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

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Work in Progress, please do not quote; comments are very welcome.

Abstract: Thanks to a vast and rich set of Swiss register data, this paper can offer new perspectives on the recently highly debated issue of sanctions and monitoring effects. The paper wants to make contributions in two issues: First, how do benefit sanctions and warning policy affect the post-unemployment earnings situation and job stability of concerned individuals? Second, of which sustainability, empirical duration, are sanction effects in the post-unemployment period? I.e., do the earnings in this phase react more or less permanently to the sanction shock encountered during unemployment? The rich dataset allows to construct appropriate measures to tackle these questions and assess their empirical relevance for Switzerland. A combination of a difference-in-differences approach and a duration model allows to handle issues of unobserved heterogeneity and selectivity, the existence of the latter yet showing up in first indicative results. A second line of empirical evaluation uses exogenous variation in sanction policy intensity to estimate its effect in a quasi-experimental setup. First (descriptive) results give indication of a negative earnings effect for the sanctioned.

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1 Introduction

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

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

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

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

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

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

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

2 Theoretical Background and Measurement

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

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

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

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

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

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

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

3 Literature Review

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

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

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

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

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

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

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

benefit sanction imposed that activates the job seeker, not the size of that sanction.

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

In a more general perspective, interest in benefit sanctions is motivated by the observation that, on one hand, the frequently used policy of active labor market programs is often not successful in getting the unemployed immediately back to work. On the other hand, the potentially successful policy of close monitoring and benefit sanctions is not frequently used. The overview by Grubb (2000) shows a wide range of experiences in terms of sanction policies. For instance, sanctions enforced on unemployed job seekers are frequently applied in Switzerland and the Czech Republic, while in Denmark they are hardly used. Furthermore, an interesting result in the recent evaluation literature is that, among the broad range of active labor market policies, programs with intensive counseling and job search assistance did much better than other programs, in particular when combined with close monitoring and enforcement of the work test. Typically these programs do not involve risks that participants are locked into programs with reduced search activity as a consequence.[2]

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

[2]Martin and Grubb (2001) in their survey on the success of ALMPs in OECD countries conclude that governments should rely as much as possible on in-depth counseling, job-finding incentives and job-search assistance programs. The prototypical country that relied heavily on active labor market policies is Sweden. Recent evidence by Calmfors et al. (2001) suggests that Swedish programs were not very effective in maintaining regular employment. Furthermore, Swedish labor market training had no or negative employment effects, whereas a lot of other programs had a locking-in effect. Participants are not willing to exit from the programs before they are completed. In an earlier study Calmfors (1994) concludes that intensified counseling and job search assistance raise re-employment probabilities substantially. In Lalive et al. (2008) and Gerfin and Lechner (2002) similar pessimistic conclusions are drawn with respect to the effectiveness of Swiss active labor market programs.

[3]See also Meyer (1995) who reviews empirical evidence on compliance with unemployment insurance rules in the U.S. Using data from a randomized experiment Ashenfelter et al. (2005) do not find a significant impact of stricter sanctions on unemployment insurance claims and benefits. Ashenfelter et al. (2005) focus on the effect of stricter review of benefit claims and information regarding job search obligations during the first 6-9 weeks of the unemployment spell. The treatment results in a rather modest 2 percentage point reduction in the initial qualification rate and does not affect the total claim duration. Our findings are different in two important respects.

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

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

4 Institutional Procedures in the Swiss UI System

4.1 Unemployment Benefits

Job seekers are entitled to unemployment benefits if they meet two requirements. First, the unemployed must have paid unemployment insurance taxes for at least six months in the two years prior to registering at the public employment service (PES). The contribution period is extended to 12 months for those individuals who have been registered at least once in the three previous years. Individuals entering from non-employment who are looking for work are exempted from the contribution requirement if they have been in school, in prison, employed outside of Switzerland or have been taking care of children. Second, job seekers must possess the capability to fulfill the requirements of a regular job - they must be 'employable'. If a job seeker is found not to be employable there is the possibility to collect social assistance. Social assistance is means tested and relatively generous. For instance, social assistance is roughly 76% of unemployment benefits for a single job seeker with no other sources of income (OECD, 1999).

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

First, our study focuses on the effects of very strong financial sanctions that can be imposed on individuals who have passed the initial review during the entire unemployment spell. Second, the ex-ante effect we report is based on very salient differences in the likelihood of detecting a failure of adequate job search behavior. These two key differences explain why our study finds strong evidence for the effectiveness of benefit sanctions in shortening unemployment spell.

unemployment insurance contribution.

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

The second on-going obligation concerns participation in active labor market programs during the unemployment spell.[4] The exact nature and scope of the participation requirement is determined at the beginning of the unemployment spell and in monthly meetings with the caseworker.

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

4.2 Sanction Procedures

In the legal regulations[5], basically two motives for a sanction by benefit cut are brought out: First, to participate in an adequate amount at the loss to the UI that was caused by the noncompliance of the job seeker. However, the size of the benefit cut does finally not depend on the amount of the loss incurred by the UI but on the extent of the noncompliance. Secondly, these sanctions are aimed at exerting pressure on the job seekers to fulfil their obligations. In order to support a learning effect, sanction strength is increased in the case of repeated noncompliance for the same reason. It is useful to distinguish two types of sanctions. First, benefits can be withheld for quitting the previous job, i.e. for causing 'unnecessary' job loss. Second, job-seekers can be punished for lack of compliance with eligibility requirements during the spell. The first type of sanctions are inflicted upon workers at the start of the unemployment spell. The second type of sanctions are imposed during the spell of unemployment. In this paper we focus on this second

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

[5]The legal bases for the sanction procedure are mainly given by Art. 30 of the Swiss UI Law (AVIG), Art. 44 and Art. 45 of the corresponding UI Ordinance (AVIV) and part D ("Sanctions") of the Decree about Unemployment Benefits (Kreisschreiben) issued by the Swiss State Secretariat for Economic Affairs seco. The right of job seekers under suspicion of noncompliance to get the opportunity to justify themselves is based on Art. 42 of the Federal Social Insurance Law (ATSG) and the paragraphs D8 and D9 of the above-mentioned decree.

type of sanctions. Sanctions are private information and potential employers do not know whether a job applicant got sanctioned or not.

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

The second stage of the sanction process starts as soon as the clarification period ends. Depending on the nature of the clarification provided by the job seeker the CMEA decides whether or not the sanction will be enforced. If there is sufficient ground for an excuse the sanction process will be stopped. If the excuse is deemed not valid, the sanction is enforced. A benefit sanction entails a 100% reduction of benefits for a maximum duration of 60 work days. The UI law distinguishes four levels of sanction strengths: (i) Noncompliance of small degree leads to a benefit cut of 1 to 15 workdays. These short sanction durations are typically imposed if an unemployed person fails to apply to the minimum number of jobs (which is fixed by the caseworker at the PES) or doesn't show up at a meeting at the PES office. (ii) Benefit cut sanctions of medium duration (16 to 30 days), which can be imposed suite to an unappropriate rejection of a temporary job offer or suite to a second small sanctions noncompliance. (iii) Benefit cut sanctions of long duration (31 to 60 days). They are applied mainly as a consequence to a rejection of a 'suitable' longer temporary (half a year) or permanent job offer. (iv) There exists finally an "ultima ratio" sanction type which comes to application in cases of repeated noncompliance with demanded obligations in the UI system. For those people benefit entitlement will be reconsidered and potentially cancelled for a certain period. These cancellations can last some months or even more than a year. If the reluctant job seeker shows willingness to participate at the obligations again, the case will be reconsidered. In the dataset used for the empirical analysis, 88 % of the sanctions imposed were of short duration, 8% of all benefit reduction were of medium duration, and 9 % of long duration. Benefit entitlement cancellations are a rare phenomenon, a group of only some hundred people is observed in the data sample.

Benefits are immediately stopped after the CMEA has decided on legitimacy and duration of the sanction. Once the sanction has been imposed, the unemployed can appeal to a cantonal court within 30 days of the start of the benefit sanction. The court then decides whether the sanction conforms to current legal practice. However, it takes at least one year until the court reaches a decision. Appeal to the court does not keep the CMEA from imposing the sanction.

Sanctions have to be executed within an enforcement period of six months. The enforcement

period for the benefit cut starts at the first day of the committed noncompliance[6].

The actual application of these rules is delegated to the CMEA of the 26 cantons of Switzerland. All cantons have delegated the first phase of the sanction process to the public employment service. Some cantons have also delegated the second phase of the sanction process to the public employment service. Thus, the actual application of the sanction policy may differ both across cantons and within cantons. For instance, in December 1998 the average sanction rate was 10.8 sanctions enforced per 100 unemployment spells. The variation of the sanction rate across cantons is big, the minimum being 4.5 (Jura, Zug), the maximum being 25.3 (Obwalden) sanctions per 100 spells. See more on that in the section 6.2.

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

5 Data Sources and Data Structure

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

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

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

Finally, rich administrative data on income and its origins as well as on job tenure for the first time an empirical study on the income effects of sanctions. The data allow to construct long-term income histories of individuals who were one in unemployment, ranging from 5 years before up to 2 years after the unemployment spell. In addition, also individual job tenure histories are observed – giving indication on job stability. This offers an interesting new sustainability perspective on sanctions effects which will be exploited in this paper.

5.1 Used Databases

This paper relies on three high quality data sources for Switzerland. All three contain official register data from social security and can be linked by a common ID number.

Unemployment Insurance Database AVAM/ASAL This database contains (almost) all the available information which is reported by the regional placement offices (RPO) about all the job seekers registered in the UI. Our available database spans from 1992 to (recently) March 2008. Relevant for our project are the years 1997 to 2002. The information in this database about individual UE spells defines our basic data structre.

The database allows to construct UE spells with information on exact durations, socio-demographic data about the job seekers and some information about the previous job and the one they search for. Additionally, information about ALMP program participation is also available for those people.

