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HOW EFFECTIVE ARE UNEMPLOYMENT BENEFIT SANCTIONS? LOOKING BEYOND UNEMPLOYMENT EXIT

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SUMMARY

This paper provides a comprehensive evaluation of the effects of benefit sanctions on post-unemployment outcomes such as post-unemployment employment stability and earnings. We use rich register data which allow us to distinguish between a warning that a benefit reduction may take place in the near future and the actual withdrawal of unemployment benefits. Adopting a multivariate mixed proportional hazard approach to address selectivity, we find that warnings do not affect subsequent employment stability but do reduce post-unemployment earnings. Actual benefit reductions lower the quality of post-unemployment jobs both in terms of job duration as well as in terms of earnings. Copyright © 2012 John Wiley & Sons, Ltd.

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1. INTRODUCTION

All OECD countries provide income replacement for workers who lose their jobs. Insurance smooths consumption but it entails a cost in terms of reduced search for new jobs. To restore search incentives often activation measures are introduced. Unemployed people are required to attend intensive interviews with employment counsellors, to apply for job vacancies as directed by the employment counsellors, to independently search for job vacancies and to apply for jobs, to accept offers of suitable work, and to attend training programmes. If unemployed workers are unwilling to participate in such activities, search insufficiently for a job or reject job offers, they may face a reduction in their unemployment benefits, i.e. they may have a benefit sanction imposed. Such a benefit sanction may be permanent or temporary and may involve a partial reduction or a complete removal of unemployment benefits.

This paper asks how benefit sanctions affect job seekers' post-unemployment earnings. The answer to this question is not trivial. Sanctions have been shown to increase the rate of leaving unemployment among affected job seekers (Van den Berg *et al*., 2004; Abbring *et al*., 2005). Faster exit from unemployment boosts post-unemployment labour earnings since sanctioned job seekers start working earlier than non-sanctioned ones. The key issue, however, is whether sanctioned job seekers are able to leave unemployment for jobs that are as stable and as well paid as those for non-sanctioned job seekers. If sanctioned job seekers sacrifice some stability and/or a part of their wage to leave unemployment more quickly, it is not clear that sanctioned job seekers will end up earning more than non-sanctioned job seekers.

Understanding the net effects of benefit sanctions on post-unemployment labour earnings is important for at least three reasons. Unemployment insurance is a central component of social insurance against income

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shocks that is a feature of all OECD countries' policy mix. Understanding how one central component, benefit sanctions, affects earnings and employment stability of insured job seekers is therefore crucial in thinking about how to redesign these systems. Second, in contrast to active labour market programmes, sanctions seem to enhance exits from unemployment. This explains the recent shift of large European economies such as Germany towards stiffer sanction regimes. Yet, unless we understand more closely how this policy affects post-unemployment labour market trajectories, the policy option of adopting a stiff sanction regime is based on incomplete evidence: the effects of sanctions on leaving unemployment. A comprehensive evaluation of benefit sanctions can fill the gap in also providing evidence on the phase beyond unemployment.

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

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

The small body of recent empirical literature on benefit sanctions is mainly of European origin and supports the positive short-term effects on the exit rate from unemployment.[1] Two Dutch papers find that benefit sanctions double the outflow from unemployment to a job (Van den Berg *et al*., 2004; Abbring *et al*., 2005). Using Danish data Svarer (2011) finds that the unemployment exit rate increases by more than 50% following enforcement of a sanction. Jensen *et al*. (2003) find a small effect of the sanctions that are part of the Danish youth unemployment programme. Schneider (2008), studying benefit sanctions in Germany, finds no significant effect of sanctions on reported reservation wages. Hofmann (2008), on the other hand, reports positive effects of benefit sanctions on the employment probability of West German unemployed. A common element in these benefit sanction studies is that they are restricted to the analysis of the effects on the duration of unemployment. This is not surprising as suitable data to perform an analysis of post-unemployment jobs are often not available. Even in the context of much more frequently investigated effects of changes in level or duration of unemployment benefits, the post-unemployment dimension of these effects is rarely considered.[2]

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

[2] Three recent studies which do look at the post-unemployment effects are Card *et al*. (2010), Van Ours and Vodopivec (2008), and Lalive (2007). These studies assess the effects of a change of potential duration of unemployment benefits in Austria and Slovenia. Both find no or little effect on job match quality or wages.

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This paper is most similar to Van den Berg and Vikström (2009) and Lalive *et al*. (2005). Van den Berg and Vikström (2009) assess the effects of benefit sanctions on post-unemployment outcomes in Sweden. Lalive *et al*. (2005) use similar data and apply multivariate mixed proportional hazard modelling to assess the effects of warnings and enforcements on unemployment exit. This paper differs in at least three important respects. First, the main focus of this paper is on post-unemployment outcomes such as employment stability and earnings. Whereas Van den Berg and Vikström (2009) study effects on wages and job tenure, they do not focus on earnings. Lalive *et al*. (2005) disregard post-unemployment outcomes. Second, this paper provides key simulations that can help in assessing the overall assessment of benefit sanctions. Specifically, this paper compares the earnings-enhancing effects of benefit sanctions due to faster exit from unemployment to the earnings-reducing effects of benefit sanctions due to accepting jobs that pay less and/or are less stable. Third, this paper constructs and develops multivariate mixed proportional hazard models that do not restrict the correlation between heterogeneity components in any of the processes that are involved. This goes beyond existing studies such as Bonnal *et al*. (1997) and Van den Berg and Vikström (2009), who use factor structure modelling to reduce dimensionality, or Lalive *et al*. (2005), whose main results imply degenerate distributions of unobserved heterogeneity.

The remainder of this paper is structured as follows. Section 2 discusses institutional procedures in the Swiss UI system, both concerning unemployment benefits and sanction procedures. In Section 3 we briefly outline possible behavioural explanations for sanction effects in the post-unemployment period. Section 4 presents our data and a descriptive analysis. In Section 5 we provide the set-up for the econometric analysis, while in Section 6 we provide our parameter estimates. Section 7 concludes.

2. INSTITUTIONAL PROCEDURES IN THE SWISS UI SYSTEM

Job seekers are entitled to unemployment benefits if they meet two requirements. First, they must have paid unemployment insurance taxes for at least six months in the two years prior to registering at the public employment service (PES). The contribution period is extended to 12 months for those individuals who have been registered at least once in the three previous years. Job seekers entering the labour market are exempted from the contribution requirement if they have been in school, in prison, employed outside of Switzerland or have been taking care of children. Second, job seekers must possess the capability to fulfil 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 replaces roughly 76% of unemployment benefits for a single job seeker with no other sources of earnings (OECD, 1999).

The potential duration of unemployment benefits is two years for individuals who meet the contribution and employability requirements. After this period of two years unemployed people have to rely on social assistance. The replacement ratio is 80%; and 70 % for job seekers who earned more than CHF 4030 prior to unemployment and are not caring for children.[3] Job seekers have to pay all earnings and social insurance taxes except the unemployment insurance tax rate (which stands at about 2%). This means that the gross replacement rate is similar to the net replacement rate.

The entitlement criteria during the unemployment spell concern job search requirements and participation in active labour market programmes. Job seekers are obliged to make a minimum number of applications for 'suitable' jobs each month.[4] Also, they are obliged to participate in active labour

[3] 1 CHF = 0.86 euros.

[4] A suitable job has to meet four criteria: (i) the travel time from home to job must not exceed 2 hours; (ii) the new job contract cannot 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.

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market programmes during the unemployment spell. Compliance with the job search and programme 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. Caseworkers monitor job search by checking that job seekers use to fill in the details of the jobs to which they have applied. Job seekers are typically required to apply to about 10 vacancies per month. Caseworkers have some discretion to adjust this target. Caseworkers count the number of new applications in all cases and they may also check up on the applications claimed by job seekers. Participation in a labour market programme is monitored by the caseworker because programme suppliers only get paid for the actual number of days a job seeker attends the programme.

In this paper we focus on benefit sanctions because of non-compliance with eligibility requirements. 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 misbehaviour by the unemployed person is detected and reported to the Cantonal Ministry of Economic Affairs (CMEA), either by the caseworker, by a prospective employer, or by the active labour market programme 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 by which the clarification is to be sent back. The average duration between the date job seekers are informed and the date by 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 are sufficient grounds 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.

Once the CMEA has decided on the legitimacy and duration of the sanction, benefit payments are stopped for the time specified in the warning letter. The CMEA has to take this decision within an enforcement period of six months. The enforcement period for the benefit cut starts on the first day of the committed non-compliance. Owing to administrative delay at the CMEA, there is no strict one-to-one relationship between receiving a warning letter and the day when benefits are stopped. Once the sanction has been imposed, the unemployed person 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.

Note that whether or not a job seeker has been warned of a sanction or whether a sanction has been informed is private information. Neither caseworkers nor potential employers know about the current sanction status since this is decided at the CMEA. Moreover, job seekers are not forced to disclose sanction status.

