UNIL | Université de Lausanne

FACULTÉ DES HAUTES ÉTUDES COMMERCIALES

DÉPARTEMENT D'ÉCONOMETRIE ET ÉCONOMIE POLITIQUE

EVALUATING THE DURING AND POST UNEMPLOYMENT EFFECTS OF LABOR MARKET POLICIES

THÈSE DE DOCTORAT

présentée à la

Faculté des Hautes Etudes Commerciales
de l'Université de Lausanne

pour l'obtention du grade de
Docteur en Sciences Économiques, « mention Économie Politique »

par

Patrick P. ARNI

Directeur de thèse
Prof. Rafael Lalive

Jury

Prof. Yves Pigneur, Président
Prof. Lorenz Götte, expert interne
Prof. Jan C. van Ours, expert externe
Prof. Michael Rosholm, expert externe

LAUSANNE
2011

UNIL | Université de Lausanne
HEC Lausanne
Le Doyen
Bâtiment Internef
CH-1015 Lausanne

IMPRIMATUR

Sans se prononcer sur les opinions de l'auteur, la Faculté des hautes études commerciales de l'Université de Lausanne autorise l'impression de la thèse de Monsieur Patrick ARNI, titulaire d'un Master of Arts de l'Université de Zurich, en vue de l'obtention du grade de docteur en Sciences Economiques, mention "Economie Politique".

La thèse est intitulée :

EVALUATING THE DURING AND POST UNEMPLOYMENT EFFECTS OF LABOR MARKET POLICIES

Lausanne, le 13 mai 2011

Le doyen

Daniel Oyon

Tél. ++41 21 692 33 66 | Fax ++41 21 692 34 35 | hecdoctorats@unil.ch

Year : 2011

Evaluating the During and Post Unemployment Effects of Labor Market Policies

PATRICK P. ARNI

UNIL | Université de Lausanne

MEMBRES DU JURY

Le jury est formé de :

- Monsieur Rafael LALIVE, Professeur, Faculté des Hautes Etudes Commerciales, Université de Lausanne. Directeur de thèse.

- Monsieur Lorenz GÖTTE, Professeur, Faculté des Hautes Etudes Commerciales, Université de Lausanne. Expert interne.

- Monsieur Michael ROSHOLM, Professeur, Department of Economics, University of Aarhus, Danemark. Expert externe.

- Monsieur Jan C. van OURS, Professeur, Department of Economics, Tilburg University, Pays-Bas. Expert externe.

- Monsieur Yves PIGNEUR, Professeur et Vice-doyen de la Faculté des Hautes Etudes Commerciales, Université de Lausanne. Président du jury.

University of Lausanne
Faculty of Business and Economics

Doctorate in Economics

I hereby certify that I have examined the doctoral thesis of

Patrick ARNI

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature : R. Lalive Date : 12 mai 2011

Prof. Rafael LALIVE
Supervisor of the doctoral committee

University of Lausanne
Faculty of Business and Economics

Doctorate in Economics

I hereby certify that I have examined the doctoral thesis of

Patrick ARNI

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature : Lorenz Goette Date : 13.5.2011

Prof. Lorenz GOETTE
Internal member of the doctoral committee

University of Lausanne
Faculty of Business and Economics

Doctorate in Economics

I hereby certify that I have examined the doctoral thesis of

Patrick ARNI

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature : ______________ Date : May 6, 2011

Prof. Michael ROSHOLM
External member of the doctoral committee

University of Lausanne
Faculty of Business and Economics

Doctorate in Economics

I hereby certify that I have examined the doctoral thesis of

Patrick ARNI

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature : ________________ Date : 26 April 2011

Prof. Jan C. van OURS
External member of the doctoral committee

Evaluating the During and Post Unemployment Effects of Labor Market Policies

Getting off the Beaten Track

Patrick P. Arni

Dissertation, Université de Lausanne, HEC/DEEP

Supervisor: Prof. Dr. Rafael Lalive

April 2011

Contents

Preface

Doing a Ph.D. corresponds to a long journey. Over the course of more than four years of travel time, you travel – through doctoral courses, infinite numbers of cups of coffee and inspiring discussions with many people – into the exciting field of research. As every well-informed travel guide book advises you to do, you should get off the beaten track from time to time. This is also a core idea of research, I think. I tried to follow the travel guide book's advise, from time to time.

This dissertation is about the evaluation of during and post unemployment effects of labor market policy. Labor market policy – focused on avoiding and reducing unemployment and on improving the chances of matching of job offers and demands – can be understood as the implementation of economic incentive and support mechanisms which are supposed to help individuals in reaching the mentioned aims. To what extent are these aims met, is labor market policy successful? Providing answers to this question is of high policy relevance: The individual's well-being is crucially dependent on not being disconnected from the labor market. Moreover, European countries often spend more than 1 percent of GDP on labor market policy – to avoid economic inefficiencies linked to unemployment, to invest in their citizen's labor market chances, and to reduce potential of social unrest. To meet this demand for answers, the modern econometric program evaluation literature and the economic job search literature have, since the seventies, developed powerful tools to evaluate labor market policy. My dissertation work is based on this tradition and motivated by these policy issues.

As the word 'tradition' implies, the ideas about evaluation of labor market policy are not new. But the challenges on the labor market remain high, become even more salient due to globalisation and dynamic development. Labor market policy – and therefore its evaluation – needs therefore to develop too. And innovative development means getting off the beaten tracks of existing standard approaches, from time to time. I would like to contribute a bit to this development with my dissertation.

My dissertation chapters go off the beaten track of standard labor market policy evaluation in the following respects. First, I consider alternative outcomes: Two of the three dissertation chapters are mainly focused on post-unemployment outcomes. I go beyond modeling only unemployment duration, as most of the standard European evaluation literature does. In particular, I consider as well employment stability and the evolution of earnings, as results of the preceding unemployment spell. From a policy point of view, considering post-unemployment outcomes is of obvious importance (optimisation of economic welfare rather than only minimisation of unemployment costs). But – due to the high demands on data (microdata that allow to construct individual unemployment *and* employment histories) and rather challenging methodological questions – this extension of the scope of policy evaluation only starts to really getting implemented in empirical work in these recent years. Thus, I would like to contribute with my dissertation chapters to the development of this scope extension.

The second leaving of the beaten paths is in terms of the methodological approach. Two of my three dissertation chapters are based on a social experiment. Randomised field trials for unemployment insurance evaluation are still very (or better: too) rare. Besides two smaller experiments in the Netherlands and in Sweden at the beginning of the last decade (and a series of older trials at the beginning of the nineties in the US and the UK) there is only one randomised evaluation experiment of a larger scale which is recently/permanently running: with the Danish unemployment insurance. However, in other fields of public policy like development and education economics randomised field experiments have become much more common yet and prove their comparative advantages: the cleanness of design and thus clarity of interpretation. The new randomised field experiment that I present in two dissertation chapters – the first of this form in Switzerland – shall thus contribute to paving the way (in methodological and policy respects) for more social experiments in the labor context in future.

Third, the principle of getting off the beaten track was also followed in the context of the data: The mentioned social experiment is documented by a unique combination of data: Besides all the typical register data of unemployment insurance, I dispose of a set of repeated surveys that covers a broad range of questions which are crucial for getting more insights into the job seeker's behavior *behind* the directly visible outcome. The fact that these surveys are repeated and timed in parallel to the different stages of the treatment plan allows a narrow combination of the data and, as a consequence, the evaluation of causal effects of sequential treatment on behavior. Thus, this new type of data combination, supported by the experimental setup, provides the opportunity to extend the scope of content in policy evaluation to behavioral questions: What did the job seekers really do in order to achieve the higher job finding proportion (found in the experiment)? Which role did the forming of labor market expectations (embodied in reservation wages) and of beliefs play in determining job search success? How do job seekers really search? And does this behavior change in response to the incentives and support given by labor market programs? Answers to such questions (potentially) allow the design of policies and support mechanisms which are more (and, hopefully, clearer) targeted than those in use today. In particular the last chapter of my dissertation aims at giving some impulses to the development of approaches answering such questions.

So, to wrap up, the arch over the three dissertation chapters is built by the intention to bring in some fresh ideas into the evaluation of labor market policies: in terms of alternative outcomes, alternative methodological approaches and alternative data – and to combine these with rigorous application of the state-of-the-art econometric policy evaluation methods. These intentions and the choice of the content of my dissertation chapters reflect my personal attitude towards research: I would like to produce applied research which always combines innovation and methodological rigor with the focus on questions which are relevant as well outside academia, mainly in policy. Thus, I hope that the results of these dissertation chapters will also be perceived by some policy makers, and that the professional practitioners in the field may take along some insights for their work. In fact, I already could contribute to the dissemination by giving a series of presentations for that target group and by providing a non-scientific policy report.

What are the specific topics and motivations of my three dissertation chapters? The first chapter focuses on incentive measures that aim at avoiding non-compliance with the rules of the unemployment insurance system: I analyse, in co-authored work, the benefit sanctions system of the Swiss unemployment insurance. How effective are such systems in helping to re-establish job seekers in the active labor force? Up to now, this question only had been analysed with respect to unemployment duration (sanctions reduce it). But what was missing was the broader economic perspective: Being quicker out of unemployment does not forcefully mean that the individual reached sustainable re-establishment in the labor force. From a policy perspective of maximising individual welfare (earnings) and aggregate economic productivity/activity one needs to analyse the net impact of the sanction system on the generated economic value, i.e. earnings. Thus, evaluating the net effect implies looking *jointly* at the effects of the sanctions system on unemployment duration, post-unemployment employment stability and post-unemployment earnings levels – and then to trade these elements off (in terms of net earnings generated). This is what we do in the first chapter.

The second chapter analyses supportive labor market policy. How can those job seekers with the highest risk of longterm unemployment – the older job seekers – be best supported to re-improve their employability? For older job seekers, non-compliance or shirking (exerting too less effort) is normally not the crucial problem – but rather the fact of maybe not being up-to-date any more in terms of labor market skills. This is what is trained in the social experiment that we performed in the North of Switzerland (Kanton of Aargau): It features an intense treatment plan which combines bi-weekly counseling with a very intense coaching program of 20 working days. The pre-fixed timing of these measures allows a proper identification of the effects of the different treatment stages. In particular, this ex-ante timing, combined with randomisation at t_0, provides the opportunity of a clean identification of anticipation effects of a program. Literature shows the importance of anticipatory behavior of individuals: Labor market policy often operates through a 'threat effect' – job seekers leave unemployment before the program start since they do not like it. Interestingly, I observe here the opposite phenomenon, the 'attraction effect' (which is barely documented in the literature so far). After coaching, in the later stages of unemployment, it turns out that the policy intervention more and more improves job finding success. At the end, the proportion of job finders among the treated is 9 percentage points higher. The analysis of post-unemployment outcomes reveals that the beneficial effects on subsequent employment stability were more than big enough to pay the program costs.

The third and final chapter of the dissertation goes into the recently emerging research strand that aims at combining the analysis of job search with behavioral approaches. Such a combination necessitates the respective combination of data, in order to be able to perform suitable empirical analyses. These data – which augment register data by surveys – are still very rare to find. Even rarer is the combination of such data with a social experiment that exogenously varies labor market policy. This dissertation paper is, to my knowledge, the first contribution to the literature that can empirically evaluate the interaction of labor market policy and different dimensions of job search behavior. The mentioned combination of coaching and counseling is a good candidate of policy to analyse these interactions; since this type of policy directly aims at improving/changing some aspects of job search behavior. So, the results presented in this chapter are supposed to contribute to the literature by giving some first insights into the 'blackbox' of job search behavior which is manipulated by labor market policy. In particular, they show how behavioral variables like reservation wages, beliefs about job finding success, job search effort and -strategy evolve in response to the policy intervention. Remarkable treatment effects on these dimensions of behavior and beliefs are found. Seemingly, this supportive labor market policy induced a learning process.

Acknowledgements

First of all, I would like to express a big Thank You to my PhD supervisor, Rafael Lalive. The very inspiring discussions with him, his support and his friendship are of invaluable value. I learnt a lot of him. His creative, precise and positive attitude towards research, and life in general, is definitely a remarkable example to me that I will put into the backpack for the journey towards future. Then, I would like to warmly thank Jan van Ours. It was and is a very inspiring, and funny, experience to work with him as a co-author, and he often acted as a co-supervisor. Moreover, he opened the door to an amazing experience I had when staying for half a year at Tilburg University. Next, I would like to present many thanks to Lorenz Goette and Michael Rosholm. They helped, as committee members, a lot to push this thesis forward to its final version. I very much appreciated their constructive comments and interest towards my work. Finally, a sincere thank you goes to Raphael Weisz and the members of the project teams in the PES of the field experiment in the Canton of Aargau. It was great to experience such a constructive, committed and likeable cooperation.

Finally, I would like to express the most precious thanks to my family. Their role and support in all respects of life is simply irreplaceable.

Chapter 1

How Effective are Unemployment Benefit Sanctions? Looking Beyond Unemployment Exit

Co-Authors: Rafael Lalive[1], *Jan C. van Ours*[2]

[1]CEPR, CESifo, IFAU, IZA, and Faculty of Business and Economics, University of Lausanne, CH-1015 Lausanne, Rafael.Lalive@unil.ch

[2]CEPR, IZA, and CentER, Department of Economics, Tilburg University; Department of Economics, University of Melbourne; email: vanours@uvt.nl

Abstract[3]: Unemployment benefit sanctions – temporary reductions in unemployment benefits – are effective in reducing unemployment duration. This paper provides a comprehensive evaluation of the effects of benefit sanctions on post-unemployment outcomes such as post-unemployment employment stability, non-participation, and on earnings. The analysis is based on rich register data which allow us to distinguish the effects between a warning that a benefit reduction may take place in the near future and the actual withdrawal of unemployment benefits. Adopting a multivariate mixed proportional hazard approach to address selectivity, we find that warnings do not affect subsequent employment stability but do reduce post-unemployment earnings. Actual benefit reductions lower the quality of post-unemployment jobs both in terms of job duration as well as in terms of earnings. Simulations indicate that workers who got a benefit sanction imposed see their labor earnings reduced by 4 percentage points during the two years after leaving unemployment. Beyond this treatment effect on the sanctioned job seekers we estimate and simulate as well the impact of a stricter implementation of the sanction regime on all job seekers (ex ante effect). Stricter monitoring of (non-)compliance reduces labor earnings by 0.6 percentage points.

JEL Classification: J64, J65, J68

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

[3]We thank Lorenz Goette, Michael Rosholm, Jaap Abbring, Jan Boone, Bart Cockx, Bo Honoré, Bruno van der Linden, Blaise Melly, Arthur van Soest, and seminar participants at Copenhagen, Ghent, IAB Nuernberg, IZA, Lisbon, Louvain-la-Neuve, Tilburg University, EALE 2009, and ESSLE 2009 for comments on previous versions of the paper. Jonathan Gast provided excellent support in interpreting administrative data on job seekers (Seco data), and Jacek Micuta and David Sanchez were extremely helpful in providing access to pension register data. Financial support from the Swiss National Science Foundation projects (No. 100012-130114, 100012-130114, and PBLAP1-127652/1) is gratefully acknowledged. All remaining errors are our own. Address: Faculty of Business and Economics, University of Lausanne, CH-1015 Lausanne, email: Patrick.Arni@unil.ch

1.1 Introduction

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

This paper asks how benefit sanctions affect job seeker's post unemployment earnings. The answer to this question is not trivial. Sanctions have been shown to increase the rate of leaving unemployment among affected job seekers (Abbring et al., 2005, and Van den Berg et al., 2004). Faster exit from unemployment boosts post-unemployment labor earnings since sanctioned job seekers start working earlier than non-sanctioned job seekers. The key issue is, however, whether sanctioned job seekers are able to leave unemployment to jobs that are as stable and as well-paying as non-sanctioned job seekers. If sanctioned job seekers sacrifice some stability and/or a part of their wage to leave unemployment more quickly, it is not clear that sanctioned job seekers will end up earning more than non-sanctioned job seekers.[4]

Understanding the net effects of benefit sanctions on post-unemployment labor earnings is important for at least three reasons. Unemployment insurance is a central component of social insurance against income shocks that is a feature of all OECD countries policy mix. Understanding how one central component, benefit sanctions, affect insured job seekers is therefore crucial in thinking about how to redesign these systems. Second, in contrast to active labor market programs[5], sanctions seem to enhance exists from unemployment. This explains the recent shift of large European economies such as Germany towards stiffer sanction regimes. Yet unless we

[4] Note that this discussion focuses on post unemployment earnings rather than income thus neglecting all transfers (unemployment benefits). An earnings analysis can therefore only inform on the efficiency aspects of the benefit sanction system but not on the issue of how benefit sanctions affect economic well-being as proxied by income.

[5] Lack of success of ALMP has been blamed on the lock-in effect of training programs. Training programs typically exempt participants from the job search requirement. This mechanically leads to an initial unemployment duration prolonging effect. Lalive et al. (2008) and Gerfin and Lechner (2002) provide evaluations of Swiss ALMPs and find that training and employment programs prolong unemployment duration whereas temporary wage subsidies may reduce unemployment duration. Note that active labor market policies with intensive counseling and job search assistance do better than other programs, in particular when combined with close monitoring and enforcement of the work test – elements that come closer to the "stick" than the "carrot". See the survey on the success of active labor market policy programs in OECD countries Martin and Grubb (2001) who conclude that governments should rely as much as possible on in-depth counseling, job-finding incentives and job-search assistance programs as other more intense programs are not very effective. A recent meta-study by Card et al. (2009) which covers 97 studies between 1995 and 2007 confirms these findings.

understand closer how this policy affects post unemployment labor market trajectories, the policy option of adopting a stiff sanction regime is based on incomplete evidence: the effects of sanctions on leaving unemployment. A comprehensive evaluation of benefit sanctions can fill the gap in also providing evidence on the phase beyond unemployment.

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

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

The small body of recent empirical literature on benefit sanctions is mainly of European origin and supports the positive short-term effects on the exit rate from unemployment.[6] Two Dutch papers find that benefit sanctions double the outflow from unemployment to a job (Abbring et al. (2005) and Van den Berg et al. (2004)). Using Danish data Svarer (2007) finds that the unemployment exit rate increases by more than 50% following enforcement of a sanction. Jensen et al. (2003) find a small effect of the sanctions that are part of Danish youth unemployment program. Schneider (2008) studying benefit sanctions in Germany finds no significant effect of

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

sanctions on reported reservation wages. Hofmann (2008) on the other hand reports positive effects of benefit sanctions on the employment probability of West-German unemployed. A common element in these benefit sanction studies is that they are restricted to the analysis of the effects on the duration of unemployment. This is not surprising as suitable data to perform an analysis of post-unemployment jobs are often not available. Even in the context of much more frequently investigated effects of changes in level or duration of unemployment benefits the post-unemployment dimension of these effects is rarely considered.[7] The same holds for investigations of the effect of job search requirements or job search assistance.[8]

This paper is most similar to Lalive et al. (2005) use similar data and apply multivariate mixed proportional hazard modelling to assess the effects of warnings and enforcements on unemployment exist. This paper differs from Lalive et al. (2005) in at least three important respects. First, the main focus of this paper is on post-unemployment outcomes such as employment stability and earnings. These outcomes have neither been covered by Lalive et al. (2005) nor most of the existing studies on post unemployment effects of benefit sanctions.[9] Second, this paper provides key simulations that can help in assessing the overall assessment of benefit sanctions. Specifically, this paper compares the earnings enhancing effects of benefit sanctions due to faster exit from unemployment to the earnings reducing effects of benefit sanctions due to accepting jobs that pay less and/or are less stable. Third, this paper constructs and develops multivariate mixed proportional hazard models that do not restrict the correlation between heterogeneity components in any of the processes that are involved. This goes beyond existing studies such as Bonnal et al. (1997) and Van den Berg and Vikström (2009) who use factor structure modelling to reduce dimensionality, or Lalive et al. (2005) whose main results imply degenerate distributions of unobserved heterogeneity.

The remainder of this paper are structured as follows. Section 1.2 discusses institutional procedures in the Swiss UI system, both concerning unemployment benefits and sanction proce-

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

[8]Recent contributions from the US and UK include Black et al. (2003), Klepinger et al. (2002) and Petrongolo (2008). These studies evaluate reemployment services, including job search assistance, or strengthened work-search requirements. They find some positive, no, and persistently negative effects on subsequent earnings, respectively. Note that these studies differ substantially from the sanctions literature even though job seekers may get penalised by losing eligibility in the case of non-compliance. Unlike ours, these studies do not dispose of information on individual non-compliance and sanctions. Therefore they cannot distinguish whether the measured effects come from compliance or non-compliance behaviour. It's sensible to assume that they are mainly driven by compliance since the majority of job seekers normally complies. In contrast, our study explicitly evaluates the behaviorally different case of effects of detected and penalised *non-compliance* behaviour. Theoretically, this kind of behavior implies an additional element of uncertainty about incidence and timing of sanction enforcement. Moreover, non-complying individuals remain in UI and must continue to fulfill all related obligations, which is not the case in the above-mentioned studies. Only very recently we became aware of Van den Berg and Vikström (2009), who also investigate post-unemployment effects of unemployment benefit sanctions. Using Swedish data on post-unemployment jobs - wage rates, hours of work and occupational level - they find that sanctions lower wages and hours of work and lead to a lower occupational level.

[9]Van den Berg and Vikström (2009) study the effects of benefit sanctions on job quality but not on earnings.

dures. In Section 1.3 we briefly outline possible behavioral explanations for sanction effects in the post-unemployment period. Section 1.4 presents our data and a descriptive analysis. In section 1.5 we provide the set-up of the econometric analysis while in section 1.6 we provide our parameter estimates. Section 1.7 concludes.

