		***	**
Non-Swiss, %	44.3	43.1	50.7	48.4	***	***	
French-speaking region, %	68.5	69.1	67.3	62.8	*	***	***
2nd foreign language, %	37.1	37.8	32.7	36.2	***		**
Non-reg. mother tongue, %	44.2	43.2	50.1	47.0	***	***	**
Reg. UE in last 2 years, %	9.3	9.1	9.5	10.9		**	
Monthly average earnings, CHF (6 months before UE)	3601	3682	3281	3186	***	***	
N	22'383	18'252	2'434	1'697			

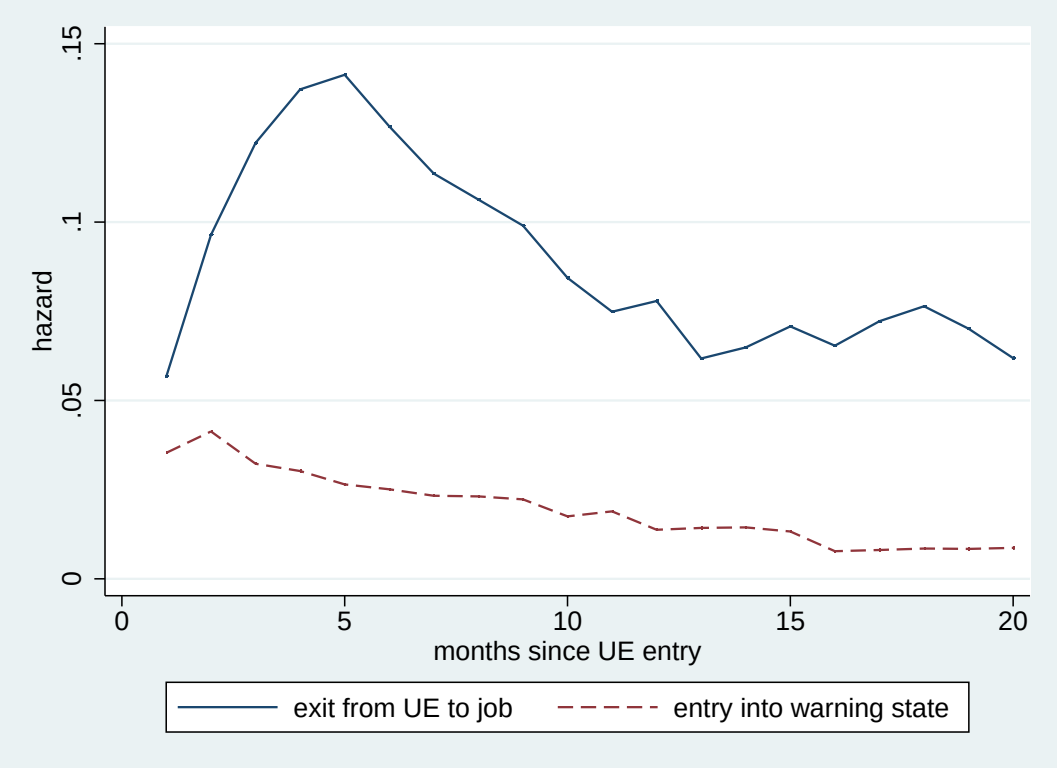
Notes: Spells are grouped in 0=non-sanctioned, 1=warned only and 2=warned and sanction enforced. T-tests for sample mean or median comparison (assuming unequal variances) between the respective groups were performed: * significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Own calculations based on merged UIR-SSA database.

seem also to be a reason for sanctions – as the higher percentage of spells belonging to people who do not speak the language of the region suggests. Finally, spells with another registered unemployment in the last two years (i.e. repeated UE) are relatively overrepresented in the group of the sanctioned. Non-sanctioned individuals earned almost 3700 CHF on average in the 6 months prior to entering unemployment. In contrast, individuals who were warned or saw their benefits reduced earned 3300 or 3200 respectively.

Interestingly, whereas there are striking differences between non-sanctioned individuals and individuals who came into contact with the sanction system, the differences between the latter two groups are much less pronounced. The warned and enforced groups do not differ in terms of age, professional degree, part-time unemployment status, nationality, previous registration as unemployed, and pre-unemployment earnings. Individuals who saw their benefits reduced are more likely to be female, single, less likely to live in the French speaking region, and more likely to be able to speak a second language than individuals who left unemployment after being warned but without seeing their benefits reduced. This suggests that the comparison between warned individuals and individuals who saw their benefits reduced is less strongly affected than the comparison between the non-sanctioned and any individual coming into contact with the sanction regime.

Figure 2 shows the empirical Kaplan-Meier estimates of the transition rate from unemploy-

Figure 2: Transition rate to jobs and & sanction warning rate



ment to regular jobs and the sanction warnings rate. The unemployment exit rate starts at a rather low level of 5 % per month, peaks at 15 % per month after 5 months of job search have elapsed, and tapers off gradually to the original of just above 5 % per month after 10 months of elapsed unemployment duration. Respectively, 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. Median duration of the observed UE spells is 141 days, mean amounts to 244 days.

The second hazard rate in Figure 2 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. A possible explanation for this shape bases on twofold arguments: First, people who got warned (and maybe enforced) are more aware of the functioning and the consequences of the sanctioning system; thus, they either know how to improve their strategy to comply or they want to avoid further sanction events at a higher effort in the future (rather a combination of both explanations is reasonable). Second, the UI law in Switzerland stipulates that repeated noncompliance will be punished markedly harder; this improves the incentive for people who are hit by a first sanction event to invest more in compliance in the future. In median, 77 days (mean 120) of the UE duration elapse until the first warning is announced for sanctioned people.