Sanctions Information This database contains a separate extract from AVAM/ASAL, focussed on the process of sanctioning. These are the same data that were used for the LvOZ paper. They report information on all the sanction warnings and imposed sanctions in the Swiss UI system from 1-9-1997 to 30-6-2000. The variables of main interest are:

- Dates of issue of sanction warnings and sanction impositions
- Sanction status (related to the dates above): 1 - Warning ("Rechtfertigung"), connected with the request to justify a potential noncompliance; 2 - Report to UI authority ("Meldung"); 3 - Imposition of the sanction ("Sanktionsverfügung")

– Strength of a sanction: Number of days of benefit cut (1 to 60 days)

Social Security/Pension Claims Database AHV This database contains income information on all Swiss people which are eligible for the public retirement pension system AHV (compulsory for everybody) and – depending on incidence – for invalidity insurance (IV), replacement payments for military service (EO) or military insurance ("Militärversicherung" (MilV); very few people). Our database ranges from 1990 to 2002. Due to the coverage of the whole Swiss population, we can construct, based on a monthly precision, income histories for all the job seekers that are included in AVAM/ASAL. It is possible to distinguish different sources of income (and their amounts): employment, self-employment, non-employment, eligible employment income from abroad, UI, IV, EO, MilV benefits (as well as virtual income transfers only relevant for the calculation of pensions: income splitting for married couples, child care benefits).

Additionally, it is also possible to priorise income sources in the case of multiple income sources. And we can identify job/income changes in the income history using a variable which reports an ID number of the entity that is liable to pay contributions to AHV/IV. Unfortunately, it is not possible – to my knowledge – to link this number with information about branches of employers.

5.2 Data Structuring and Sampling

The core inflow sample basically consists of **all UE spells in Switzerland with entries from 1-8-1998 to 31-7-1999** (called 'spellinfo' below).

The following data with respective sample/date ranges are merged to this core sample:

– *Sanctions data* for the job seekers concerned which are available for the period from 1-9-1997 to 30-6-2000. As discussed below, it makes sense to reduce the core sample to a group of cantons depending on sanctions practices and sanctions registration. The appropriate choice of cantons and the variation of sanctions practices between them allows the estimation of the ex-ante effect of sanction policy using this variation in practice and respective cantons and regions as control groups.

– *Control variables* from *AVAM/ASAL* for the same entry period: Socio demographic variables, ALMP information, full/parttime UE status.

– *AHV income* information from 1-8-1993 to 31-7-2001. I.e. to construct income history up to 5 years before and until 2 years after the UE spell; also job change rates in this time span are constructed.

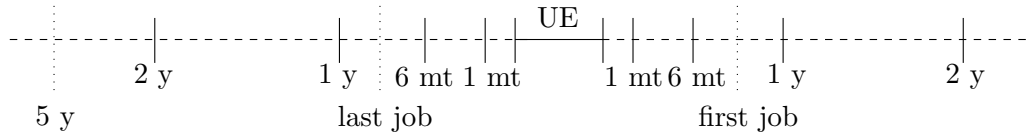
– *Reentry rates (into UE)* also from *AVAM/ASAL* over the period from 1-8-1996 to 31-7-2001. This allows the observation of the reentry history up to 2 years before and after the respective UE spell.

The above-mentioned inflow sample period was chosen for several reasons: The availability of the sanctions data restricts our attention (for the moment) to the a bit less than 3 years starting at 1-9-1997. Furthermore, the starting point of August 1998 was chosen since, from then on, justification durations are completely registered. To avoid seasonality bias, it is natural to observe an inflow sample of one year. The restriction of the inflow to end of July 1999 allows to reduce the number of spells that are affected by censoring of information about sanctions, as outlined in the next paragraph. Finally, one clearly can state that – thanks to the advantageous data availability (see Table 1): complete coverage, big size – an inflow sample of one year, ... spells, allows for very detailed analyses of high precision.

As a consequence of data availability spans, some issues of *censoring* (of minor proportion) arise. People who enter UI between 30-6-1998 and 30-6-2000 potentially can have an UE spell (→ max. 2 years of UI benefits) that goes beyond the end of the observation period of the sanctions information, 30-6-2000. The appearance of this small proportion of information censoring will be taken into account in the econometric model.

A second source of censoring are spells with entry date between 31-12-98 and 30-6-2000. A person who fully exploits the two years of benefit duration will have an exit date of his/her spell beyond 31-12-2000. So, for those people AHV income information (which ends 31-12-2002) does not last long enough for the construction of the income indicators for the point two years after exit (see figure below). This very small proportion of missings in long-term income indicators will be handled as well in the econometric model.

In detail, the **individual histories** that are constructed out of the sample(s) above have the following structure:



The different outcome indicators, which are the crucial data element of this paper, are constructed in order to measure the labour market situation of the former job seekers at different periodicities: The indicator one month after/before gives us an uncontaminated measure of the income and employment situation recently after and before the UE spell. Short- to midterm effects are measured at the points 6 months and 1 year before/after the spell, long-term effects are considered two years after and up to 5 years before unemployment. Interesting additional value gives us information on job switches and tenures before and after unemployment (e.g. last job, first job) – especially valuable for the evaluation of effects on job stability.

In general the constructed indicators are averaged measures for the respective periods. For example, the indicators '1 year' and '2 years' contain the average from month 7 to month 12 and month 13 to month 24, respectively. Though, one can properly distinguish short-term/mid-

term/longer-term income situation (2-6/7-12/13-24 mt after/before UE) without "averaging out" possible differences depending on the time distance to the UE spell.

At every point – 1 month, 6 months, 1 year, 2 years after exit and for the duration of the first job after exit; and the same measures before entry, additionally also 5 years before entry – the following measures/indicators are constructed:

- *wage*: Averaged wage per month over the above mentioned periods.
- *disposable income*: Averaged total disposable income per month.
- *labour market status*: Number of months working or not in the respective period.
- *reentry into UE*: Reentry in UE yes/no in the respective periods. Allows the construction of reentry probabilities/ratios.
- *job changes/job tenures*: Number and date of job changes in the respective periods.

See remarks and formula below for more details on the first three indicators.

The construction of symmetric time spans before and after the UE spell allows to create income, wage etc differentials which reflect income changes before vs after the UE spell. Additionally, one can construct differentials to the indicators for 5 years before UE entry – to get a even longer-term difference. This is also part of the difference-in-differences strategy outlined in the methodology section.

5.3 Some further Remarks on the Construction of the Income Indicators

The detailed information in the AHV database on income origins allows to separate the different income sources as mentioned in the section 5.1 on the other hand. Out of this categorisation, it is particularly possible to distinguish work income flows from other income sources which results in the construction of precise measures for wages, total disposable income and labour force status. In the following, I provide some more details on how they are constructed:

- *Labour force status of a person*: A person is considered as working if

$$income\_work > (income\_total - income\_work)$$

  i.e. if the total amount of work income in a month is bigger than the total amount of other income sources (UI, non-employment, IV, MilV) in that month. Income_work contains inc_empl, inc_ind (self-employed), inc_foreign (very rare) and inc_EO (replacement for work income during military service). Otherwise labour market status is 'no work' (i.e. UE, NE or handicapped). Note that a person can have multiple income sources – e.g. have work income and UE or IV benefits. In this case one has to set up a decision rule, up to which point a person is considered as being part of the labor force/working – the one above. Alternatively, one could calculate in detail the ratios of labor force participation.

The main issue behind this is would be the proper control for part-time un/employment: We have the option either to handle it with a discrete categorisation work=1/0 or to use a continuous participation ratio.

– *Wage*: As a consequence of the last point, the construction of an averaged wage over the above-mentioned time periods is:

$$wage = \frac{income\_work}{sum(work\_dummy)\ over\ the\ given\ period}$$

– *Disposable total income*: As opposed to the "pure" work income, it is useful to construct also the total amount of income that is at disposal per month, aggregated over all income sources. Disposable income is in particular relevant in issues like income elasticity with respect to benefit cuts by sanctions. Thus, to look at the effect of the incentives from sanctions, both perspectives – wage and disposable income – are relevant and of interest. So,

$$inc\_disp = \frac{income\_total}{\#months\ of\ given\ period}$$

6 Descriptive Statistics

Due to the considerations in the last section, the inflow sample used in this paper is reduced to 1-8-1998 to 31-7-1999. This period falls into a recovery phase of the Swiss economy. At the end of July 1998, 208'370 people were registered in official statistics as job seekers – one year later, the number of job seekers amounted to 162'771. Our flow sample covers completely all entries in this period. In this paper, I deal with a sample of 225'143 unemployment spells 17.5% of which contain at least one sanction event. So, in roughly every sixth unemployment spell some sanction measures – warnings, enforcements of benefit cuts or entitlement cancellations for a certain period – come to application.

6.1 Overview: Sampling, Spells and Sanctioned Population

Table 1: Sample draw: all Swiss UE spells with entry between 1-8-1998 and 31-7-1999

	obs	*percent*
total dataset: entries 1992–2004	3'159'557	
entries 1-9-1997–30-6-2000	632'670	
sample: entries 1-8-1998–31-7-1999	**225'143**	
... spells with sanction event	39'346	17.48
... spells without sanction event	185'797	82.52

Table 2 shows that 6.3% of the entry population was registered for several UE spells. Correspondingly, they are represented with multiple spells in our dataset. This value clearly underlines

that in our inflow sample of one years's entries repeated unemployment spells are a rather rare case. The table shows as well that 211'214 people are part of our sample.