1.2 Institutional Procedures in the Swiss UI System

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

The potential duration of unemployment benefits is 2 years for individuals who meet the contribution and employability requirements. After this period of two years unemployed have to rely on social assistance. The replacement ratio is 80%; and 70 % for job seekers who earned more than CHF 4030 (3650 USD) prior to unemployment and are not caring for children. Job seekers have to pay all earnings and social insurance taxes except the unemployment insurance tax rate (which stands at about 2 %). This means that the gross replacement rate is similar to the net replacement rate.

The entitlement criteria during the unemployment spell concern job search requirements and participation in active labor market programs. Job seekers are obliged to make a minimum number of applications to 'suitable' jobs each month.[10] And, they are obliged to participate in active labor market programs during the unemployment spell.[11] 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

[10] 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.

[11] The exact nature and scope of the participation requirement is determined at the beginning of the unemployment spell and in monthly meetings with the caseworker. Gerfin and Lechner (2002) and Lalive et al. (2008) contain background information on and an evaluation of the active labor market programs.

caseworker's availability. Job seekers have to meet at least once a month with the caseworker. Caseworkers monitor job search by checking that job seekers use to fill in the details of the jobs to which they have applied. Job seekers are typically required to apply to about 10 jobs per month. Caseworkers have some discretion to adjust this target. Caseworkers count the number of new applications in all cases and they may also check up on the applications claimed by job seekers. Participation in a labor market program is monitored by the caseworker because program suppliers only get paid for the actual number of days a job seeker attends the program.

In this paper we focus on benefit sanctions because of noncompliance with eligibility requirements.[12] 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[13]. 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[14]. 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.[15]

Once the CMEA has decided on legitimacy and duration of the sanction, benefit payments are stopped for time specified in the warning letter. The CMEA has to take this decision within an enforcement period of six months. The enforcement period for the benefit cut starts at the first day of the committed noncompliance[16]. Due to administrative delay at the CMEA, there is no strict one-to-one relationship between receiving a warning letter and the day when benefits are stopped. Once the sanction has been imposed, the unemployed can appeal to a cantonal court within 30 days of the start of the benefit sanction. The court then decides whether the sanction

[12]We disregard a second type of benefit sanctions which refer to 'unnecessary' job loss and are inflicted upon workers at the start of the unemployment spell.

[13]The timing of the warning process is, thus, not linked to the meeting with a caseworker. The mentioned authorities can monitor and warn at any time – e.g. whenever they detect that a claimed application was not sent to the employer, or they get to know that the job seeker did not participate in the ALMP, etc.

[14]This warning letter does not explicitly state the size of the potential penalty. The reason of the sanction gives, however, some indication. But note that the CMEA has considerable leeway in the decision on sanction strength.

[15]Depending on the nature of the infringement, there are four levels of sanction strengths; in workdays: 1 to 15, 16 to 30, 31 to 60, several months up to more than a year. The last level is barely applied. Note that individuals stay in unemployment insurance when sanctioned.

[16]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.

conforms to current legal practice. However, it takes at least one year until the court reaches a decision. Appeal to the court does not keep the CMEA from imposing the sanction.

Note that sanctions are private information and neither caseworkers nor job seekers share information on benefit sanctions with potential employers.

1.3 How Sanctions Affect Behavior

Which are the possible behavioral explanations that can elucidate the effects of the sanction system on labor market outcomes after unemployment exit? Job search theory provides a convenient framework for understanding this issue.[17] There are two behavioral responses of unemployed workers to benefit sanctions. First, they might increase *search intensity*. Second, sanctions could make them lower their demands concerning post-unemployment jobs, i.e. reduce their *reservation wage*. Benefit sanctions affect behavior because they reduce the value of being unemployed. Two effects may be distinguished. The first effect is the *ex-post effect*, the effect that a benefit reduction increases costs of being unemployed thereby changing the behavior of the unemployed. However, unemployed may already change their behavior in anticipation of a benefit sanction, to avoid getting one imposed. This second effect is the *ex-ante effect*, the effect that the *risk of getting a benefit sanction* influences behavior as well.

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

Moreover, the *effects of warnings and of enforcing the benefit sanction may differ if job seekers search for jobs of different quality*. Job seekers who receive a warning letter know that the probability of a benefit reduction has substantially increased but they continue to receive the same benefits. In contrast, job seekers who receive the information that their benefits are cut experience a strong, temporary reduction in the stream of benefits received. Differences in the effects of a warning and the effects of an actual benefit reduction may be related to the quality of jobs workers

[17] 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.

are looking for. Suppose there are two types of jobs; "good" jobs referring to full-time permanent positions and "bad" jobs referring to part-time and/or temporary positions. Job seekers entering unemployment will be searching for good jobs while disregarding bad jobs. Receiving the warning letter decreases the value of remaining unemployed. This will increase intensity of searching for good jobs while leaving unaffected intensity of searching for bad jobs. Seeing the benefits actually reduced decreases the value of staying unemployed more substantially leading job seekers to search for bad jobs as well as for good jobs. So, warnings may have different effects from actual benefit reductions with respect to the quality of jobs accepted. It is therefore theoretically fruitful to *distinguish between search for a temporary vs a permanent job*. The key idea is that job seekers may not search for temporary jobs until they experience actual benefit reductions[18]. This can explain why sanction warnings have no effect on employment stability whereas benefit reductions clearly shorten employment spells after UI exit – a result we find in this study. In *Appendix A*, we outline this theoretical explanation more in detail.

Finally, a further dimension of effects of benefit sanctions – which has been ignored so far in the empirical literature – is *their impact on labor force attachment*. For some subpopulation of unemployed workers sanctions may not promote but discourage search effort. This group of job seekers attaches only slightly more value to being in registered unemployment than to being in a state of unregistered unemployment which imposes no obligations. For these individuals the shock of a sanction – or already the announcement of it – reduces the value of registered unemployment such that they now decide to leave UI for unregistered non-employment. This status is more attractive for them since it avoids the cost of job search and compliance to the obligations of the UI. In addition, they can avoid the pressure of being monitored and the risk of further sanctions. Note, moreover, that an ex-ante effect for this kind of behavioral reaction is conceivable: that the mere threat of potential sanctions influences the labor force participation decision. It is a priori not clear if suchlike labor force exits are of rather temporary or permanent nature. This will be empirically discussed in section 1.6.3.

1.4 Data and Descriptive Analysis

1.4.1 Data Sources and Data Structure

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

[18] Our theoretical explanation in Appendix A comprises as well an alternative set-up where the unemployed search for a bad job with low(er) intensity already before the enforcement of a sanction, but increase search for these jobs relatively more thereafter. See footnote 53 for details.

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

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

From the merged UIR-SSA database, we draw an inflow sample covering individuals entering the UIR between August 1998 and July 1999. From these, we selected UI eligible job seekers aged 30 to 55 entering unemployment from a job with positive earnings in the year prior to entering unemployment[19]. Moreover, we restrict the sample to individuals who are entering unemployment in cantons with reliable information on warnings. Cantons differ in terms of the number of actual benefit reductions that are preceded by a warning letter. We interpret this as missing information on warning letters because job seekers must be informed before actual benefit reductions take place. The analysis focuses on cantons where almost all warnings preceding actual benefit reductions are present[20]. This sample is not representative for Switzerland.[21] Yet this sample restriction allows understanding both the effects of a warning and the effect of enforcing the benefit sanction. The resulting sample covers 23,961 spells. The median duration of unemployment is 153 days, 80.0% of the unemployed found a job, 19.8% of the unemployed received a sanctions warning, while 8.4% actually got a benefit sanction imposed (see for more details Appendix E).

1.4.2 Descriptive Analysis

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

The key piece of descriptive evidence concerns earnings *histories* of individuals who never experience a sanction, individuals who receive a warning but this warning does not lead to an actual

[19]The latter selection was chosen in order to focus the sample on individuals who acquired at least some benefit rights. This excludes individuals who are registered in UI only to follow ALMP's. Note that individuals with zero benefit rights are not at risk of being sanctioned.

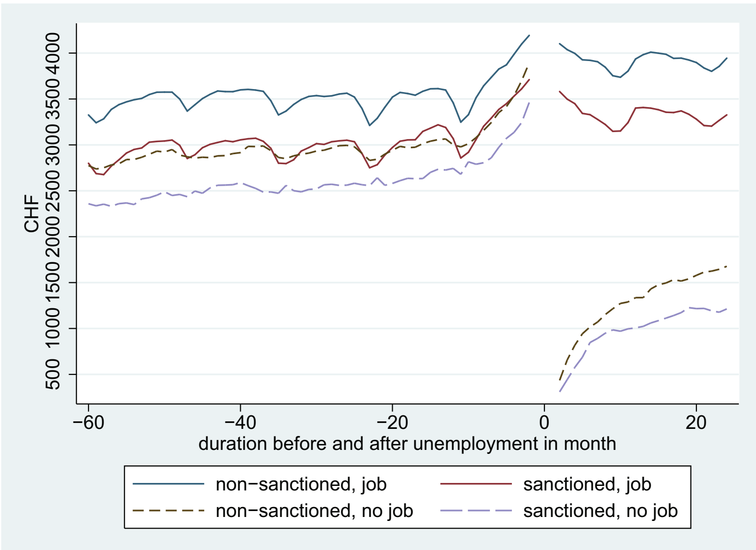
[20]These cantons are Vaud, Valais and Fribourg in the West, Solothurn and Uri in the center, and Appenzell-Innerrhoden and Graubünden in the East. On average, 5% of the warnings are missing. Cantons with at least 87.5% warnings present were chosen for the sample. We predict warning times for the remaining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

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

Figure 1.1: Duration-dependent employment earnings histories: by sanction status.

a. By sanction status

b. By employment status



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

reduction in benefits, and individuals who receive a warning and the benefit cut is also realized. Recall that our earnings data span the time period 1993 to 2002. This allows constructing average (deflated) earnings in the 5 years prior to entering unemployment and in the 2 years after leaving unemployment by sanction status (top graph of Figure 1.1). Results indicate that non-sanctioned and sanctioned differ tremendously with respect to earnings levels. Whereas non-sanctioned earn almost 3500 CHF per month[22], individuals with either a warning or an actual benefit reduction earned on the order of 2750 CHF per month.

Interestingly, while the earnings gap between individuals who were warned only and those who are warned and enforced is visible 5 years before entering unemployment, the gap disappears around the time when individuals enter unemployment. This suggests that while selectivity is important in comparing the non-sanctioned to either warned or warned plus enforced individuals, direct comparisons within the latter two groups are more informative. Moreover, enforcing the sanction appears to lower post-unemployment monthly earnings for the group with a sanction by about 200 CHF in comparison with the warned group. This is a first descriptive hint that benefit sanctions may reduce post-unemployment earnings. But this picture could be misleading since the descriptive effect may be confounded by unobserved characteristics and endogenous selectivity. These will be taken into account in the estimated models. The bottom graph of Figure 1.1 distinguishes the earnings paths with respect to the exit destination – into employment or nonemployment. This figure supports the previous one, pointing to an increased earnings difference between the sanctioned and non-sanctioned after unemployment exit for both, the exit to employment and to non-employment group.[23]

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

Figure 1.2 shows the empirical Kaplan-Meier estimates of the transition rate from unemployment to employment or non-employment and the sanction warnings rate. Job seekers leave unemployment for employment if their labor earnings in the first month after unemployment exceed zero. Job seekers leave unemployment for non-employment if labor earnings in the first month after unemployment are zero.[24] The exit rate to employment starts at a rather low level of 5 % per month, peaks at 14 % per month after 5 months of job search have elapsed, and tapers off gradually to a level of about 7% per month after 10 months of elapsed unemployment

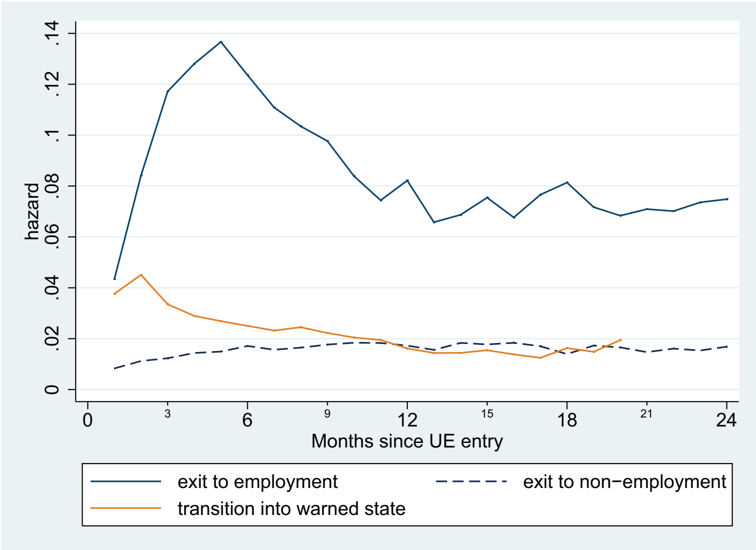
[22]When interpreting the absolute earnings levels in this and the previous figures, one has to consider that: (i) individuals may be partly employed, partly non-employed in their earnings history; (ii) also part-time workers are in the sample; (iii) the sample contains all the individuals who gained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

[23]Note that the upward-tendency of the earnings paths in the last year before unemployment entry in the two graphs in Figure 1.1 is generated by the sampling: The fact that having at least once positive earnings in the year before unemployment entry is one of the conditions of being sampled and leads to a higher proportion of individuals in employment in this year. Consequently, average earnings are higher. This causes no problems for estimation later on because we will control for the full past earnings and employment history.

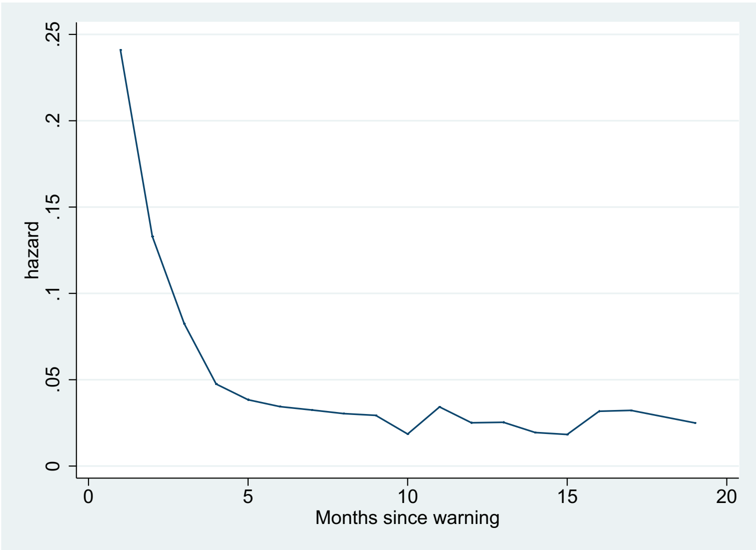
[24]Note that the pension data covers labor earnings and earnings from some transfer programs (unemployment, disability, and military insurance) but not on social assistance. Job seekers leaving for non-employment could be drawing social assistance. This is, however, unlikely since social assistance would send job seekers who are eligible for unemployment benefits back to unemployment insurance.

Figure 1.2: Unemployment transition rates and sanction enforcement rates

a. Exit rates from unemployment & sanction warning rates



b. Sanction enforcement rates



duration. The transition rate to non-employment, on the other hand, doesn't show a peak in the early months of unemployment: It slightly increases in the first 6 months from 1 to 2% of exits to non-employment. From then on, it remains on this level. In general, the distribution of the UE durations in the sample (not illustrated) shows the well-known shape with a peak in the first four months of unemployment and another peak, though smaller, at the end of the normal benefit entitlement period after two years. The third hazard rate in Figure 1.2 is the sanction warning rate. The sanction warning rate measures the probability of a sanction warning in the next month for those who are still unemployed at the start of each month. The sanction warnings rate shows a peak of almost 5% in the second month of UE, gradually decreasing afterwards. The median duration until the first warning was 77 days.

The bottom graph of Figure 1.2 shows the enforcement hazard, i.e. the rate at which sanctions are enforced among those who have been warned. Clearly, there is a strong tendency to enforce a sanction in the first month after giving the warning. The enforcement hazard peaks at about 23 % in the first month, and decreases strongly to 7 % in month 2, and more gradually to levels below 5 % per month thereafter. This evidence suggests on one hand that at least one quarter of all warnings immediately lead to withdrawal of benefits. On the other hand, the fact that the enforcement hazard is substantially below 100 % in the first month after the warning also suggests that not all warnings are actually enforced.

1.5 Econometric Analysis

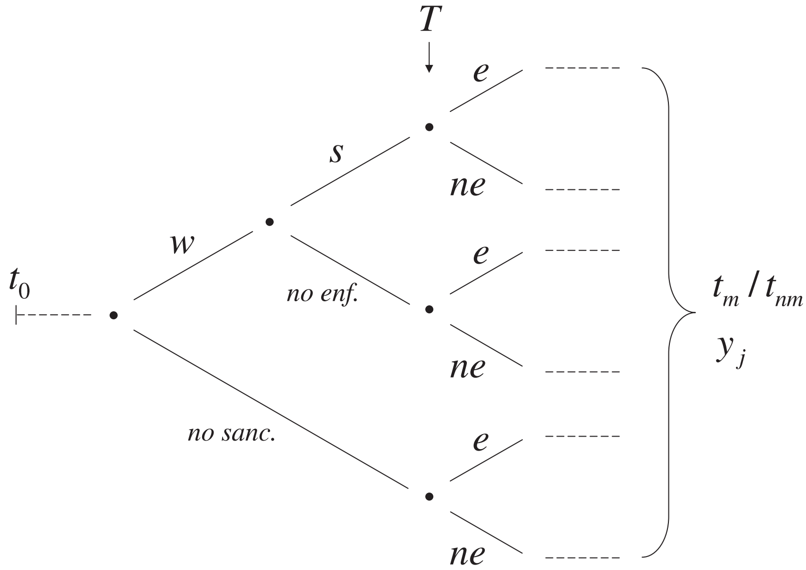
Our dataset allows the use of detailed duration analysis methods. In particular, we use a multi-state duration model that combines information on the timing of benefit sanctions with information on unemployment dynamics and the quality of post-unemployment jobs.

1.5.1 Modeling Individual's Event Histories

As a base for the evaluation of sanction effects on post-unemployment outcomes, we model the event history of an individual during and after unemployment. As depicted in Figure 1.3, the individual experiences *multiple stages*, starting at t_0, the entry into unemployment. The first selection is the treatment assignment: to be sanctioned or not. Since we dispose of non-experimental data, this *assignment is non-random and endogenous*. It comprises two stages, the warning (subscript w) that a sanction investigation has started, and later the possible sanction enforcement (s). Thus, at the point of exit from unemployment (T), the individual can be potentially in three different states (s, w or not sanctioned). In addition, unemployment spells can be censored if they last longer than 720 days.

By T, the third selection takes place, individuals exit to employment (e) or non-employment (ne). Job seekers are defined to exit for employment if their labor earnings exceed any other source of income in the first full month after leaving unemployment. To clarify, suppose a job seeker leaves April 15^{th}. We then check the entire month of May and compare labor earnings to earnings from other social insurance transfers that we observed in the data (disability insurance, military insurance). If labor earnings exceed these other income sources, we say that the job seeker has left unemployment for employment. If labor earnings are equal or below other sources of income,

Figure 1.3: Multiple states of the individual's process history



Note: Abbreviations of states: w=warned, s=sanction enforced, e=exit to employment (i.e. positive labor earnings in the first month after unemployment exit), ne=exit to nonemployment (zero earnings in the first month). Note that for Model III, the exit destinations e and ne are replaced by y=positive labor earnings over 24 months after unemployment exit and 0=zero earnings over that period. See the econometrics and results sections (1.5 and 1.6) for more explanations and discussion.

we say that the job seeker has left unemployment for non-employment[25]. Note that in most cases other sources of social insurance transfers are zero. Thus, we mainly classify exits by whether there are some or there are no labor earnings in the first full month after leaving unemployment.

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

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

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

points distribution for the unobserved heterogeneity.

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

There are two central assumptions for the nonparametric identification of causal effects of dynamic treatments (Abbring and van den Berg 2003a). The first assumption states that job seekers do not know *the exact date* when a warning or actual reduction of a benefit sanction takes place but it does not exclude that forward looking individuals act on properties of the sanction warnings and benefit reduction process. In other words, we assume that there is no *deterministic* anticipation effect where workers are informed exactly, while we allow for a *probabilistic* anticipation effects, the ex-ante effect where workers may behave differently because they know they may be confronted with a benefit sanction. The ex-ante effect is constant over the spell of unemployment, depending only on the local sanction system. The (deterministic) no anticipation assumption is crucial to rule out changes in behavior before the actual treatment takes place. Arguably, anticipation of the exact date of warnings and benefit reductions is not possible in the present context. Job seekers may have some information regarding the monitoring technology used by caseworkers, but they can not anticipate the actual date of receiving the warning letter. This is because issuing the warning letter takes several steps. First, caseworkers, firms, or program staff need to detect non-compliance and decide to report it. Second, the official at the CMEA will look into the case and decide whether non-compliance is present. Third, job seekers can not anticipate the actual day of receiving the letter because administrative delays are introducing a strong degree of uncertainty. Moreover, job seekers also can not anticipate the day when benefits are reduced. Justification introduces uncertainty with regard to whether the warning leads to a benefit reduction. Moreover, even if justification is not valid, the CMEA can take up to 6 months until the benefit sanction is actually enforced.