Figure 3: Enforcement Hazard

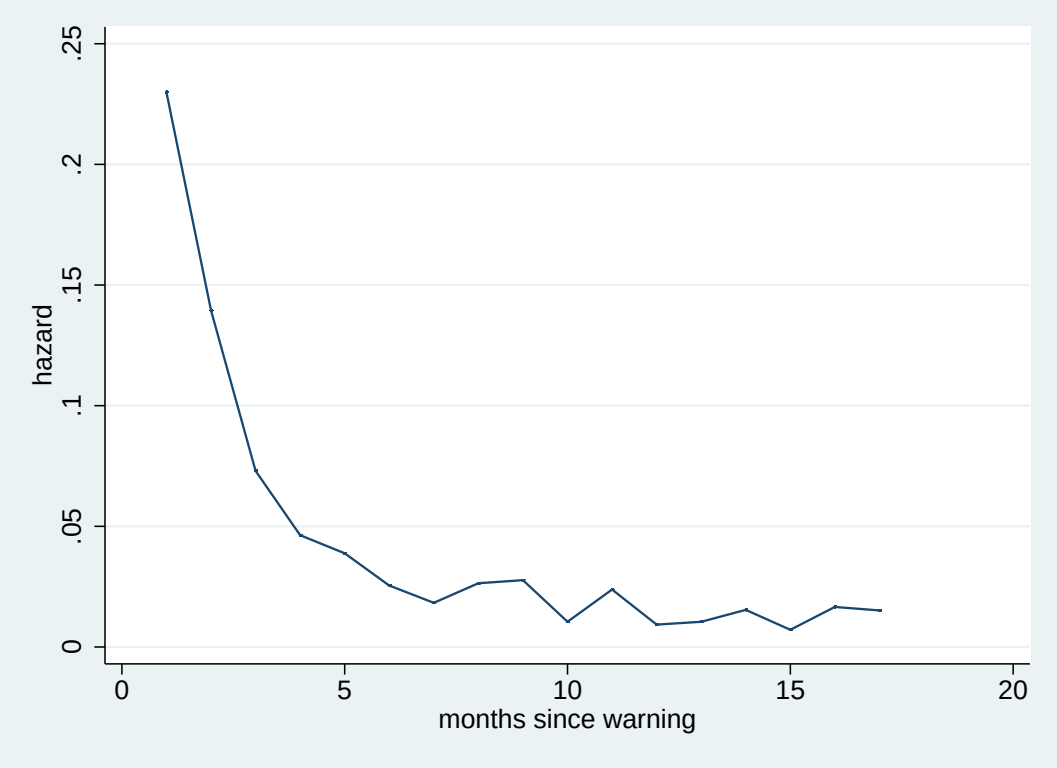
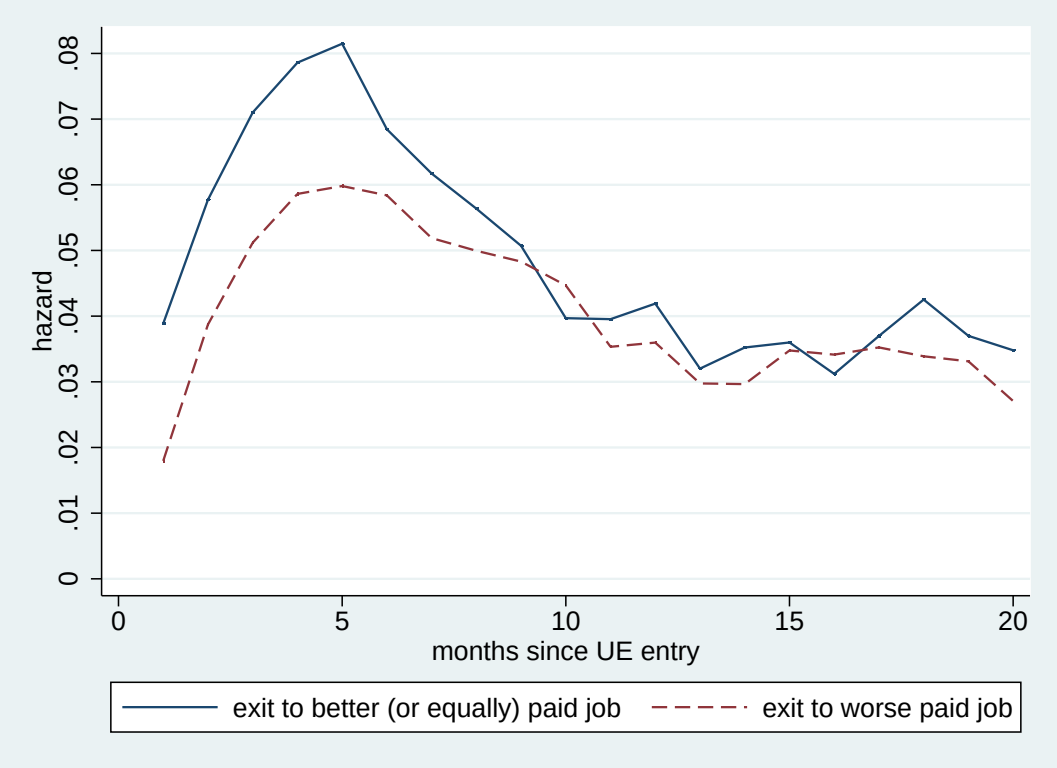


Figure 3 shows the enforcement hazard, i.e. the rate at which sanctions are enforced among those who have been warned. Clearly, there is a strong tendency to enforce a sanction in the first month after giving the warning. The enforcement hazard peaks at about 23 % in the first month, and decreases strongly to 7 % in month 2, and more gradually to levels below 5 % per month thereafter. Indeed, a supplementary analysis (not shown) that zooms in on the first 50 days reveals that the main part of the enforcement takes place right after the first 2 weeks of potential justification duration – between 2 and 3 weeks of duration since warning. After 3 weeks, the transition rate to enforcement declines gradually at a lower level. Thus, the normal case of the sanction procedure is indeed that the sanction – if justified – gets enforced immediately after receipt of the justification (taking into account maybe two or three days of administrative delay).

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. This suggests that if job seekers can anticipate imperfect correspondence between enforcement and warnings, there should be an incremental effect of enforcing the sanction.