Table 2: Number of UE spells per person in sample period, non-sanctioned vs sanctioned

# spells per pers	*Non-sanctioned*	*Sanctioned*	*Total*
1	162'872	35'086	197'958
2	10'179	2'433	12'612
3	486	130	616
4	22	5	27
5	0	1	1
Total	173'559	37'655	211'214

Table 3: Number of sanction events per person

# sanction events	*warnings*		*enforcements*		*entitlement canc.*	
per person	*people*	*cum %*	*people*	*cum %*	*people*	*cum %*
1	15441	61.20	15950	66.52	372	97.89
2	5261	82.05	4480	85.21	8	100
3	2007	90.00	1785	92.66		
4	1147	94.55	850	96.20		
5	586	96.87	435	98.01		
6	341	98.22	253	99.07		
7	172	98.91	120	99.57		
8	121	99.39	42	99.75		
9	62	99.63	31	99.87		
10	44	99.81	11	99.92		
11	17	99.87	7	99.95		
12	13	99.92	9	99.99		
13	7	99.95				
14	5	99.97	3	100		
15	3	99.98				
16	2	99.99				
17	1	100				
18	1	100				
Total	25231		23976		380	

Multiple sanction events – on the other hand – are not that rare. As Table 3 suggests, looking at the first four warnings and enforcements and at the first two cancellation events (if persent) leads to a coverage of 95%, 96% and 100% of the available information. This is done in the data modelling for this paper. Therefore, we can profit as well from the rich information about multiple sanction events, including the strength of these sanctions (in enforcements and cancellations[7]).

[7]Due to their very rare application – see Table 4 – entitlement cancellations will not be used as a separate state of sanctioning. So, the focus is on warnings and enforcements. Entitlement cancellation are thus only relevant in assessing the strength of a sanction – to identify the strongest possible reaction on repeated noncompliance.

Table 4: Sanction events in the sample, by spells

sanction event	*spells*
warning	25'801
enforcement	24'367
entitlement cancellation	398
total spells with sanction event	39'346

The *chronological order* of sanction events is crucial to be taken into account: It is important to implement the information on durations between the arrival of different events (e.g. the period between the first warning and enforcement of a sanction) to be able to specifically identify their effect. As we will see later in the indicative results of duration analysis, arrival times of warnings and enforcements are really relevant to separate their effects on outcome.

6.2 Cantonal Variation: Differences in Sanctions and Registration Policy

Another interesting dimension in the data to exploit for this paper are observations on cantonal and regional (per PES unit) policy variations. These variations are due to the strongly federalist execution of UI regulations in Switzerland which leave remarkable leeway in practice for cantons and PES unities. These different "practice philosophies" cultivated in the regions – described and verified in many studies about Swiss institutions – represent a variation in policy/treatment intensity that is exogenous to individual behaviour and choices in the Swiss UI system. This paper wants to make use of this opportunity of exogenous treatment variation – by implementing a quasi-experimental design. See more on that in section 8.3.

Table 5 shows clearly the remarkable policy variations in the sanctions procedures between cantons. For example, the enforcement rate of sanctions differs from 4.5% (Jura, Zug) to over 20% in several cantons. The warnings rate differs in even bigger amount. The study of LvOZ has shown additionally that there exists as well a remarkable within canton policy variation. Both types of variation will be used in the paper for the assessment of changes in the treatment intensity on the one hand and for the control of fixed effects on the other hand. In the Figures 2 and 3 in the Appendix, the variations in warning and enforcement rates are graphically represented. The green cantons are those with higher rates, the red ones those with lower rates.

In addition what becomes also clear from the first column of Table 5, is that practices in registration of warnings differ markedly between cantons. As outlined in the respective section, administrative authorities are in theory obliged by law to offer the potential noncompliant job seeker the opportunity to justify him- or herself. As a consequence, every enforcement of a sanction should be preceded by an announcement (warning). Due as well to federalist execution practices, different procedures whether these announcements are issued and registered locally (PES) or centrally (CMEA) lead to practice cases where local registration was obviously not transmitted to central registration and therefore is not observable in our data.

Due to the importance of completeness of information on exact timing, as outlined more in detail in the next sections, we restrict our attention to cantons with a small quota of missing warnings registration (i.e. no announcement registered before enforcement of sanction(s) starts). This consideration leads us to a sample of eleven cantons: Aargau, Appenzell Innerrhoden, Fribourg, Glarus, Graubuenden, Jura, Solothurn, Uri, Vaud, Valais and Zurich. Consult Figure 4 for a graphical representation. The sample is very representative in the sense that it covers all different regions and cultures (except Italian-speaking Ticino), all different sizes of cantons and the different degrees of urbanisation. So, in the analysis of warning's effects, this canton sample will be used, in the analysis of enforcements of sanctions, whole Switzerland can be included.

Table 5: Differences in cantonal sanctions policy and warnings registration strictness

	missing	*rates of ...*			
canton	*warning reg.*	*sanction events*	*warnings*	*enforcements*	*entit. canc.*
AG	**0.1635**	**0.2474**	**0.2171**	**0.1216**	**0.0056**
AI	**0.0769**	**0.0942**	**0.0870**	**0.0652**	**0.0072**
AR	0.8791	0.1072	0.0200	0.1048	0.0000
BE	0.4991	0.1541	0.0853	0.1192	0.0001
BL	0.8132	0.0797	0.0100	0.0650	0.0196
BS	0.9968	0.2393	0.0006	0.2387	0.0018
FR	**0.0423**	**0.1826**	**0.1771**	**0.0524**	**0.0009**
GE	0.9863	0.1071	0.0029	0.1058	0.0006
GL	**0.2713**	**0.1730**	**0.1380**	**0.1067**	**0.0120**
GR	**0.0164**	**0.2051**	**0.2025**	**0.1413**	**0.0024**
JU	**0.1165**	**0.1853**	**0.1669**	**0.0445**	**0.0005**
LU	0.9960	0.1182	0.0005	0.1181	0.0002
NE	0.9964	0.0848	0.0003	0.0846	0.0000
NW	1.0000	0.2363	0.0000	0.2363	0.0000
OW	1.0000	0.2534	0.0000	0.2534	0.0000
SG	0.5107	0.1803	0.0916	0.1549	0.0010
SH	0.9713	0.2495	0.0143	0.2480	0.0253
SO	**0.1312**	**0.1993**	**0.1796**	**0.1024**	**0.0003**
SZ	0.9976	0.1349	0.0003	0.1349	0.0016
TG	0.9930	0.1118	0.0009	0.1113	0.0000
TI	0.3641	0.1277	0.0877	0.0643	0.0001
UR	**0.0156**	**0.2144**	**0.2094**	**0.0972**	**0.0017**
VD	**0.0281**	**0.1794**	**0.1758**	**0.0901**	**0.0013**
VS	**0.0877**	**0.2512**	**0.2319**	**0.0768**	**0.0011**
ZG	1.0000	0.0452	0.0000	0.0452	0.0004
ZH	**0.2759**	**0.2120**	**0.1666**	**0.1073**	**0.0004**
Total	0.3739	0.1748	0.1146	0.1082	0.0018

6.3 Non-Sanctioned vs Sanctioned: Group Differences

A crucial question to clarify is how non-sanctioned and sanctioned individuals differ in their characteristics. If differences can already be observed in their observable characteristics, the

Table 6: Comparison of the groups of non-sanctioned and sanctioned people in individual characteristics

	total	non-sanctioned	sanctioned
UE duration,days (median)	141.0	126.0	226.0
Women, %	44.7	45.6	40.4
Age	35.0	35.4	33.3
W/o professional degree, %	28.1	26.5	35.2
Part-time UE, %	11.1	11.4	9.8
Unmarried, %	42.2	41.4	46.1
Non-Swiss, %	41.0	39.7	47.2
French-speaking region, %	28.8	29.3	26.7
2nd foreign language, %	46.1	47.0	41.9
Non-regional mother tongue, %	40.7	39.4	46.8
Registered UE in last 2 years, %	9.1	8.8	10.7
Monthly average earnings, CHF (6 months before UE)	3140	3214	2789

point is yet made clear that the assignment to the sanctioned group is not random (with respect to characteristics). As to be expected, this is indeed the case: the population of sanctioned people – or more correct in our setup: sanctioned spells – differs clearly from the non-sanctioned. Thus, it is of central importance in this paper to deal appropriately with the issue of *selectivity* in the assignment of treatment and control groups. As a consequence, this question will be the main point of discussion in the modeling propositions in the Empirical Strategy section 8.

Table 6 shows a series of individual characterstics the statistics of which are separately calculated by groups of the non-sanctioned and the sanctioned. The big difference in the median duration of UE between the two groups has to be relativised. Given the fact that these are unconditional descriptive statistics, there is a "mechanical" reason inherent to this difference: The descriptive analysis here does not yet condition on elapsed duration when building treatment and control group. So, "naturally" the sanctioned individuals represent a selection that shows longer average durations, as it takes time until the first sanction event appears (see more on that in the next section). Spells with quick exit (after two or three months, e.g.) face, due to their shorter duration, a smaller probability to be sanctioned.

Thus, to get a better picture, we have to look at this issue by means of a duration-dependent concept. This is done in Figure 5. This figure displays the (monthly) exit rates from UE for the two groups dependent on the elapsed duration of UE. The difference between the two groups is evident: Non-sanctioned people have a higher propensity to exit in the first ten months of elapsed duration. The peak is after 3 months where their exit rate amounts to 17%; afterwards, the exit rate declines. Sanctioned people instead show an exit rate of only 11% after 3 months – but its pattern is different afterwards: the rate stays at this level for longer terms. Thus, the effect of a sanction (increasing the exit rate) seems to kick in after a certain period. This confirms the findings of the earlier study by LVoZ for Switzerland. Be aware that these rates are not yet controlled for the influence of other variables – the picture is still indicative.

The other characteristics in Table 6 illustrate the difference between the two groups as well. To give an indicative profile: Sanctioned spells belong to people who are in comparison rather male, younger, without professional degree, full-time unemployed, unmarried and not Swiss. Sanctioning is less frequently practiced in French-speaking regions (see also section 6.2). People who are good in foreign languages (understand at least two foreign languages) have less tendency to be sanctioned. Communication problems seem also to be a reason for sanctions – as the higher percentage of spells belonging to people who do not speak the language of the region suggests. Finally, spells with another registered unemployment in the last two years (i.e. repeated UE) are relatively overrepresented in the group of the sanctioned. To end with, the raw difference in average earnings per month amounts to CHF 425. See more on that in the Earnings Analysis section 7.2. [In a next version, significance tests on the group means have to be added to Table 6.]