3. HOW SANCTIONS AFFECT BEHAVIOUR

What are the possible behavioural explanations that can elucidate the effects of the sanction system on labour market outcomes after unemployment exit? Job search theory provides a convenient framework for understanding this issue.[5] There are two behavioural responses of unemployed workers to benefit

[5] See Boone and Van Ours (2006) and Boone *et al.* (2007) for recent analyses of this issue in the labour 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 offences would be minimized by setting fines high enough to eliminate all offences. If unemployed workers are risk averse this result may not hold for the labour market and a combination of intensive monitoring and small fines may be the optimal outcome.

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sanctions. First, they might increase *search intensity*. Second, sanctions could make them lower their demands concerning post-unemployment jobs, i.e. reduce their *reservation wage*. Benefit sanctions affect behaviour because they reduce the value of being unemployed. Two effects may be distinguished. The first effect is the *ex post effect*—the effect that a benefit reduction increases costs of being unemployed, thereby changing the behaviour of the unemployed. However, unemployed people may already change their behaviour in anticipation of a benefit sanction, to avoid getting one imposed. This second effect is the *ex ante effect*—the effect that the *risk of getting a benefit sanction* influences behaviour as well.

Both increased search intensity and lower reservation wages lead to a reduction of unemployment duration. But how will benefit sanctions affect post-unemployment earnings and job stability? From a theoretical point of view, increased search intensity could lead to a post-unemployment job that is at least as good as the job that would have been found without a sanction. This is particularly so if skill depreciation or employer signalling is important. If job seekers search harder for a new job and find one earlier, their skills depreciate less and they will be offered better jobs because they have spent less time in unemployment. However, to the extent that a reduction of the reservation wage leads to acceptance of lower-quality jobs, wage loss and reduced job duration may be expected. Thus theoretical predictions are *inconclusive* concerning post-unemployment sanction effects. It is up to an *empirical evaluation* to establish which effects dominate in practice.

Moreover, the *ex post effects of warnings and of enforcing the benefit sanction may differ if job seekers search for jobs of different quality*. Job seekers who receive a warning letter know that the probability of a benefit reduction has substantially increased but they continue to receive the same benefits. This will change behaviour after a warning letter has been issued—the *ex post* effect of a warning. Note that this *ex post* effect should not be confused with the *ex ante* effects of benefit sanctions. The *ex ante* effect refers to the behaviour of job seekers before a warning letter has been issued. In contrast, job seekers who receive the information that their benefits are cut experience a strong, temporary reduction in the stream of benefits received. This suggests that the effect of a benefit reduction will be quantitatively stronger than the effect of a warning that benefits may be reduced in the future.

Finally, a further dimension of effects of benefit sanctions—which has been ignored so far in the empirical literature—is *their impact on labour force attachment*. For some subpopulation of unemployed workers sanctions may not promote but discourage search effort. This group of job seekers attaches only slightly more value to being in registered unemployment than to being in a state of unregistered unemployment which imposes no obligations. For these individuals the imposition—or even the warning—of a sanction reduces the value of registered unemployment such that they now decide to leave UI for unregistered non-employment. This status is more attractive for them since it avoids the cost of job search and compliance with the obligations of the UI. In addition, they can avoid the pressure of being monitored and the risk of further sanctions. Note, moreover, that an *ex ante* effect for this kind of behavioural response is also conceivable: the mere threat of a potential sanction influences the labour force participation decision. It is a priori not clear if such labour force exits are of a temporary or permanent nature. The existence and nature of such a behavioural response is a matter of empirical research. We will come back to this in Section 6.3.

4. DATA AND DESCRIPTIVE ANALYSIS

4.1. Data Sources and Data Structure

Our study is based on data from the Swiss unemployment register. Our main sample is drawn from the unemployment insurance register database (UIR) covering the time period 1998–2003. It contains information on all individuals registering with the public employment service (PES)—which can be

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job seekers who are eligible for unemployment benefits but also other individuals asking the PES for assistance. The database also contains information on unemployment benefit 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 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 earnings provided from the social security administration (SSA) covering the period 1993–2002. This database contains earnings information on individuals who are eligible for the public retirement pension system. The data provide information on earnings but also on non-labour earnings sources such as unemployment benefits, disability benefits, military benefits, etc. Earnings and non-labour earnings information is available at monthly precision. The SSA does not record information on hours worked.

From the merged UIR-SSA database, we draw an inflow sample covering individuals entering the UIR between August 1998 and July 1999. From these, we selected UI-eligible job seekers aged 30–55 entering unemployment from a job with positive earnings in the year prior to entering unemployment to focus the sample on individuals who acquired at least some benefit rights. Moreover, we restrict the sample to individuals who are entering unemployment in cantons with reliable information on warnings. Cantons differ in terms of the number of actual benefit reductions that are preceded by a warning letter. We interpret this as missing information on warning letters because job seekers must be informed before actual benefit reductions take place. The analysis focuses on cantons where almost all warnings preceding actual benefit reductions are present.[6] This sample is not representative for Switzerland. Yet this sample restriction allows understanding both the effects of a warning and the effect of enforcing the benefit sanction. The resulting sample covers 23,961 spells. The median duration of unemployment is 153 days; 80.0% of the unemployed found a job, 19.8% of the unemployed received a sanctions warning, while 8.4% actually had a benefit sanction imposed (for details the online Appendix, available as supporting information).

4.2. Descriptive Analysis

This section provides a descriptive analysis of the earnings of warned, sanctioned, and non-sanctioned job seekers, along with information on the sanction process.

The key piece of descriptive evidence concerns earnings *histories* of individuals who never experience a sanction, individuals who receive a warning but this warning does not lead to an actual reduction in benefits, and individuals who receive a warning and the benefit cut is also realized. Recall that our earnings data span the time period 1993–2002. This allows constructing average (deflated) earnings in the five years prior to entering unemployment and in the two years after leaving unemployment by sanction status (top graph of Figure 1). Results indicate that non-sanctioned and sanctioned differ tremendously with respect to earnings levels. Whereas non-sanctioned individuals earn almost 3500 CHF per month, those with either a warning or an actual benefit reduction earned on the order of 2750 CHF per month. The regular fluctuations in earnings are due to a strong seasonal pattern in unemployment for one of the regions considered in the sample.

Interestingly, while the earnings gap between individuals who were warned only and those who are warned and enforced is visible five years before entering unemployment, the gap disappears around the time 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

[6] These cantons are Vaud, Valais and Fribourg in the west, Solothurn and Uri in the centre, and Appenzell-Innerrhoden and Graubünden in the east. On average, 5% of the warnings are missing. Cantons with at least 87.5% warnings present were chosen for the sample. We predict warning times for the remaining 5% of sanctioned job seekers using a Tobit regression based on information on observed characteristics. Results are unaffected by disregarding these job seekers. The sample covers 26.4% of the inflow in the Swiss UIR during the respective year.

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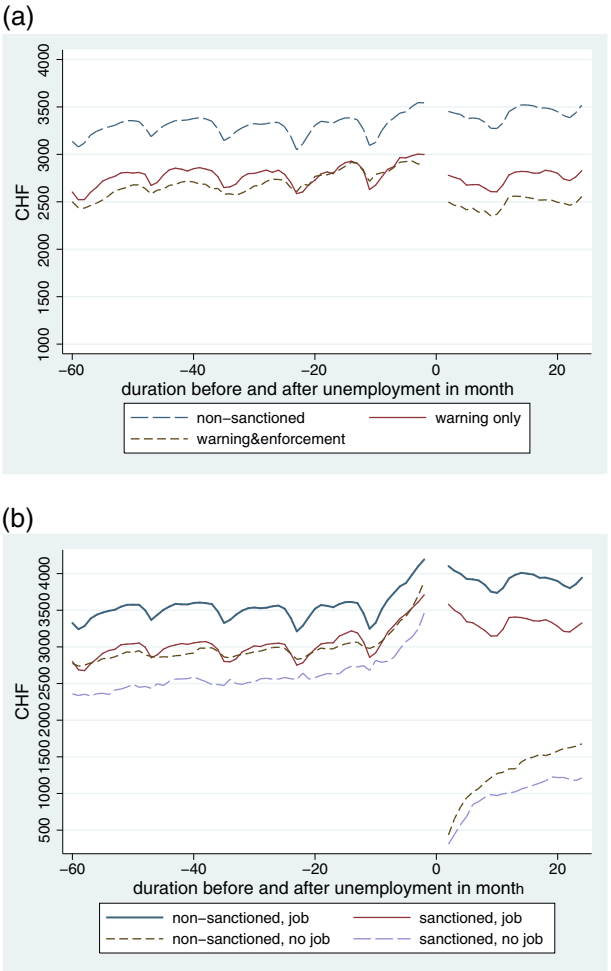

Figure 1. Duration-dependent employment earnings histories: (a) by sanction status; (b) by employment status. These lines average earnings histories dependent on the duration before entry in unemployment (negative values) or after exit from unemployment (positive) for all spells belonging to the inflow sample and to the respective subgroup

latter two groups are more informative. Moreover, enforcing the sanction appears to lower post-unemployment monthly earnings for the group with a sanction by about 200 CHF in comparison with the warned group. This is a first descriptive hint that benefit sanctions may reduce post-unemployment earnings. This picture could be misleading, however, since the descriptive effect may be confounded by unobserved characteristics and endogenous selectivity. These will be taken into account in the estimated models. The bottom graph of Figure 1 distinguishes the earnings paths with respect to the exit destination—into employment or non-employment. This figure supports the previous one, pointing to an increased earnings difference between the sanctioned and non-sanctioned after unemployment exit for both the exit to employment and to the non-employment group.