A further set of descriptive analyzes refer to upward and downward mobility. Figure 4 provides Kaplan-Meier estimates of the hazard of leaving unemployment for a job that pays at least as much as the job prior to entering unemployment (upward mobility) and the corresponding

Figure 4: Upward and downward mobility

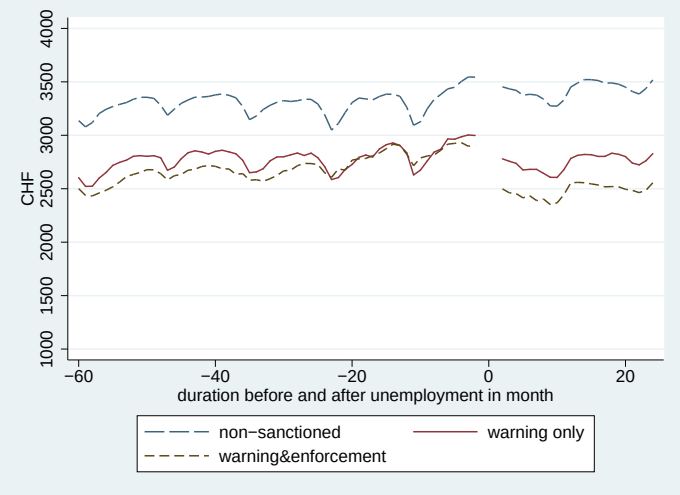


hazard of leaving unemployment for a job that pays worse than the job prior to entering unemployment. Descriptive evidence clearly suggests that the upward mobility dominates in the early parts of the unemployment spell. The upward hazard starts at a level of 4 %, peaks at a maximum of 8 % and decreases to a level between 3 and 4 % in months 10 and after. In contrast, downward mobility is quite restricted during the early parts of the spell. The downward hazard starts at 2 %, peaks at 6 % in month 5, and decreases to a level of between 3 and 4 % in months 10 and thereafter. Interestingly, the basic shape of these hazard rates appears to be quite similar.

What does evidence in Figure 4 have to say regarding the quality of post unemployment jobs? The fact that the downward hazard appears to catch up to the upward hazard suggests that job seekers are becoming less selective in terms of the jobs accepted compare to the job held before entering unemployment. This fact has been modeled in job search theory as a decreasing reservation wage. The reduction in the reservation wage (and corresponding increase in the hazard of leaving unemployment for worse jobs) is particularly strong during the first months of the spell, i.e. in months 0 to 10. Thereafter, both hazard rates remain at qualitatively the same level. A second interpretation of this pattern is based on heterogeneity. Suppose the hazard of leaving unemployment for a better job is more sensitive to characteristics such as age or gender. Heterogeneity then introduces more spurious negative duration dependence into the upward hazard rate than into the downward hazard rate. Since dynamic weeding out of good risks takes place more quickly in the upward mobility hazard, the two hazard

rates tend to be different in the initial phases of the spell but tend to become more similar thereafter. The empirical analysis below will address the question of how benefit sanctions affect the probability of moving to a job that is at least as good as the pre-unemployment job compared to moving down. Analyzing this question provides first rough information on the effects of benefits sanctions on post unemployment job quality. Addressing this issue requires specifying a model that takes joint determination of benefit sanction warnings, enforcements, and hazards of leaving unemployment upward or downward into account.

Figure 5: Duration-dependent employment earnings histories: by sanction status. (Averages 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.)



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. Figure 5 shows average (deflated) employment earnings in the 5 years prior to entering unemployment and in the 2 years after leaving unemployment by sanction status. Results indicate that non-sanctioned and sanctioned differ tremendously with respect to earnings levels. Whereas non-sanctioned earn almost 3500 CHF per month[7], individuals with either a warning or an actual benefit reduction earned on the order of 2750 CHF per month. 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

[7]When interpreting the earnings level of this graph, 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 labour force or not in the years before). This explains the low level of average employment earnings reported in the graph.

lower post-unemployment monthly earnings for the group with a sanction by about 200 CHF in comparison with the warned group. This is first descriptive evidence that benefit sanctions may reduce post-unemployment earnings.

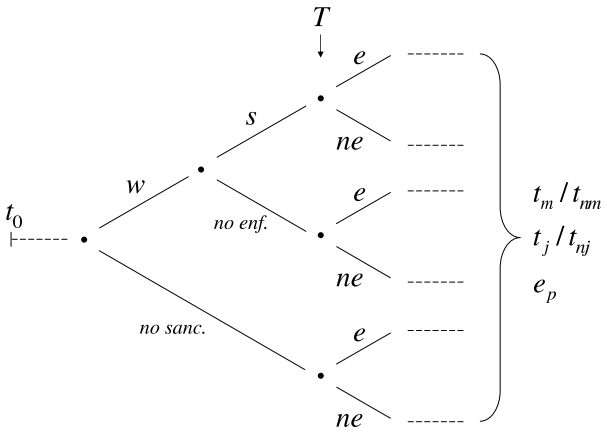
7 Econometric Analysis

The constructed dataset, as outlined in the section 5, allows the use of detailed duration analysis methods. In particular, a multi-states duration model can be implemented. This model combines information on the timing of benefit sanctions with information on the quality of post-unemployment jobs.

7.1 Modelling Individual's Event Histories

As a base for the evaluation of sanction effects on the post-unemployment outcomes, we model in the following the event history of an individual during and after unemployment.

Figure 6: Multiple states of the individual's process history



As depicted in Figure 6, 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 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. Beyond T, we observe the post-unemployment outcome – in the form of subsequent (non-)employment or (no) job durations (t_m/t_{nm} or t_j/t_{nj}) or of earnings (e_p) over a certain period. Due to the fact that our post-unemployment observation period ends by 31-12-2002, we analyse

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 be observed only 1.5 years in their outcomes.

We implement the described individual's process history 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. The central assumption for the nonparametric identification of such a causal multivariate duration model is the one of no anticipation of the treatment (Abbring and van den Berg 2003a). In our context, it is plausible to assume that individuals do not anticipate a warning because the time between detecting misconduct and actual warnings being issued is very short. Once an individual got such a warning, he or she may anticipate getting the benefit sanction enforced. Because our data provide specific information about the date of warning, this latter anticipation effect can be explicitly modelled and taken into account in the empirical analysis.

Let $D_1 \equiv I(t_w < t_l)$ i.e. duration until exit from unemployment to employment or nonemployment ($l = \{e, ne\}$, depending on which one gets realised first) is longer than duration until the first sanction warning. Let $D_2 \equiv I(t_w + t_s < t_l)$ i.e. duration until unemployment exit is longer than the time until a benefit sanction is imposed. 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 is then:

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

$\lambda_l(t)$ stands for individual duration dependence in our proportional hazard model, x represents observable individual characteristics, r is a vector of public employment service dummy variables and v_l represents the unobserved heterogeneity that accounts for possible selectivity in the exit process (see more on that in subsection 7.3). 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 modelling flexible duration dependence is the use of a step function (piecewise-constant duration model)

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

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. In the case of Switzerland where median UE duration amounts to a bit less than half a year, it makes sense to distinguish four time intervals: 1-3 months, 3-6 months, 6-12 months and 12 and more months. Because estimation includes as well

a constant term, normalization is necessary which is achieved by setting $\lambda_{l,0} = 0$ (i.e., baseline exit rate of 1).