7 Indicative and Expected Results

7.1 Duration Analysis by Groups

The intention of this section is to shed light on the "mechanics" of the sanction process. To understand the timing of the different events in this process is crucial in two ways: First, this descriptive duration analysis can be seen as a first step to introduce this information on the exact timing of the events in a structural model where the transition processes to different stages of the sanctioning and its outcomes are explicitly modelled (see section 8.2). Second, detailed insights in the timing of events are the base to create a meaningful quasi-experimental design (see section 8.3).

The first process of interest is the exit from registered UE. As displayed in Figure 6 in the Appendix, this process shows the familiar shape for Switzerland: The hazard rate[8] increases in the first months up to a peak of a bit more than 15%, then the exit rate gradually decreases. Respectively, the distribution of the UE durations in the sample (not illustrated) shows the well-known shape with a peak in the first four months of unemployment and another peak, though smaller, at the end of the normal benefit entitlement period after two years. Median duration of the observed UE spells is 141 days, mean amounts to 244 days.

The second hazard rate in Figure 6 is the warning rate. It shows a peak of almost 5% in the second month of UE, gradually decreasing afterwards. A possible explanation for this shape bases on twofold arguments: First, people who got warned (and maybe enforced) are more aware

[8]The hazard rate is the common concept in duration analysis that relates the number of spells with, e.g., an exit in the respective month to the total number of spells "at risk" (i.e. at "risk" to come into the stage of exiting UE). This ratio is corrected for censoring. The empirical calculation of the ratios follows the concept of Kaplan-Meier. The underlying sample consists of all spells of our mentioned total sample. In the cases where information on warnings announcements is used, the sample is restricted to the cantons with correct registration of warnings (see section 6.2). Be aware that in months of yet longer elapsed durations the sample gets naturally smaller and the estimation therefore less precise. In our case, this is only relevant for the graphs dealing with subsamples of spells with warnings and/or enforcements for durations beyond ten months (i.e. Figures 9 and 10 in the Appendix).

of the functioning and the consequences of the sanctioning system; thus, they either know how to improve their strategy to comply or they want to avoid further sanction events at a higher effort in the future (rather a combination of both explanations is reasonable). Second, the UI law in Switzerland stipulates that repeated noncompliance will be punished markedly harder; this improves the incentive for people who are hit by a first sanction event to invest more in compliance in the future. In median, 77 days (mean 120) of the UE duration elapse until the first warning is announced for sanctioned people.

Another 24 days (mean 47 days) go by until the first enforcement of a sanction is normally executed. So, after a bit more than 100 days in median – and 170 days in mean – the first sanction is enforced in Switzerland. As Figure 7 shows, the enforcement rate (the probability at which job seekers are confronted with an enforced sanction in a certain month) displays the same shape as the warning rate. After a peak in month two (2.7%), the rate gradually decreases. The explanations for the shape are analog to those mentioned in the last paragraph.

How does the transition process from the point in time of an announced warning to the point of enforcement of a sanction look like in detail? Figure 8, on monthly basis, shows clearly that the vast majority of sanctions is enforced in the first month, afterwards the hazard falls quite immediately. This conforms very well to the legal practice that allows warned job seekers a justification period of about two weeks. Indeed, in the data we observe justification periods of 10 to 14 days for all the cantons. After this justification period, the cantonal authority (CMEA) has the entitlement to decide about the sanction. Looking at the second panel of Figure 8, the zoom on the first 50 days, using daily transition rates, reveals an interesting picture: The main part of the enforcement takes place right after the first 2 weeks of potential justification duration – between 2 and 3 weeks (the linear fits underline this) of duration since warning. After 3 weeks, the transition rate to enforcement declines gradually at a lower level. Thus, the normal case of the sanction procedure is indeed that the sanction – if justified – gets enforced immediately after receipt of the justification (taking into account maybe two or three days of administrative delay).

The final question in looking at the sanction procedure is: What happens to the propensity to exit from UE, once a warning is announced or a sanction is enforced? Figure 9 shows strong indication that the announcement of a warning indeed results in an increased exit rate (confirming results of LvOZ) – in the first two months after the announcement it reaches the high level of a bit more than 18%, higher than the peak of the total exit rate in the first months. Roughly, this effect wears out after nine months and the exit rate is back at the general level, between 10% and 12%. A very similar picture results from Figure 10: Once the enforcement took place, the exit rate jumps up to almost 18% going gradually back to a "normal" level between 10% and 12%.

So, from duration analysis up to this point – hazards which are controlled for observables have to be calculated in a later step – already some important conclusions arise: First, the effect

of the sanction events – warnings and enforcements – reflects obviously in the exit rates, which increase as expected. As a consequence, secondly, the timing of those events is crucial to identify their effect. Third, the process warning - justification - (possible) enforcement works according to the legal conditions: The picture of execution is quite homogenous (for the cantons which register warnings) in the sense that they normally apply immediately the steps in the described chronology. Finally, the discrepancy in reaction (with respect to exit from unemployment) between the different sanction states is of remarkable importance: Warnings on their own happen to show an important effect as well as enforcements. Therefore the hypothesis seems natural that these gradual reactions translate as well in the post-unemployment job outcomes. This will be highlighted further in the next subsection.

7.2 Earnings Effects by Groups

A difference in earnings development, depending on the different sanction states, is probable to be observed as hypothesised above. On the other hand, from theory one may derive – as outlined in the respective section and the next subsection in more detail – opposite effects of higher search effort on one hand (leading to shorter UE duration and therefore a positive earnings effect) versus a reduction in reservation wages (and other quality indicators of post-unemployment jobs which lead to a negative earnings effect) on the other hand. This could cancel out in the observed net earnings – i.e. a zero difference in the earnings development between non-sanctioned and sanctioned people could be the result of observation. Therefore, it is, from a theoretical point of view, not clear a priori that one should observe a sanction-caused difference in aggregated income or earnings histories of the two groups. Or in other words: An observed zero earnings effect of sanctions does not mean automatically that they don't have any effect on earnings at all. [In the following, I will report total income histories in the figures since the preparation of the corresponding earnings figures is not yet finished. Qualitatively, they should reflect the same picture as the ones reported.]

Figure 11 shows the monthly total income between July 1993 and July 2002, averaged over all people who were unemployed in the sample period (i.e. the observed sample period from August 1998 to end of July 1999) – grouped by the non-sanctioned versus the sanctioned[9]. The figure shows very obviously the constant level difference of income between the two groups before entry into the sampled UE. It amounts to almost CHF 500 per month. The stable difference demonstrates clearly the differences in the characteristics between the group of the non-sanctioned and the sanctioned – as concluded already in the respective descriptive statistics section 6.3. What happens to this difference *after* unemployment, including the possible treatment by a sanction? The figure suggests a slightly higher level difference in post-UE income as well as a

[9]This and the following figures are based on a 10% sample of the mentioned sample (due to computational restrictions when using the complete income panel as done here). This means that a bit more than 22'000 spells are used for these figures. Per month, the population of the non-sanctioned varies between 12'000 and 15'000 spells, the one of the sanctioned between 2500 and 3200 spells.

small divergence. Divergence in the post-UE income paths would also imply an increase of the discrepancy in income development over elapsed time after exit from UE. Sanctioned individuals encounter a more fierce income drop during the UE phase which remains in the post-UE period – at least at this level of descriptive analysis. So, these differences could be a first indication of the sustainability of sanction effects – this suggests that permanent sanction effects are of relevance, i.e. reductions in the reservation wages are probable to occur.

Very interestingly, we can observe in Figure 12 that a gap in the development of income level opens between individuals who are warned only and those who are confronted with a sanction. This markedly big gap originates from the unemployment spell – where the biggest difference between the two groups is their different treatment in terms of sanctions (income development before is very similar, so that no systematic differences in characteristics become obvious). So, conclusion seems to be straightforward that a permanent negative income effect arises from the enforcement of a benefit sanction. Again, note that of course no causality and validation of the effect is established yet. With respect to this issue, one main variable of influence that needs to be accounted for is *duration dependence*: The figures above smooth out potential income reactions due to the fact that in all data points after July 1998 there can be spells which are still in unemployment, also after July 1999 (for longer UE durations). Averaging incomes of employed and unemployed of course confuses the picture of the effects of interest. This can be overcome when plotting the income information dependent on elapsed duration.

As a consequence, the next step to proceed is to plot the income figures with respect to duration to take duration dependence into account. This is done in Figure 13 which bases on the same sample and grouping as the figures before, but now ordered by duration *before* entry into UE (negative values) and duration *after* exit from UE. Interestingly, the picture remains the same as above: The level difference in income gets a bit bigger after UE, with a slight tendency to divergence in development. When doing the same exercise as above, i.e. splitting up the cases of warning only and warning and enforcement, we get the picture of Figure 14. And this picture remains, interestingly again, the same as above when plotting the information in time-dependent manner. The figure suggests again increased level differences in income after UE, with a bigger income difference of warned vs warned & enforced compared to before unemployment. As an important side-remark: the same picture emerges when using only the cantons with correct warning registration (not illustrated) – only difference: the income level of the non-sanctioned is in general very slightly higher, about CHF 50.

What happens to these income differences when taking into account the different observable characteristics of the groups of non-sanctioned and sanctioned spells? As displayed in Figure 15, income profiles before UE flatten but still show a constant difference of about CHF 350 in the residual income. These residual incomes are the product of an OLS regression of total income on the observable variables that are mentioned in Table 6 as well as year and canton fixed effects – and then taking residuals. The use of time dummies may explain the flattening of the profiles before UE. Now, after eliminating observable income parts, the fall of income in the months just

before entering UE gets becomes significantly more prominent. This could be an indication of job and wage instability already before unemployment takes place. Even more striking is the income rebound after the end of unemployment for both groups – despite the control for time fixed effects. It could be interpreted as an effort of individuals to catch up after the incurred income losses during unemployment. What speaks against this interpretation is that the upward tendency is permanent over 20 months and that levels are reached that are markedly higher than before unemployment. To get to more reliable explanations, in a next step, a similar income history has to be added for people without unemployment period to get a further control group.