The second key identifying assumption is that the hazards of leaving unemployment have a mixed proportional hazard structure (MPH). This assumption states that selectivity can be modeled assuming time invariant unobserved heterogeneity that is independent of observed characteristics. The assumption of time invariance appears warranted (referring to individual specific characteristics such as motivation for job search, etc.). In contrast, the assumption of independence between observed and unobserved characteristics appears to be more questionable. However, note that while correlation between observed characteristics and unobserved characteristics is likely to bias parameter estimates attached to control variables, the bias to the treatment effects are likely to be less severe since selectivity is explicitly taken into account. Assuming an MPH structure also means that observed covariates shift the hazard rate proportionately. Proportionality is one of the most common assumptions in duration studies and earlier work on Switzerland suggests that it is not driving results on the effects of dynamic treatments (Lalive, van Ours and Zweimüller 2008).

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

$$\theta_l(t_l|x, r, p, D_{wl}, D_{sl}, v_l) = \lambda_l(t_l)\exp(x'\beta_l + r'\alpha_l + p'\gamma_l + \delta_{wl}D_{wl} + \delta_{sl}D_{sl} + v_l) \tag{1.1}$$

$\lambda_l(t)$ stands for individual duration dependence in our proportional hazard model, x represents a vector of observable individual characteristics, r is a vector of public employment service dummy variables, p is a vector of controls for state dependence[26] and v_l represents the unobserved heterogeneity that accounts for possible selectivity in the exit process (see subsection 1.5.3 for the empirical specification of unobserved heterogeneity). Appendix E provides a detailed description of the set of control variables x, r and p. Note that this full set is used for all the models described in the following. The parameters δ_{wl} and δ_{sl} measure the effect that a warning and an enforcement have on the exit rate from unemployment. Note that δ_{sl} measures the additional effect of enforcement relative to the effect of a warning. A common approach to modeling flexible duration dependence is the use of a step function (piecewise-constant duration model)

$$\lambda_l(t_l) = \exp(\sum_k (\lambda_{l,k} \cdot I_k(t_l)) \tag{1.2}$$

where $k = 0, .., 3$ is a subscript for time-intervals and $I_k(t)$ are time-varying dummy variables that

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

are one in subsequent time-intervals. Taking into account the shape of the descriptive hazards (see section 1.4.2) and the fact that for our Swiss data we observe median unemployment durations of a bit less/more than half a year for the exit to e/ne groups, we fix the four time intervals as follows: 1-40/1-90 days, 40-210/90-270 days, 210-360/270-480 days and 360/480 and more days. Because estimation includes as well a constant term, normalization is necessary which is achieved by setting $\lambda_{l,0} = 0$ (i.e. the constant measures the baseline exit rate in interval 0).

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

$$\theta_h(t_h|x, r, p, v_h) = \lambda_h(t_h) \exp(x'\beta_h + r'\alpha_h + p'\gamma_h + v_h) \tag{1.3}$$

where for $h = \{w, s\}$, $\lambda_h(t_h) = \exp(\sum_k (\lambda_{h,k} \cdot I_k(t_h))$ with normalization $\lambda_{h,0} = 0$ and v_h representing the respective unobserved heterogeneity.[27]

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

$$\mathcal{L} \;=\; \prod_{i=1}^{I} \int_v \theta_{w,i}^{c_w}(t_w) S_{w,i}(t_w) \theta_{s,i}^{c_s}(t_s) S_{s,i}(t_s) \theta_{e,i}^{c_e}(t_e) S_{e,i}(t_e) \theta_{ne,i}^{c_{ne}}(t_{ne}) S_{ne,i}(t_{ne}) \mathcal{L}_{p,i} \, dG(v) \tag{1.4}$$

where c_m ($m \in \{e, ne, w, s\}$) designates a censoring indicator, being 1 if the respective duration is not censored, and zero otherwise, and $S_{m,i}(t_m) \equiv exp(-\int_0^{t_m} \theta_{m,i}(z)dz)$ is a time-to-event specific "survivor" function, v is a vector of unobserved heterogeneity components (further discussed in section 1.5.3), and $G(v)$ is the corresponding cumulative joint distribution. Note that 1.4 accounts for both right-censoring and the competing risks nature of unemployment exits.

The most important element in (1.4) is $\mathcal{L}_{p,i}$ containing information on the individual likelihood contribution of the post-unemployment period. This element of our model varies, depending on which post-unemployment outcome we evaluate.

1.5.2 Modeling the Post-unemployment Outcome Measures

Considering the post-unemployment labor market histories adds a second selection problem to the model: Not only the selection into the treatment state is endogenous, but as well the selection into the post-unemployment state – finding a job or not is clearly endogenous. This implies that the composition of the subsample of job finders with respect to observables and unobservables is different from the one of the non-employed. This has to be taken into account when estimating

[27] Based on descriptive analysis of the duration distributions and hazards, duration splits to implement the piecewise-constant design are set to 30/90/240 days for the warnings hazard and 10/30/150 days. Note that enforcements usually take place already 10 to 20 days after the warning, therefore the early splits (see section 1.4.2 for descriptive details).

labor market outcomes for these subsamples separately. Intuitively, handling this selection problem implies the control for observable and unobservable differences as well as allowing for a correlation structure between the unemployment and the different post-unemployment processes. This is done by simultaneous estimation with correlated unobservables. We model this approach in the following subsections.

1.5.2.1 Employment stability

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

Note that we control here as well for the realized duration of unemployment, t_u ($= \min(t_e, t_{ne})$). To allow for nonlinear unemployment duration dependence we add a polynomial function $g(ln\ t_u)$[28] to the controls. This implies for the complete likelihood functions – which describe the joint distribution of t_w, t_s, t_e, t_{ne}, t_m and t_{nm} – that we claim independence between the distributions of these durations *conditional on* x, r, p, D_w, D_s, the respective unobserved heterogeneity v and duration t_u in the case of the two post-unemployment processes.

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

$$\begin{aligned} \mathcal{L}_{p,i} &= \left[[S_m(t_m - 1) - S_m(t_m)]^{c_m} \, S_m(t_m)^{1-c_m} \right]^{c_e} \cdot \\ & \left[[S_{nm}(t_{nm} - 1) - S_{nm}(t_{nm})]^{c_{nm}} \, S_{nm}(t_{nm})^{1-c_{nm}} \right]^{c_{ne}} \end{aligned} \tag{1.5}$$

Note that this likelihood contribution takes into account that employment and non-employment durations can only be observed in monthly precision (see Appendix E for clarification). Since these contributions are at the third stage of the selection (see Figure 1.3), double-censoring occurs. First, censored employment or non-employment durations (with c_m or c_{nm} equal zero) may occur since the post-unemployment observation window is restricted to the end of 2002. Second, uncensored unemployment spells with c_e or c_{ne} equal 1 are censored in the other exit destination and therefore as well in the respective post-unemployment process. Finally, in the case of a censored unemployment spell, c_e *and* c_{ne} are zero and $\mathcal{L}_{p,i}$ equals 1.[29]

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

[29] 19,149 of total 23,961 spells (i.e. 79.9%) exit from unemployment to employment ($c_e = 1$), 2985 (12.5%) exit to non-employment ($c_{ne} = 1$); 1827 (7.6%) exhibit censored unemployment durations. After exit, 42.5% and 34.9% of the respective populations are censored in their first employment/non-employment spell (i.e. $c_m = 0$ or $c_{nm} = 0$). These high censoring rates point to the fact that an important share of the sample show stable labor force participation statuses after unemployment exit.

1.5.2.2 Post-unemployment earnings

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

We use an MPH structure to model the post-unemployment earnings distribution for at least two reasons. First, the MPH model structure is more flexible than assuming a specific parametric distribution – e.g., log-normality – by applying the same flexible hazard function design as for the durations above. Second, results from the duration literature show that the earnings hazard model is identified.[30] We extend this approach additionally in two respects: First, we use this multiple states hazard framework with earnings to evaluate a specific treatment. Accordingly, we introduce dynamic treatment effects in this context. Second, we handle the double selectivity problem that is implied by our framework: Selection at the *entry* into the two sanction states and at the *exit* from those states into (non-)employment.

The earnings hazard describes the (instantaneous) probability of earning y conditional on earning at least y. Thus, like the unemployment exit hazard, the earnings hazard has an upward-directed interpretation: the probability of generating an earnings level of exactly y conditional on earning at least y. What are the implications of assuming that the earnings hazard follow an MPH structure? In case earnings are exactly exponentially distributed, the MPH structure implies that both observed and unobserved characteristics change log expected earnings in an additive fashion – quite similar to modeling log earnings using linear models.[31] In case earnings are not exponential, assuming an MPH structure generally implies modeling proportionate shifts on the integrated earnings hazards. Moreover, it can be shown that assuming an MPH structure implies that the effect of benefit sanctions on mean earnings as well as on all the quantiles of earnings are of opposite sign as the effect on the hazard.[32]

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

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

[32] To see this, suppose that earnings without sanction are Y_0 with hazard $\theta_0(y|x) = \lambda(y)exp(x'\beta)$ and Y_1 follow a distribution with hazard $\theta_1(y|x) = \theta_0(y|x)exp(\delta)$ where δ is the effect of a benefit sanction on the earnings hazard. Since $E(T_1|x) = \int_0^\infty exp(-\int_0^y \theta_1(z|x)dz)dy$, it follows $E(T_1|x) < E(T_0|x) \iff \delta > 0$. Moreover, note that the α quantile treatment effect is $y_1^\alpha - y_0^\alpha = \Lambda_0^{-1}(-log(1-\alpha)exp(-\delta)) - \Lambda_0^{-1}(-log(1-\alpha))$ where $\Lambda_0^{-1}()$ is the inverse of the integrated hazard of the counterfactual earnings distribution. This means that $y_1^\alpha - y_0^\alpha < 0 \iff \delta > 0$ since $\Lambda_0^{-1}()$ is a monotonically increasing function. Finally, consider the log likelihood ratio of earnings with sanction and counterfactual earnings without sanction, i.e. $lnf_1(y|x)/f_0(y|x) = \delta - (exp(\delta) - 1)\Lambda_0(y)$. This shows that the likelihood ratio satisfies the monotone likelihood ratio property, and benefit sanctions shift the earnings distribution in the sense of first order stochastic dominance.

For the earnings data, we implement the estimation of sanction effects on earnings in the same way as in Model I one above – we just replace t_o by y_j, i.e. by one of the mentioned earnings measures (whereby $j = \{1, 24\}$). Since the earnings data are considered as being continuous we use continuous hazards. Depending on the descriptive hazards and medians of the respective measures, we define suitable splits of the earnings values to design the respective piecewise-constant earnings-level-dependence functions $\lambda_{y_j}(y_j)$[33].

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

$$\mathcal{L}_{p,i} = \left[\theta_{y_j}^{c_{yj}}(y_j) S_{y_j}(y_j)\right]^{c_e} \tag{1.6}$$

Model III is very similar in the design – except that it uses different exit destinations. Going back to Figure 1.3, this means that at time T individuals are not separated by exiting to e or to ne as described in Model III, but the exit destinations are now $y_{24} > 0$ and $y_{24} = 0$. So, we separate individuals with a sum of earnings over 24 months which is positive from those with zero sum of earnings[34]. The second group represents the part of the sample that permanently exits labor force over 24 months. The comparison of the Models II and III allows interesting statements about the effect of sanctions on individuals who *temporarily* exit to nonemployment, thus who reenter labor force during the 24 months (i.e. the subgroup which has different exit destinations in the two models). See more on that comparison in the respective results subsection 1.6.3. Consequently, the likelihood contribution for Model III has the same structure as the one for Model II:

$$\mathcal{L}_{p,i} = \left[\theta_{y_{24t}}^{c_{y24t}}(y_{24t}) S_{y_{24t}}(y_{24})\right]^{c_y} \tag{1.7}$$

where c_y represent the non-censoring indicator, being one if $y_{24} > 0$. Note that in the Models II and III we estimate five processes. There is no sixth process here (like in Model II) since earnings are not defined for individuals exiting to nonemployment[35].

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

[34] Note that these exit destination definitions imply the use of information over the 24 months *after* exit. This may seem unusual. However, this does not require any change in the econometric modeling of the competing risks. The same basic identifying assumption (see Abrring and van den Berg 2003b) must hold: the latent durations of the different risks must be independent, *conditional* on x and v. Here, the estimation of v is influenced by the 24 months of labor market history after UE exit. This additional information may be helpful for the precision of the estimation of v. On the other hand, this longer time span may increase the risk that the time invariance assumption on v gets violated.

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

As described for Model I, the post-unemployment process is again confronted with double censoring. First, c_{yj}/c_{y24t} can be zero for two reasons: earnings can't be observed over 24 months[36] after unemployment exit (since this was late in the observation window); in addition, earnings are right-censored at 10,000/200,000 CHF over 1/24 months due to the top coding of social security earnings. In our data, very small proportions had to be censored due to these reasons[37]. The second hierarchy of censoring (c_e/c_y) is the same as for Model I.

Note that we divide all the earnings measures by 1000, in order to avoid extreme value levels in estimation. Again, we condition on the unemployment duration by adding the polynomial $g(ln\ t_u)$[38] to the controls.

1.5.3 Dealing with Multiple Selectivity

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

To take into account this multiple selectivity on the level of unobserved characteristics, we follow the approach of Gritz (1993) and Ham and LaLonde (1996). They point out that addressing the selection problem consists in *simultaneously* modeling the selection processes into the treatment and later into (non-)employment and in allowing for *correlation* between the different stages of the individual's history. The first point is met by the model presented above. The second is handled by allowing for correlation between the unobserved heterogeneity components of the different processes. For example, an individual who leaves unemployment for employment may have above average unobserved characteristics. This positive composition and selection effect (linked to the fact of having indeed found a job) may mask the potentially negative effect of a sanction on

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

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

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

subsequent employment duration – if we don't control for the correlation in unobservables between the unemployment exit process and the subsequent employment process. Such arguments may be made for all our proposed models.

Combining such a design and our precise data, the effect of interest – the *causal* effect of benefit sanctions – can be separated from the discussed selectivity effects due to availability of information on the exact timing of the sanction process and the exit process. Causal effects of sanction warnings and enforcements on unemployment exit and the post-unemployment process create a conditional dependence between the five or six processes: i.e., the outcome measure changes *only* in the case a warning has been issued or a sanction has been enforced. On the other hand, selectivity creates a global dependence between the outcome and the sanction processes, captured by the correlation of the unobserved heterogeneity components.

In estimation we handle unobserved heterogeneity in the standard way by integrating it out over the joint density function $G(v)$, as shown in equation (1.4) above. The vector $v \in \mathbb{R}^6_+$ or $v \in \mathbb{R}^5_+$ comprises all the unobserved heterogeneity components of the respective model: In the Model I, $v = (v_w, v_s, v_e, v_{ne}, v_m, v_{nm})$, in the Models II and III we replace the last two elements by v_{y1}, v_{y24} or v_{y24t}.

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

$$Pr(v_w = v_{wg}, v_s = v_{sg}, v_e = v_{eg}, v_{ne} = v_{neg}, v_m = v_{mg}, v_{nm} = v_{nmg}) = p_i \tag{1.8}$$

$$Pr(v_w = v_{wg}, v_s = v_{sg}, v_e = v_{eg}, v_{ne} = v_{neg}, v_r = v_{rg}) = p_i \tag{1.9}$$

whereby expression (1.8) applies to Model I and expression (1.9) to the Models II and III. In the latter case, we distinguish $r = \{y1, y24, y24t\}$. All unobserved heterogeneity level combinations with $g = \{a, b\}$ for each process are possible. This generates probabilities p_i for $i = 1, \ldots, 64$ in Model I and for $i = 1, \ldots, 32$ in the Models II and III. To ensure that the probabilities p_i are between zero and one, and sum to one, we model $p_i = exp(a_i)/\sum_i \exp(a_i)$ and normalize the last a as being $a_I = 0$. Note that we specify the correlated unobserved heterogeneity in a more flexible way than in Ham and LaLonde (1996), who rely on a one-factor structure, and most of the applications (e.g. Van den Berg and Vikström 2009 or Bonnal et al. 1997).

1.6 Estimation Results

We report in the following the results of the parameter estimates of the *Models I to III* as described in the econometrics section 1.5. Then, we proceed to the analysis of the ex-ante effects. Thereafter, we discuss how we explain our findings from a theoretical point of view. The section ends with simulation exercises based on the reported estimation results, which allow to quantify the different treatment effects.

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

Table 1.1: The effect of benefit sanctions on exit behavior and subsequent non-/employment duration

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

Notes: We report coefficients and their transformations: Transformed treatment effects are changes in %. Asymptotic z-values.

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

Table 1.1 provides information on the econometric estimates of Model I. *Model I* focuses on the effects of benefit sanctions on the exit behavior of concerned individuals, assuming correlated unobserved heterogeneity. How do benefit sanctions affect the non-/employment stability? To answer this question, the duration of the first spell of employment (M) for job seekers leaving unemployment to employment and the duration of the first spell of non-employment (NM) for job seekers leaving unemployment for non-employment is analyzed. Individuals of the E group who face a sanction warning are confronted with an immediate increase of the exit rate from the employment spell M by 1.9%. This change is not significant. In contrast, the additional treatment effect coming from imposing the sanction is highly significant and amounts to 15.0% for the M spells. The point estimate of the warning effect for the NE group on the NM spell is markedly higher, 15.7%, but not significant either. Again, the additional enforcement effect is significant; it results in a considerable increase of the NE hazard by 30.7%.

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

Turning to results on the effects of benefit sanctions on leaving unemployment, we find that the point estimates of the treatment effects indicate that the log hazard rate of exits into employment (E) goes up by 0.147 once individuals get warned that they are under suspicion of having committed a non-compliance. Once the sanction is enforced, the exit to E rate increases by additional 0.148. Both effects are substantial and highly significant. Expressed in percentage changes (i.e. $exp(\delta)-1$), results indicate that a sanction warning caused a 15.9 % increase relative to non-sanctioned, whereas actually imposing the sanction adds a further increase of the rate by 16.0 % relative to the job seekers with a warning.

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

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

In the Appendix B, Table 1.7, we report additionally the baseline transition rates for all processes of Model I as well as the estimated mass point probabilities. Besides the estimated constant of the first piece of the baseline hazard (λ_1), we indicate the transition rate of an "average" individual (see notes of Table 1.7 for details) for the same first split period. Our estimates allow for two levels of unobserved heterogeneity in all four hazard rates. Starting from a restrictive specification with only a small number of mass points, we add more of them as long as they increase the log likelihood. As recommended by Gaure et al. (2007), we select the model that provides the best fit according to the log likelihood.

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

mainly with respect to the enforcement of a sanction[39]. Finally, we may note that in Model II the exit to E and to NE treatment effects as well as the four transitions in the unemployment period are very similar to the corresponding estimates of Model I. This is a comfortable and sensible result since there is no obvious argument that adding post-unemployment information should crucially alter the estimation results for the unemployment processes.

1.6.2 The Effects on Earnings and their Persistence

Table 1.2: The effect of benefit sanctions on earnings: over 1 vs. 24 months after unemployment exit; E (exit to employment) group

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

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

The impact of sanction effects on the sustainability of post-unemployment jobs is the key contribution of an analysis of UI sanction systems that looks beyond unemployment exit. But in order to gain an even more comprehensive view on how a sanction system may influence post-unemployment job quality, the analysis of earnings is essential. A glimpse on the duration-dependent earnings histories of Figure 1.1 in the descriptive analysis may lead to the hypothesis that sanctions reduce subsequent earnings. But as mentioned as well, this analysis could be misleading since it doesn't incorporate the issue of selectivity. This problem is addressed in the *Models II* which feature simultaneous estimation of the sanctioning and unemployment processes together

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

with the earnings process of the exit to E group, allowing for correlated unobserved heterogeneity in all the 5 processes.

Table 1.2 reports two versions of Model II: First, we analyze as outcome the earnings in the first (complete) month after exit to employment, i.e. for the E group (Model IIa). Second, we build the sum of realized earnings over 24 months as outcome in the fifth process (for the same E group; Model IIb). The comparison of the two sub-models of Model II allow statements on the *persistence* of the sanction effects in the development of the earnings flow. Whereas the first analysis gives insights on how the individual's reaction on a sanction (warning) is reflected in the take-up of the first job after unemployment, the second analysis aims for a comprehensive view on the total effect of sanctions on earnings generation in mid-terms for the E group. Thereby, the latter allows for and incorporates the effects of switches between employment and non-employment over the two years, directly or indirectly driven by previous sanctions.

How do sanctions affect earnings in the first month after leaving unemployment? Results from Table 1.2 clearly suggest a *negative effect*. Already the act of warning a job seeker that a sanction procedure has been started increases the earnings hazard by 8.0 % for job seekers who leave unemployment after having been warned that a benefit reduction may take place in the future. The earnings hazard increases somewhat more, albeit statistically insignificantly, for job seekers who experience an actual benefit reduction. Both effects translate into lower average earnings for sanctioned job seekers. We defer a discussion of the magnitude of the effects of benefit sanctions on average earnings to section 1.6.5.

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

Estimations of the earnings Models II are affected much less by the inclusion of unobserved heterogeneity than Model II. Comparison with corresponding models without unobserved heterogeneity (not reported) reveals that unobserved heterogeneity only plays a (rather small) role in shaping the enforcement effect[40]. Selectivity into earnings is not relevant[41]. The small role of

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

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

unobserved heterogeneity in this model is presumably due to the inclusion of extensive controls for state dependence into the model. Controlling for earnings and employment paths in the last five years before unemployment seems to capture pretty well the heterogeneity in future earnings development as well. This is consistent with the long-term stability of earnings paths that we observed in the descriptive Figure 1.1.