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 and v as

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

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.

Using those elements, we can describe the density of the realised durations until exit from unemployment to a job (t_e) as

$$f(t_e|x, r, t_w, t_s, v_e) = \theta_e(t_e|x, r, t_w, t_s, v_e) \exp\left(-\int_0^{t_e} \theta_e(z|x, r, t_w, t_s, v_e)dz\right) \tag{4}$$

In the censored case, only the survivor $S(t_e|x, r, t_w, t_s, v_e) = \exp\left(-\int_0^{t_e} \theta_e(z|x, r, t_w, t_s, v_e)dz\right)$ builds the contribution to the likelihood. Note that t_w and t_s enter into the density expression above to model the time-dependent treatment variables. We allow the hazard to switch up- or downwards immediately at the time the treatment occurs (timing-of-events approach). We profit from the advantage of having highly precise data available that record in daily precision the occurrence of sanction warnings and enforcements (and unemployment duration in general).

The likelihood contribution of the transition process from unemployment to nonemployment has exactly the same structure as above. The contributions of the warning and enforcement states are analogue, but without treatment effects. Individuals who are not warned or not enforced enter as censored, thus just with the respective survivor function. This leads us to the following likelihood function (suppressing the conditioning on x, r, v and the treatments):

$$\mathcal{L} = \prod_{i=1}^{I} \int_v \theta_{w,i}^{c_w}(t_w) S_{w,i}(t_w) \theta_{s,i}^{c_s}(t_s) S_{s,i}(t_s) \theta_{e,i}^{c_e}(t_e) S_{e,i}(t_e) \theta_{ne,i}^{c_{ne}}(t_{ne}) S_{ne,i}(t_{ne}) \mathcal{L}_{p,i} \, dG(v) \tag{5}$$

where c designates a censoring indicator, being 1 if the respective duration is not censored. v is a vector of unobserved heterogeneity components which is discussed in section 7.3, $G(v)$ is the corresponding cumulative joint distribution. $\mathcal{L}_{p,i}$ contains the individual likelihood contribution of the post-unemployment period. This element of our model varies, depending on which post-unemployment outcome we evaluate.

7.2 Modelling the post-unemployment outcome measures

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

exit.

Following Figure 6 above, (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 between two survivor functions of two consecutive months, be it t_{b-1} and t_b, divided by the survivor of the earlier month. 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. Thus (ignoring the conditioning in the survivors for space reasons)

$$\theta_o(t_o|x, r, D_{wo}, D_{so}, t_w, t_s, t_e, t_{ne}, v_o) = \frac{S_o(t_{b-1,o}) - S_o(t_{b-1,o})}{S_o(t_{b-1,o})} \tag{6}$$

The survivors are modelled as described above. Note that we control here as well for the durations of the unemployment history (t_w, t_s, t_e, t_{ne}). In the post-unemployment period, the treatment effect results in a constant upward or downward shift of the respective hazard.

The corresponding likelihood contribution consists in the difference of the two survivors if the observation is not censored and in just the survivor if censored. Then, the individual likelihood contribution of the post-unemployment period (suppressing again the conditioning) is simply

$$\mathcal{L}_{p,i} = [\theta_m^{c_m c_e}(t_m) S_m(t_m) \theta_{nm}^{c_{nm} c_{ne}}(t_{nm}) S_{nm}(t_{nm})]^{c_e + c_{ne}} \tag{7}$$

Since these contributions are at the third stage of the selection (see Figure 6), double-censoring occurs. In the case of a censored unemployment spell, $\mathcal{L}_{p,i}$ equals 1.

Our *second model* is designed to analyse the effect of benefit sanctions on *job stability* in the post-unemployment period. We look at the first job/no job spell starting right after unemployment exit. A job spell is defined as the number of subsequent months with earnings registered from the same employer. Thus, this second model differs only in one respect from the first: employment spells can be split up in several job spells. No-job-spells, on the other hand, correspond exactly to the nonemployment spells. As a consequence, median duration of the first job after unemployment exit is shorter than the respective employment spell: ... months. Accordingly, we adapt the piecewise-constant duration splits to ... Otherwise, the only difference to model one is that now we have the states $o = \{j, nj\}$.

Our *third model* incorporates *earnings* as an outcome measure in the post-unemployment period. We evaluate the effects of benefit sanctions on the following measures: log earnings in the first month after unemployment exit (e_{p1}), the log of the sum of earnings in the first 6, 12 and 24 months after unemployment exit (e_{p2}, e_{p3}, e_{p4}).

We use a hazard structure to model the post-unemployment earnings distribution. This offers two advantages: First, we can avoid fix parametric distribution assumptions – like, e.g.,

log-normality – by applying the same flexible hazard function design as for the durations above. Second, it fits nicely into the competing risk duration model framework developed so far. 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. 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.

A distribution function modelled on the base of a hazard framework is applicable for non-negative random variables – which is exactly the case for earnings. Interpretation is suitable as well: Such a distribution function describes the (instantaneous) probability of realising a certain level of earnings. The related hazard represents the probability to realise a certain earnings level, given one hasn't realised a lower level ("survival" in the earnings distribution). Thus, the hazard has an upward-directed interpretation: the probability of not having reached a job that pays higher (than the realised level).

For the earnings data, we implement the estimation of sanction effects on earnings in the same way as in model one above – we just replace t_o by e_p, i.e. by one of the mentioned earnings measures. Since the earnings data are considered as continuous we use continuous-time hazards. Depending on the medians of the respective measures, we define suitable splits of the earnings values in the respective piecewise-constant earnings-level-dependence functions $\lambda_{e_{py}}(e_{py})$ with $y = \{1, ..., 4\}$[8].