This discussion suggests that it is central to further understand the sanction process. This process allocates job seekers to a group that is warned but not enforced, a group that experiences a warning plus a benefit reduction, and the remaining group of job seekers who do not get in touch with any of the sanction stages.

Figure 2 shows the empirical Kaplan–Meier estimates of the transition rate from unemployment to employment or non-employment and the sanction warnings rate. Unemployment duration refers to total

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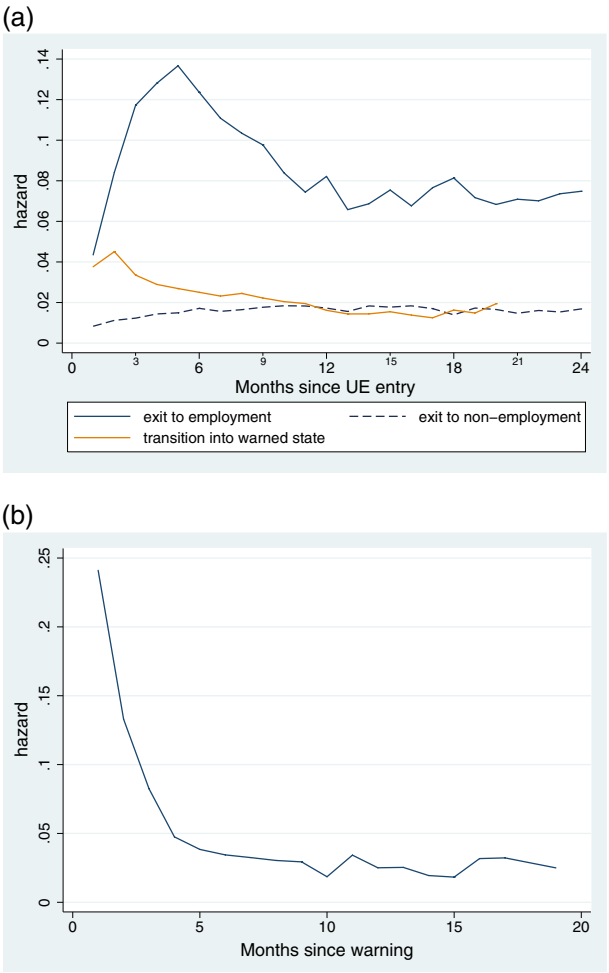

Figure 2. Unemployment transition rates and sanction enforcement rates: (a) exit rates from unemployment and sanction warning rates; (b) sanction enforcement rates

unemployment duration including participation in active labour market programmes. Job seekers leave unemployment for employment if their labour earnings in the first month after unemployment exceed income from other sources of income, else job seekers leave unemployment for non-employment. These exits represent exits to temporary inactivity, sickness insurance, education, etc. Our data do not allow us to distinguish between the nature of these exits.

The exit rate to employment starts at a rather low level of 5% per month, peaks at 14% per month after five months of job search have elapsed, and gradually tapers off to a level of about 7% per month after 10 months of elapsed unemployment duration. The transition rate to non-employment, on the other hand, does not show a peak in the early months of unemployment: it slightly increases in the first six months from 1% to 2% of exits to non-employment. From then on, it remains on this level. In general, the distribution of the unemployment duration in the sample (not illustrated) shows the well-known shape with a peak in the first four months of unemployment and another peak, though smaller, at the end of the normal benefit entitlement period after two years. The third hazard rate in Figure 2 is the sanction warning rate, which 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 warning rate

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shows a peak of almost 5% in the second month of the unemployment spell, gradually decreasing afterwards. The median duration until the first warning was 77 days.

The bottom graph of Figure 2 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 two, and more gradually to levels below 5% per month thereafter. This evidence suggests on one hand that at least one-quarter of all warnings immediately lead to withdrawal of benefits; on the other hand, the fact that the enforcement hazard is substantially below 100% in the first month after the warning also suggests that not all warnings are actually enforced.

5. ECONOMETRIC ANALYSIS

Our dataset allows the use of detailed duration analysis methods. In particular, we use a multi-state duration model that combines information on the timing of benefit sanctions with information on unemployment dynamics and the quality of post-unemployment jobs.

As a base for the evaluation of sanction effects on post-unemployment outcomes, we model the event history of an individual during and after unemployment. 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). Job seekers are defined to exit for employment if their labour earnings exceed any other source of income in the first full month after leaving unemployment. To clarify, suppose a job seeker leaves on 15 April. We then check the entire month of May and compare labour earnings to earnings from other social insurance transfers that we observed in the data (disability insurance, military insurance). If labour earnings exceed these other income sources, we say that the job seeker has left unemployment for employment. If labour earnings are equal or below other sources of income, we say that the job seeker has left unemployment for non-employment.[7] Note that in most cases other sources of social insurance transfers are zero. Thus we mainly classify exits by whether there are some or no labour earnings in the first full month after leaving unemployment.

Beyond T, we observe the post-unemployment outcome—in the form of subsequent employment (t_m) or non-employment (t_{nm}) duration or of earnings (y) over a certain period. Due to the fact that our post-unemployment observation period ends by 31 December 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 years' benefit availability can only be observed for 1.5 years.

We implement the event histories of individuals by using a competing risk, mixed proportional hazard (MPH) framework, with dynamic treatment effects. The 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,

[7] Note that self-employment is considered as employment, as long as the earnings are above the minimum threshold at which social security contributions become compulsory. If earnings are below, they are not capturedby the social security data; but these cases are rare.

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piecewise-constant duration dependence function and specify a discrete mass points distribution for the unobserved heterogeneity.

The dynamic treatment effects can be modelled and identified by the MPH approach due to the availability of the exact dates of the implementation of the warning and enforcement treatments in the data. At these dates, the unemployment hazard is allowed to shift. The size of this shift provides an estimate of the respective treatment effect. Intuitively, this identification strategy implies that the hazards are equal for the two (potential) counterfactuals before the shift date, conditional on observables and unobservables. This corresponds to the no-anticipation assumption, as outlined in Abbring and van den Berg (2003a). They state, moreover, that the dynamic treatment effect estimation by use of hazards cannot be done fully non-parametrically: the assumption of proportionality between covariates and baseline hazard as well as the assumption of the unobserved characteristics being independent from observables and time invariant are necessary. The latter allows distinguishing the distribution of unobservables from the duration dependence pattern of the baseline hazard. The plausibility and implications of these assumptions are further discussed in the following.

There are two central assumptions for the non-parametric identification of causal effects of dynamic treatments.[8] The first assumption states that job seekers do not know *the exact date* when a warning or actual reduction of a benefit sanction takes place but it does not exclude that forward-looking individuals act on properties of the sanction warnings and benefit reduction process. In other words, we assume that there is no *deterministic* anticipation effect where workers are informed exactly, while we allow for a *probabilistic* anticipation effect, the *ex ante* effect where workers may behave differently because they know they may be confronted with a benefit sanction. The *ex ante* effect is constant over the spell of unemployment, depending only on the local sanction system. The (deterministic) no-anticipation assumptionis crucial to rule out changes in behaviour before the actual treatment takes place.

Anticipation of the exact date of warnings and benefit reductions is very unlikely in the present context. Job seekers may anticipate that a sanction is pending from the moment a caseworker fixed the requirements to be fulfilled by the next meeting. But the time between meetings is typically quite short, usually about one month. Moreover, anticipating the exact date when the warning letter arrives is difficult because issuing the warning letter takes several steps. First, caseworkers, firms, or programme staff need to detect non-compliance and decide to report it. Second, the official at the CMEA will look into the case and decide whether non-compliance is present. Third, job seekers cannot anticipate the actual day of receiving the letter because administrative delays are introducing a strong degree of uncertainty. Moreover, job seekers also cannot anticipate the day when benefits are reduced. Justification introduces uncertainty with regard to whether the warning leads to a benefit reduction. Moreover, even if justification is not valid, the CMEA can take up to six months until the benefit sanction is actually enforced.[9]

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

[8] Abbring and van den Berg (2003a) discuss identification of dynamic treatment effects in a single risk context; Drepper and Efraimidis (2011) extend the identification results to the competing risks setting.