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

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

Table 1.3: The effect of benefit sanctions on earnings over 24 months: E group (excluding temporary and permanent labor force exits) vs. total population with positive earnings (excluding only permanent labor force exits)

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

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

In a final step, we analyze *Model III* – by comparing it to *Model II* – which features as well earnings over 24 months as outcome. But whereas Model II only focuses on earnings for job seekers who start earning immediately after leaving unemployment, Model III adds those job seekers who temporarily leave the labor force. Thus, the key difference between the two models lies in the feature that individuals exiting first to non-employment and taking up a job later on are part of the analyzed earnings group in Model III, whereas they are not in Model II. Table 1.3

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

Note that the estimation of Model III implies different competing risks destinations with respect to unemployment exit than the Models I to II did[42]. Here, we distinguish the exits to positive earnings over the 24 subsequent months versus the exit to *permanent* labor force exit over 24 months. Accordingly, the exit treatment effects and the four respective transition rates estimates may be different from the ones of the previous models. Indeed, they are – albeit not to large amount. The warning and enforcement effects on the two exit destinations are stronger (in the case of the permanent labor force exit group only when looking at the total effect). The higher increases in the respective hazard rates are sensible: The temporary labor force exit individuals who are now in the Y group contribute with their tendency to exit labor force (which is quantitatively higher as the exit to E effect, as we know from the previous models) to the now higher treatment effects.

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

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

Table 1.4: Ex-ante effects: Regression of PES-specific outcomes on monitoring/warning policy and unemployment rates by PES

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

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

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

1.6.4 Ex-ante Effects

Previous theory and evidence in the small UI sanctions literature pointed to the importance of *ex-ante effects* of benefit sanctions (see section 1.1). The mere "threat" of the presence of a sanction system may induce job seekers to behave more according to the search, job acceptance and obligations to participate in active labor market programs imposed by unemployment insurance. The estimated Models I to III allow us to investigate this kind of policy effect for the Swiss sanction system. In all the models, we estimated public employment service (PES) fixed effects for all the respective processes. The PES effects in the warning process, α_w, represent, presumably, a measure of *how strictly a certain PES office monitors and consequently warns.* Being the result of the very federalist way of policy implementation in Switzerland, these PES fixed effects – and PES-specific warning rates in general (as descriptive analyses show) – vary considerably. We exploit this variation to estimate the effect of monitoring strictness on the PES-specific level of the different outcomes. Since the regional labor market conditions could influence PES-specific sanction policy, we control in addition for the regional unemployment rates by PES (averaged over 1998 and 1999).[43]

Table 1.4, featuring the respective OLS regressions (population-weighted and with boot-

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

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

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

1.6.5 Quantifying the Effects of Benefit Sanctions

The key result of the empirical analysis is that sanction warning and enforcement speed up exit from registered unemployment thereby increasing post unemployment earnings due to earlier start on the job. However, sanction warnings and enforcements also reduce the level of post-unemployment earnings. How do these two effects on post unemployment earnings add up?[44] We provide two sets of simulations on the effects of sanctions on earnings in a two year period after leaving unemployment. Note that we focus on post unemployment earnings rather than post unemployment income.

The first set of simulations provides information on the *ex post effects* of benefit sanctions. The simulation compares the actual pattern of leaving unemployment and post unemployment earnings with counterfactual unemployment exit and post unemployment trajectories. Actual and counterfactual trajectories only differ with respect to the post warning unemployment experience. Whereas the actual trajectory imposes our estimates of the warning and enforcement treatment effects from Model III, the counterfactual scenario sets these treatment effects to zero (see appendix

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

Table 1.5: Simulations: Effects of sanctions on expected earnings and unemployment durations

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

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

section B for further details).[45] Note that all simulations fully take the competing risks nature (exits to paid post unemployment vs exits to unpaid post unemployment) of the exit destination

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

into account.

Table 1.5, panel A provides the results. Actual time in unemployment until an exit with at least some earnings in the two year period after leaving unemployment lasts for 244 days. Counterfactual time to leaving unemployment is 277 days. Thus, sanction warning and enforcement reduce job search duration by 33 days or a bit more than 1 month. Clearly, reduced unemployment duration implies earlier exit to paid post unemployment. But is one month of earlier exit enough to undo the reductions in post unemployment earnings? Earnings simulations indicate that individuals who are sanctioned have, on average, post unemployment earnings of 71,944 CHF in the two years after unemployment. In contrast, had they not been sanctioned, they would have earned 78,113 CHF in a period of two years. This means that post unemployment earnings have been reduced by 6,170 CHF or by 8.6 % compared to earnings with a sanction or about 63 days of pay with a sanction. On net, this means that while sanctioned individuals gain about one month of pay, they lose the equivalent of two months of earnings with sanction. How about individuals who leave unemployment to non-employment? Actual time to leaving unemployment is 309 days, whereas the counterfactual duration is 343 days, or 34 days shorter (reduction of 10 %).[46] Yet since the labor earnings of individuals who leave to non-employment are zero, earlier exit to unpaid post unemployment does not affect post unemployment earnings.

The second set of simulations provides information on the *ex ante effect*. Here, we first simulate actual time to paid and unpaid post unemployment, as well as subsequent earnings in the former case, for *all* job seekers using *actual* estimates of the PES dummies in the respective exit and earnings processes. We then ask, how much earlier job seekers would leave unemployment if PES were asked to *increase their warning intensity to a minimum standard*, and what effect that would have on the earnings thereafter. We set this minimum standard equal to the mean estimated warnings intensity plus one standard deviation of the estimated PES dummies. This means that PES with estimated warnings intensities below that level are required to increase warnings intensity while PES which already fulfil that minimum standard will face no adjustment. How does this affect the hazards of leaving unemployment and generating earnings thereafter? We use estimates of the ex ante effects in Table 1.4 to assess how changes in warning rates translate into changes in exit rates and earnings hazards.

Results indicate that job search until leaving for paid post unemployment lasts for about 203 days (Table 1.5 panel B). With increased warnings intensity, job search would last for 193 days. Thus, job search is reduced by about 10 days due to the ex ante effect. In contrast, leaving unemployment earlier due to more strict warning also leads to earnings reductions. Whereas job

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

Table 1.6: Simulations: Proportions by unemployment exit destinations

A: Ex-post effects (on the sanctioned)

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

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

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

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

Source: Own calculations from merged UIR-SSR database.

seekers earn 84,684 CHF in the two years after leaving unemployment in the actual situation, their earningss would be reduced to 83,201 CHF or 1,483 CHF (1.8 % of actual earnings) in the counterfactual situation with more intense warning. This means that, in the intensified warning regime, leaving unemployment earlier by 10 days is associated with an earnings loss that is equivalent 13 days of full pay. Interestingly, in contrast to our finding for the ex post effects, the ex ante effects on leaving unemployment and post unemployment earnings roughly balance for those individuals who leave unemployment for paid post unemployment situation. But one has to take into account that this rather small net ex ante effect of 4 days of loss concerns *everyone* of the leavers to paid post unemployment, i.e. 89.3% of the Y group (see Table 1.6, panel A).

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

Based on the simulations, we can calculate the proportions of individuals leaving for the two possible exit destinations (Y and 0). These proportions, shown in Table 1.6, support the observation from above about countervailing effects in the 0 group. Under actual warning, 7.2%

of the job seekers exit to unpaid post unemployment (panel B), whereas under the intensified warning policy only 6.1% exit to 0. The opposite is the case for exiting to Y. This highlights the mechanism of reaction on the policy change in the 0 group: Due to intensified warnings, some job seekers now rather exit to a paid job instead of entering the unpaid post unemployment as they would in the status quo. Thus, an intensified warning policy brings some individuals back to reentering labor market. This is, over the whole, not the case for the ex post effects (panel A): Being sanctioned leads to some more entries into Y, but the proportion of exits to 0 increases even more[47].

1.6.6 Discussion

Our findings for the ex post effects of benefit sanctions suggest that, consistent with job search theory, benefit warnings and reductions increase the rate of leaving unemployment. *Yet, there is also a significant reduction in post unemployment earnings, possibly because of lower reservation wages. On net, the positive effects of leaving unemployment more quickly do not outweigh these negative effects of benefit sanctions. This suggests that costs of on-the-job search could be substantial for workers who have recently left unemployment.* Job seekers who are confronted with a warning or a benefit sanction tend to reduce their demands concerning the quality of the post-unemployment job. On average, they accept quicker a job offer – at the cost of a reduced employment stability and/or lower earnings. This cost is financially more important for the individual than her/his gain in terms of earlier unemployment exit.

In terms of ex ante effects, we find that job seekers who are confronted with higher warning probabilities leave unemployment more quickly. Yet again, faster exit from unemployment is accompanied by lower earnings leading to a net reduction in post unemployment earnings. Regarding warning and enforcement effects, we find that while mere warnings increase the rate of leaving unemployment, they do not affect employment and non-employment durations. In contrast, actual benefit reductions do not only lead to a faster exit from unemployment but they also tend to reduce the duration of employment thereafter. Arguably, this result can be explained by the fact that job seekers search for jobs of different quality – temporary and permanent jobs. As outlined in section 1.3 and Appendix A, job seekers may not search for temporary jobs until they experience actual benefit reductions. Such a sequential job search strategy, – that job seekers tend to primarily focus on the search for higher quality permanent jobs as long as they are not yet harmed by a benefit reduction – can explain why only the benefit sanction itself harms employment stability but not the warning.

The clear *persistence* of negative sanction effects on earnings up to two years after unemploy-

[47] Not that the remainders, i.e. the difference between the sum of the proportions of the Y and 0 group and 100%, are the censored spells. Thus, what appears less often in the ex post sanctioned case are the long, censored durations.

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

1.7 Conclusions

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

In terms of ex post effects, we find that both warnings and actual enforcement of benefit sanctions increase the unemployment exit rate. Whereas warnings do not affect the duration of subsequent employment they have a persistent negative impact on post-unemployment earnings. Enforcement of benefit sanctions reduces the quality of post-unemployment jobs both in terms of job duration as well as in terms of earnings. We also find evidence of benefit sanctions increasing exits out of the labor market. In terms of ex ante effects, we find that stricter monitoring of job search leads to faster exit from unemployment but also reduces post unemployment earnings while leaving employment durations unchanged.

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

Turning to policy options, recall that benefit sanctions in the Swiss system entail full reduction of unemployment benefits. We show that these full reductions in unemployment benefits lead to substantially lower post unemployment earnings. Moreover, we show that increased monitoring is effective in generating incentives to leave unemployment without inflicting a large post unemployment penalty on job seekers. Taken together, these results suggests that an alternative policy could be constructed that preserves search incentives but moderates the post unemployment consequences of benefits sanctions: a system with increased monitoring of search behavior but decreased penalties in case of non-compliance. It is, however, up to future research to quantitatively assess the elasticity of the net effect of sanctions on changes in penalty size[48].

[48]The existing empirical evidence shows different results concerning the elasticity of unemployment exit rates on penalty size. Svarer (2007) found for Denmark that severity matters, Van den Berg et al. (2004) found no such variety for Dutch welfare recipients. Note, moreover, that estimating heterogenous sanction effects by penalty size is a non-trivial exercise: Subgroups by sanction strength are (endogenously) selective, and the decision process on sanction severity is not mechanic (decision leeway of administration) and therefore not fully exogenous either. So, further sources of unobserved heterogeneity would need to be modeled.

References

Abbring, Jaap H., Van den Berg, Gerard J., and Jan C. van Ours (2005). "The Effect of Unemployment Insurance Sanctions on the Transition Rate from Unemployment to Employment." *Economic Journal*, 115, 602-630.

Abbring, Jaap H. and Gerard van den Berg (2003a). "The Non-Parametric Identification of Treatment Effects in Duration Models." *Econometrica,* 71, 1491-1517.

Abbring, Jaap H. and Gerard J. van den Berg (2003b). "The Identifiability of the Mixed Proportional Hazards Competing Risks Model," *Journal of The Royal Statistical Society Series B*, 65(3), 701-710.

Ashenfelter, Orley, Ashmore, David, and Olivier Deschênes (2005). "Do Unemployment Insurance Recipients Actively Seek Work? Randomized Trials in Four U.S. States." *Journal of Econometrics*, 125, 53-75.

Becker, Gary (1968). "Crime and punishment: an economic approach." *Journal of Political Economy*, 76, 169-217.

Black, Dan A., Smith, Jeffrey A., Berger, Mark C., and Brett J. Noel (2003). "Is the Threat of Reemployment Services More Effective than the Services Themselves? Evidence from Random Assignments in the UI system." *American Economic Review*, 93, 1313-1327.

Bloom, Dan and Don Winstead (2002). "Sanctions and Welfare Reform." *Policy Brief No.12*, January 2002, Brookings Institution, Washington DC.

Bonnal, Liliane, Fougère, Denis, and Anne Sérandon (1997). "Evaluating the Impact of French Employment Policies on Individual Labor Market Histories." *Review of Economic Studies*, 64, 683-713.

Boone, Jan and Jan C. van Ours (2006). "Modeling Financial Incentives to Get Unemployed Back to Work", *Journal of Institutional and Theoretical Economics*, 162(2), 227-252.

Boone, Jan, Fredriksson, Peter, Holmlund, Bertil and Jan C. van Ours (2007). "Optimal Unemployment Insurance with Monitoring and Sanctions." *Economic Journal*, 117, 399-421.

Boone, Jan, Sadrieh, Abdolkarim, and Jan C. van Ours (2009). "Experiments on Unemployment Benefit Sanctions and Job Search Behavior." *European Economic Review*, forthcoming.

Card, David, Kluve, Jochen, and Andrea Weber (2009). "Active Labor Market Policy Evaluations: A Meta-Analysis." *IZA Discussion Paper* 4002.

Card, David, Chetty, Ray, and Andrea Weber (2007). "Cash-on-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market." *Quarterly Journal of Economics*, 122(4), 1511-1560.

Cockx, Bart and Matteo Picchio (2008). "The Cost of Early Unemployment Duration.", mimeo, Université Catholique de Louvain.

Donald, Stephen G. and David A. Green (2000). "Differences in Wage Distributions Between Canada and the United States: An Application of a Flexible Estimator of Distribution Functions in the Presence of Covariates." *Review of Economic Studies*, 67, 609-633.

Gaure, Simen, Roed, Knut, and Thao Zhang (2007). "Time and causality: A Monte Carlo assessment of the timing-of-events approach." *Journal of Econometrics* 141(2): 1159-1195.

Gerfin, Michael, and Michael Lechner (2002). "Microeconometric Evaluation of Active Labor Market Policies in Switzerland." *Economic Journal* 112, 854-893.

Gritz, Mark (1993). "The Impact of Training on the Frequency and Duration of Employment." *Journal of Econometrics* 57, 21-51.

Grubb, David (2000). "Eligibility Criteria for Unemployment Benefits." *OECD Economic Studies No. 31*, Paris, OECD.

Ham, John C. and LaLonde, Robert J. (1996). "The Effect of Sample Selection and Initial Conditions in Duration Models: Evidence from Experimental Data on Training," *Econometrica*, 64(1), 175-205.

Heckman, James and Singer, Burton (1984). "A Method for Minimizing the Impact of Distributional Assumptions in Econometric Models for Duration Data," *Econometrica*, 52(2), 271-320.

Hofmann, Barbara (2008). "Work Incentives? Ex Post Effects of Unemployment Insurance Sanctions – Evidence from West Germany." *CESifo Working Paper 2508.*

Jensen, Peter, Svarer Nielsen, Michael and Michael Rosholm (2003). "The Response of Youth Unemployment to Benefits, Incentives, and Sanctions." *European Journal of Political Economy*, 19, 301-316.

Klepinger, Daniel H., Johnson, Terry R., and Jutta M. Joesch (2002). "Effects of Unemployment Insurance Work-Search Requirements: The Maryland Experiment." *Industrial and Labor Relations Review*, 56 (1), 3-22.

Lalive, Rafael, Van Ours, Jan C., and Josef Zweimüller (2005). "The Effect of Benefit Sanctions on the Duration of Unemployment." *Journal of the European Economic Association*, 3 (6), 1386-1417.

Lalive, Rafael (2007). "Unemployment Benefits, Unemployment Duration, and Post-Unemployment Jobs: A Regression Discontinuity Approach." *American Economic Review*, 91(2), 108-112.

Lalive, Rafael, Van Ours, Jan C., and Josef Zweimüller (2008). "The Impact of Active Labor Market Programs on the Duration of Unemployment" *The Economic Journal*, 118, 235-257.

Martin, John P., and David Grubb (2001). "What Works and for Whom: a Review of OECD Countries' Experience with Active Labor Market Policies." *Working Paper*, OECD, Paris.

OECD (1999). "*The Battle against Exclusion: Social Assistance in Canada and Switzerland*." OECD, Paris.

Petrongolo, Barbara (2009). "The Long-Term Effects of Job Search Requirements: Evidence from the UK JSA Reform."*Journal of Public Economics*, 93, 1234-1253.

Schneider, Julia (2008). "The Effect of Unemployment Benefit II Sanctions on Reservation Wages." mimeo, IAB, Nurnberg.

Svarer, Michael (2007). "The Effect of Sanctions on the Job Finding Rate: Evidence from Denmark." *IZA Discussion Paper* 3015.

Van den Berg, Gerard J., Van der Klaauw, Bas, and Van Ours, Jan C. (2004). "Punitive Sanctions and the Transition Rate from Welfare to Work." *Journal of Labor Economics*, 22, 211-241.

Van den Berg, Gerard J. and Johan Vikström (2009). "Monitoring Job Offer Decisions, Punishments, Exit to Work, and Job Quality." *IFAU Working Paper* 2009:18.

Van Ours, Jan C. and Milan Vodopivec (2008). "Does Reducing Unemployment Insurance Generosity reduce Job Match Quality?" *Journal of Public Economics*, 92, 684-695.

Appendices

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

The Swiss data allow us to make a distinction between warnings and enforcement of benefit sanctions. Furthermore, the data contains information about the quality of post-unemployment jobs. To illustrate how benefit sanctions may affect the quality of post-unemployment jobs we extend the benefit sanctions part of the search-matching model of Boone and Van Ours (2006) accordingly.[49] Workers are assumed to be risk-neutral and cannot save; hence they consume all their earnings each period. This assumption rules out the possibility that agents save to insure themselves against the loss of earnings due to unemployment. Once a worker becomes unemployed, he receives an unemployment benefit that is constant over the unemployment spell unless a benefit sanction is imposed in which case the benefits are canceled. Workers have only one instrument of search, their search intensity.[50] Different from Boone and Van Ours (2006) we introduce two sanction "states": the warning state and the enforcement state. Thus there are three types of unemployment: unemployment without benefit sanctions ($u1$), unemployment with a warning ($u2$) and unemployment with sanctions imposed ($u3$). Also different from Boone and Van Ours (2006), to investigate the relationship between benefit sanction and the quality of post-unemployment jobs we introduce two types of jobs: temporary and permanent jobs. So there are two types of employment, permanent ($e1$) and temporary ($e2$). The jobs pay the same wage w and differ only in the job destruction rate $\delta_1 < \delta_2$.[51]

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

The search for the jobs generates flows of job offers, which follow a Poisson process with arrival rate $\mu_1 s_1$ and $\mu_2 s_2$. The arrival rates of job offers consist of two parts, one part (μ_1 and μ_2)

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

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

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

is determined by the state of the labor market i.e. the number of vacancies and unemployed and the other part (s_1 and s_2) is determined by the optimizing behavior of the unemployed worker. Unemployed without a benefit sanction are monitored and they face the risk of receiving a warning if they search less than required. The monitoring intensity is ϕ_1, and the required intensity of search equals λ. Workers will never search more than required: $s_1 + s_2 \leq \lambda$.

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

$$\rho V_{u1} = \max_s\{b - \gamma(s) + \mu_1 s_1(V_{e1} - V_{u1}) + \mu_2 s_2(V_{e2} - V_{u1}) + \phi_1(\lambda - s_1 - s_2)(V_{u2} - V_{u1})\} \quad (1.10)$$

where V_{e1} is the value of being employed with a permanent job, V_{e2} is the value of being employed with a temporary job, V_{u2} is the value of being unemployment with a sanction warning and ρ is the discount rate. The flow value of unemployment without benefit sanctions consists of two parts: the flow of utility during unemployment (utility of benefits minus search costs) and the expected flow of additional earnings after the job is found. The optimal search intensities follow directly from differentiating equation (1.10):

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

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

The Bellman equation for the unemployed workers with a sanction warning:[52]

$$\rho V_{u2} = \max_s\{b - \gamma(s) + \mu_1 s_1(V_{e1} - V_{u2}) + \mu_2 s_2(V_{e2} - V_{u2}) + \phi_2(\lambda - s_1 - s_2)(V_{u3} - V_{u2})\} \quad (1.11)$$

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

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

differentiating equation (1.11):

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

Note that the differences $s_{21}^* - s_{11}^*$ and $s_{22}^* - s_{21}^*$ represent the ex post effect of a warning. Finally, the Bellman equation for the unemployed workers with a sanction enforced:

$$\rho V_{u3} = \max_s\{-\gamma(s) + \mu_1 s_1(V_{e1} - V_{u3}) + \mu_2 s_2(V_{e2} - V_{u3})\} \tag{1.12}$$

where the penalty imposed is equal to the benefits. We assume that unemployed with a benefit sanction are no longer monitored because their benefits are equal to zero. Once again, the optimal search intensities can be found by differentiating equation (1.12):

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

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

$$\rho V_{e1} = w + \delta_1(V_{u1} - V_{e1}) \tag{1.13}$$
$$\rho V_{e2} = w + \delta_2(V_{u1} - V_{e2}) \tag{1.14}$$

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

$$V_{e1} > V_{u1} > V_{u2} > V_{e2} > V_{u3} \tag{1.15}$$

workers will initially only search for jobs of type 1. Receiving a warning will induce them to search with a higher intensity for jobs of type 1, but they will still not look for jobs of type 2. Only once they get a benefit sanction imposed will they start looking for jobs of type 2. Then, their average expected job duration will be lower because now they start accepting temporary jobs.[53]

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

B. Simulations

B1. Ex post Effects

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

$$f_{y24}^{D_w,D_s}(y|x,v) = \theta_{y24}^{D_w,D_s}(y|x,v) S_{y24}^{D_w,D_s}(y|x,v).$$

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

$$E(y|x,v,D_w,D_s) = \int_0^{199} y\ f_{y24}^{D_w,D_s}(y|x,v)dy + \left[1 - \int_0^{199} f_{y24}^{D_w,D_s}(y|x,v)dy\right] \cdot 200 \tag{1.16}$$

whereby y is earnings in 1000 CHF. The second term of the equation (1.16) above accounts for the high earnings censored at 200,000 CHF. In the treated case, i.e. with both sanction warning and enforcement imposed, we set $D_w = 1$ and $D_s = 1$. This amounts to increasing the earnings hazard in (1.16) by the estimated treatment effects δ_{wy24t} and δ_{sy24t} over the whole support. In the non-treated counterfactual, equation (1.16) is evaluated at $D_w = 0$ and $D_s = 0$. The difference between these two mean earnings results in the ex post effect. Note that we simulate first conditional on unobserved heterogeneity and then we integrate unobserved heterogeneity out.