The third model results in an individual post-unemployment likelihood contribution (suppressing conditioning) of

$$\mathcal{L}_{p,i} = \left[\theta_{e_{py}}^{c_{epy}}(e_{py}) S_{e_{py}}(e_{py})\right]^{d_{ey}} \tag{8}$$

Only in the case that the individual disposes of registered employment earnings in the observation period of the respective earnings measure e_{py}, $\mathcal{L}_{p,i}$ is different from 1. In these cases $d_{ey} = 1$.

A last difference of the design of this model from the first one comes from the fact that realised earnings are *dependent on the duration of the unemployment spell before*. We take this into account by adding $g(ln\ t_e)$, a polynomial in the log duration until exit into employment[9], to the observable's index of the respective hazards. This implies for the complete likelihood functions – which describe the joint distribution of t_w, t_s and t_e, t_{ne} and e_{py} – that we claim independence between the distributions of these durations and earnings *conditional on* x, r, D_w, D_s, the respective unobserved heterogeneity v *and* duration t_e in the case of earnings.

Remark: The results on the earnings models reported below stem from a preliminary version

[8] For... we find a median of ... which results in splits of ...

[9] Estimation shows that log duration terms up to the ... power get significant.

of the model above where we assumed a log-normal distribution for the earnings.

7.3 Dealing with multiple selectivity

Our evaluation setup implies, as Figure 6 demonstrates, an 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 unemployment and the (potential) treatment into a state of employment or nonemployment 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* modelling 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.

In other words, 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 sanction warning, enforcement and unemployment exit on the post-unemployment process create a local dependence between the four processes, i.e. the outcome measure changes directly after a warning has been issued, a sanction has been enforced or an exit from UE takes place. 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 (5) above. The vector $v \in \mathbb{R}^6_+$ comprises all the unobserved heterogeneity components of the respective model: In the first model, $v = (v_w, v_s, v_e, v_{ne}, v_m, v_{nm})$, in the second we replace the last two elements by v_j and v_{nj}, in the third by v_{epy} for the respective earnings measure e_{py}.

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 the first two models and five in the third, this implies that the joint distribution has 64 or 32 mass points. The associated probabilities are of the form

$$Pr(v_w = v_{wg}, v_s = v_{sg}, v_e = v_{eg}, v_{ne} = v_{neg}, v_r = v_{rg}, v_{nr} = v_{nrg}) = p_i \tag{9}$$

where all combinations with $g = \{a, b\}$ are possible. The last two elements of the probability are specified according to $r = \{m, j, epy\}$ for the three models, whereby for the last model v_{nr} doesn't exist. Thus, this generates probabilities p_i for $i = 1, \ldots, 64$ in the first two models and for $i = 1, \ldots, 32$ in the last model.

We specify the correlated unobserved heterogeneity in a more flexible way than Ham and LaLonde (1996) who rely on a one-factor structure. The only restriction we impose on the correlation probabilities is to give them a multinomial logit specification in order to fix them between zero and one. I.e., we model $p_i = exp(a_i)/\left[1 + \sum_i \exp(a_i)\right]$ and estimate the 64 or 32 a_i parameters.

7.4 Upward and Downward Mobility

An informative extension of the described model consists in separating transitions to regular jobs that pay at least equally well as the pre-unemployment job from transitions to regular jobs that pay worse. Note that the transition rate from unemployment to regular jobs, $\theta_e(t|x, r, D_1, D_2, v_e)$, can be separated into the rate of leaving unemployment to jobs that pay at least as well as or better (b) than the previous job $\theta_b(t|x, r, D_1, D_2, v_b)$, and into the rate of leaving unemployment to jobs that pay worse (w) than the previous job $\theta_w(t|x, r, D_1, D_2, v_w)$. Because moving up and moving down are mutually exclusive events, the two hazard rates add up to the original total transition rate from unemployment to regular jobs.

The key advantage of separating the effects of benefit sanctions on upward and downward mobility is that doing so allows us to understand how benefit sanctions change the composition of the pool of jobs accepted when leaving unemployment. Consider the likelihood ratio of leaving unemployment for better jobs compared to worse jobs, i.e. $\frac{\theta_b(t|x,r,D_1,D_2,v_b)}{\theta_w(t|x,r,D_1,D_2,v_w)}$. The treatment parameters provide information on how this likelihood is affected by benefit sanctions. For instance, $\delta_{1b} - \delta_{1w}$ provides information on how warnings affects the likelihood of leaving for better jobs (relative to the non-sanctioned). By the same reasoning, $\delta_{1b} - \delta_{1w} + \delta_{2b} - \delta_{2w}$ provides information on how sanction enforcements change the likelihood of leaving for a better job (relative to the non-sanctioned). Thus, separating post-unemployment transitions into upward and downward transitions allows estimating the effect of benefit sanctions on the composition of jobs accepted.

Moreover, a second rationale for separating upward and downward transition relates to the job search decisions. The baseline job search model provides a structure that pins search intensity $s(t)$ and the reservation wage $\rho(t)$. Let $F(.)$ denote the c.d.f. of the job offers. The

rate of leaving unemployment for regular jobs is

$$\theta(t) = \lambda(s(t))(1 - F(\rho(t)))$$

This unemployment exit rate can be decomposed into one component that is affected by search and into a second component that is affected by search and reservation wage. The key idea is to break the total exit rate into upward and downward mobility. Let w_p be the wage in the pre-unemployment job. Upward mobility is then the probability of receiving a wage offer times the probability of the wage offer paying more than the previous wage, or $\theta_b(t) = \lambda(s(t))(1 - F(w_p))$. Note that upward mobility depends only on the search intensity. Downward mobility is the product between the arrival rate of job offers and the probability of accepting a job that pays less the the previous wage, or $\theta_w(t) = \lambda(s(t))(F(w_p) - F(\rho(t)))$. Downward mobility reflects both, search intensity and reservation wage. Thus, decomposing total exits into transitions to better jobs and worse jobs allows speculating about the relative effects of sanctions on upward and downward mobility.

8 Econometric results

Remark: Note that the results below come from a preliminary version of the econometric model that doesn't include all the competing risk selections described in Figure 6. Specifically, the models below feature correlated unobserved heterogeneity, but they do not yet explicitly estimate the non-employment (ne) option. The results of the full model and the other outcome analyses described above (on long-term earnings effects, up to two years after exit, as well as on employment and job stability) will follow in April.