But the decisive point for this paper is of course that the level difference of income *after* unemployment between non-sanctioned and sanctioned spells has become bigger in Figure 15. The tendency for divergence disappeared after the removal of the explained parts of income. To make the point more obvious, in Figure 16 the difference in income residuals of non-sanctioned and sanctioned is plotted. It results clearly – at this stage of research – that the income difference level is higher in the post-UE phase, showing a difference of about CHF 100 in income residuals per month. Thus, the preliminary conclusion of income analysis at this point is that spells of sanctioned people show an additional income loss in the post-UE phase, compared to before UE. So – without being able yet to give causal statements – these results suggest a *negative income effect* of sanctions which is surprisingly permanent.

One further control that has to be implemented in a next step of the income/earnings analysis is the one for unemployment duration: In the figures above, duration before and after die unemployment phase are taken into account – but the hypothesis is still probable that UE spells with an enforced sanction are on average longer than those with a warning only. Since it is widely known that a longer UE duration reduces post-UE chances, it is conceivable that at least a part of the level difference between those two groups is due to UE duration dependence. In the econometric models, as sketched in the section 8, this will have to be taken into account.

7.3 Expected Results

Based on the theoretical considerations outlined in section 2, the combination of the two main channels of reactions on sanctions – adaptation of search effort and adjustment of quality demands on post-unemployment jobs – can be observed in the development of earnings as well as job change and unemployment reentry histories as follows. In the *border case* where only search effort is affected by sanctions and quality demands (or reservation wages, in a simplified view) remain, post-unemployment earnings history should exhibit no long-term difference between sanctioned and non-sanctioned people. The same counts for job change and reentry rates. As effects of increased job search act naturally rather in short-term (for the first job), in shorter periods earnings and job stability effects of sanctions could still be observed in this border case. In the other extreme case where only quality demands adjust, one should detect clearly negative effects on earnings development of sanctioned people. Assuming that lower quality also includes

a reduction in the sustainability of the job match, higher job change and reentry rates into unemployment should appear as well.

It is most probable that empirical evidence shows a *combination* of the two reaction patterns, as outlined earlier. Therefore, it is likely that I will detect a *small reduction of post-unemployment earnings*, compared to non-sanctioned individuals – as a net effect of increased search effort and reduced quality demand. The reduction appears indeed in the indicative results of the last subsection. And the finding of earlier studies that sanctions reduce UE duration can be linked with a higher search effort. The further evaluation of the relative size of differences in earnings development on one hand versus differences in job change and reentry rates on the other hand allows some inference on the relative importance of search effort versus quality effects.

Of high interest will be as well the development of these histories over time – addressing the sustainability issue of sanctions effects on one hand and indirect insights on the potential relevance of search effort vs quality effects on the other hand. It is intuitive that job search effects will exert their main influence rather in short- and mid-terms – say the first six months. Quality effects are to be expected to have longer-lasting influence on both, earnings development and job change/reentry rates.

What concerns the second strain of literature mentioned in the theory section 2: It is rather improbable that the decrease of UE duration due to benefit sanctions is long enough to expect a reduction of skills depreciation of scarring effects.

So, mainly two insights can be gained from this kind of analysis: First, it is possible to a certain degree to disentangle quality and search effects by these kinds of indirect observations mentioned above. Secondly, the duration of sanction effects can be empirically evaluated – allowing for a classification of benefit sanctions as policy instruments of rather short-term or also long-term impact. Both insights are of high interest for policy design: From an individual utility perspective, higher effects on search effort are more favorable than the reduction in quality (demands) of post-unemployment jobs. From an aggregate welfare perspective, tradeoffs from an earlier job match due to sanctions and potential lower post-unemployment earnings generation of sanctioned people have to be considered. Further, if imposing sanctions "only" leads to a more abrupt reentry into the labour market followed by negative quality effects like reduction in wage income level and job tenure – clearly the issue about the political intentions behind sanctions will arise: if these policy measures need to be sustainable or not.

Finally, the results on the *ex-ante effects of sanction policy on the whole population of job seekers* are expected to be significant in mid- and long-term. As LvOZ have shown that these anticipation effects exist in short-term, it seems consequent that regional differences in treatment intensity reflect as well in the development of post-UE earnings. Higher strictness of sanction policy should result in increased search and quality effects at least in mid-term. In the case of a dominance of search effort reactions – which seem to be more probable in the ex-ante effects on

the whole job seeker population – mid-term effects should appear stronger than long-term effects.

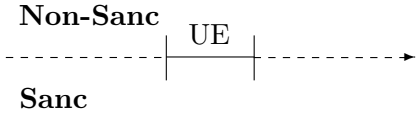
The results of the appropriate econometric models will allow for more detailed evaluations of these questions.

8 Empirical Strategy

8.1 Two possible Strategies to follow

As seen in the last sections, the *selectivity* in the assignment to the groups of sanctioned and non-sanctioned people is of remarkable importance. As a consequence, the crucial issue in modelling the empirical and econometric strategy is how to deal with this selectivity properly – or in other words: how to construct proper treatment and control groups.

The first and foremost tool to be used in our data setting is the construction of a *difference-in-differences* (DID) strategy, as illustrated here:



This strategy bases on the hypothesis that most of the unobserved heterogeneity which differs between groups is reflected in different income levels. Thus, through control of the differences in income level between sanctioned and non-sanctioned individuals we should tackle most of the unobserved heterogeneity that brings us away from a suitable random assignment setup. This is done by DID: By taking differences *before vs after* the treatment – which is in our case the UE duration that contains the sanction event(s) – we get level-independent income differentials. They reflect the effect of sanctions and unemployment on income realisation, before vs after unemployment. The second difference is taken between *non-sanctioned* and *sanctioned* people to isolate the effect of sanctioning from general effects of unemployment. Thus, in general notation, the *treatment effect* δ can be described as

$$\delta \quad = \quad E(i_a^1 - i_b^1 | x, D = 1) - E(i_a^0 - i_b^0 | x, D = 0) \tag{1}$$

where i_a/i_b is income after/before treatment and D is the treatment variable which is 1 if the treatment (sanction event) took place (the superscripts have the same meaning); x contains (time-varying) control variables. Transforming this DID setup into a regression, we get

$$\Delta \quad = \quad \alpha_1 + x'\beta + r'\alpha + \delta_1 D_1 + \delta_2 D_2 + u \tag{2}$$

where $\Delta = i_a - i_b$ and $u = (1 - 0.5D_1 - 0.5D_2)(\varepsilon_a^0 - \varepsilon_b^0) + D_1(\varepsilon_a^1 - \varepsilon_b^1) + D_2(\varepsilon_a^2 - \varepsilon_b^2)$, assuming that $E(u) = 0$. r is a vector of PES dummies to control for fixed effects related to different PES units. The constant α_1 reflects the general income change over time ('before' to 'after'), e.g.

due to changing economic environment. In equation (2), the treatment effect is split up into an effect of the warning – δ_1 – and the effect of the enforcement of a sanction – δ_2. In addition to the classical DID, it could become useful – if the further progress of the project gives respective indication – to apply a semi-parametric estimation framework for DID[10].

Besides Δ, the rich dataset allows to construct *additional outcome measures* which reflect aspects of post-unemployment job quality. First, it is useful to construct *job change rates* before and after UE. The income dataset allows us to identify changes of the employer over the whole individual history; accordingly, job tenures and job change rates can be constructed. These rates can be used in the same DID-manner as described above. A second additional measure of interest is the *reentry rate into unemployment* in the post-UE phase. This measure cannot be implemented into a DID framework, but a duration model with the transition rate into repeated unemployment as dependent variable can be estimated.

The general idea of differencing out income-level-related unobserved heterogeneity is applied in both of the *two empirical strategies* that are sketched in the following subsections. The *first* strategy aims at setting up additional behavioral equations which explicitly model the different stages of the sanction process, using the crucial information about their exact timing. Thus, a structural model with simultaneous equations, combining DID and duration modelling, will be sketched which allows for (additional) unobserved heterogeneity in all equations. *Secondly*, a completely different approach will be adopted which bases on a *quasi-experimental* setup. Starting with the hypothesis that the observed variation of sanction policy intensity among the PES units is exogenous on the individual level, this variation is used to create a suitable treatment intensity variable. Its effect on the different outcome measures is then in the scope of the analysis.

8.2 A Structural Model with DID and Duration Data

The constructed dataset, as outlined in the section 5, allows the use of detailed duration analysis methods. In particular, a multi-states duration model can be implemented together with the presented DID equation. This structural model which combines the two approaches gives rise to the joint estimation of all relevant transitions into different sanction and (un)employment states – allowing as well for unobserved heterogeneity in every stage.

The crucial main advantage of the available data is their rich information on the detailed timing of events: They allow to observe state transitions from one job to another before unemployment, entry into unemployment, transition to the state of being under suspicion of non-compliance (by announcement of a potential benefit sanction), entry into enforcement period of sanctions of different size, exit from unemployment and finally transitions from one job to another

[10]It is conceivable that – if systematic differences in unobservables exist between sanctioned and non-sanctioned people as indicated – the average outcomes (e.g. income differentials) for the treated and control groups do not follow parallel paths over the time of the treatment as classical difference-in-differences models assume. This suggests the choice of a difference-in-differences model that allows for non-linearities. A suitable estimation framework which tackles non-parallel outcome dynamics is the one by Abadie (2005) which bases on a semi-parametric estimation strategy

in the post-unemployment phase. Also, different income states before and after unemployment are observed as well.