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

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

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

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

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

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

$$E(t_y|x,v,D_w = 1, D_s(t), t_w < T_y < 720) = \int_{t_w}^{720} t \frac{f_y^{1,D_s(t)}(t|x,v)}{\int_{t_w}^{720} f_y^{1,D_s(t)}(t|x,v)dt} dt \tag{1.17}$$

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

$$E(t_y|x,v,D_w = 0, D_s = 0, t_w < T_y < 720) = \int_{t_w}^{720} t \frac{f_y^{0,0}(t|x,v)}{\int_{t_w}^{720} f_y^{0,0}(t|x,v)dt} dt \tag{1.18}$$

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

The ex post effect of benefit sanctions is the difference between actual mean duration (1.17) and counterfactual mean duration (1.18). Note again that we simulate first conditional on unobserved heterogeneity and then we integrate unobserved heterogeneity out.

B2. Simulating the ex ante Effect

We simulate the ex ante effect on the post-unemployment outcome by focusing on everyone who generated positive earnings over 24 months after unemployment exit. We set their sanction statuses D_w and D_s to zero. Now, let $\theta_{y24}^{D_w,D_s,\alpha_{e24y}}(y|x,v)$ denote the earnings hazard, depending on sanction warning status D_w, sanction enforcement D_s status, and the vector of PES dummies in the outcome, α_{e24y}. The counterfactual of expected earnings under actual warning intensity and outcome dummies, implying $\alpha_{e24y}^0 = \hat{\alpha}_{e24y}$, is described by equation (1.16) above, now evaluated for the whole $y24 > 0$ group.

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

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

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

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

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

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

$$E(t_y|x, v, D_w = 0, D_s = 0, \alpha^0_y, T_y < 720) = \int_0^{720} t \frac{f_y^{0,0,\alpha^0_y}(t|x, v)}{\int_0^{720} f_y^{0,0,\alpha^0_y}(t|x, v)dt} \tag{1.19}$$

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

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

Expected duration with the increased warning regime is

$$E(t_y|x, v, D_w = 0, D_s = 0, \alpha^1_y, T_y < 720) = \int_0^{720} t \frac{f_y^{0,0,\alpha^1_y}(t|x, v)}{\int_0^{720} f_y^{0,0,\alpha^1_y}(t|x, v)dt} \tag{1.20}$$

The ex ante effect on unemployment duration with exit in employment consists in the difference between the equations (1.20) and (1.19). The respective effect on unemployment duration that ends in medium run non-employment is calculated analogously, replacing f_y by f_0.

C. Likelihood contributions

Due to the fact that the SSR data we use are of monthly precision, we model the respective hazards in a discrete manner. The discrete hazards for t_o (with $o = \{m, nm\}$) can be represented as the difference between two survivor functions of two consecutive months, be it $t_o - 1$ and t_o, divided by the survivor of the earlier month.[54] Thus, the discrete-time hazard is the probability of failure in the interval between two consecutive months, conditioned on the probability of surviving to at least the earlier month.

The corresponding likelihood contribution consists therefore in

$$S_o(t_o - 1|x, r, p, D_{wo}, D_{so}, t_u, v_o) - S_o(t_o|x, r, p, D_{wo}, D_{so}, t_u, v_o) \tag{1.21}$$

if the observation is not censored and in $S_o(t_o|x, r, p, D_{wo}, D_{so}, t_u, v_o)$ if censored. The survivors[55] are modeled in the same way as described in the last subsection. In the post-unemployment period, the treatment effect results in a constant upward or downward shift of the respective hazard.

D. Additional Tables

Table 1.7: The effect of benefit sanctions on exit behavior and subsequent non-/employment duration

⟶ *see next page*

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

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

		Model I	
(*Coeff./Transf.*)	Coeff.	z-value	Transf.
Effect on exit from employment (M)			
warning (δ_{wm}/in %)	0.018	0.34	0.019
enforcement (δ_{sm}/in %)	0.140	2.35	0.150
Effect on exit from non-empl. (NM)			
warning (δ_{wnm}/in %)	0.146	1.14	0.157
enforcement (δ_{snm}/in %)	0.267	1.97	0.307
Effect on exit UE → E			
warning (δ_{we}/in %)	0.147	3.39	0.159
enforcement (δ_{se}/in %)	0.148	3.07	0.160
Effect on exit UE → NE			
warning (δ_{wne}/in %)	0.689	5.05	0.992
enforcement (δ_{sne}/in %)	0.513	4.05	0.670
Transition rate: exit from M			
$\lambda_{ma,1}/exp(u_{ma})$	-1.962	-3.56	3.832
$\lambda_{mb,1}/exp(u_{mb})$	-4.557	-5.27	0.286
Transition rate: exit from NM			
$\lambda_{nma,1}/exp(u_{nma})$	-0.367	-0.23	2.932
$\lambda_{nmb,1}/exp(u_{nmb})$	2.022	1.28	31.972
Transition rate: exit to E			
$\lambda_{ea,1}/exp(u_{ea})$	-5.321	-13.48	0.183
$\lambda_{eb,1}/exp(u_{eb})$	-6.478	-15.70	0.058
Transition rate: exit to NE			
$\lambda_{nea,1}/exp(u_{nea})$	-2.790	-2.69	0.052
$\lambda_{neb,1}/exp(u_{neb})$	-5.342	-5.08	0.004
Transition rate: warning			
$\lambda_{wa,1}/exp(u_{wa})$	-5.151	-4.77	0.181
$\lambda_{wb,1}/exp(u_{wb})$	-9.373	-8.54	0.003
Transition rate: enforcement			
$\lambda_{sa,1}/exp(u_{sa})$	-3.382	-2.07	0.447
$\lambda_{sb,1}/exp(u_{sb})$	-100	–	0
Probabilities			
a_1/p_1	2.937	2.87	0.088
a_2/p_2	1.494	0.95	0.021
a_3/p_3	1.334	1.12	0.018
a_5/p_5	3.645	3.72	0.178
a_6/p_6	1.927	1.69	0.032
a_7/p_7	1.481	1.32	0.020
a_9/p_9	2.026	0.72	0.035
a_{11}/p_{11}	3.650	3.42	0.179
a_{13}/p_{13}	2.656	2.40	0.066
a_{17}/p_{17}	2.168	2.10	0.041
a_{18}/ p_{18}	0.467	0.33	0.007
a_{22}/ p_{22}	0.786	0.40	0.010
a_{24}/ p_{24}	-0.008	-0.01	0.005
a_{27}/ p_{27}	3.287	3.47	0.124
a_{34}/ p_{34}	1.218	0.63	0.016
a_{37}/ p_{37}	2.135	2.02	0.039
a_{38}/ p_{38}	1.983	2.06	0.034
a_{45}/ p_{45}	2.887	2.91	0.083
a_{64}/ p_{64}	–	–	0.005
Unobserved heterogeneity		Yes	
Control variables		Yes	
Control for state dependence		Yes	
PES dummies		Yes	
-Log-Likelihood		255064	
BIC		259158	
N		23961	

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

Table 1.8: The effect of benefit sanctions on earnings: over 1 vs. 24 months after unemployment exit; E (exit to employment) group

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

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

Table 1.9: The effect of benefit sanctions on earnings over 24 months: E group (excluding temporary and permanent labor force exits) vs. total population with positive earnings (excluding only permanent labor force exits)

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

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

E. Observables

In the following table we provide means (or medians in the case of durations) for all the variables used in the estimated Models I to III (see section 1.5 for a description of the models). The means are given for the total sample as well as for the treatment subgroups: the non-sanctioned (non-sanc), those who were warned only (warn only), and those who were warned and got a benefit sanction imposed (warn&enf). The variables below, except the last paragraph, are the control variables which are present in all the Models I to III. These control variables feature as well endogenous state dependence variables (second last paragraph). Finally, the last paragraph gives a descriptive insight in how outcome levels are different depending on in which treatment subgroup an individual is. The estimated coefficients for the control variables in Models I to III are not reported in this paper due to space reasons. They are available from the authors upon request.

Table 1.10: Observable characteristics: Means by sanction status group

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

continued on next page

continued from previous page

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

continued on next page

continued from previous page

	total	non-sanc	warn only	warn&enf
FRC1	0.008	0.008	0.006	0.008
FRD1	0.010	0.011	0.008	0.005
FRF1	0.011	0.013	0.005	0.004
FRK1	0.005	0.005	0.004	0.004
FRL1	0.031	0.032	0.027	0.021
FRM1	0.019	0.017	0.039	0.011
FRM4	0.002	0.002	0.004	0.005
FRN1	0.009	0.011	0.005	0.002
GRD1	0.042	0.039	0.023	0.093
GRE1	0.009	0.009	0.008	0.018
GRF1	0.009	0.008	0.003	0.024
GRG1	0.005	0.006	0.001	0.003
GRH1	0.010	0.010	0.005	0.012
GRI1	0.015	0.015	0.010	0.022
SOA2	0.016	0.015	0.020	0.024
SOA3	0.022	0.021	0.026	0.029
SOA4	0.009	0.010	0.006	0.006
SOA5	0.016	0.015	0.019	0.018
SOA6	0.009	0.011	0.002	0.007
SOA7	0.005	0.003	0.007	0.027
SOA8	0.003	0.003	0.002	0[4]
SOA9	0.006	0.005	0.006	0.007
SOAA	0.010	0.011	0.006	0.005
SOAB	0.018	0.019	0.011	0.020
URA2	0.008	0.007	0.011	0.008
VDB1	0.091	0.096	0.066	0.073
VDB2	0.007	0.007	0.005	0.003
VDC1	0.008	0.008	0.008	0.004
VDD1	0.030	0.028	0.034	0.038
VDD4	0.003	0.002	0.005	0.006
VDE1	0.013	0.015	0.001	0.011
VDH1	0.024	0.025	0.007	0.039
VDJ1	0.022	0.025	0.009	0.005
VDL1	0.040	0.040	0.039	0.050
VDM1	0.015	0.013	0.019	0.020
VDN1	0.005	0.006	0.001	0.002
VDP1	0.023	0.026	0.012	0.005
VDQ1	0.021	0.019	0.011	0.053
VDT1	0.009	0.009	0.009	0.007
VDU1	0.027	0.027	0.023	0.031
VDV1	0.033	0.034	0.035	0.020
VDW1	0.009	0.010	0.008	0.003
VDZ1	0.006	0.006	0.007	0.007
VSL1	0.026	0.020	0.050	0.050
VSM1	0.052	0.051	0.077	0.036
VSM2	0.004	0.004	0.003	0.000
VSN1	0.053	0.047	0.113	0.029
VSO1	0.021	0.024	0.004	0.017
VSO2	0.045	0.053	0.003	0.032
VSP1	0.080	0.071	0.164	0.055

Endogenous state dependence: duration of past stage (unemployment)[5]

continued on next page

continued from previous page

	total	non-sanc	warn only	warn&enf
Log unemployment duration (median, days)	5.10	5.00	5.38	5.73
Log unemployment duration, squared (median, days)	26.01	24.97	28.99	32.87
Log unemployment duration, 3rd power (median, days)	132.6	124.8	156.1	188.5
Log unemployment duration, 4th power (median, days)	676.4	623.6	840.6	1080.5
Log unemployment duration, 5th power (median, days)	3449.8	3116.3	4526.1	6195.0
Log unemployment duration, 6th power (median, days)	17593.5	15572.8	24370.8	35517.9
Outcomes (dependent variables for Models I to III)[6]				
Unemployment duration	164	148	218	309
Duration first spell after ue: employment (E: 19149 obs)	25	26	19	22
Duration first spell after ue: nonemployment (NE: 2985 obs)	11	10	16	12
Earnings in the first month after ue exit (E: 19149 obs)	89826.85	92364.93	79733.43	75292.16
Earnings over 24 months after ue exit (E: 19149 obs)	3992.41	4087.35	3611.41	3453.90
Earnings over 24 months after ue exit (Y: 21012 obs)	85954.90	88855.57	75708.11	69206.41
Observations	**23961**	**19228**	**2714**	**2019**

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

Chapter 2

How to Improve Labor Market Programs for Older Job-Seekers? A Field Experiment

Abstract[1]: Older job seekers often face a higher longterm unemployment risk because their employability decreased over time. I evaluate an new social experiment which implements a counseling and coaching policy for older job seekers in Switzerland. To avoid the negative duration effect, which is typically generated by this type of training program, the policy design follows three principles: earlier than normal, highly intense and clearly targeted. The evaluation is based on a unique dataset that merges register data with repeated surveys. The new policy design turns out to be successful in several respects: The program does not increase, but slightly (insignificantly) decrease unemployment duration. At later stages of unemployment, a more and more positive effect on the exit rate to job is visible. This results in the proportion of job finders being 9 percentage points higher in the treatment group. The quality of found jobs does not diminish: The realised salaries of the treated are at the same level as the control group's. Remarkably, the new program increases employment stability in the 540 days after unemployment exit. This saves 23 days of future unemployment, which more than fully pays the program cost.

JEL Classification: J64, J65, J68, J14

Keywords: Social experiment, labor market policy evaluation, training, dynamic treatment effects, duration model, older workers, job search behavior, post-unemployment outcomes.

[1] I would like to thank Rafael Lalive, Jan van Ours, Lorenz Goette, Michael Rosholm, Gerard van den Berg, Josef Zweimüller, Jaap Abbring, Bas van der Klaauw, Geert Ridder, Arthur van Soest, Marco Caliendo, Brian Krogh Graversen, Konstantinos Tatsiramos, Matteo Picchio, Anna Giraldo and Olivier Deschênes for their valuable comments. Moreover, I thank the seminar participants at Tilburg University, Tinbergen Institute, Amsterdam, University of Lausanne and University of Zurich, the workshop participants in Berlin as well as the session participants at the EEA meeting in Glasgow and the IZA/IFAU conference in Uppsala for their valuable comments. I am grateful to Raphael Weisz and the Office of Economic Affairs and Labour (AWA) of Canton of Aargau, Switzerland, for providing the data and further support. I would like to thank as well the project teams in the regional PES.
A major part of the work for this paper I carried out during my research stay at Tilburg University. Financial support from the Swiss National Science Foundation (project no. PBLAP1-127652/1) is gratefully acknowledged.
Email: patrick.arni@unil.ch

2.1 Introduction

The issue of *long-term unemployment (LTU)* – i.e. unemployment that exceeds the duration of one year – gains in importance in the economic policy debate. As a lagged outcome of the recent economic crisis LTU reached, towards the end of 2010, a long-run high in several big economies like the US, UK and France and in small economies like Switzerland and Austria[2]. As of 2008, the European OECD countries were confronted with a LTU rate of 36.8%. Heterogeneity is big: the national rates range from 6.0% in Norway to 53.4% in Germany, with countries like the UK (25.5%) and Switzerland (34.3%) being in the middle. In the US, the proportion of individuals in LTU amounts to 10.6% (OECD 2009).[3] Long-term unemployment is considered as being especially harmful to the labor market prospects of concerned individuals. A longer absence from labor market implies most often a remarkable loss in human capital, employability and self-esteem. As a consequence, *avoiding* long-term unemployment – through reduction of LTU *risk* – is a prominent issue for labor market policy.

What are the key drivers of long-term unemployment? A crucial one is *advanced working age*, going often together with *decreasing employability*. The strong increase of the LTU rate as a function of age can be found in many national labor statistics (see, e.g., the ONS Bulletin for the UK). For the region under consideration in this paper, northern Switzerland, a highly age-related pattern arises as well. The proportion of individuals in unemployment insurance who face LTU climbs from 18.4% in the age group 30–34 to 39.0% in the age group 55–59 (AMOSA 2007), as Figure 2.2 reports. Thus, this strong age-relatedness of long-term unemployment calls for active labor market policy (ALMP) strategies that explicitly deal with the reduction of unemployment risk for *older workers*. The ongoing demographic change in the labor force will further improve the importance of this focus.

The *main contribution* of this paper to the literature is that it reports the results of a ***new unemployment insurance field experiment that implements a novel ALMP for older job seekers***. Social experiments are still rare in the evaluation literature on incentive policies in unemployment insurance (UI), mainly in Europe. The small amount of recent papers comprises studies on an experiment in The Netherlands (Van den Berg et al 2006), one in Denmark (Graversen et al 2008 and 2009, Rosholm 2008, Rosholm et al 2010) and one in Sweden (Hägglund 2006 and 2009). In the US, a wave of related social experiments was performed in the early nineties (see Meyer 1995 and Black et al 2003). The crucial advantage of randomised trials is that they allow for a cleaner evaluation design – since randomisation avoids problems of unobserved heterogeneity

[2]See respective national unemployment statistics (of BLS, ONS, DARES, Seco and AMS). An exception is Germany: long-term unemployment dropped, according to BA, by 5% from February 2010 to February 2011 – though, from a remarkably high level (see OECD figures above).

[3]It is well conceivable that the different generosity of the unemployment insurance benefits may play a role in determining the heterogeneity of these figures. Though, other factors are of importance as well, as the low percentages in Scandinavian countries demonstrate.

and endogenous selection. As a consequence, e.g. the recent meta-study on European ALMP by Kluve et al (2007) concludes by asking for more randomised trials in the field. Moreover, Van den Berg et al. (2006) find as a methodological conclusion that evaluation results based on social experiments are mutually consistent to a very high degree, which compares favorably to the literature based on nonexperimental data.

The experimentally evaluated new ALMP strategy is non-standard in several respects. First, the novel policy explicitly focusses on the mentioned risk group of individuals of *age 45+ and lower employability*. Interestingly, literature on the econometric evaluation of labor market policies *targeted* on older job seekers is largely missing so far. This is surprising since there is some literature on evaluation of non-targeted programs which concludes that effectiveness of respective policies could be improved by targeting them on those individuals who are most at risk (e.g. Huber et al. 2009 on German welfare-to-work programs).

Second, the new ALMP strategy differs from standard policy approaches as the intervention happens *very early* and at a *high intensity*. It features a fixed treatment plan which combines individualised coaching (in small groups) with high-frequency counseling. The coaching program starts already after 50 days of unemployment (median), intensified counseling by the caseworker takes place every second week, during the first four months of unemployment. This early intervention strategy is supposed to increase search effectiveness and to avoid long lock-in durations in the period of highest chances of job finding. This period is typically between the months 3 to 6 of unemployment, as Figure 2.1 and empirical studies like AMOSA (2007) show for the Swiss case.

Third, the new policy differs from the mainstream approaches in its focus on *investing time* into the treated individuals. Thus, unlike most of the recent ALMP strategies which aim at reaching (short-term) "activation" mainly through increased control and through the threat effect of programs (see e.g. Rosholm and Svarer 2008, Hägglund 2006, Graversen et al 2009), the new policy allows for additional time per individual job seeker which is invested into the development of labor market skills and improved search strategies. Note that approaches like the mentioned, which operate predominantly through deterrence from participation in (unpopular) programs, do not seem suitable for the risk group targeted here. In the case of older job seekers with lower employability participation in a *supportive program* is explicitly aimed. Thus, the challenge is to design and implement a supportive policy which avoids the typical lock-in effect, known from human capital training programs (Card et al 2009; Gerfin et al. 2002 and Lalive et al. 2008 for Switzerland). Early and highly intense intervention allows to keep the lock-in effect low – as this evaluation will show – while still allowing the investment of more time per individual.

So, to wrap up, the policy design aim of this new ALMP is to *combine* the effective policy elements of *monitoring* and *counseling* with a highly intense and *targeted program to train employability*. This design follows the insights gained in the program evaluation literature of the fifteen years. The latter shows that not many types of ALMP programs can be considered as being effective in terms of bringing unemployed individuals quickly back to work. For example, training and (public) employment programs use to show a zero or negative effect (Card et al. 2009) – mainly driven by lock-in problems and in the latter case as well by a certain stigmatisation. Recent studies on Swiss ALMP find comparable non-positive effects for these kinds of programs (Gerfin et al. 2002, Lalive et al. 2008). Higher effectiveness is normally found for the group of (often combined) measures which entails job search assistance, monitoring and sanctions. The threat and the use of benefit sanctions results in a considerable reduction of unemployment duration (Lalive et al. 2005, Abbring et al. 2005), though there is a remarkably big negative effect on post-unemployment earnings and job stability (Arni et al. 2009, Van den Berg et al. 2009). Monitoring seems to be effective if it is combined with some legal pressure (sanctions) or with an activation or job search assistance program, as the three recent social experiments in Denmark,Sweden and the Netherlands show (Graversen et al. 2008, Hägglund 2009, Van den Berg et al. 2006). The literature on older unemployment insurance experiments in the US finds as well some evidence for the effectiveness of job search assistance and monitoring (Ashenfelter et al. 2005, Meyer 1995).