Table 2 provides information on the econometric estimates of the baseline model. Model 1 in Table 2 provides information on the treatment effects assuming unobserved heterogeneity is absent. Results indicate that there is no change in the hazard rate once individuals get notified that the sanction process has been started. The point estimate suggests a 1.1 percent increase in the hazard rate, not statistically different from zero. In contrast, enforcing the sanction appears to increase the hazard rate from unemployment to regular jobs. Estimates indicate that the hazard rate increases by about 8 % in addition to the original increase by 1.1. percent. The increase due to enforcement is statistically significantly different from zero. The transition rate from unemployment to regular jobs attains a level of about 0.25 % per day or about 7.5 % per month. This is in line with descriptive evidence on the unemployment exit hazard according to Figure 2.

Model 2 in Table 2 provides estimates of a model that allows for unobserved heterogeneity. Our estimates allow for two levels in all three hazard rates. Starting from a restrictive two point specification, we add masspoints 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

Table 2: Benefit Sanctions and Unemployment Duration

	Model 1		*Model 2*	
	Coeff.	z-value	Coeff.	z-value
Treatment effects				
Warning (δ_1, in %)	0.011	0.42	0.072	1.80
Enforcement (δ_2, in %)	0.079	1.90	0.028	0.50
Transition rates (in % per day, first 60 days)				
Exit to Job				
$exp(u_{3a})$	0.248	4.47	0.262	3.70
$exp(u_{3b})$			0.044	3.14
Warning				
$exp(u_{1a})$	0.100	2.14	0.175	1.71
$exp(u_{1b})$			0.004	1.59
Enforcement				
$exp(u_{2a})$	0.442	1.30	0.489	1.17
$exp(u_{2b})$			0.034	0.93
Probabilities				
$p_1 = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_3 = v_{3,a})$			0.388	7.06
$p_2 = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_3 = v_{3,a})$			0.064	2.71
$p_3 = Prob(v_1 = v_{1,b}, v_2 = v_{2,a}, v_3 = v_{3,a})$			0.451	8.08
$p_5 = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_3 = v_{3,b})$			0.037	4.40
$p_6 = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_3 = v_{3,b})$			0.016	2.77
$p_8 = Prob(v_1 = v_{1,b}, v_2 = v_{2,b}, v_3 = v_{3,b})$			0.044	–
Unobserved heterogeneity	No		Yes	
Control variables	Yes		Yes	
PES dummies	Yes		Yes	
Log-Likelihood	-158547		-158402	
BIC	159789		159679	
N	22383		22383	

Notes: We report the transition rates in % per day, suitable for the first 60 days of unemployment (first level of piecewise constant hazard). Further, these rates are calculated for an "average" individual: $u_{jl} = \lambda_{j,1} + v_{j,l} + \bar{x}'\beta_j + \bar{r}'\alpha_j$ where $j = \{1 \text{ or } s_1, 2 \text{ or } s_2, 3 \text{ or } u\}$ and $l = \{a, b\}$. Asymptotic z-values. Other probabilities are zero.
Source: Own calculations based on merged UIR-SSA database.

likelihood. Results indicate that accounting for unobserved heterogeneity matters. In contrast to the baseline model without unobserved heterogeneity, the model with heterogeneity indicates that the unemployment exit hazard shifts upward immediately upon informing an individual that the sanction process has been started. The effect is quantitatively important – the hazard rate increases by 7.2 percent after a warning. However, the event of actually enforcing the sanction does not appear to lead to a further increase in the unemployment exit hazard. Whereas the enforcement effect point estimate is positive (2.8 %), it is not statistically significantly different from zero. Overall, there does not appear to be a tremendous amount of selectivity.[10]

Estimates differ from the earlier studies by Abbring et al. (2005), van den Berg (2004), and Lalive et al. (2005). 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. Lalive et al. (2005) 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. A number of reasons may account for this difference in results. 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 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).

The key problem with the existing results is that they do not provide information on the effects of benefit sanctions on the quality of post-unemployment jobs. Table 3 provides estimates of the extension of the baseline model distinguishing upward and downward mobility (Model 3). The baseline model that does not make this distinction is reproduced for convenience (Model 2). Results indicate that benefit warnings increase both the hazard of leaving unemployment for a better job and the hazard of leaving unemployment to a worse job (compared to the job held prior to entering unemployment). The hazard of leaving unemployment to a worse job increases by almost 12 % upon notification of a warning. The hazard of leaving unemployment to a better job increases by even more than 16 %. This suggests that benefit warnings affect job search strategies in ways that affect both the probability of moving upward and downward. In the context of standard job search, this would be interpreted as benefit warnings changing the

[10]This can be explained as follows. Estimates indicate that there are 6 different groups. Is there selectivity with respect to exit and warnings? To see this, consider the average baseline exit rate of the groups that have a high warnings rate (probabilities 1,2,5, and 6). The high warnings group has an average exit rate of about .23 % per day. The low warnings group (probabilities 3, and 8) has an average exit rate of .24. This means that there is small negative selection into warnings of individuals with slightly lower exit rates. This explains why the warnings effect increases but only slightly. In contrast, the group with high enforcement rates has high exit rates and vice versa. This shows that there is positive selection into enforcement and the corresponding slight decrease in the treatment effect.

arrival rate of offers.

Imposing a benefit sanction leads to a reduction in the rate of leaving unemployment for a better (or equally well) paying job by about 9 % (not significantly different from zero). Thus job seekers with a benefit reduction leave unemployment to regular jobs about 6.5 % more likely than non-sanctioned individuals. In contrast, enforcing a benefit sanction tends to further increase the rate of leaving unemployment for jobs that pay worse than the pre-unemployment job. The rate of leaving unemployment for worse jobs is about 16.5 % higher for job seekers whose benefits were actually reduced compared to job seekers who were merely informed that a benefit reduction might take place. Thus, the overall effect of warning and enforcement leads job seekers to leave unemployment by 30 % more likely than non-sanctioned individuals. In the light of a standard job search model, these results suggest that job seekers are adjusting their expectations with respect to the minimum acceptable income level offered by the post unemployment job.

Taken together, these results suggest that warnings translate into changes in search intensity but do not immediately lead to changes in reservation wages. In contrast, enforcement appears to reduce search intensity somewhat (albeit insignificantly) and lead to an important downward adjustment in the reservation wage.