In the part of our structural model that specifies explicitly the different duration dependent transitions, we make a distinction between (i) the date a warning has been issued stating that a sanction might be imposed and (ii) the date the sanction is actually enforced. Let t_{s_1} be denoted as the date of the warning and t_{s_2} as the date of enforcement (as measured from the date of the warning). We assume that individuals do not anticipate a warning. Once an individual got such a warning, he or she may anticipate getting the benefit sanction enforced. Because our data provide specific information about the date of warning, this latter anticipation effect can be explicitly modelled and taken into account in the empirical analysis.

The starting point to set up the duration model part is a specification where the treatment variables D_1 and D_2 again indicate warning and enforcement:

$$\theta_u(t|x, r, D_1, D_2, v_3) = \lambda_u(t) \exp(x'\beta_u + r'\alpha_u + \delta_1 D_1 + \delta_2 D_2 + v_3) \tag{3}$$

where $\theta_u(t|x, r, D_1, D_2, v_3)$ represents the exit rate from unemployment. $\lambda_u(t)$ stands for individual duration dependence in our proportional hazard model, x represents observable individual characteristics, r is a vector of public employment service dummy variables and v_3 represents the unobserved heterogeneity that accounts for possible selectivity in the exit process. $D_1 \equiv I(t_{s_1} < t_u)$ and $D_2 \equiv I(t_{s_1} + t_{s_2} < t_u)$ and the parameters δ_1 and δ_2 measure the effect that a warning and an enforcement have on the exit rate from UE. Note that δ_2 measures the additional effect of enforcement relative to the effect of a warning. In addition, the treatment effect of the enforcement can be specified more in detail by using a measure for the strength of the sanction (days of benefit cut). A common approach to modeling flexible duration dependence is the use of a step function (piecewise-constant duration model)

$$\lambda_u(t) = \exp(\Sigma_k(\lambda_{u,k} \cdot I_k(t)) \tag{4}$$

where $k(= 1, .., 4)$ is a subscript for time-intervals and $I_k(t)$ are time-varying dummy variables that are one in subsequent time-intervals. In the case of Switzerland where median UE duration amounts to a bit less than half a year, it makes sense to distinguish four time intervals: 1-3 months, 3-6 months, 6-12 months and 12 and more months. Because estimation includes as well a constant term, normalisation is necessary which is achieved by setting $\lambda_{u,1} = 0$ (baseline exit rate of 1).

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

$$\theta_{s_j}(t|x, r, v_j) = \lambda_{s_j}(t) \exp(x'\beta_{s_j} + r'\alpha_{s_i} + v_j) \tag{5}$$

where for $j = 1, 2$, $\lambda_{s_j}(t) = \exp(\Sigma_k(\lambda_{s_j,k} \cdot I_k(t))$ with normalization $\lambda_{s_j,1} = 0$ and v_j representing the respective unobserved heterogeneity.

So, the complete *structural model* allows for unobserved heterogeneity to affect the outcome variable, the exits from unemployment, the rate at which individuals are warned, and the rate at which sanctions are enforced. Specifically,

$$\begin{aligned}
\Delta &= \alpha_1 + x'\beta_i + r'\alpha_i + \delta_1 D_1 + \delta_2 D_2 + u && (6)\\
\theta_u(t|x, r, D_1, D_2, v_3) &= \lambda_u(t)\exp(x'\beta_u + r'\alpha_u + \delta_1 D_1 + \delta_2 D_2 + v_3) && (7)\\
\theta_{s_2}(t|x, r, v_2) &= \lambda_{s_2}(t)\exp(x'\beta_{s_2} + r'\alpha_{s_2} + v_2) && (8)\\
\theta_{s_1}(t|x, r, v_1) &= \lambda_{s_1}(t)\exp(x'\beta_{s_1} + r'\alpha_{s_1} + v_1) && (9)
\end{aligned}$$

where u, v_1, v_2 and v_3 are the components of unobserved heterogeneity in the outcome differential as well as the transition rates to the post-unemployment (job) state and to the two (subsequent) sanction states. Thus, the model specification explicitly allows for correlation of the unobservables affecting all the four parts of the model.

The causal effect of benefit sanctions – the *ex post* treatment effect – can be separated from selectivity effects that arise due to the potentially incomplete information in the data. This is possible due to availability of information on the exact timing of the sanction process and the exit process. Causal effects of sanction warning, enforcement and exit on the post-unemployment process create a local dependence between the four processes, i.e. the outcome measure changes directly after a warning has been issued, a sanction has been enforced or an exit from UE takes place. On the other hand, selectivity creates a global dependence between the outcome and the sanction processes. This is addressed by the above-mentioned difference-in-differences approach.

See Abbring and van den Berg (2003) for more on the identification of such a composite model.

8.3 A Quasi-experimental Approach

In this subsection it is outlined how exogenous regional variation in treatment intensity can be used to estimate the effect of sanction policy in a quasi-experimental setup. The federalistic decision structure in the Swiss UI system gives rise to this exogenous warning and sanction enforcement policy variation.

The *exogeneity* of this policy variation for the *individual* can be underlined as follows. On one hand, we observe variation in the sanction strictness also between PES units of the same region [to be demonstrated later with data/graphs – indication is shown already in LvOZ]; this gives indication that the variation obviously does not come from differences of the economic environment which should be very similar inside the same region (PES units can be as less as 20km afar from each other). On the other hand, studies were conducted about the behavior of PES administrators that show their importance in shaping a certain 'PES-philosophy' – which contains elements that are exogenous to the choice of the individuals. They demonstrate, e.g., that the principal reasons for the variation in the actual implementation of the Swiss sanction policy are

differences in 'PES-culture', the amount of administrative procedures until a sanction is enforced, substantial leeway in the interpretation of the law, the preferences of the head administrator of a PES, and the number of job seekers registered with the PES (Atag Ernst & Young, 1999). [Further research has to be done on the relation between PES performance and sanction policy rates.]

We can proxy the 'strictness' or intensity of the application of the sanction policy with *warning and enforcement indicators at the PES level*, conditional on observed covariates of the job seekers. These indicators will reflect incidence (rates) as well as the duration until warnings/enforcements are imposed, since these durations differ also markedly between PES units; finally, the strength of sanctions imposed can be used for the indicator as well. So, basically the aim is to create $\widehat{TI}$, the estimated treatment intensity. More specific

$$\begin{aligned} \widehat{TI}_w &= \textit{treatment intensity in warning} \\ \widehat{TI}_e &= \textit{treatment intensity in enforcement} \\ \widehat{TI}_{we} &= \widehat{TI}_w + \widehat{TI}_e \end{aligned}$$

Using those treatment intensity measures to be calculated at the PES level, we get the following relations of interest between $\widehat{TI}$ of the respective PES unit and the outcome, Δ:

Table 7: Relation between treatment intensity, outcome and sample population in different quasi-experimental models

relevant outcome	PES A $\widehat{TI}_A$	PES B $\widehat{TI}_B$	...
ex ante (only non-sanc pop.)	Δ^0_A	Δ^0_B	...
ex post (only sanc pop.)	Δ^1_A	Δ^1_B	...
both (total population)	$\Delta^1_A - \Delta^0_A$	$\Delta^1_B - \Delta^0_B$	...

When focussing on the evaluation of the *ex ante* effect of sanctions, the analysis is to be restricted on the population of unemployed who are *not* sanctioned. Using their non-treatment outcome as a dependent variable, the following relation can be estimated:

$$\Delta^0 = \alpha_0 + x'\beta + \widehat{TI} + \epsilon_0 \tag{10}$$

where $\widehat{TI}$ can be one of the above-mentioned measures (depending on the question) and the other variables have the same meaning as in subsection 8.1.

To estimate the *ex post* effect of a more intense treatment, it's natural to restrict the target

population to people who are *sanctioned*[11] As a consequence, the following model applies:

$$\Delta^1 = \alpha_1 + x'\beta + \widehat{TI} + \epsilon_1 \tag{11}$$

Finally, when looking at a model which allows the *joint* evaluation of both effects – ex ante and ex post –, the inclusion of the whole inflow population will be of interest. Since the (potential) treatment could have effect on the outcome of the treated *and* the non-treated, it is important to construct an outcome variable that contains both possible sorts of outcomes:

$$(\Delta^1 - \Delta^0) = \alpha_{1-0} + x'\beta + \widehat{TI} + \epsilon_{1-0} \tag{12}$$

In order to evaluate the importance of the ex ante effect on the sanctioned population a comparison can be done – opposing the model (12) to the difference of model (11) from model (10).

The next step in the development of this paper will be to further detail, support (by evidence) and estimate the two strains of empirical strategies that were sketched in the last subsections.

[11] If the warning effect – or the warning and the enforcement effect together – is of interest, the sanctioned population contains all people who are at least once warned. If the enforcement effect only is in the scope, the definition of sanctioned people refers to individuals who are confronted at least once with an enforced sanction.