[9] Anticipated job starts could also lead to a spurious 'effect of warning on leaving unemployment'. Job seekers who know that they will leave unemployment soon have no incentive to comply with UI regulations. This leads to increased transition rates from unemployment to regular jobs immediately after a warning among job seekers who anticipate starting a job soon. The finding of a positive warning effect on unemployment exit could also be driven by anticipated job starts. However, anticipated job starts can explain neither the strong warning effect on earnings after unemployment nor the effects of warnings on leaving unemployment for non-employment. We therefore do not find it plausiblethat all of the warning effects are generated by anticipated job starts.

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characteristics and unobserved characteristics is likely to bias parameter estimates attached to control variables, the bias to the treatment effects are likely to be less severe since selectivity is explicitly taken into account. Assuming an MPH structure also means that observed covariates shift the hazard rate proportionately. Proportionality is a very common but fundamentally untestable assumption in the present setting.[10]

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

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

where $\lambda_l(t)$ stands for individual duration dependence in our proportional hazard model, x represents a vector of observable individual characteristics, r is a vector of public employment service dummy variables, p is a vector of controls for state dependence and v_l represents the unobserved heterogeneity that accounts for possible selectivity in the exit process. The supporting online Appendix provides a detailed description of the set of control variables x, r and p. Note that this full set is used for all the models described in the following. The parameters δ_{wl} and δ_{sl} measure the effect that a warning and an enforcement have on the exit rate from unemployment. Note that δ_{sl} measures the additional effect of enforcement relative to the effect of a warning. We adopt a piece-wise constant specification to model flexible duration dependence.

To deal with selectivity, we also model the rate by which individuals are warned about a possible sanction and the rate by which a sanction is enforced at time t conditional on x, r, p and v as

$$\theta_h(t_h|x, r, p, v_h) = \lambda_h(t_h)\exp\left(x'\beta_h + r'\alpha_h + p'\gamma_h + v_h\right) \quad (2)$$

where for $h = \{w, s\}$ and $\lambda_h(t_h)$ captures the piece-wise constant duration dependence of the warnings and enforcement hazards.

We present three main types of models that assess the role of benefit sanctions for post-unemployment outcomes. Our Model I 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 non-employment spell starting right after unemployment exit. We model the effects of being warned or experiencing a benefit reduction as shifts of the hazards of leaving employment, or non-employment as in equation (1). We take the monthly precision of employment and non-employment duration into account (see supporting online Appendix for further details).

Our Models II and III feature *earnings* as an outcome measure in the post-unemployment period. We evaluate the effects of benefit sanctions on the earnings in the first (complete) month after unemployment exit and on the sum of earnings over the first 24 months after unemployment exit (y_1 and y_{24}, respectively).

[10] Our earlier work on Switzerland compares effects of active labour market programmes delivered by a flexible matching estimator and by a proportional hazard estimator (Lalive *et al*., 2008). Our results show that proportionality is not a restrictive assumption in a setting where conditional independence can be assumed. Note that this does not imply that proportionality is innocuous in the present setting where we assume that conditional independence does not hold since identification crucially depends on the assumption.

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Thus we generate measures that *incorporate* endogenous changes of the labour market status during the respective periods (see Klepinger *et al*., 2002, for a similar design). These outcome measures are global in the sense that they capture the effects of sanction warnings and enforcement on the duration of employment, on the level of wages, and on hours worked for individuals leaving unemployment.

We use an MPH structure to model the post-unemployment earnings distribution for at least two reasons. First, the MPH model structure is more flexible than assuming a specific parametric distribution, e.g. log-normality, by applying the same flexible hazard function design as for the durations above. Second, results from the duration literature show that the earnings hazard model is identified.[11] 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.

Model II considers the effects on earnings for individuals who leave unemployment directly for employment. In contrast, Model III considers all individuals who have generated positive earnings in the two-year period after leaving unemployment. Model II therefore considers the effects of benefit sanctions on those individuals who leave unemployment for jobs directly, whereas Model III also considers individuals who temporarily leave unemployment for non-employment. Again, we specify the effects of sanctions on earnings hazards according to equation (1).

In the estimation we handle unobserved heterogeneity in the way suggested by, for example, Gritz (1993) and Ham and LaLonde (1996), by integrating it out over the joint density function $G(v)$. The vector $v \in \mathbb{R}_+^6$ or $v \in \mathbb{R}_+^5$ comprises all the unobserved heterogeneity components of the respective model: In Model I, v is a vector with six dimensions, in Models II and III v as a vector with five dimensions. 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. Note that we specify the correlated unobserved heterogeneity in a more flexible way than in Ham and LaLonde (1996), who rely on a one-factor structure, and most of the applications (e.g. Van den Berg and Vikström, 2009, or Bonnal *et al*., 1997). We adopt a two-step approach to estimate the models. We first search for mass points based on estimates of unobserved heterogeneity in individual processes. We then assess whether additional mass points can be located adopting the procedure suggested in Gaure *et al*. (2007) (see supporting online Appendix for details on estimation).

6. ESTIMATION RESULTS

We report in the following the results of the parameter estimates of Models I–III as described in the econometrics section above (Section 5). We then proceed to the analysis of the *ex ante* effects. Thereafter, we discuss how we explain our findings from a theoretical point of view. The section ends with simulation exercises based on the reported estimation results, which allow quantification of the different treatment effects.

[11] 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. Note that a tight connection exists between modelling hazards and conditional means in case the outcome distribution is exponential: parameters that reflect shifts of the hazard are the (negative of) the corresponding parameters in the conditional earnings model. Moreover, we find that the treatment parameters are quite similar in absolute magnitude in the model of the earnings hazard and in the model for the conditional mean of log earnings. This suggests that even if the outcome is not exponential, the two sets of parameters can be interpreted in a roughly similar way.

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6.1. Unemployment Exit Behaviour and Subsequent (Non-)Employment Stability

Table 1 provides information on the econometric estimates of Model I. *Model I* focuses on the effects of benefit sanctions on the exit behaviour of concerned individuals, assuming correlated unobserved heterogeneity. We first discuss the effects of benefit sanctions on leaving unemployment. Findings indicate that the point estimates of the treatment effects indicate that the log hazard rate of exits into employment (E) goes up by 0.147 once individuals are warned that they are under suspicion of having committed a non-compliance. Once the sanction is enforced, the exit to E rate increases by an additional 0.148. Both effects are substantial and highly significant. Expressed as percentage changes (i.e. $exp(\delta) - 1$), results indicate that a sanction warning caused a 15.9% increase relative to non-sanctioned, whereas actually imposing the sanction adds a further increase of the rate by 16.0% relative to the job seekers with a warning.

However, sanctions and warnings do not only foster a quicker take-up of a regular job; they also cause an increase in labour force exit. An announcement of a sanction leads to a remarkable rise in the exit to non-employment (NE) rate by 99.0%. Enforcing the sanction results in an additional increment of the exit to NE rate by 67.0%. This insight, that *the present and future disutility of a sanction (warning) influences the labour supply decision*, is new in the literature, to our knowledge. The (highly significant) effect is non-trivial: adding up the warning and enforcement effects amounts to more than doubling the exit to NE rate (+116%), but one has to put this result into the right context of interpretation: first, by taking into account that 'only' 12.5% of the sample exits to non-employment; second, as shown below, exit to NE is often temporary and can partly be read as an unpaid prolongation of unemployment.

Estimates differ from the earlier studies by Abbring *et al*. (2005), van den Berg *et al*. (2004), and Svarer (2011). The two Dutch studies report increases in the exit rate due to sanctions of 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 one, where job seekers are informed of the sanction process starting. Svarer (2011) finds

Table I. The effect of benefit sanctions on exit behaviour and subsequent (non-)employment duration

	Model I		
(Coeff./transf.)	Coeff.	z-value	Transf.
Effect on exit from employment (M)			
Warning (δ_{wm}/in %)	0.018	0.34	0.019
Enforcement (δ_{sm}/in %)	0.140	2.35	0.150
Effect on exit from non-employment (NM)			
Warning (δ_{wnm}/in %)	0.146	1.14	0.157
Enforcement (δ_{snm}/in %)	0.267	1.97	0.307
Effect on exit UE → E			
Warning (δ_{we}/in %)	0.147	3.39	0.159
Enforcement (δ_{se}/in %)	0.148	3.07	0.160
Effect on exit UE → NE			
Warning (δ_{wne}/in %)	0.689	5.05	0.992
Enforcement (δ_{sne}/in %)	0.513	4.05	0.670
Unobserved heterogeneity		Yes	
Control variables		Yes	
Control for state dependence		Yes	
PES dummies		Yes	
-Log-likelihood	255,064		
N	23,961		

Note: We report coefficients and their transformations: transformed treatment effects are changes in %. Asymptotic z-values. In total, 811 parameters are estimated.
Source: Own estimations based on merged UIR-SSA database.