On methodological grounds, the distinctive feature of this paper, as compared to most of the existing literature, is the combination of the experimental setup with *strict ex-ante timing of the treatment plan.* At t_0, the first interview at the public employment service (PES) office, full information on the future treatment steps is provided: Thus, job seekers know about the exact start and end date of the upcoming coaching program and about the bi-weekly rhythm of counseling. This allows a clean separation of treatment periods and therefore precise identification of sub-treatment-effects: the pre-coaching – or (gross) anticipation –, during-coaching and post-coaching effects. Ex-ante timing, randomisation and full information at t_0 are the basic conditions that are necessary to identify anticipation effects (from t_0 on) without further substantial econometric assumptions, as Abbring and Van den Berg (2005) show. The setup of the here evaluated program fulfills all three conditions – whereas the existing recent literature on estimating anticipatory behavior of job seekers before program entry (Crépon et al 2010, Rosholm and Svarer 2008, Black et al 2003[4]) needs to impose further structure or assumptions.

The data used for the evaluation of this field experiment are very rich and cover a *long observation period, including post-unemployment.* From the time of inflow into unemployment (December 2007 to December 2008), individuals have been observed, by means of rich register data, throughout the whole unemployment spell as well as *the 1.5 years after unemployment exit.* In addition, a *linked survey* provides data about salaries and related information in the first job after exit. For this type of policy intervention it is especially important to consider the longer run policy outcomes. It is conceivable that it takes some time until the impacts of employability

[4]Black et al (2003) rely their effect identification on a "tie-breaking experiment" where randomisation, due to capacity constraints, was performed in a pre-profiled subgroup. Note that in this case randomisation happened after t_0 (which implies some selection issues, see Abbring and Van den Berg 2005), and the times of randomisation and information (via letter) were different.

training and optimisation and reorientation of job search behavior are "digested" (assimilated) by the job seekers and finally translate into positive labor market outcomes, i.e. job finding. Unlike pressure-oriented restrictive policies like sanctions and threats, supportive policies imply learning processes which forcefully consume some time. And indeed I find in the evaluation of this new policy that its positive main impacts – increased job finding proportion and stability of subsequent employment – materialize in the later stages of unemployment and in the period thereafter.

The paper is organised as follows. In the next section, I will outline the different aspects of the performed social experiment: its treatment plan, institutional background, implementation and potential effects (from the viewpoint of job search theory); finally, the used data are described. Section 2.3 provides a nonparametric analysis of the main impacts of the intervention. This section shows what can be learned by the pure use of the experiment (by means of means comparisons and survivor analysis), without imposing any structure beyond. In section 2.4, I set up a duration model framework which allows to identify sub-treatment-effects and effects on post-unemployment durations. Then, n Section 2.5, I proceed to the discussion of the results of this model, being guided by four questions: (i) How do the treatment effects of different treatment periods look like? (ii) Based on these results, how can policy implementation be optimised? (iii) What about the quality of post-unemployment outcomes of the new policy (in terms of employment stability and salaries)? (iv) Does the program pay off for the unemployment insurance? Section 2.6 concludes.

2.2 The Experiment

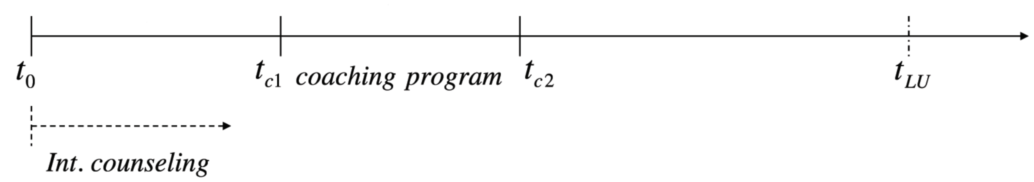
In this section, I will first describe the interventions that constitute the treatment plan. Then, I will shortly outline the institutional background: the Swiss unemployment insurance system and some facts about the (long-term) unemployment situation in the region of the project. Next, the specific implementation of the experiment (sampling and randomisation procedure) will be presented. Then, I discuss potential effects of the treatments in the context of job search theory. Finally, the data – a combination of register and survey sources – are presented.

2.2.1 The Treatment Plan

The treatment plan consists of two main measures and a specific timing of the interventions. The two main measures are high-frequency counseling by the caseworker at the public employment service (PES) office and an intense external coaching program performed in small groups.

The *timing* of the interventions is highly relevant – mainly for two reasons. On one hand, *early intervention* is crucial in order to fight long-term unemployment (see introduction). If the (intense) interventions start too late, the risk is high that the concerned job seeker is already on a vicious circle of being too long away from the labor market and therefore facing a decrease in employability – especially in the case of older job seekers who are often confronted with decreasing labor market attractiveness anyway. On the other hand, to impose a *clearly structured treatment order* for which the *timing is fixed ex-ante* is crucial for the identification of treatment effects. The fact that order and timing of the treatments are known from start on – which is the case here – makes this part of the treatment plan *exogenous*. I will use this fact when discussing econometric modeling and identification, see section 2.4.

The timing of the treatment plan can be visualised in the following way:



High-frequency counseling starts right from the beginning of the unemployment insurance spell, from the first interview on. Job seekers meet with their caseworkers every second week – thus in a double frequency compared to the normal monthly rhythm of interviews. Counseling goes on in high frequency for the treated during the first four months of the unemployment spell. Then, the frequency goes back to normal (monthly rhythm).

The basic idea behind increasing counseling frequency is that the caseworkers have *more* *time* available for the respective job seeker (see also introduction). This has as an effect that the job seeker is better known to the caseworker: counseling can therefore be more *targeted* and individualised. Moreover, more time remains in the interviews to go beyond administrative and application monitoring tasks; this time can be used to coach the job seeker in job search strategies. Note, however, that this intensified support implies as well a certain tightening of monitoring (higher frequency of control).

The *coaching program*, the second main measure, starts in median after 50 days (48.5 days for those who really participate, 52 days until potential coaching entry for the others[5]). Thus, the principle of early intervention is taken literally. The coaching was performed in small groups of 10-15 persons. An external, private-sector coaching firm was mandated to perform the coaching program. One coach ran all the coaching programs which took place during the year of inflow (December 2007 to December 2008; last program started in January 2009). The content and strategy of the coaching focused on three points: (i) increasing the self-marketing skills for the labor market; (ii) improving self-assessment which should result in a better and more realistic self-profiling, which helps again for successful self-marketing and efficiency of job search; (iii) optimisation of search strategy with a particular focus on assessing the potential of reorientations (towards other industries, regions, working times, search channels etc.). Thus, the coaching program features a strong element of human capital development (in terms of core competences and employability). The coaching program lasts 54 or 70 days (due to Christmas/New Year break). Job seekers were 3 to 4 full days per week in the program; in addition, homework had to be done as well. So the coaching program is highly intense and features a high work load (which results in a restriction of job search time, see section 2.2.4 on potential effects).

The *control group* followed the 'status quo', i.e. was in the normal procedures and standard programs. This means in particular that they were interviewed by caseworkers only monthly and entry into active labor market programs normally started clearly later since the status quo doesn't feature an early intervention principle. A typical ALMP trajectory in the control group starts with participating in a short job search assistance sequence of 3 to 7 working days, roughly after 3 to 4 months of unemployment. Thus, this short program is normally the only ALMP activity in the control group that takes place during the period of intense intervention in the treatment group (first 4 months). After the four months (end of treatment) both groups follow status quo procedures (featuring monthly interviews and further ALMPs, dependent on individual needs). It is important to note that the individuals of the control group had no possibility to enter the coaching program. This newly designed program was exclusively open and assigned to the

[5]Note that, due to the fact that the timing of the measures was fixed ex-ante, I can identify the *potential coaching entry date* for every person in the project, i.e. also for coaching non-participants and for the control group. The series of dates for coaching program starts was fixed with the coaching program provider before project start. Approximatively every 1.5th month a new coaching programs started; there were 9 in total over the year of inflow. The algorithm for identifying the potential coaching entry date is: next program start date which is $\geq$ (availability date + 5 days).

treatment group. As the treated, the control group was surveyed as well.

2.2.2 Institutional Background

This social experiment for individuals aged 45+ was performed in the frame of the rules of the Swiss unemployment insurance (UI). The maximum duration of unemployment benefits in the Swiss UI system is 1.5 years (400 days) for individuals who meet the eligibility requirements. The two requirements are (i) that they must have paid unemployment insurance taxes for at least 12 months in the two years prior to entering registered unemployment, and (ii) that they must be 'employable' (i.e. fulfill the requirements of a regular job). After this period of two years or in the case of non-employability the unemployed have to rely on social assistance. From the 55th birthday on, job seekers profit of a benefit duration which is prolonged by about half a year (120 working days). Beyond the age of 61, benefit rights get extended by another 120 days.

The marginal replacement ratio is 80% for job seekers with previous monthly income up to CHF 3797 (about 2550 €). For income between 3797 CHF and 4340 CHF (2900) the replacement ratio linearly falls to 70%. For individuals with income beyond 4340 CHF the ratio is 70%, whereby the insured income is capped at 10500 CHF (7000 €). For job seekers with dependent children, the marginal replacement ratio is always 80% (up to the same maximal insured income cap). Job seekers have to pay all income and social insurance taxes except for the unemployment insurance contribution.

It is important to note that all the assignments to active labor market policy programs and the interview appointments – i.e. the described treatment plan of this experiment – are compulsory for job seekers[6]. If they do not comply to these rules, they risk to be sanctioned (as well if they refuse suitable job offers or do not provide the amount of applications demanded by the caseworker). Sanctioning is comparably frequent in Switzerland (about every sixth job seeker is sanctioned) and implies benefit reductions of 100% during 1-60 days, for details see Arni et al. (2009). This strict sanctioning regime results in high compliance with the rules. This is the case as well here, see section 2.3.1 for details.

The typical *unemployment exit rate path* for the case of Switzerland shows a similar shape as in most European countries. In an early stage, up to 4 to 5 months, the (monthly) exit rate rises pretty sharply – in the case of the sample of this experiment it tops at 18%, see Figure 2.1. Thereafter, the exit hazard goes down remarkably and remains on a level of 6 to 12%. In the last months before benefit exhaustion (beyond the time period of Figure 2.1 and this project) it typically rises sharply to levels comparable to the first peak.

Long-term unemployment (LTU) incidence is highly age-dependent. For the region under

[6]During ALMPs all the standard duties (job search effort, interviews at PES) and rights (benefits) remain. In practice, caseworkers normally demand a slightly smaller number of applications per month than during periods without ALMP. This potentially supports the lock-in effect.

Figure 2.1: Unemployment exit hazard

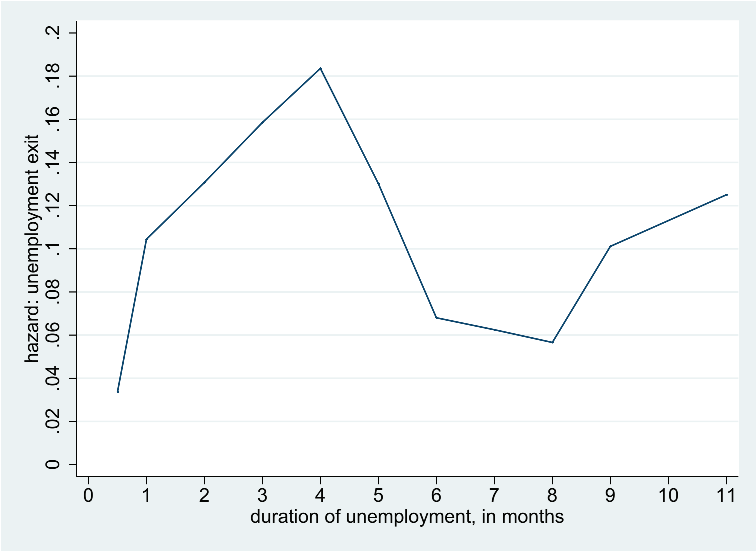
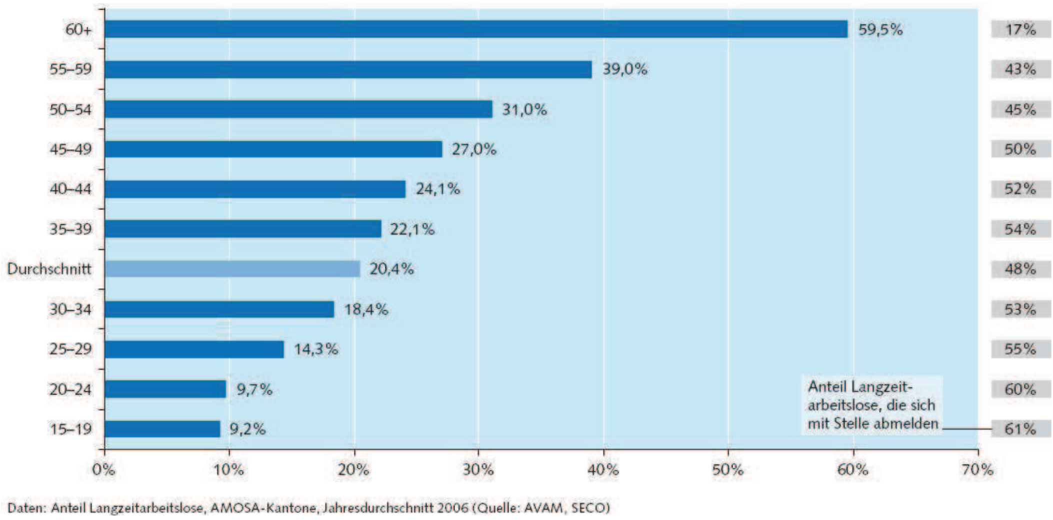


Figure 2.2: Incidence of long-term unemployment by age groups



Note: The bars represent the proportion of long-term unemployed (1 year or more) individuals among the registered unemployed of the respective age category. The figure to the right reports the age-related proportions of the long-term unemployed who deregister from unemployment insurance due to having found a job.
Source: AMOSA 2007.

consideration, Figure 2.2 shows this strong pattern in terms of proportion of LU in the unemployed population of a certain age category. Figure 2.2 (AMOSA 2007) reveals that this proportion amounts to 18.4% for individuals aged 30-34 – and increases up to 39.0% for individuals aged 55-59. Note that the last figure may be affected by the above-mentioned fact that job seekers of age 55+ and 61+ receive a benefit duration extension. The precentage numbers to the right of Figure 2.2 represent the age-related proportions of the long-term unemployed who deregister from unemployment insurance due to having found a job. This percentage remarkably decreases from age 45 on, from around 50% to less than 30% beyond age of 60. Figure 2.2 clearly shows that individuals of age 45+ face a markedly increased risk of long-term unemployment.

2.2.3 Implementation of the Experiment

This experimental project was performed in two PES offices in the Canton of Aargau in north-western Switzerland. The PES belong to a quite urbanised region in the agglomeration of Zurich (about 45 minutes of commuting distance to the centre of the city). So, the region belongs to the "Greater Zurich Area" which features the biggest and economically most productive labor market in Switzerland (population: 3.7 million). Thus, given the relative size of the experiment compared to the size of the labor market, general equilibrium effects of the experimental intervention can be excluded. The treatment consisted in the two main measures and the timing strategy which are described in the treatment plan section 2.2.1. The members of the control group followed the status quo procedures.

Job seekers who were flowing into the two PES between December 2007 and December 2008 and met the participation eligibility conditions *were randomly assigned to treatment and control group at time* t_0, i.e. at registration before the first interview.

Thus, the assignment procedure, run separately for each of the two PES, consisted in three steps: First, the complete inflow of the respective PES was filtered with respect to the *eligibility conditions:* Age 45+, employability level medium or low, only full-time or part-time unemployed above 50%, enough (language) skills to follow the coaching, no top management and no job seekers who have found a longer-term temporary subsidised job (longer than a couple of days). Second, the remaining individuals were assigned to the caseworker pool. 16 caseworkers were involved in the project, whereby 10 bore the main load of cases. The *assignment mechanism* follows a fixed rule: assignment by occupation. It is therefore *exogenous* to the treatment (caseworkers took, thus, automatically cases in the treatment and the control group). Note, moreover, that caseworker and PES fixed effects will be taken into account in the estimations.

As a third step, the cases were *randomly assigned* to the treatment group (60%) and the control group (40%)[7], by use of a randomised list. Like that, the final sample amounts to *327 individuals* with 186/141 in the treatment/control group.

It is important to know which *information* was available for the treatment and control group

[7] In the first quarter of 2007, the random assignment ratio was 50%–50%. As a consequence of good economic conditions, inflow was lower than expected. We therefore decided to switch to a 60%–40% assignment rule. This explains why the treatment-control ration reported in the descriptive analysis in section 2.3.1 is in-between the two rules. Note that this switch has no impact on the quality of randomisation.

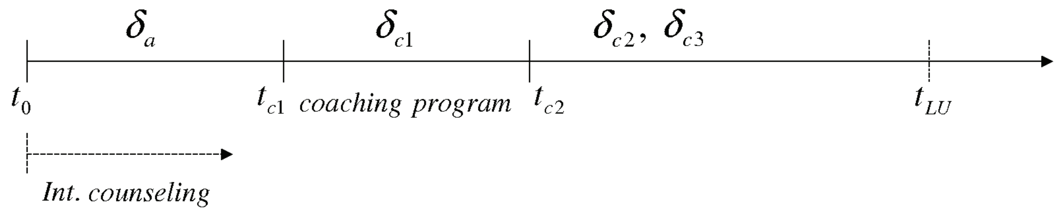
at time t_0. In their first interview with the caseworker, the job seekers of both groups were informed in written form that they participate in a project for "quality control". This was necessary since both groups had to fill out repeated surveys over the duration of their unemployment spell (see section 2.2.5). On the other hand, the caseworkers were not allowed to use the terms 'long-term unemployment (risk group)' and 'randomisation'. The former was to avoid stigmatisation biases, the latter to prevent discussions which could potentially increase the risk of non-compliance.

Note, finally, that all the assignments to the treatment measures were compulsory (and could be sanctioned in the case of non-compliance, see last section). Still, non-compliance by the treated job seeker in terms of intentionally avoiding the coaching program can not be excluded with 100% certainty. But, as the non-compliance analysis in section 2.3.1 shows, intentional non-compliance could only be observed in a negligibly small number of cases.

2.2.4 Potential Effects

It is fruitful to discuss shortly the *potential effects* that the treatment plan could generate. To do so, I first focus on discussing the potential effects of every stage of the treatment plan on the outcome (job finding propensity). Secondly, I relate the potential effects to the two crucial decision variables in job search theory: job search effort and reservation wage.

Following the strict timing of the treatment plan as described in section 2.2.1, the treatment effects can be shaped as follows:



The first treatment period, from t_0 to t_{c1}, is the *anticipation* period. Two things may happen in this period. First, the anticipation of the upcoming coaching (whereby t_{c1} is known ex-ante) may result in an "attraction effect" or a "threat effect". If individuals expect support and positive impact of the coaching, the former effect will materialise – δ_a will be negative; if individuals do not have positive expectations and consider the coaching as a disturbing factor in their job search, the latter effect will prevail and δ_a becomes positive. Second, the intensified counseling could result in a quick job finding success, thus δ_a would increase. But note that the anticipation period is rather short (it takes in median 50 days until (potential) coaching entry, see section 2.2.1), such that the full effect of double-frequency counseling is normally not yet developed. Not as well that a quick job finding success in general, i.e. not driven by the doubling of counseling, will not result in a treatment effect. Due to randomisation such a treatment-unrelated event can happen with the same probability in the control group. In other words, such events of treatment-unrelated dynamic selection do not affect the balancing of the two groups.

The second treatment period, from t_{c1} to t_{c2}, is shaped by the effect of (potentially) being in the coaching program. For δ_{c1} it is therefore most probable that a *lock-in effect* can be found. Due to the high intensity and work load of the coaching program it is well conceivable that job search effort suffers from a certain lack of time.

The third treatment period, from t_{c2} on until unemployment exit, captures the *post-coaching* effects. These are the cumulative outcome of coaching and the parallely ongoing high-frequency counseling (in the first four months of unemployment). I split this effect up into a short-run effect δ_{c2}, which operates in the first 180 days after coaching, and in the mid-run effect δ_{c3} thereafter. The aim of the policy is clearly that this effect should become positive. Note, though, that if coaching results in a substantial job search strategy change (which is one of the core assessment elements in the coaching, see section 2.2.1), the potential effects could be twofold: In the short run, reorientation of search strategy may lead to a further lock-in situation; the job seeker first needs to learn and to put the effort in the development of the new strategy instead of fully searching for the same kind of jobs. In the longer run, the change of job search strategy could result in a higher success rate in job finding.

If one considers these potential effects in the context of the job search theory decision variables job search effort and reservation wage, it gets quite obvious that *overlapping effects* are highly probable. Looking at *job search* effort, it may be concluded that more intense and/or more *effective* search – the latter is a crucial aim of coaching and counseling – should be the result of the treatment. On the contrary, the high time consumption of the coaching program and of a potential reorientation may reduce job search effort (lock-in effect). Thus, it is ex ante not clear which of the two effect directions will prevail.

Also when considering *potential reservation wage development*, arguments for a potential increase or decrease of this variable can be put forward. More realistic self-assessment due to coaching and the increased pressure generated by the intense treatment could lead to a lowered demand towards the quality of future jobs, which would result in a positive effect on job finding. But self-assessment could also reveal an underestimation of the labor market qualities of an individual; furthermore, if human capital is successfully developed by means of the coaching, the labor market value and thus reservation wage could as well increase – with a potentially negative effect on the probability to find a job. Finally, a successful improvement of job search strategy and self-marketing could bring the individual to reach a job match of higher quality and thus higher salary.