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Appendices

Figures

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

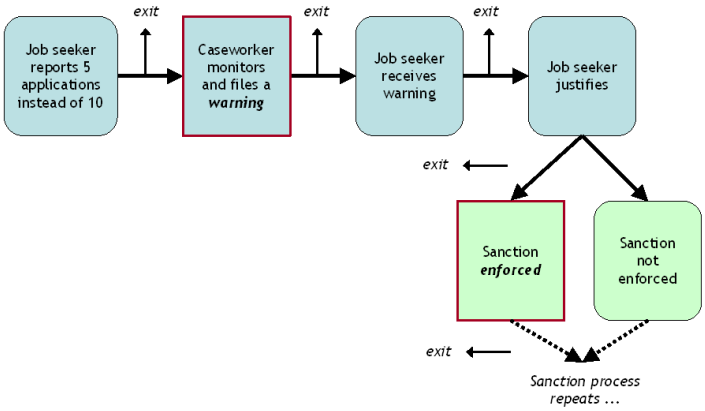

Figure 2: Warning rates by canton (see Table 5 for details)

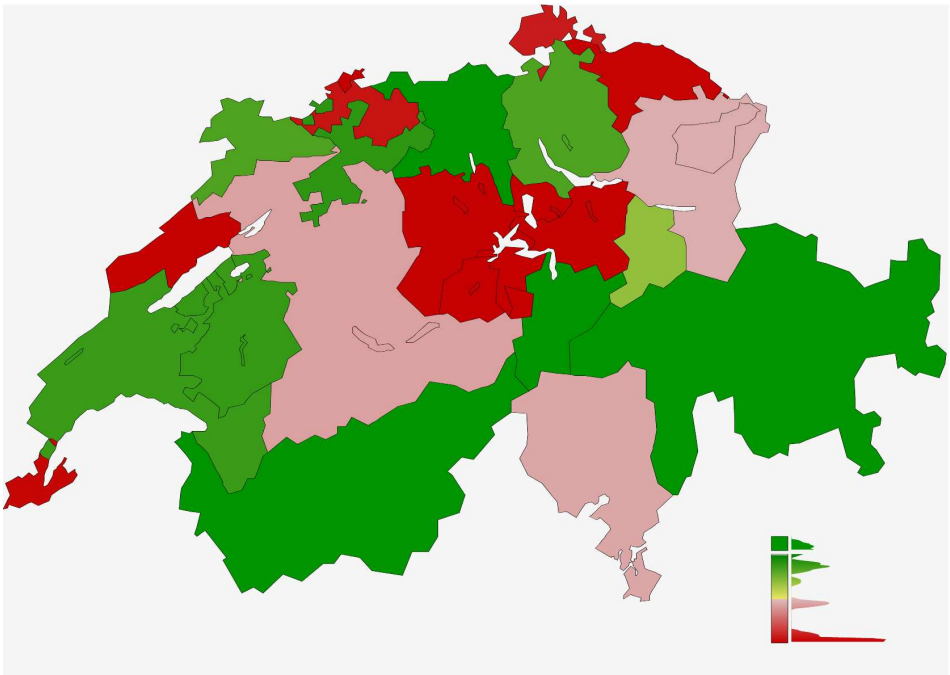

Figure 3: Enforcement rates of sanctions by canton (see Table 5 for details)

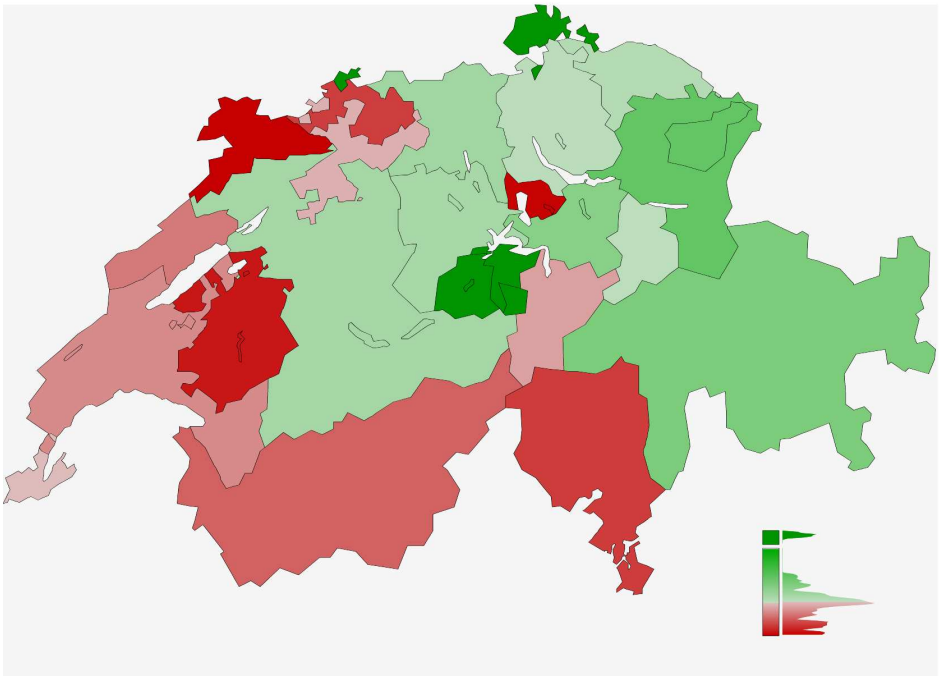

Figure 4: Sample of cantons used for warning effects analysis (see Table 5 for details)

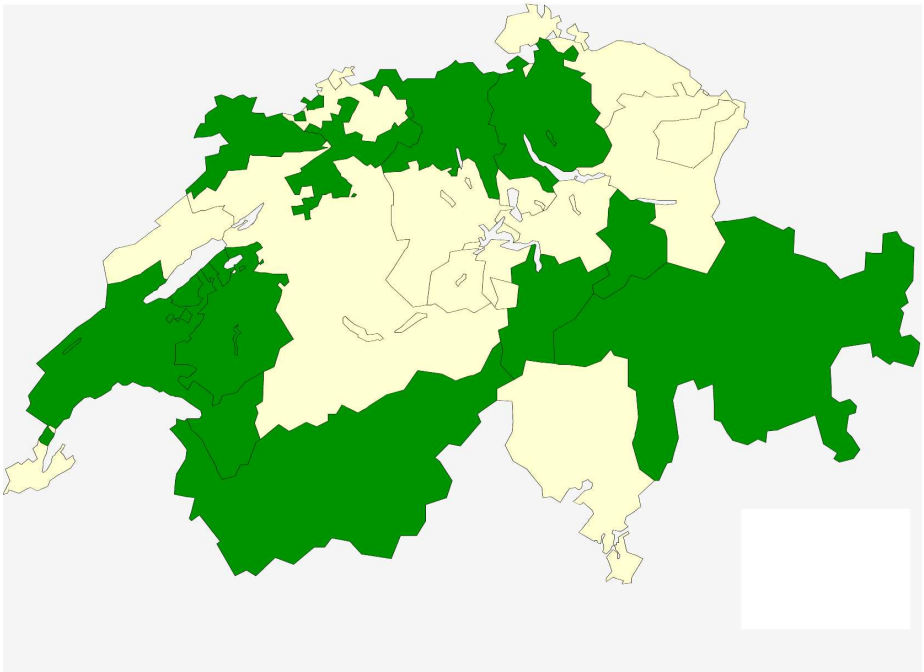

Figure 5: Exit rate from unemployment (monthly hazard rate), group of non-sanctioned vs sanctioned spells (sample period as mentioned in section 5.2; KANT=reduction to canton sample with warnings, as mentioned in section 6.2)

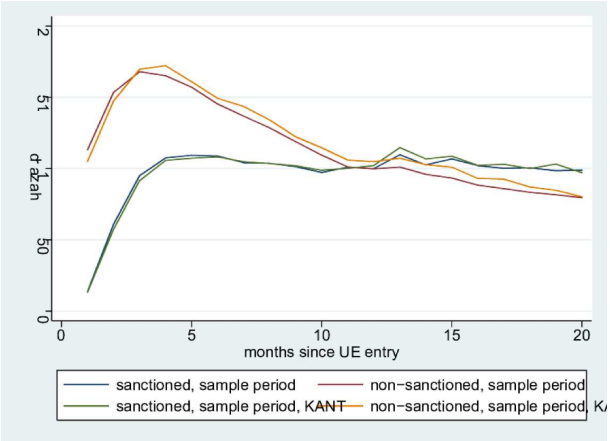

Figure 6: Exit rate from UE and warning rate

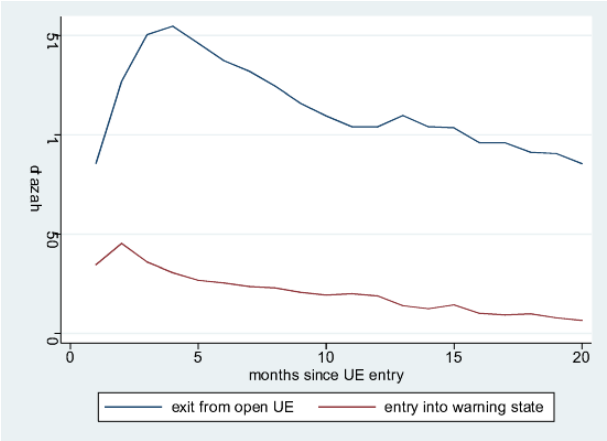

Figure 7: Enforcement rate

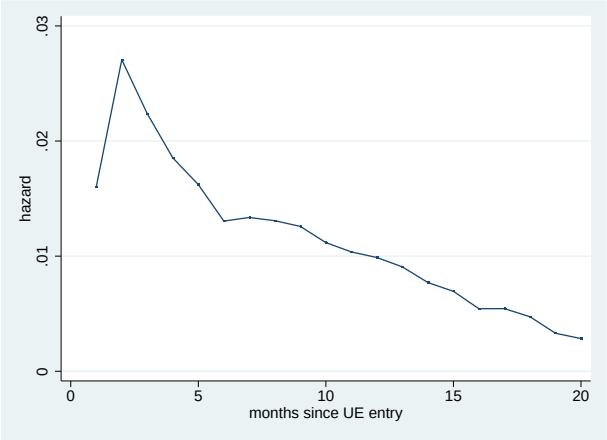

Figure 8: Transition rate from warning into enforcement, monthly and daily for the first 50 days with linear fits