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for Denmark an increase in the unemployment exit rate of more than 50% following enforcement. Our results are close to those of Lalive *et al*. (2005), who use a similar dataset. They find that warnings increase the hazard rate by 25% and a further increase by 20% is estimated to take place after benefits have been reduced for Swiss job seekers entering unemployment in late 1997. Some differences between the studies have to be taken into account: First, Lalive *et al*. (2005) do not have access to information on previous earnings. Arguably, previous earnings capture labour market success quite tightly, leaving little room for unobserved heterogeneity. Second, the current study is using information on benefit sanctions covering a broader range of cantons in Switzerland than Lalive *et al*. (2005). To the extent that warnings and enforcement effects vary across Swiss regions, this also gives rise to differences in estimates. Third, the distribution of unobserved heterogeneity is more comprehensively estimated in this paper than in Lalive *et al*. (2005). Finally, endogenous selection of the exits into E and NE is explicitly taken into account in this study by modelling the exit to NE process, thereby allowing for correlated unobserved heterogeneity in this destination as well.

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

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

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

Finally, we take a look at the role of the unobserved heterogeneity in Model I. Unobserved heterogeneity plays a relevant role in shaping the treatment effects on the duration of the (non-)employment spells. The corresponding version of Model II without unobserved heterogeneity (not reported) exerts sanction effects of $\delta_{wm} = 0.053/\delta_{sm} = 0.035$ for the E group and of $\delta_{wnm} = -0.094/\delta_{snm} = 0.141$ for the NE group. Except for the warning effect on the M spell (which falls from weak to no significance), all the effects go up once unobserved heterogeneity is taken into account. A certain amount of selectivity into the post-unemployment spells is present, too—mainly with respect to the enforcement

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of a sanction.[12] Finally, we may note that in Model II the exit to E and to NE treatment effects as well as the four transitions in the unemployment period are very similar to the corresponding estimates of Model I. This is a comfortable and sensible result since there is no obvious argument that adding post-unemployment information should crucially alter the estimation results for the unemployment processes.

6.2. The Effects on Earnings and their Persistence

The impact of sanction effects on the sustainability of post-unemployment jobs is the key contribution of an analysis of UI sanction systems that looks beyond unemployment exit. In order to gain an even more comprehensive view on how a sanction system may influence post-unemployment job quality, however, the analysis of earnings is essential. A glimpse at the duration-dependent earnings histories of Figure 1 in the descriptive analysis may lead to the hypothesis that sanctions reduce subsequent earnings. But, as also mentioned, this analysis could be misleading since it does not incorporate the issue of selectivity. This problem is addressed in Model II, which features simultaneous estimation of the sanctioning and unemployment processes together with the earnings process of the exit to E group, allowing for correlated unobserved heterogeneity in all five processes.

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

How do sanctions affect earnings in the first month after leaving unemployment? The results shown in Table 2 clearly suggest a *negative effect*. Already the act of warning a job seeker that a sanction procedure has been started increases the earnings hazard by 8.0% for job seekers who leave unemployment after having been warned that a benefit reduction may take place in the future. The earnings hazard increases somewhat more, albeit statistically insignificantly, for job seekers who experience an actual benefit reduction. Both effects translate into lower average earnings for sanctioned job seekers. We defer a discussion of the magnitude of the effects of benefit sanctions on average earnings to Section 6.5.

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

Estimations of the earnings Model II are affected much less by the inclusion of unobserved heterogeneity than Model I. Comparison with corresponding models without unobserved

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

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

	Model IIa: earn 1 mt				Model IIb: earn. 24 mths		
(Coeff./transf.)	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Effect on earnings over 1/24 months							
Warning (δ_{wy1}/in %)	0.077	2.40	0.080	δ_{wy24}/%	0.102	3.27	0.107
Enforcement (δ_{sy1}/in %)	0.050	1.18	0.051	δ_{sy24}/%	0.076	1.78	0.079
Effect on exit UE → E							
Warning (δ_{we}/in %)	0.154	3.41	0.167		0.154	3.39	0.167
Enforcement (δ_{se}/in %)	0.152	3.02	0.165		0.147	2.93	0.159
Effect on exit UE → NE							
Warning (δ_{wne}/in %)	0.612	4.66	0.843		0.625	4.66	0.869
Enforcement (δ_{sne}/in %)	0.522	4.16	0.686		0.518	4.12	0.679
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-likelihood		231,704				289,436	
N		23,961				23,961	

Note: We report coefficients and their transformations: transformed treatment effects are changes in %. Asymptotic z-values. In total, 669/668 parameters are estimated.
Source: Own estimations based on merged UIR-SSA database.

heterogeneity (not reported) reveals that unobserved heterogeneity only plays a (rather small) role in shaping the enforcement effect.[13] Selectivity into earnings is not relevant. The small role of unobserved heterogeneity in this model is presumably due to the inclusion of extensive controls for state dependence in the model. Controlling for earnings and employment paths in the last five years before unemployment seems to capture quite well the heterogeneity in future earnings development as well. This is consistent with the long-term stability of earnings paths that we observed in the descriptive Figure 1.

Summing up, we can clearly state that sanctions not only negatively affect stability and duration of employment (of the job seekers leaving unemployment to employment), but also the level of earnings that is generated from this employment after unemployment exit. This suggests that sanctions not only affect the search behaviour by favouring more temporary jobs, but that *they also reduce earnings after leaving unemployment*.

6.3. The Effects on Earnings: Temporary versus Permanent Labour Force Exits

In a final step, we analyse Model III—by comparing it to Model II—which also features earnings over 24 months as outcome. But whereas Model II only focuses on earnings for job seekers who start earning immediately after leaving unemployment, Model III adds those job seekers who temporarily leave the labour force. Thus the key difference between the two models lies in the feature that individuals exiting first to non-employment and taking up a job later on are part of the analysed earnings group in Model III, whereas they are not in Model II. Table 3 reports the treatment effects on this total population with positive earnings and compares them to the results of Model II with earnings over 24 months, which is reproduced here for convenience (see Table A.III for detailed results). The effects

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

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

	Model IIb: earn. 24 mths				Model III: earn. 24 mths		
(Coeff./transf.)	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Effect on earnings over 24 months							
Warning (δ_{wy24}/in %)	0.102	3.27	0.107	δ_{wy24t}/%	0.117	4.02	0.124
Enforcement (δ_{sy24}/in %)	0.076	1.78	0.079	δ_{sy24t}/%	0.104	2.66	0.109
Effect on exit UE → E/Y							
Warning (δ_{we}/in %)	0.154	3.39	0.167	δ_{wy}/%	0.181	4.33	0.198
Enforcement (δ_{se}/in %)	0.147	2.93	0.159	δ_{sy}/%	0.211	4.55	0.235
Effect on exit UE → NE/0							
Warning (δ_{wne}/in %)	0.625	4.66	0.869	δ_{w0}/%	0.830	2.59	1.294
Enforcement (δ_{sne}/in %)	0.518	4.12	0.679	δ_{s0}/%	0.294	1.73	0.342
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-likelihood		289,436				294,752	
N		23,961				23,961	

Note: We report coefficients and their transformations: Transformed treatment effects are changes in %. Asymptotic z-values. In total 668/665 parameters are estimated.
Source: Own estimations based on merged UIR-SSA database.

of announcing to an individual the start of a sanction investigation and of effectively imposing a temporary benefit reduction are both stronger in Model III than in the corresponding Model II. A warning increases the earnings hazard by 12.4%, whereas imposing the sanction leads in addition to an increase in the earnings hazard by 10.9%.[14] What does the fact that warnings and sanctions exert a *higher* reductive effect on earnings in Model III mean? This suggests that *individuals coming back from a transitory non-employment period after unemployment are faced with a stronger sanction effect in total over 24 months*. Thus the additional non-paid time for job search does not allow them to get a job that is so much better that it would compensate for the incurred additional earnings loss during the non-employment period. Exiting the labour force to avoid sanction pressure is truly costly.

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

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

[14] An alternative model that assumes earnings are log-normal gives similar results (see supporting online Appendix, Section 3).