This shows that as well the sign of *post-unemployment effects* is not clear a priori. A reduced, more realistic reservation wage could improve the job finding proportion – but as well reduce the quality of the found job (and thus salary). A more comprehensive job search strategy could increase job finding propensity and reduce job quality, too – but job quality could as well increase, as mentioned, if job search becomes more effective in the sense of improving the matching quality. Thus, empirical evaluation is necessary to assess which effect dominates. The data in this paper allow this assessment.

2.2.5 The Data: Register and Survey

The evaluation of this social experiment is based on a unique combination of administrative records of the unemployment insurance (UI) and a series of repeated surveys on behavioral measures which cover the behavioral dynamics and labor market outcomes beyond the UI registers. For this paper I use the first and the final survey (the repeated surveys are analysed in the companion paper Arni 2011), in order to cover issues of job quality, and the register data for the unemployment and post-unemployment periods.

The *register data* are available for all job seekers who flow into registered unemployment between December 2007 and December 2008 in the region under consideration, the Canton of Aargau. The individuals are observed from start of their unemployment spell until the end of March 2010 (exogenous censoring date). Thus, all individuals are observed for at least 454 days and maximum 835 days. During these periods, repeated unemployment spells can be observed. Thus, this allows not only to construct unemployment spells but also post-unemployment durations. More specifically, the here constructed post-unemployment spell is defined as the duration from exit from unemployment to a job until a possible reentry into unemployment (otherwise it is censored). To avoid the overweight of some long durations, the post-unemployment durations will be (exogenously) censored at 540 days (1.5 years).

The register data include a rich set of observable characteristics (see table in section). Beyond socio-demographics, education and occupation, they track as well past unemployment histories up to three years before entry in the spell under consideration. The tables in the descriptive section 2.3.1 and, in particular, the first table in the section 2.4.1 on the results of the duration model (Table 2.3) report the collection of used observables.

The additional *survey data* used here stem from the final and the first caseworker survey of the LZAR data base. This data base, which features repeated surveys of job seekers and caseworkers over the unemployment spells in this project (see Arni 2011 for details), is fully linked to the register data. After the respective interviews, the caseworkers had to fill in an online tool which complemented the information of the register data base. Here, I extract information on the gross monthly salary in the first job right after unemployment exit, as reported by the job seeker to the caseworker. This is supplemented by information on the pensum (contractual workload in hours per week). Note that reporting of this information is not compulsory for the job seekers. I will analyse response rates and balancing in the next section. Beyond the final caseworker survey I use as well the first caseworker survey (filled in after the first interview) to analyse pre-unemployment salaries (last monthly gross salary before entry into unemployment).

2.3 Nonparametric Analysis of Main Impacts

2.3.1 Descriptive Analysis

In this section, I compare observable characteristics of the treatment and the control group in order to assess if initial randomisation worked fine and to characterize the experimental population in general. Moreover, I check how balancing of the observables looks like in the first and the final caseworker survey of the LZAR data which feature imperfect response rate. Finally, I report a series of analyses to describe several aspects of dynamic selection into the coaching program, the core part of the new policy: the variation of the timing of the program; who participated in the coaching program; the amount of intentional non-compliance.

Table 2.1: Comparison of characteristics of treatment vs control group

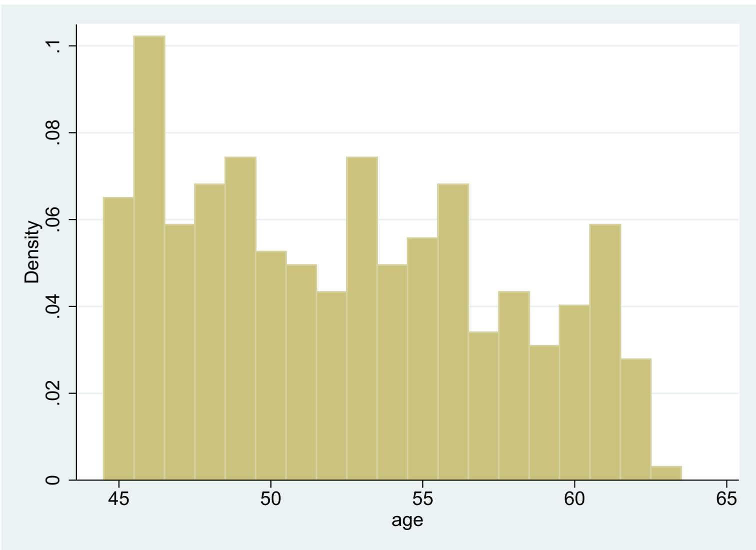
	Treatment Group	*Control Group*	*t-values*
Gender: Woman	44.1%	43.3%	0.15
Married (incl. separated)	56.4%	49.7%	1.22
Age	52.5	51.9	1.04
Nationality: CH	84.4%	85.1%	-0.17
Qualification: (semi-)skilled	96.2%	95.7%	0.22
Employability: 3/4	77.4% / 21.5%	78.0% / 21.3%	(-)0.05
At least 1 foreign language	55.4%	53.2%	0.39
Job < 100%	17.7%	17.7%	0.00
PES 2	14.5%	10.6%	1.04
Duation to availability (median, days)	11	13	-0.49
Past UE duration (median, days)	0	0	0.00
Observations	186	141	
... in %	56.9%	43.1%	

Notes: Frequency percentages for different observable characteristics by treatment and control group are reported. t-values are based on unpaired t-tests with equal variances.
Source: Own calculations based on merged UIR-LZAR database.

The comparison of observable characteristics between treatment and control group, see Table 2.1, shows that *randomisation worked very well.* No remarkable group differences can be detected for this sample of 327 job seekers (186 in treatment group, 141 in control group). Note that the initial sampling according to the project eligibility criteria (see section 2.2.3) shapes the absolute values of the figures in Table 2.1. This explains, for example, the high proportion of skilled and of Swiss job seekers. Moreover, the project is focussed to individuals of middle (3) and low (4) employability. Less than 18% of the job seekers were looking for a job of higher part-time charge (above 50%). The treatment group features, by random, a slightly higher proportion of married people.

The median duration of unemployment history in the past three years is zero for both groups. 27.5% of the participants have a positive duration (median 113 days). 'Duration to availability' indicates the number of days until an individual gets available for active labor market programs (ALMP). The main reason for initial non-availability is that the respective individuals already registered at the unemployment insurance during the cancellation period[8]; this restricts their availability to participate in interviews and labor market policy. A second reason is that some job seekers may be engaged in a shorter temporary subsidized job such that they get available some weeks later. A majority of 57% is available for ALMP within 20 days. Note that the PES 2 joined the experiment inflow later, from June 2008 on. This, combined with the slightly changed random assignment ratio over time (see footnote 7), mechanically explains the slightly higher percentage of random assignments to the treatment group. Since this was all fixed ex-ante, it doesn't affect randomisation.

Figure 2.3: The age structure of the sample



The median *age* of the participants in the social experiments is 52 years. The total age range of the participants lies between 45 and 63 years. Figure 2.3 shows the age distribution of the sample. 40% of the individuals in the sample are of age 45-49, 27.5% of age 50-54, 21.7% of age 55-59 and 10.7% of age 60-63. Note that none of this latter group had the possibility to pass to early retirement by means of unemployment insurance.

As compared to Table 2.1, to which degree are the used survey items balanced? The response

[8]This behavior is promoted by the unemployment insurance authority – for the same reason as the early intervention principle. The earlier the caseworker interventions start, the lower the potential risk to stay long in unemployment, see also introduction.

rates are not perfect but high in the first and the final caseworker survey: 92.4 and 81.3%, respectively. The fact that not all the job seekers found a job and that reporting of job/salary information is not compulsory results in 163 remaining observations. This means that 68.5% of the individuals responded to the salary questions, measured as a proportion of the total of the job finders. This response rate is highly balanced between treatment and control group (68.1 vs. 69.2%)[9]. Slightly more women and part-time workers are among these job finders (salary info sample). Otherwise, observable characteristics are highly comparable to the full sample. The three survey samples are well balanced in their observable characterstics, as Table B1 in the Appendix reports. No significant differences in observables between treatment and control group are found, except from the proportion of married people. In total, there is *no indication of a significant response bias.*

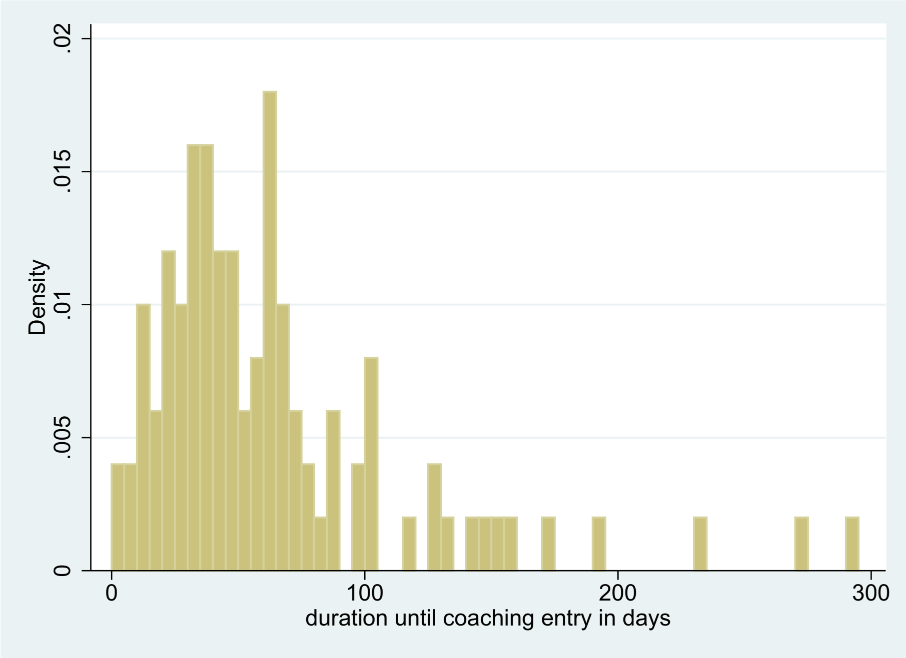
In the following, I analyse three aspects of the *coaching program participation*: (i) the variation of the time to program start; (ii) the impacts of dynamic selection on the characteristics of the participating population; (iii) the size of intentional non-compliance to compulsory participation. This information is helpful to understand the empirical background of the treatment plan and the importance of selection issues for the identification of treatment effects by period.

Figure 2.4 shows that there is considerable variation in the duration until entry into the coaching program. Median duration from start of unemployment until coaching entry is 50 days. Duration to coaching entry varies from 0 (coaching start by coincidence at the day of unemployment entry) to 290 days. It is important to mention that this variation is predominantly exogenous – due to the fact that all the dates of the coaching program (see footnote 5 for details) were *fixed in advance* with the coaching supplier. The exogeneity of the mechanism could be compromised by the following factors: duration to availability, a temporary subsidized job, calling in sick. I perform some sensitivity analyses on whether these factors affect the labor market outcome when discussing the anticipation effect in section 2.5.2. I do not find such evidence. The variation in coaching entry timing offers therefore the opportunity to estimate the elasticity of the anticipation effect with respect to anticipation duration, see section 2.5.2.

Next, in order to get to know more about which characteristics codetermine *early dynamic selection* and therefore *coaching entry*, I perform a respective probit regression. The analysis on coaching entry propensity, see Table B2 in the Appendix, reveals the following pattern of dynamic selection in the pre-program stage of the unemployment spell: The probability to enter coaching (in the treatment group) is higher for individuals who are of older age, unmarried, male, relatively less skilled (”only” one foreign language and not two, low-skill- and unskilled occupations). Inversely, one can state that early exits are more prominent among younger (age 45-49), married, female people speaking 2+ languages. Individuals with a longer duration to availability show a lower probability to enter coaching – this can also be explained by dynamic selection: it seems that

[9]Since I use pre-unemployment salaries to construct pre-to-post-unemployment salary differences, this response rate analysis is the same for the final as for the first survey.

Figure 2.4: Variation in coaching entry timing



those people who registered at the UI already during cancellation period had a higher propensity to quickly find a job. Moreover, non-German-speaking individuals had a lower probability to enter coaching; the two possible explanations are early exit from unemployment or insufficient knowledge of the German language to follow the coaching[10]. The significance of the inflow dummy for Nov/Dec 2008 points to a small overbooking of the coaching programs starting at the end of 2008. Note that since the booking was made in order of inflow, potential non-compliance behavior cannot influence the booking process.

The described pattern of coaching entry propensities that arises above is typical for early exit behavior: The relatively younger and better skilled exit more quickly from unemployment such that more of them are not unemployed any more at the time of planned program entry (either they already exited from UI or they found a job starting in the near future such that coaching participation was not of use any more). Thus, this points to common dynamic selection behavior over the course of the unemployment spell. As far as this dynamic selection is independent from the anticipation behavior with respect to the upcoming coaching program and from the early impacts of intensified counseling, it does not harm the balancing between treatment and control group. But, however, the part of dynamic selection that gets reinforced by coaching anticipation can potentially harm the comparability of the two groups. This is a problem if the imbalance

[10]In this case the insufficient language proficiency was, seemingly, not yet visible at t_0, otherwise they would have been filtered out at the beginning, see section 2.2.3.

is correlated with the labor market outcomes. In such a case of un-balanced impact of dynamic selection controls of observables and unobservables need to be introduced by use of a respective econometric model. This is done in section 2.4 – the analysis in section 2.4.4 shows, though, that the importance of unobservables is insignificant over the course of the unemployment spell, given the control for the observables characteristics.

A final dimension of the selection process during unemployment is *intentional non-compliance*, i.e. individuals who intentionally ignored the (compulsory and exogenous) treatment assignment. Intentional non-compliance behavior can, potentially, be correlated with unobservables that influence as well the labor market outcomes; this would generate another reason for introducing unobservables into an econometric model. I use a filtering algorithm that features several steps to analyse this question. First, I restrict the focus to people who are in the treatment group but did not participate in the coaching program. This is the case for 86 of the 186 individuals. Second, I identified the cases of early exits in this subgroup[11]: The majority of this subgroup (53.5%) did not participate by default since they found a job early in unemployment, i.e. before potential coaching entry. This has obviously nothing to do with non-compliance and corresponds to the above-described "normal" dynamic selection process. After this filter step, 40 individuals remained to be further analysed. The caseworkers of these individuals were surveyed about the reason for the non-participation in coaching. The vast majority of these cases turned out to have valid (and legally accepted) reasons for non-participation: 35% found a temporary subsidised job shortly after unemployment start, so that they became unavailable for coaching; 22.5% had an offer for a job starting in the near future (within the next 2-3 months normally); 27.5% had other valid reasons which are unrelated to non-compliance (like caseworker error or the fact that the job seeker recently followed another coaching). The remaining cases – 4 to 6 individuals – can be considered as having shown intentional non-compliance. 2 cases reported health problems, 4 cases showed 'high unwillingness to participate' in the coaching. Thus, the *non-compliance rate amounts only to 3.2%* – which is negligible.

2.3.2 Comparison of Means & Survivor Analysis of Main Outcomes

What can be learned on the impacts of the social experiment without imposing any econometric structure? Given the successful randomisation at t_0 (see section 2.3.1), *causal statements on the total/net effect of the treatment plan as a whole* can be inferred in a nonparametric manner – by use of means comparisons and Kaplan-Meier survivor analysis. This is done in the following. Four main results materialise. They are documented in Table 2.2 and a series of survivor graphs.

[11] The filtering conditions for this step are: (availability date + 5 days) < potential coaching entry date < (exit date - 30). If a person did not participate in coaching even though there was a program available within these conditions, the case was labeled as 'unexplained non-participation'. These conditions imply (i) that the job seeker must be available minimum 5 days before coaching start, and (ii) that the caseworker will not send a job seeker to the coaching program if (s)he starts a newly found job within the next 30 days.

Table 2.2: Non-parametric comparison of main outcomes: unemployment duration (means, medians), proportion in longterm unemployment, job finders, gross salaries, recurrence to unemployment

	TG	CG	difference =TE	t-value	TG$^{(2)}$	CG$^{(2)}$	diff.$^{(2)}$ =TE	t-value$^{(2)}$
Unemployment duration, means and **medians** (2)	**234.7**	**241.9**	**-7.28**	**-0.324**	**139.5**	**138**	**1.5**	**0.060**
...for short anticipation durations (1-34 days)	207.2	233.0	-25.77	-0.602	125	155.5	-30.5	-0.662
...for median anticipation durations (35-70 days)	218.2	234.8	-16.53	-0.491	131	128.5	2.5	0.067
...for long anticipation durations (70+ days)	286.4	263.9	22.53	0.534	231.5	184	47.5	1.067
Δ TE median → short anticipation			-9.24	-0.170			-33	0.556
Δ TE long → short anticipation			-48.30	-0.798			-78	1.217
...for ages 45-54	204.3	214.4	-10.03	-0.397	131	131.5	-0.5	-0.018
...for ages 55+	306.9	290.6	16.30	0.379	283	191	92	2.034
Proportion in longterm unemployment	**0.2796**	**0.3191**	**-0.0396**	**-0.774**				
Proportion leaving for a job	**0.7204**	**0.6312**	**0.0892**	**1.718**				
Prop. in job, incl. people voluntarily leaving UI	0.7742	0.6667	0.1075	2.173				
Gross salary, mean	**5357.6**	**5392.4**	**-34.78**	**-0.105**				
... difference to pre-UE salary	-402.7	-242.3	-160.37	-0.737				
pensum: working hours per week	38.72	37.62	1.10	0.850				
Recurrence to unemployment	**0.2308**	**0.2809**	**-0.0501**	**0.837**				

Note: Means are reported, in the case of the unemployment durations as well medians (2). Observations: 327, 186 in treatment group (TG) and 141 in control group (CG); subsamples for short/median/long anticipation are of size 95/141/91; for ages 45-54/55+ they are 221/106. Observations on salary data: 163; on recurrence: 219. TE=treatment effect.
Source: Merged UIR-LZAR database

The first result arises from the nonparametric analysis of the question: How did the new labor market policy affect the (total) unemployment durations of individuals? The first row in Table 2.2 reports the comparison of the mean and median unemployment durations by treatment group (TG) vs. control group (CG). This yields a clear result: There is *no significant effect of the treatment plan on the unemployment duration*. The respective t-values report that the TG-CG differences are clearly not significant. Median unemployment durations do differ only marginally (139.5 vs 138 days). The mean unemployment duration of TG members (235 days) is 7 days shorter than the corresponding mean duration for the CG (242 days). Note that in order to provide a realistic picture of mean durations and to restrict the impact of extreme outlier values, durations have been (exogenously) censored at 570 days (19 months)[12].

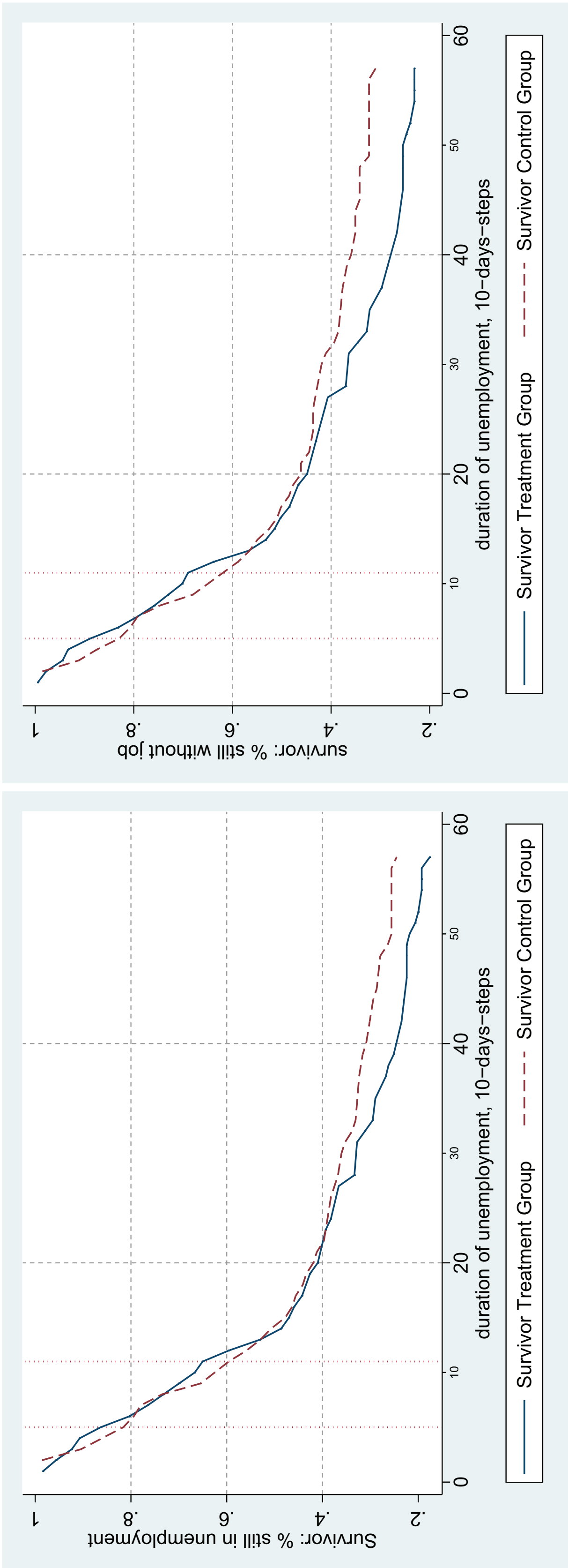
In the light of the existing ALMP evaluation evidence (see references in introduction) the result of *no prolongation* of unemployment duration due to the new ALMP can be interpreted as being positive. The predominant result in the literature on training-oriented ALMPs is that they increase unemployment duration due to the lock-in effect (less search during the program) and/or uneffectiveness of the program with respect to labor market chances. Even though the new program evaluated here implies high workload and time consumption in the first four months of unemployment, this did not translate into a prolongation of unemployment duration. Possible explanations are a reduced lock-in effect and/or a substantial improvement of effectiveness in job finding after coaching. This can and will be tested in the upcoming sections 2.4 and 2.5 by use of a duration model.