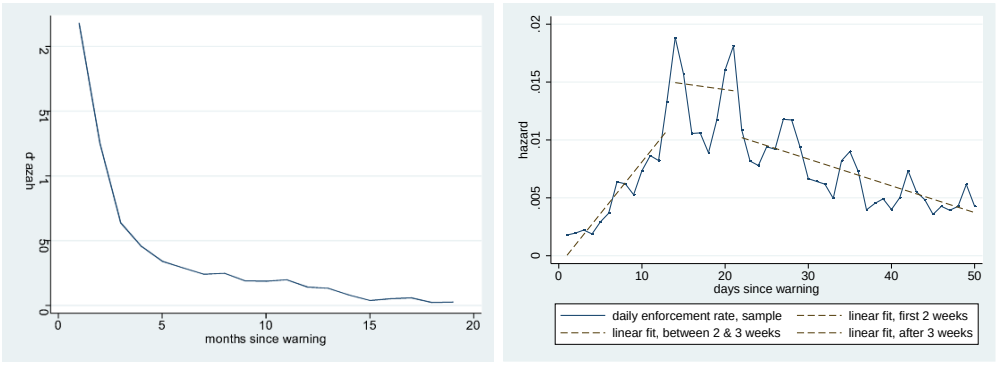

Figure 9: Exit rate from UE, by the elapsed duration since warning

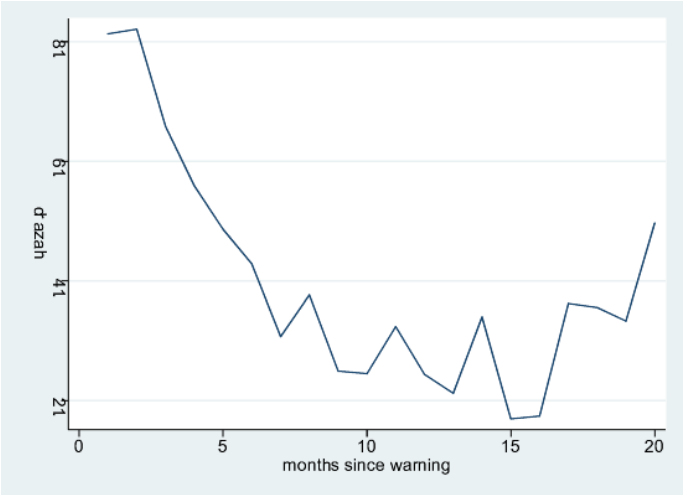

Figure 10: Exit rate from UE, by the elapsed duration since enforcement of a sanction

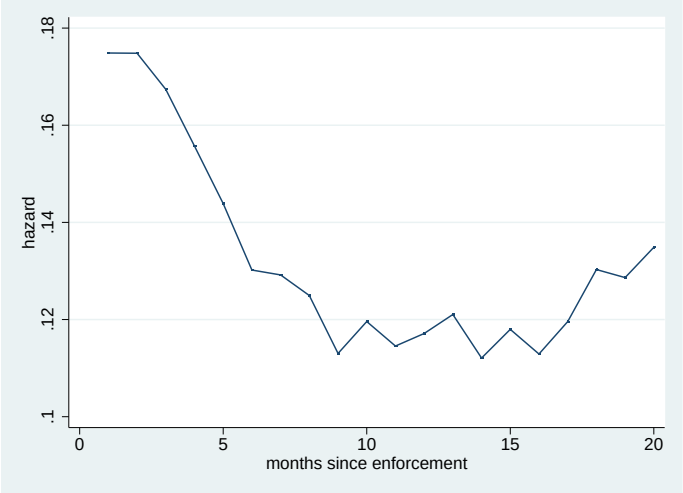

Figure 11: Aggregated income development: Non-Sanctioned vs sanctioned spells. (Averages all pre- and post-UE income histories belonging to the inflow sample, i.e. inflow/start unemployment between the two dotted lines.)

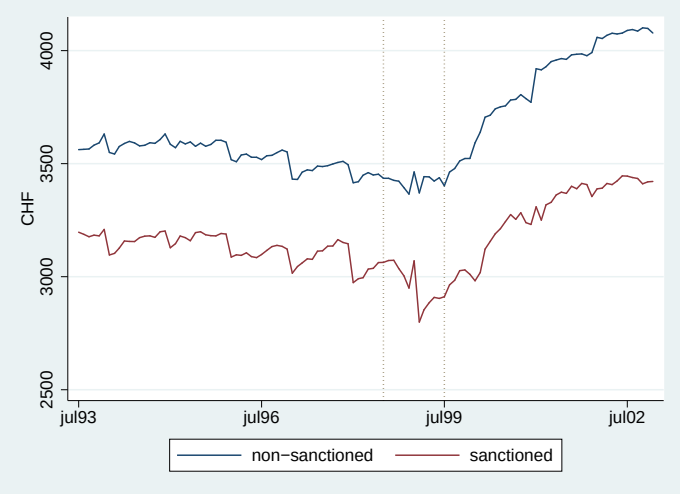

Figure 12: Aggregated income development: By sanction status. (Averages all pre- and post-UE income histories belonging to the inflow sample, i.e. inflow/start unemployment between the two dotted lines.)

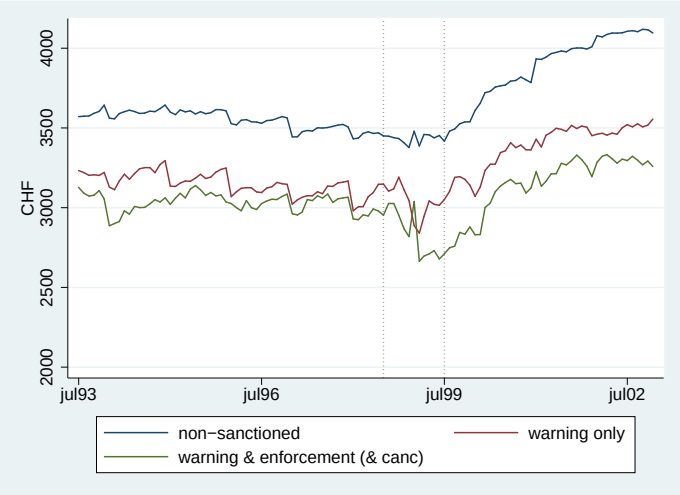

Figure 13: Duration-dependent income histories: Non-Sanctioned vs sanctioned spells. (Averages income histories dependent on the duration before entry in UE (negative values) or after exit from UE (positive) for all spells belonging to the inflow sample.)

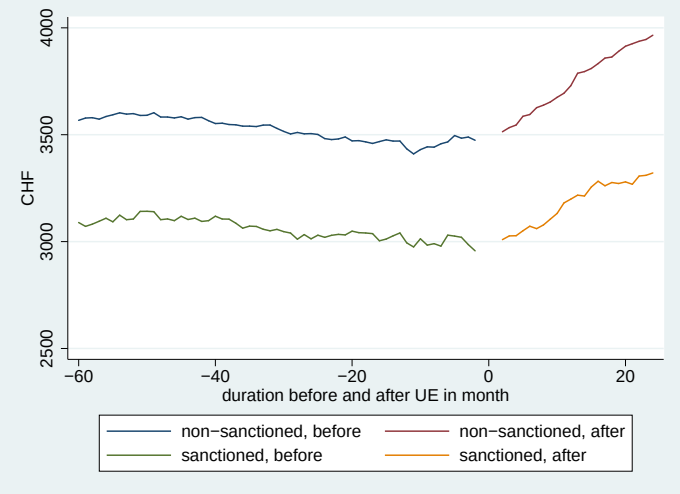

Figure 14: Duration-dependent income histories: By sanction status. (Averages income histories dependent on the duration before entry in UE (negative values) or after exit from UE (positive) for all spells belonging to the inflow sample.)

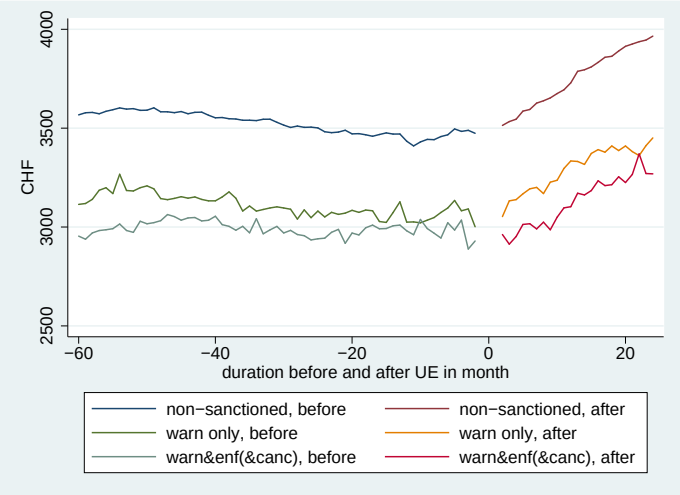

Figure 15: Duration-dependent income histories: Unexplained part of income (residuals after control for observables), non-sanctioned vs sanctioned. (Averages income histories dependent on the duration before entry in UE (negative values) or after exit from UE (positive) for all spells belonging to the inflow sample.)

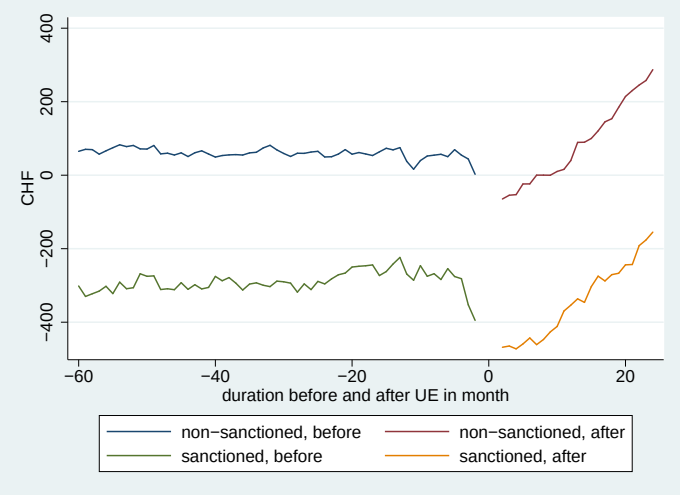

Figure 16: Duration-dependent income histories: Difference of income residuals between non-sanctioned and sanctioned.