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6.4. *Ex Ante* Effects

Previous theory and evidence in the small UI sanctions literature pointed to the importance of *ex ante effects* of benefit sanctions (see Section 1). The mere 'threat' of the presence of a sanction system may induce job seekers to behave more according to the search, job acceptance and obligations to participate in active labour market programmes imposed by unemployment insurance. The estimated Models I–III allow us to investigate this kind of policy effect for the Swiss sanction system. In all the models, we estimated public employment service (PES) fixed effects for all the respective processes. The PES effects in the warning process, α_w, represent, presumably, a measure of *how strictly a certain PES office monitors and consequently warns*. Being the result of the very federalist method of policy implementation in Switzerland, these PES fixed effects—and PES-specific warning rates in general (as descriptive analyses show)—vary considerably. We exploit this variation to estimate the effect of monitoring strictness on the PES-specific level of the different outcomes. Since the regional labour market conditions could influence PES-specific sanction policy, we control in addition for the regional unemployment rates by PES (averaged over 1998 and 1999).[15]

Table 4, featuring the respective OLS regressions (population-weighted and with bootstrapped standard errors), shows that *ex ante effects are in most of our estimated models a relevant issue*. In the case of exit to employment, we find a significant *ex ante* effect: when increasing monitoring intensity (measured as the PES-specific log warnings rate) by one standard deviation (0.887), the PES-specific log exit to E rate increases by 0.095 or a quarter of a standard deviation. Moreover, for the *ex ante* effect we find a trade-off that is very similar to the *ex post* effect. While higher warnings rates increase the probability of leaving unemployment for employment, they tend to reduce post-unemployment earnings. A one standard deviation increase in warnings increases the earnings hazard by 2.8% in the first month after leaving unemployment, suggesting that non-sanctioned job seekers leave unemployment for jobs that are paid worse or that offer shorter hours. Moreover, a one standard deviation increase in monitoring intensity increases the earnings hazard in the first two years after leaving unemployment by 4.9%. This persistent earnings reduction suggests that job seekers are locked into jobs of worse quality. In addition, we find a considerable negative *ex ante* effect on employment stability. Increasing the monitoring intensity by one standard deviation causes the exit rate from first employment to increase by 12.9%. Thus shorter employment duration provides a second explanation for the persistent negative *ex ante* effect of the sanctions system on earnings.

Interestingly, the sanction policy is not relevant for those leaving unemployment for non-employment suggesting that those who have tendency to extend unemployment duration by leaving for temporary non-employment do not yet react on the mere "threat" of a stricter sanction policy.

6.5. Quantifying the effects of benefit sanctions

In this subsection we first quantify the effects of benefit sanctions on unemployment duration and post-unemployment earnings. We then use these effects to establish the overall effects on benefit payments during unemployment and post-unemployment earnings over a two-year period following the warning for a benefit sanction (see the supporting online Appendix for details).

[15] Note that accounting for regional unemployment rate is important for transitions from paid and unregistered unemployment to employment, suggesting that this rate captures key differences in labour demand across Swiss PES. Moreover, note that this analysis can nevertheless still suffer from endogeneity problems. For instance, if stricter regions use training less, then job seekers will be leaving unemployment more quickly (because fewer of them are locked into programmes) and they will earn less after leaving unemployment (because of lower productivity enhancement). Moreover, other dimensions of active labour market policy (meetings) could be correlated with warnings. This means that evidence on the *ex ante* effects have to be interpreted with due caution.

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Table IV. *Ex ante* effects: regression of PES-specific outcomes on monitoring/warning policy and unemployment rates by PES

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

Note: OLS regressions, weighted by the population of the PES (registered unemployed during inflow period). Robust standard errors in parentheses. ***$p<0.01$, **$p<0.05$, *$p<0.1$. α_w is averaged over the five estimated models in order to reduce measurement error. The alphas and the unemployment rates are in logs.
Source: Own estimations based on merged UIR-SSA database.

The first set of simulations provides information on the *ex post effects* of warnings and benefit sanctions. We compare the actual pattern of leaving unemployment and post-unemployment earnings with counterfactual unemployment exit and post-unemployment trajectories. The actual trajectory imposes our estimates of the warning effects from Model III for all job seekers who experience a warning. It also imposes our estimates of the enforcement effect on job seekers who experience a benefit reduction and on job seekers who left unemployment before benefits were reduced but would have experienced a benefit reduction before the end of the observation period. The counterfactual scenario sets all these treatment effects to zero.

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

Row A of Table 5 provides the simulation results for those who have zero earnings over 24 months after leaving unemployment. For these unemployed only the effect on the duration of unemployment matters. As shown, sanctions reduce the time in unemployment by 33 days (from 346 to 313 days). Increased warning intensity also reduces duration until exit to unpaid post-unemployment, by 11 days (from 281 to 270 days).

Row B of Table 5 shows the simulation results for workers with post-unemployment earnings. For them there is both an effect on the duration of unemployment and on post-unemployment earnings. Warned and potentially enforced job seekers search for 253 days before they leave unemployment for positive earnings. Non-sanctioned job seekers would have searched for jobs for 280 days.

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Table V. Simulations: effects of sanctions on expected earnings and unemployment durations

	Ex post effects (on sanctioned)			*Ex ante* effects (on everyone, non-sanctioned)		
	With sanction	Without sanction	Effect sanction	With sanction	Without sanction	Effect sanction
I. *If no earnings over 24 months after unemployment*						
A. Duration of unemployment (days)	313	346	−33	270	281	−11
II. *If earnings over 24 months after unemployment*						
B1. Duration of unemployment (days)	253	280	−27	193	202	−9
B2. Post-unemployment earnings (CHF)	73,251	78,089	−4838	83,201	84,684	−1483
III. *Cost–benefit analysis (CHF)*						
C1. Post-unemployment earnings (= B2)			−4838			−1483
C2. Earnings due to earlier re-entry			2702			1120
C3. Total earnings (= C1 + C2)			−2136			−363
D1. Benefit payments due to sanction			−590			0
D2. Benefit payments due to earlier re-entry			−3552			−1389
D3. Total benefits (=D1 + D2)			−4142			−1389
E1. Individual income effect (= C3 + D3)			−6278			−1752
E2. Income effect in %			−6.2			−1.5
E3. Income effect in % per month of unemployment (= E2/(B1/30))			−6.9			−4.9
F. Earnings loss in % of benefit loss (=100 × *C3*/*D3*)			52			26

Note: See supporting online Appendix for details of the simulations. Treated group = at least one warning. Income effects in E2 and E3: % of income without benefit sanction.

Thus sanctions reduce job search duration by 27 days or a bit less than one month. Earnings simulations indicate that job seekers with a benefit sanction earn 73,251 CHF in the two years after leaving unemployment. In contrast, non-sanctioned job seekers would have earned 78,089 CHF in the same period. Benefit sanctions therefore reduce post-unemployment earnings by 4838 CHF. How does an increased warnings intensity affect unemployment duration and post-unemployment earnings? Results indicate job seekers search for about 202 days on average until they leave for positive earnings. With increased warnings intensity, job search would last only 193 days on average. Thus job search is reduced by 9 days due to the *ex ante* effect. Leaving unemployment earlier due to stricter warning also leads to earnings reductions. Whereas job seekers earn 84,684 CHF in the two years after leaving unemployment in the actual situation, their earnings would be reduced to 83,201 CHF or 1,483 CHF.

The simulation results in row B show that both job seekers who experience an actual benefit sanction as well as job seekers who experience increased warning intensity leave unemployment faster but accept lower post-unemployment earnings. What is the net effect on earnings? How do earnings effects compare to effects on benefit payments? How do these add up to effects on income?[16] We contrast

[16] Note that income simulations only take unemployment benefit payments in the current unemployment spell into account. Moreover, our data do not have information on social assistance as a possible source of income for job seekers. We also do not discount future earnings or benefit payments. Setting the discount rate to zero affects the comparison of the earnings gain due to earlier re-entry with the effect on post-unemployment earnings. But since the timescale is small, discounted comparisons are similar to those we report in the paper.

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benefit payments and earnings patterns from the day when a job seekeris warned until two years after he or she would have left unemployment without a warning in these simulations.

Row C1 of Table 5 replicates the net *ex post* and *ex ante* effects of benefit sanctions on post-unemployment earnings. Row C2 shows that sanctioned workers experience an increase in earnings of 2702 CHF because they leave unemployment earlier than before, which implies a net negative *ex post* effect on earnings of 2136 CHF. Workers with a benefit sanction lose benefit payments directly because of the benefit sanction and due to earlier re-entry. As shown in row D3, total benefit loss is 4142 CHF. As shown in row E1 total income loss because of the loss in earnings and benefits due to the *ex post* effects equals 6278 CHF, which is equal to 6.2% of the income without benefit sanctions. The equivalent income loss due to the *ex ante* effect is 1752 CHF, equivalent to 1.5% of income without benefit sanctions. In terms of earnings loss per month of reduced unemployment, the overall *ex post* effect is 6.9%, while the overall *ex ante* effect equals 4.9%.

For the government the system of benefit sanctions implies a reduction in benefit payments of 4142 CHF due to the *ex post* effect and 1389 due to the *ex ante* effect (row D3).[17] This effect on benefit payments is positive but should be compared to the earnings losses for the unemployed workers. The *ex post* effect induces earnings losses of 52 % (=2136/4142 × 100 %) of the reduction in benefit payments. For the *ex ante* effect the earnings losses amount to 26% of the reduction in benefit payments.

Our simulations uncover three interesting findings. First, benefit sanctions reduce income. Job seekers find jobs more quickly because of sanctions but these jobs do not pay enough so as to compensate the reduction in benefits. Second, sanctions do not reduce government expenditure at no cost. Sanctions reduce benefit payments strongly but they also incur a considerable cost in terms of reduced earnings. Third, the policy of increased warning is more cost effective than the actual benefit sanction. The earnings cost of increased warnings is only 26% of the benefit reduction, whereas imposed benefit sanctions incur a cost of 52% of the original benefit reductions.