Table 4 provides results on the effects of benefit sanctions on post-unemployment earnings (controlling for pre-unemployment earnings levels). Results indicate that benefit sanction warnings reduce post unemployment earnings by 1.9 percent, and benefit sanction enforcements reduce earnings by a further 2.2 percent (effect marginally significant at the 10 % level).

Do these downward adjustments in reservation wages lead to effects on earnings changes? Table 5 provides estimates of the baseline model extended by an equation modeling the change in log earnings in the first month after leaving unemployment compared to log earnings in the last month before entering unemployment [Further results will extend this to longer horizons]. Importantly, the model for the change in log earnings takes the realized duration of unemployment into account. This means that the resulting treatment effects do not confound duration effects of benefit sanctions with the effects of sanctions conditional on duration. Results for the effects of benefit warnings and enforcements are broadly consistent with the corresponding estimates from the baseline model (see Table 2).

Results for the effects of benefit warnings and enforcement on changes in log earnings suggest that sanctions are not important in affecting the mean changes in log earnings. How do these results square with those reported in Figure 5? Results that do not allow for unobserved heterogeneity indicate that job seekers who are warned leave unemployment for jobs that pay 2.3 percent less. Moreover, job seekers who are leaving for jobs after benefits have been reduced experience an incremental reduction of 4 percent. Thus, simple earnings changes are consistent with Figure 5. Yet, allowing for unobserved heterogeneity changes the picture because earnings changes tend to be low for individuals with high warnings hazards and vice versa. This suggests that simple descriptive analysis is affected by selectivity bias. Once that selectivity has been taken into account, there are no further differences in earnings changes.

Table 3: Benefit Sanctions and Upward vs Downward Mobility

	Model 2			*Model 3*	
	Coeff.	z-value		Coeff.	z-value
Treatment effects					
Warning (δ_1, in %)	0.072	1.80	up (δ_{1b})	0.166	2.64
			down (δ_{1w})	0.118	1.74
Enforcement (δ_2, in %)	0.028	0.50	up (δ_{2b})	-0.090	-1.39
			down (δ_{2w})	0.165	1.77
Transition rates					
Exit to Job					
$exp(u_{3a})$	0.262	3.70	up ($exp(u_{3ab})$)	0.166	2.96
			down ($exp(u_{3aw})$)	0.090	2.20
$exp(u_{3b})$	0.044	3.14	up ($exp(u_{3bb})$)	0.030	2.80
			down ($exp(u_{3bw})$)	0.013	2.01
Warning					
$exp(u_{1a})$	0.175	1.71		0.016	1.57
$exp(u_{1b})$	0.004	1.59		0.223	1.59
Enforcement					
$exp(u_{2a})$	0.489	1.17		0.482	1.16
$exp(u_{2b})$	0.034	0.93		0.037	0.94
Probabilities					
p_1	0.388	7.06	p_1	0.513	1.40
p_2	0.064	2.71	p_2	0.061	0.65
p_3	0.451	8.08	p_3	0.278	0.99
p_5	0.037	4.40	p_8	0.012	0.55
p_6	0.016	2.77	p_{12}	0.026	0.68
p_8	0.044	–	p_{13}	0.025	0.79
			p_{14}	0.040	0.63
			p_{15}	0.041	0.70
			p_{16}	0.004	–
Unobserved heterogeneity	Yes			Yes	
Control variables	Yes			Yes	
PES dummies	Yes			Yes	
Log-Likelihood	-158402			-169788	
BIC	159679			171506	
N	22383			22383	

Notes: We report the transition rates in % per day, suitable for the first 60 days of unemployment (first level of piecewise constant hazard). Further, these rates are calculated for an "average" individual: $u_{jl(m)} = \lambda_{j,1} + v_{j,l} + \bar{x}'\beta_j + \bar{r}'\alpha_j$ where $j = \{1 \text{ or } s_1, 2 \text{ or } s_2, 3 \text{ or } u\}$, $l = \{a, b\}$ and $m = \{b, w\}$ for model 3. Asymptotic z-values. Other probabilities are zero. For the definition of the probabilities in model 2 see Table 2. The probabilities in model 3 are the following: $p_1 = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_b = v_{b,a}, v_w = v_{w,a})$, $p_2 = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_b = v_{b,a}, v_w = v_{w,a})$, $p_3 = Prob(v_1 = v_{1,b}, v_2 = v_{2,a}, v_b = v_{b,a}, v_w = v_{w,a})$, $p_8 = Prob(v_1 = v_{1,b}, v_2 = v_{2,b}, v_b = v_{b,a}, v_w = v_{w,b})$, $p_{12} = Prob(v_1 = v_{1,b}, v_2 = v_{2,b}, v_b = v_{b,b}, v_w = v_{w,a})$, $p_{13} = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_b = v_{b,b}, v_w = v_{w,b})$, $p_{14} = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_b = v_{b,b}, v_w = v_{w,b})$, $p_{15} = Prob(v_1 = v_{1,b}, v_2 = v_{2,a}, v_b = v_{b,b}, v_w = v_{w,b})$, $p_{16} = Prob(v_1 = v_{1,b}, v_2 = v_{2,b}, v_b = v_{b,b}, v_w = v_{w,b})$.