7. CONCLUSIONS

Activating unemployed workers through the introduction of a system of benefit sanctions may be relatively cheap and effective in bringing unemployed people back to work more quickly. However, a comprehensive policy evaluation of such a system should not only consider direct effects in terms of reduced unemployment durations and reductions in benefit payments, but also consider indirect effects in terms of employment stability, earnings and attachment to the labour market. This is what we do in our study using a rich set of Swiss register data. We present one of the first empirical studies that looks beyond unemployment exits, providing a comprehensive evaluation of benefit sanctions. In our analysis, we distinguish between *ex post* effects which refer to the behavioural response after a benefit sanction has been imposed and *ex ante* effects which occur if an unemployedworker responds to the threat of an increase in sanction rate that makes a benefit sanction more likely to occur.

Our findings for the *ex post* effects of benefit sanctions suggest that, consistent with job search theory, benefit warnings and reductions increase the rate of leaving unemployment. Yet there is also a significant reduction in post-unemployment earnings, possibly because of lower reservation wages. On net, the positive effects of leaving unemployment more quickly do not outweigh these negative effects of benefit sanctions. This suggests that costs of on-the-job search could be substantial for workers who have recently left unemployment. Job seekers who are confronted with a warning or a benefit sanction tend to reduce their demands concerning the quality of the post-unemployment

[17] We ignore general equilibrium effects, costs of administering sanctions and effects on taxes due to the change in earnings.

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job. On average, they accept a job offer more quickly at the cost of a reduced employment stability and/or lower earnings. This cost is financially more important for the individual than her/his gain in terms of earlier unemployment exit.

In terms of *ex ante* effects, we find that job seekers who are confronted with higher warning probabilities leave unemployment more quickly. Yet again, faster exit from unemployment is accompanied by lower earnings, leading to a net reduction in post-unemployment earnings. Regarding warning and enforcement effects, we find that while mere warnings increase the rate of leaving unemployment, they do not affect employment and non-employment durations. In contrast, actual benefit reductions not only lead to a faster exit from unemployment but they also tend to reduce the duration of employment thereafter. Arguably, this result can be explained by the fact that job seekers search for jobs of different quality—temporary and permanent jobs. As outlined in Section 3, job seekers may not search for temporary jobs until they experience actual benefit reductions. Such a sequential job search strategy can explainwhy only the benefit sanction itself harms employment stability but not the warning.

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

We use simulations to quantify the overall effect on a job seeker's income and discuss the costs and benefits to society. Clearly, a system of benefit sanctions leads to a reduction in benefit payments. However, this comes at a cost. We find that a benefit sanction reduces income by 6.2% or by 6.9% per month of reduced unemployment duration. The policy of increased monitoring and warning also reduces a job seeker's income by 1.5% or by 4.9% per month of reduced unemployment duration. Thus earnings losses make up more than 50% of the reductions in benefit payments for actual sanctions and about 26% for the policy of increased warning.

What are the policy implications of our findings? Our analysis shows that the Swiss system of benefit sanctions reduces benefit payments but at the expense of reductions in post-unemployment earnings. However, the *ex ante* effect of the system has less negative consequences for post-unemployment earnings than the *ex post* effect. Keeping in mind that benefit sanctions in the Swiss system entail full reduction of benefits, i.e. a penalty of 100%, the policy implication is straightforward. Our analysis suggests that the current system of benefit sanctions can be improved at the margin by reducing the size of benefit sanctions and increasing monitoring intensity. Obviously, we have to be careful as any change in the size of the penalty might also affect the effects of monitoring. If the size of the benefit sanctions were reduced substantially monitoring might become less effective as well. Nevertheless, it is clear that improvements are possible. We leave an analysis of the optimal combination of penalty size and monitoring intensity to future research.

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APPENDIX

Table A.I. The effect of benefit sanctions on exit behaviour and subsequent (non-)employment duration

	Model I		
(Coeff./transf.)	Coeff.	z-value	Transf.
Effect on exit from employment (M)			
Warning (δ_{wm}/in %)	0.018	0.34	0.019
Enforcement (δ_{sm}/in %)	0.140	2.35	0.150
Effect on exit from non-empl. (NM)			
Warning (δ_{wnm}/in %)	0.146	1.14	0.157
Enforcement (δ_{snm}/in %)	0.267	1.97	0.307
Effect on exit UE → E			
Warning (δ_{we}/in %)	0.147	3.39	0.159
Enforcement (δ_{se}/in %)	0.148	3.07	0.160
Effect on exit UE → NE			
Warning (δ_{wne}/in %)	0.689	5.05	0.992
Enforcement (δ_{sne}/in %)	0.513	4.05	0.670
Transition rate: exit from M			
$\lambda_{ma,1}/exp(u_{ma})$	−1.962	−3.56	3.832
$\lambda_{mb,1}/exp(u_{mb})$	−4.557	−5.27	0.286
Transition rate: exit from NM			
$\lambda_{nma,1}/exp(u_{nma})$	−0.367	−0.23	2.932
$\lambda_{nmb,1}/exp(u_{nmb})$	2.022	1.28	31.972
Transition rate: exit to E			
$\lambda_{ea,1}/exp(u_{ea})$	−5.321	−13.48	0.183
$\lambda_{eb,1}/exp(u_{eb})$	−6.478	−15.70	0.058
Transition rate: exit to NE			
$\lambda_{nea,1}/exp(u_{nea})$	−2.790	−2.69	0.052
$\lambda_{neb,1}/exp(u_{neb})$	−5.342	−5.08	0.004
Transition rate: warning			
$\lambda_{wa,1}/exp(u_{wa})$	−5.151	−4.77	0.181
$\lambda_{wb,1}/exp(u_{wb})$	−9.373	−8.54	0.003
Transition rate: enforcement			
$\lambda_{sa,1}/exp(u_{sa})$	−3.382	−2.07	0.447
$\lambda_{sb,1}/exp(u_{sb})$	−100	—	0
Probabilities			
a_1/p_1	2.937	2.87	0.088
a_2/p_2	1.494	0.95	0.021
a_3/p_3	1.334	1.12	0.018
a_5/p_5	3.645	3.72	0.178
a_6/p_6	1.927	1.69	0.032
a_7/p_7	1.481	1.32	0.020
a_9/p_9	2.026	0.72	0.035
a_{11}/p_{11}	3.650	3.42	0.179
a_{13}/p_{13}	2.656	2.40	0.066
a_{17}/p_{17}	2.168	2.10	0.041
a_{18}/p_{18}	0.467	0.33	0.007
a_{22}/p_{22}	0.786	0.40	0.010
a_{24}/p_{24}	−0.008	−0.01	0.005
a_{27}/p_{27}	3.287	3.47	0.124
a_{34}/p_{34}	1.218	0.63	0.016
a_{37}/p_{37}	2.135	2.02	0.039
a_{38}/p_{38}	1.983	2.06	0.034
a_{45}/p_{45}	2.887	2.91	0.083
a_{64}/p_{64}	—	—	0.005
Unobserved heterogeneity		Yes	
Control variables, incl. state dependence		Yes	
PES dummies		Yes	
-Log-likelihood		255,064	
BIC		259,158	
N		23,961	

Note: We report coefficients and their transformations: Transformed treatment effects are changes in %. Transition rates are in % per day (exception: M/NM in % per month), suitable for the first split period of the piecewise constant hazards (see respective footnotes); the transformations are calculated for an 'average' individual: $u_{jg} = \lambda_{jg,1} + v_{jg} + \bar{x}'\beta_j + \bar{r}'\alpha_j + \bar{p}'\gamma_j$, where $j = \{m, nm, e, ne, w, s\}$, $g = \{a, b\}$ and

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the bars are means, except for the past earnings variables in the state dependence (p) where we use medians. Asymptotic z-values. Other probabilities are zero. In total 811 parameters are estimated.
Source: Own estimations based on merged UIR-SSA database.