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

Table 4: Benefit Sanctions and Post Unemployment Earnings

	Model 4	
	Coeff.	z-value
Treatment effects		
... on earnings level		
Warning (δ_3, in %)	-1.931	-2.01
Enforcement (δ_4, in %)	-2.230	-1.67
... on exit to job		
Warning (δ_1, in %)	0.002	0.07
Enforcement (δ_2, in %)	0.044	0.95
Earnings level (in CHF)		
$exp(u_{4a})$	3782.87	15.09
$exp(u_{4b})$	1065.308	14.90
Transition rates (in % per day, first 60 days)		
Exit to Job		
$exp(u_{3a})$	0.253	3.97
$exp(u_{3b})$	0.016	2.17
Warning		
$exp(u_{1a})$	0.005	1.57
$exp(u_{1b})$	0.175	1.69
Enforcement		
$exp(u_{2a})$	0.445	-8.42
$exp(u_{2b})$	0	–
Probabilities		
$p_1 = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_3 = v_{3,a}, \nu = \nu_a)$	0.450	2.61
$p_3 = Prob(v_1 = v_{1,b}, v_2 = v_{2,a}, v_3 = v_{3,a}, \nu = \nu_a)$	0.458	2.70
$p_5 = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_3 = v_{3,a}, \nu = \nu_b)$	0.019	1.43
$p_7 = Prob(v_1 = v_{1,b}, v_2 = v_{2,a}, v_3 = v_{3,a}, \nu = \nu_b)$	0.033	1.50
$p_{10} = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_3 = v_{3,b}, \nu = \nu_a)$	0.024	1.43
$p_{15} = Prob(v_1 = v_{1,b}, v_2 = v_{2,a}, v_3 = v_{3,b}, \nu = \nu_b)$	0.014	1.25
$p_{16} = Prob(v_1 = v_{1,b}, v_2 = v_{2,b}, v_3 = v_{3,b}, \nu = \nu_b)$	0.003	–
Unobserved heterogeneity	Yes	
Control variables	Yes	
PES dummies	Yes	
Log-Likelihood	-166713	
BIC	168411	
N	22383	

Notes: The earnings level in Swiss Francs is calculated for an "average" individual: $u_{4l} = \alpha_L + \nu_l + \bar{x}'\beta_j + \bar{r}'\alpha_j + \tilde{t}_U\gamma$ where $\tilde{t}_U$ is the median unemployment duration. We report the transition rates in % per day, suitable for the first 60 days of unemployment (first level of piecewise constant hazard). Further, these rates are calculated for an "average" individual: $u_{jl} = \lambda_{j,1} + v_{j,l} + \bar{x}'\beta_j + \bar{r}'\alpha_j$ where $j = \{1 \text{ or } s_1, 2 \text{ or } s_2, 3 \text{ or } u\}$ and $l = \{a, b\}$. Asymptotic z-values. Other probabilities are zero.

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

Table 5: Benefit Sanctions and Earnings Changes

	Model 5	
	Coeff.	z-value
Treatment effects		
... on earnings growth		
Warning (δ_3, in %)	-0.006	-0.48
Enforcement (δ_4, in %)	-0.010	-0.58
... on exit to job		
Warning (δ_1, in %)	0.075	1.85
Enforcement (δ_2, in %)	0.063	1.13
Earnings growth level (in %)		
u_{4a}	0.082	0.90
u_{4b}	-1.386	-14.71
Transition rates (in % per day, first 60 days)		
Exit to Job		
$exp(u_{3a})$	0.264	3.65
$exp(u_{3b})$	0.050	3.25
Warning		
$exp(u_{1a})$	0.177	1.69
$exp(u_{1b})$	0.005	1.57
Enforcement		
$exp(u_{2a})$	1.406	1.00
$exp(u_{2b})$	0.186	1.00
Probabilities		
$p_1 = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_3 = v_{3,a}, \eta = \eta_a)$	0.091	2.22
$p_2 = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_3 = v_{3,a}, \eta = \eta_a)$	0.329	3.17
$p_4 = Prob(v_1 = v_{1,b}, v_2 = v_{2,b}, v_3 = v_{3,a}, \eta = \eta_a)$	0.446	5.49
$p_5 = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_3 = v_{3,a}, \eta = \eta_b)$	0.009	0.32
$p_6 = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_3 = v_{3,a}, \eta = \eta_b)$	0.010	0.34
$p_7 = Prob(v_1 = v_{1,b}, v_2 = v_{2,a}, v_3 = v_{3,a}, \eta = \eta_b)$	0.007	0.20
$p_9 = Prob(v_1 = v_{1,a}, v_2 = v_{2,a}, v_3 = v_{3,b}, \eta = \eta_a)$	0.008	0.21
$p_{10} = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_3 = v_{3,b}, \eta = \eta_a)$	0.045	0.45
$p_{12} = Prob(v_1 = v_{1,b}, v_2 = v_{2,b}, v_3 = v_{3,b}, \eta = \eta_a)$	0.040	0.31
$p_{14} = Prob(v_1 = v_{1,a}, v_2 = v_{2,b}, v_3 = v_{3,b}, \eta = \eta_b)$	0.008	0.13
$p_{16} = Prob(v_1 = v_{1,b}, v_2 = v_{2,b}, v_3 = v_{3,b}, \eta = \eta_b)$	0.008	–
Unobserved heterogeneity	Yes	
Control variables	Yes	
PES dummies	Yes	
Log-Likelihood	-169992	
BIC	171710	
N	22383	

Notes: The earnings growth level is calculated for an "average" individual: $u_{4l} = \alpha_\Delta + \eta_l + \bar{x}'\beta_j + \bar{r}'\alpha_j + \tilde{t}_U\gamma$ where $\tilde{t}_U$ is the median unemployment duration. We report the transition rates in % per day, suitable for the first 60 days of unemployment (first level of piecewise constant hazard). Further, these rates are calculated for an "average" individual: $u_{jl} = \lambda_{j,1} + v_{j,l} + \bar{x}'\beta_j + \bar{r}'\alpha_j$ where $j = \{1 \text{ or } s_1, 2 \text{ or } s_2, 3 \text{ or } u\}$ and $l = \{a, b\}$. Asymptotic z-values. Other probabilities are zero.

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

9 Conclusions

This paper studies the effect of benefit sanctions on unemployment duration and on post-unemployment job quality as measured by earnings and employment stability.

Remark: Further results from the extended model (described above) which assess long-term earnings effects (up to two years after exit) and effects on employment/job stability will follow in April.

The empirical analysis is based on rich, administrative data covering a sanction system that informs job seekers of pending sanction procedures prior to enforcing such benefit reductions.

Results indicate that warnings that a benefit sanction may be inflicted reduce unemployment duration. There is a small but statistically insignificant effect on unemployment exit rates once the sanction has been enforced. Separating the sanction effects on hazards of leaving to better jobs and worse jobs (compared to the pre-unemployment job) indicates that warnings lead to increases in both exit hazards whereas enforcements only leads to higher hazards of leaving unemployment to worse jobs. This suggests that benefit enforcements do but benefit warnings do not reduce post-unemployment job quality. A final set of estimates finds a reductive effect of sanction warnings / enforcements on earnings levels (one month after unemployment exit). On earnings changes, no statistically significant effect is found.

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