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

	Model IIa: earn. 1 mths				Model IIb: earn. 24 mths		
(Coeff./transf.)	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Effect on earnings over 1/24 months							
Warning (δ_{wy1}/in %)	0.077	2.40	0.080	δ_{wy24}/%	0.102	3.27	0.107
Enforcement (δ_{sy1}/in %)	0.050	1.18	0.051	δ_{sy24}/%	0.076	1.78	0.079
Effect on exit UE → E							
Warning (δ_{we}/in %)	0.154	3.41	0.167		0.154	3.39	0.167
Enforcement (δ_{se}/in %)	0.152	3.02	0.165		0.147	2.93	0.159
Effect on exit UE → NE							
Warning (δ_{wne}/in %)	0.612	4.66	0.843		0.625	4.66	0.869
Enforcement (δ_{sne}/in %)	0.522	4.16	0.686		0.518	4.12	0.679
Earnings realization rate for Y1/24							
$\lambda_{y1a,1}/exp(u_{y1a})$	−3.008	−7.31	4.613	$\lambda/exp(u_{y24a})$	−5.094	−12.41	0.352
$\lambda_{y1b,1}/exp(u_{y1b})$	−4.785	−11.37	0.781	$\lambda/exp(u_{y24b})$	−7.311	−16.49	0.038
Transition rate: exit to E							
$\lambda_{ea,1}/exp(u_{ea})$	−5.302	−13.51	0.183		−5.312	−13.54	0.183
$\lambda_{eb,1}/exp(u_{eb})$	−6.442	−15.69	0.059		−6.430	−15.68	0.060
Transition rate: exit to NE							
$\lambda_{nea,1}/exp(u_{nea})$	−2.686	−2.66	0.051		−2.734	−2.70	0.052
$\lambda_{neb,1}/exp(u_{neb})$	−5.308	−5.11	0.004		−5.303	−5.12	0.004
Transition rate: warning							
$\lambda_{wa,1}/exp(u_{wa})$	−5.083	−4.81	0.181		−5.055	−4.79	0.180
$\lambda_{wb,1}/exp(u_{wb})$	−9.300	−8.66	0.003		−9.276	−8.64	0.003
Transition rate: enforcement							
$\lambda_{sa,1}/exp(u_{sa})$	−3.323	−2.12	0.448		−3.300	−2.11	0.443
$\lambda_{sb,1}/exp(u_{sb})$	−100	—	0		−100	—	0
Probabilities							
a_1/p_1	4.102	3.34	0.148	a_1/p_1	4.158	5.21	0.146
a_2/p_2	2.907	2.37	0.045	a_2/p_2	2.948	3.55	0.044
a_3/p_3	1.301	0.48	0.009	a_3/p_3	0.822	0.19	0.005
a_4/p_4	1.003	0.58	0.007	a_4/p_4	1.189	0.85	0.008
a_5/p_5	4.291	3.47	0.179	a_5/p_5	4.441	5.68	0.194
a_6/p_6	3.407	2.89	0.074	a_6/p_6	3.511	4.51	0.077
a_7/p_7	2.471	1.90	0.029	a_7/p_7	2.552	2.80	0.029
a_8/p_8	−1.562	−0.18	0.001	a_8/p_8	−1.852	−0.15	0.000
a_9/p_9	3.069	1.26	0.053	a_9/p_9	2.826	0.92	0.039
a_{11}/p_{11}	4.741	3.74	0.281	a_{11}/p_{11}	4.848	5.84	0.291
a_{13}/p_{13}	4.099	3.34	0.148	a_{13}/p_{13}	4.236	5.34	0.158
a_{21}/p_{21}	1.759	1.51	0.014	a_{21}/p_{21}	0.689	0.74	0.005
a_{22}/p_{22}	−0.218	−0.10	0.002	a_{22}/p_{22}	−0.127	−0.10	0.002
a_{29}/p_{29}	1.233	0.82	0.008	a_{32}/p_{32}	—	—	0.002
a_{32}/p_{32}	—	—	0.002				
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-likelihood		231,704				289,436	
BIC		235,077				292,804	
N		23,961				23,961	

Note: We report coefficients and their transformations: transformed treatment effects are changes in %. Transition rates are in % per day (earnings Y1/24: in % per 1000 CHF), suitable for the first split period of the piecewise constant hazards (see respective footnotes); the transformations are calculated for an 'average' individual: $u_{jg} = \lambda_{jg,1} + v_{jg} + \bar{x}'\beta_j + \bar{r}'\alpha_j + \bar{p}'\gamma_j$, where $j = \{y1, y24, e, ne, w, s\}$, $g = \{a, b\}$ and the bars are means, except for the past earnings in the state dependence (p) where we use medians. Asymptotic z-values. Other probabilities are zero. In total 669/668 parameters are estimated.
Source: Own estimations based on merged UIR-SSA database.

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

	Model IIb: earn. 24 mths				Model III: earn. 24 mths		
(Coeff./transf.)	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Effect on earnings over 24 months							
Warning (δ_{wy24}/in %)	0.102	3.27	0.107	δ_{wy24t}/%	0.117	4.02	0.124
Enforcement (δ_{sy24}/in %)	0.076	1.78	0.079	δ_{sy24t}/%	0.104	2.66	0.109
Effect on exit UE → E/Y							
Warning (δ_{we}/in %)	0.154	3.39	0.167	δ_{wy}/%	0.181	4.33	0.198
Enforcement (δ_{se}/in %)	0.147	2.93	0.159	δ_{sy}/%	0.211	4.55	0.235
Effect on exit UE → NE/0							
Warning (δ_{wne}/in %)	0.625	4.66	0.869	δ_{w0}/%	0.830	2.59	1.294
Enforcement (δ_{sne}/in %)	0.518	4.12	0.679	δ_{s0}/%	0.294	1.73	0.342
Earnings realization rate for Y24/24t							
$\lambda_{y24a,1}/exp(u_{y24a})$	−5.094	−12.41	0.352	$\lambda/exp(u_{y24ta})$	−4.696	−12.24	0.418
$\lambda_{y24b,1}/exp(u_{y24b})$	−7.311	−16.49	0.038	$\lambda/exp(u_{y24tb})$	−6.850	−16.09	0.048
Transition rate: exit to E/Y							
$\lambda_{ea,1}/exp(u_{ea})$	−5.312	−13.54	0.183	$\lambda/exp(u_{ya})$	−4.797	−12.70	0.211
$\lambda_{eb,1}/exp(u_{eb})$	−6.430	−15.68	0.060	$\lambda/exp(u_{yb})$	−5.887	−15.06	0.071
Transition rate: exit to NE/0							
$\lambda_{nea,1}/exp(u_{nea})$	−2.734	−2.70	0.052	$\lambda/exp(u_{0a})$	—4.785	—[a]	0.002
$\lambda_{neb,1}/exp(u_{neb})$	−5.303	−5.12	0.004	$\lambda/exp(u_{0b})$	−2.812	−6.29	0.011
Transition rate: warning							
$\lambda_{wa,1}/exp(u_{wa})$	−5.055	−4.79	0.180		−5.086	−4.85	0.181
$\lambda_{wb,1}/exp(u_{wb})$	−9.276	−8.64	0.003		−9.261	−8.68	0.003
Transition rate: enforcement							
$\lambda_{sa,1}/exp(u_{sa})$	−3.300	−2.11	0.443		−3.358	−2.17	0.446
$\lambda_{sb,1}/exp(u_{sb})$	−100	—	0		−100	—	0
Probabilities							
a_1/p_1	4.158	5.21	0.146	a_1/p_1	4.473	5.59	0.241
a_2/p_2	2.948	3.55	0.044	a_2/p_2	3.561	4.59	0.097
a_3/p_3	0.822	0.19	0.005	a_3/p_3	2.744	3.54	0.043
a_4/p_4	1.189	0.85	0.008	a_5/p_5	3.527	3.14	0.094
a_5/p_5	4.441	5.68	0.194	a_6/p_6	2.160	1.62	0.024
a_6/p_6	3.511	4.51	0.077	a_8/p_8	0.570	0.47	0.005
a_7/p_7	2.552	2.80	0.029	a_9/p_9	2.397	0.48	0.030
a_8/p_8	−1.852	−0.15	0.000	a_{11}/p_{11}	3.949	4.34	0.143
a_9/p_9	2.826	0.92	0.039	a_{13}/p_{13}	4.736	5.46	0.314
a_{11}/p_{11}	4.848	5.84	0.291	a_{17}/p_{17}	0.175	0.16	0.003
a_{13}/p_{13}	4.236	5.34	0.158	a_{18}/p_{18}	0.248	0.27	0.004
a_{21}/p_{21}	0.689	0.74	0.005	a_{32}/p_{32}	—	—	0.003
a_{22}/p_{22}	−0.127	−0.10	0.002				
a_{32}/p_{32}	—	—	0.002				
Unobserved heterogeneity		Yes				Yes	
Control variables		Yes				Yes	
Control for state dependence		Yes				Yes	
PES dummies		Yes				Yes	
-Log-likelihood		231,704				294,752	
BIC		235,077				298,110	
N		23,961				23,961	

Note: We report coefficients and their transformations: transformed treatment effects are changes in %. Transition rates are in % per day (earnings Y24/24t: in % per 1000 CHF), suitable for the first split period of the piecewise constant hazards (see respective footnotes); the transformations are calculated for an 'average' individual: $u_{jg} = \lambda_{jg,1} + v_{jg} + \bar{x}'\beta_j + \bar{r}'\alpha_j + \bar{p}'\gamma_j$, where $j = \{y24, y24t, e, ne, w, s\}$, $g = \{a, b\}$ and the bars are means, except for the past earnings in the state dependence (p), where we use medians. Asymptotic z-values. Other probabilities are zero.

[a] Constant could not be estimated in final model, value fixed. Its value was estimated from a version of the model with fixed probabilities. In total 668/665 parameters are estimated.

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

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