Some important evidence concerning this question can already be gained when looking at the nonparametric survivor analysis of unemployment duration and of duration to job finding, see Figures 2.5. The first figure reports the proportion of individuals in the TG and CG who are still in unemployment. The dotted vertical lines indicate the median starting and ending of the (potential) coaching program[13]. The two curves of the survivor overlap over the course of the first 270 days of unemployment; thereafter, they slightly begin to diverge, in favor of more exits from unemployment in the treatment group. This picture is consistent with the above-found slight but insignificant reduction of the mean unemployment duration due to the treatment. The survivor shows that a positive impact of the treatment on the rate of unemployment exit begins to kick in in later stages of unemployment.

[12] Besides restricting the impact of extreme outlier values the censoring time at 570 days (21.4% censored durations) was chosen to avoid too small numbers of observations in the calculation of the Kaplan-Meier survivor rate data points in the figures below. Moreover, this censoring time helps yielding a realistic picture of mean durations since it is located between the maximum benefit durations for individuals aged below 55 (18 months) and above (24 months). A sensitivity analysis using the latest possible censoring date (march 31, 2010; 16.5% censored durations) shows that the treatment effect results do not change qualitatively and statistically.

[13] In the upcoming analysis by treatment period in section 2.5.1 I will use, of course, the exact timing by individual

Figure 2.5: Total treatment effect on duration of unemployment and on duration to job finding, survivors treatment vs control group



This conclusion gets reinforced when analysing the durations until job finding (second figure in Figure 2.5). Unlike the first survivor comparison, the analysis here defines only those cases as a positive transition out of the initial status which end up in job finding; other cases of exits are censored. Beyond 250 days, the survivors of treatment and control groups more remarkably diverge, leading to a higher job finding proportion in the treatment group in the later stages of unemployment. As discussed further below, this effect of more frequent job finding is significant in total. Thus, this analysis shows that the new ALMP takes some time until it develops beneficial effects on job finding. So, *unemployment duration does not get shorter, but more individuals end up in a job in the treatment group.*

This result of a longer-run positive effect has not yet fully materialised at the threshold of long-term unemployment. The proportion of individuals remaining in unemployment for longer than 360 days is visibly smaller in the treatment group, but the difference does not get statistically significant as Table 2.2 shows. Thus, if the success of the new ALMP is narrowly judged by a reduction of the LTU ratio, this evaluation cannot provide a significantly positive result. However, this is not the case, the policy makers who ordered this pilot project defined more general policy goals: they mainly focus on the question whether the new policy was able to increase labor market chances of older job seekers. If labor market chances are measured by job finding, the program can be considered as being successful.

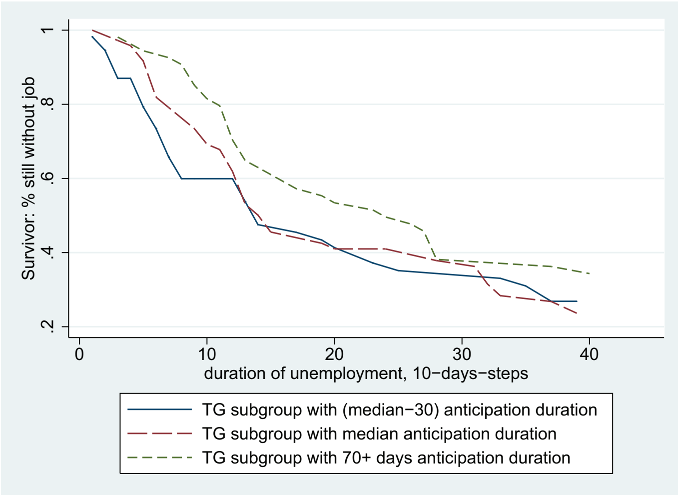
Which part of the population in the treatment group did especially profit from the new policy, which not? To explore this question two dimensions are further analysed: age and the timing of intervention[14]. Do individuals in the upper and the lower part of the considered age distribution behave differently as a result of the treatment? They do, but not much gets significant in terms of total/net unemployment durations. Table 2.2 reports that individuals below age 55 show some insignificant reduction of the mean unemployment duration, medians do not differ. This group dominates thus the above-discussed total effect on mean and median unemployment duration. Individuals aged 55+ do, however, clearly not profit from the treatment intervention in terms of unemployment duration: this gets prolonged by 16 days in mean and 92 days in median, the latter result being highly significant. So, the mentioned positive interpretation of the new program not prolonging unemployment duration does not hold for oldest subgroup of job seekers beyond age 55.

Can the impacts of the program be improved if interventions take place earlier? As discussed in the descriptive analysis of durations to coaching program start (see section 2.3.1), the core mechanism assigning anticipation durations to individuals is exogenous (timing of coaching fixed ex-ante); some factors (mentioned there) may compromise exogeneity (prolong anticipation), but sensitivity analysis (see section 2.5.2) shows that they do not significantly affect the outcome.

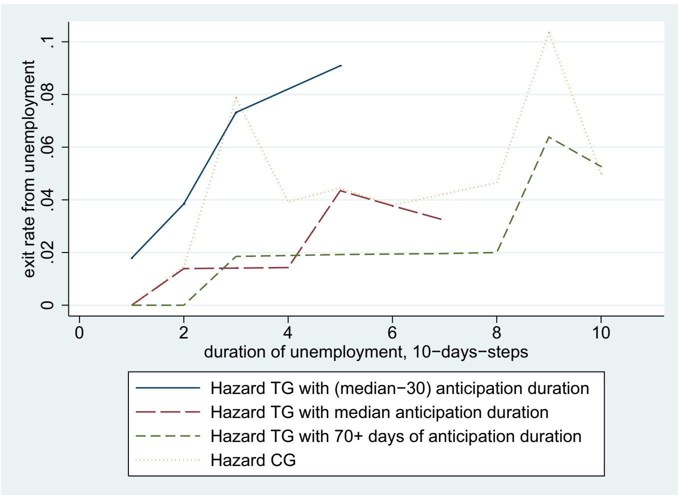
[14]Note that no distinct behavior with respect to gender could be found.

Figure 2.6: Anticipation effect: the impact of anticipation (time to program) duration

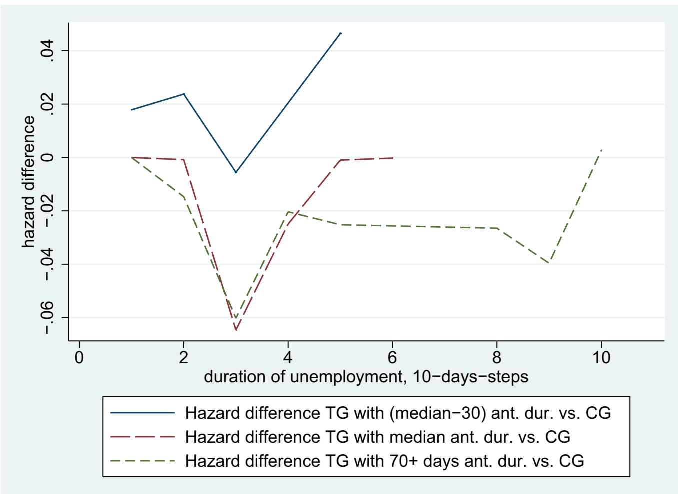
a. Is early intervention better?: Survivor rate with [median-30 days] anticipation duration vs survivor rate with median and with long anticipation duration



b. Exit to job rates by anticipation duration [= duration until (real or potential) coaching entry]



c. Comparison of anticipation effect (hazard difference to control group) for individuals with short (< 35 days) vs median (35-70 days) vs long (70+ days) anticipation duration



Thus, variation in time to coaching program entry can be used to assess a potential saving (or extension) of unemployment duration if the intervention takes place earlier (or later). I distinguish three subgroups: median anticipation durations of 35 to 70 days – yielding a median of exactly 50 days, thus the default group – versus short anticipation durations (1 to 34 days, median 19 days, thus intervention 1 month earlier) or long anticipation durations (70+ days, median 102 days). Analysis of mean and median unemployment durations and of differences in treatment effects, see Table 2.2, reveals that the pattern indeed goes in the expected direction, but differences do not get significant. Note that the sizes of the used subsamples are quite small such that standard errors naturally get quite large and the threshold for significance quite high.

Taking into account the nature of the treatment plan and its potential effects (see section 2.2.4), early intervention could have distinct impacts in different periods: In the anticipation period, the attraction effect – which I find in the analysis by treatment period in section 2.5.1 – could be reduced by early intervention (higher early exit hazard); this would, though, help to reduce unemployment duration. In the stages thereafter, early intervention could be beneficial as well since individuals leave coaching, and therefore the related lock-in period, earlier. The respective survivor analysis is presented in Figure 2.6c. The solid line, representing early intervention (coaching start one month earlier), reveals why the total duration effect of early intervention is not stronger: In the anticipation period and during coaching (thus up to 80 days), the exit to job rate was indeed higher – this effect is clearly significant as the duration model in section 2.5.2 will show. But thereafter, from day 80 to 120, individuals remained in some lock-in. Finally, from day 120 on, the survivor curve is not distinguishable any more from the default group's. Thus, early intervention works to reduce the duration-prolonging attraction effect, but earlier exit from coaching could not be translated into earlier job finding. The latter fact can be explained by learning: individuals need some time until they efficiently apply the inputs of coaching (see also introduction). This learning time seems to be longer in the case of early intervention. A possible explanation for this is that the early-intervention-individuals had less opportunity to profit from the support of intensified counseling (only through 80 days, instead of the default of 120 days). Finally, the 70+ days-survivor in Figure 2.6a shows that late intervention resulted in some procrastination of job finding in all stages of unemployment.

The second main result documents the impact of the new policy on job finding. Table 2.2 shows that the *proportion of individuals who found a job is significantly higher in the treatment group – by 9 percentage points.* Whereas 63% of the CG individuals left unemployment to a job, the proportion of TG individuals leaving for a job amounts to 72%. Combining this insight with the survivor analysis above about duration to unemployment exit and to exit to job (see Figure 2.5) yields the following conclusion: *The treatment caused significantly more individuals to find a job. But since it took some time until treatment resulted in increased job finding, the total unemployment durations did not significantly reduce.*

A more detailed look on the exit destinations[15] reveals interesting supplementary insights to the result of more job finding in the treatment group. The TG individuals left less often unemployment for non-employment (8.6% vs 13.5% in CG) and were less often censored (i.e. less long unemployment durations, 14.0% vs 19.9% in CG). "Unknown status after unemployment exit" is a bit more frequent in the TG (5.4% vs 3.5%). More than two thirds of these cases deregistered from unemployment insurance in order to avoid controls or to renounce to services of the UI; the rest left the country to search for a job elsewhere. Since it is most probable that a clear majority of these individuals found in the near future a job too, I report these percentages (77.4% vs 66.7%) as well in Table 2.2. For this measure, importance and significance of the TG-CG-difference is even higher.

A final interesting observation with respect to job finding is that the additional job finding in the treatment group predominantly originates from "referrals by PES". It has to be noted that this subcategory is also used as part of the performance reporting of the PES. So, caseworkers have an incentive to report a found job as "referred by PES" even if the job does not directly stem from the PES-run job database, but the job finding procedure was substantially supported by the caseworker. Thus, it is most probable that this result reflects the stronger guidance by the caseworker due to intensified counseling in the treatment group. This would mean that intensified counseling was an important complement to the coaching program in generating the positive treatment effect on job finding[16].

Was the higher proportion of job finders in the treatment group probably reached through the acceptance of lower quality jobs? The answer is clearly no, as the third main result of nonparametric analysis of this experiment shows. The *monthly gross salaries realised after unemployment exit are not lower in the treatment group*, as Table 2.2 reports. It has to be noted that this result is based on a subsample of those individuals who found a job and reported their salary. So, there are two potential sources of bias: selectivity with respect to job finding and unbalanced non-response behavior. The analysis in section 2.3.1 shows that the latter is not the case. The selection issue with respect to job finding will be further discussed in the next section.

In older working age, reestablishment on the labor market after unemployment often implies a wage loss (due to weaker negotiation power, among other reasons). This is found for the here analysed population as well. On average, a pre-to-post-unemployment gross salary loss of 341 CHF is incurred, which is significantly different from zero. However, when comparing treatment and control group I do not find a significant difference in the size of the salary loss (see Table 2.2).

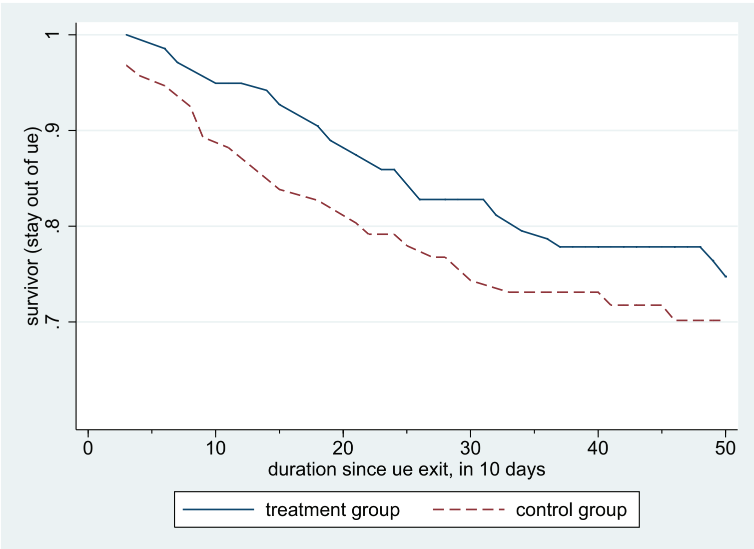
[15] Note that this exit destination and job finding information comes from the register data. To refine it, I supplemented it by survey information. This helps detailing 'unknown status' and 'other reasons' categories. By pure register data, job finding proportions would amount to 71.0 vs 60.3% (treatment effect of 10.7%); the small difference originates from the identification of some cases of exit to self-employment (considered as exits to job) by the survey.

[16] A further theoretical explanation for the increased referrals by the PES would point to an interaction effect: Given the fact that the TG members were present at the PES in double frequency, job offers available to the caseworkers could have been predominantly referred to TG members. However, I found so far no evidence for decreased job finding chances in the CG. This will be further explored by means of an external control group.

This confirms the result discussed above that the treated did not choose jobs of lower quality than the controls. Moreover, a glimpse on the weekly average pensum (official working hours per week) reveals that there is no significant difference in this job quality dimension too.

Finally, let's adopt the long-run view on how the labor market outcomes evolved *beyond* unemployment exit. Was maybe the long-run job quality diminished due to the treatment? This is measured by means of *recurrence behavior* – i.e. by analysing the probability that the job finders fell back into unemployment within 1.5 years. Such a measure reports, thus, *employment stability* within the given post-unemployment period. The question above can be answered with no: Table 2.2 reveals that 23% of the treated reentered unemployment within 1.5 years, whereas the recurrence propensity in the control group amounts to 28%. This difference is, though, statistically not significant.

Figure 2.7: Post-unemployment job stability: Survivor of the reentry rate into unemployment



How does employment stability compare between TG and CG in a time-dynamic perspective? Figure 2.7 shows that the post-unemployment survivor curve of the treatment group is located clearly above the one of the control group – *treated individuals remain, thus, on average longer outside unemployment*. 300 days after unemployment exit, about 83% of the job finders in the TG remain in employment, whereas the same rate in the CG amounts to about 74%. In other words, the reentry rate back into unemployment is on average smaller in the TG over the course of 1.5 years of post-unemployment.

However, it is important to note that this long-run measure of recurrence is prone to a selectivity issue: Selection into jobs is, as we found above, (positively) different between treatment and control groups; this potential imbalance in observables and unobservables between the two groups could affect recurrence behavior. Taking this into account will indeed show in section 2.5.3 that the treatment effect on employment stability gets more distinct: The difference in the recurrence (hazard) rates in the post-unemployment period becomes bigger and significant – the new policy caused a significant reduction of unemployment reentry.

To wrap up, the four nonparametric results on the main outcomes of the new ALMP can be summarized as follows: *The field experiment shows that the new policy caused more treatment group individuals to find a job than in the control group. They didn't find their jobs quicker – unemployment duration remained at the same levels. The quality of post-unemployment jobs was not worse in the TG than in the CG: reentry salaries were on average at the same levels and employment stability is in tendency even better – the latter result gets significant in a parametric model.*

The last statement and the discussion above about different contributing sub-treatment-effects demonstrate that putting more structure on the analysis of labor market outcomes can be valuable to gain further insights. Therefore, I apply, as a next step, a timing-of-events approach. Doing so yields at least three key advantages for the identification of components of the above-found total treatment effects and of further post-unemployment effects, as the next section will show.

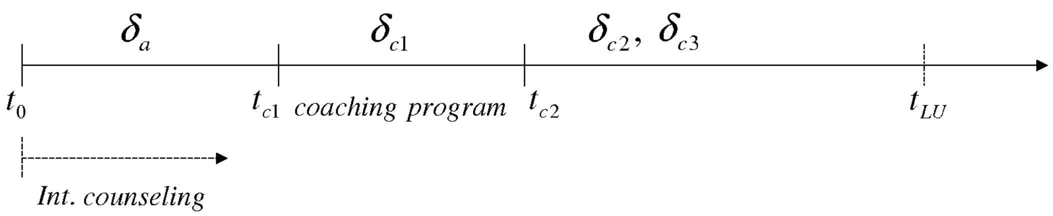
2.4 Econometric Framework

In this section, I will apply the *timing-of-events approach* to the treatment plan setup of the new policy (see section 2.2.1). This provides three key advantages for gaining more detailed insights into the (short- and long-run) *dynamics* of the treatment effects of the new policy: First, the identification of sub-treatment-effects by use of the exact timing of the different treatment periods allows to further explain what really happened during the program. Which *part of the treatment plan* did contribute in which way to the observed net/total effect? Those results by treatment period help as well to search for *policy improvements* (section 2.5.2 is dedicated to that issue). Second, this duration model approach allows to take *dynamic selection* into account. This is mainly of importance when analysing *post-unemployment* recurrence outcomes as they base on a sub-sample of job finders, which implies additional potential selectivity. Finally, this modeling approach allows to quantify the *employment stability effect* (in days of avoided future unemployment), which is done in section 2.5.3.

In the following, I will first set up the duration model with subsequent treatment periods (section 2.4.1). Then, I discuss the advantages of randomisation in the context of the timing-of-events approach – more treatment effects can be modeled under alleviated assumptions (section 2.4.2). Next, I will demonstrate how post-unemployment job stability is introduced as a second process (section 2.4.3). Finally, dynamic selection and the outcomes from controlling for unobserved heterogeneity in the context of these data will be discussed (section 2.4.4).

2.4.1 Duration Model with Subsequent Treatment Periods

In this section, I model the subsequent steps of the treatment plan implemented by this field experiment using a duration model framework. As described earlier, two crucial treatments were implemented: the *intensified counseling* (interviews with caseworker every second week), from t_0 on over 4 months, and the *targeted coaching program* which starts in median 50 days after unemployment entry and lasts approximatively 60 days. Thus, this may be represented in the following way:



Following the timing-of-events approach of Abbring and van den Berg (2003), with extension to an experimental setup with anticipation effect (Abbring et al. 2005), the (mixed) proportional hazard (MPH) model may be constructed based on the outlined setup as follows:

$$\theta_u(t_u|x, M_j, C_k, D_i, v_u) = \lambda_u(t_u)exp(x'\beta_u + \sum_{j=1}^{6}\tau_j M_j + \sum_{k=1}^{11}\gamma C_k + \sum_i \delta_i D_i(t_u) + v_u) \quad (2.1)$$

where θ_u is the exit rate from unemployment to a job and t_u is the unemployment duration. x is a vector of individual characteristics[17], including the control for the unemployment history in the past 3 years, and M_j represents a series of time dummies which control, in 2-months-steps, for the specific time and business cycle conditions at inflow into the sample. C_k are caseworker fixed effects and v_u represents the unobserved heterogeneity component which will be further discussed in section 2.4.4. The component $\sum_i \delta_i D_i(t_u)$ will be differently specified according to the gradual steps of the upcoming analysis. These specifications will be further discussed below.

The duration dependence function $\lambda_u(t_u)$ in this model is designed as being a piecewise-constant function of the form

$$\lambda_u(t_u) = exp(\sum_k(\lambda_{u,k} \cdot I_k(t_u)) \quad (2.2)$$

where $k = 0, \dots, 5$ time intervals are distinguished and $I_k(t_u)$ represent time-varying dummy variables that are one in the respective intervals. Based on the descriptive hazard for the unemployment exit process (see Figure 2.1) I define the six time intervals as follows: 0-50/51-100/101-150/151-250/251-350/351+ days. Unemployment durations are exogenously censored at March 31, 2010 (end of observation window), if necessary. Note that the analysis in this paper focuses on *exits to job* rather than on general unemployment exits. This is done in the light of the results found in section 2.3.2 that the new policy significantly increased job findings. Therefore, we are explicitly interested in the effects of different parts of the treatment on job finding hazards[18]. Moreover, this concept is consistent with the goal of this paper to study as well the long-run impacts of the new policy on employment persistence and quality. Accordingly, the non-censoring indicator in this model is 1 for individuals who found a job (see section 2.3.2 for details on exit destinations).

Based on this model setup, I perform a sequence of analyses whereby the specification of $\sum_i \delta_i D_i(t_u)$ changes gradually. The first model I estimate is a (simplified) replication of the nonparametric survivor analysis of the total effect (see section 2.3.2) by means of a (M)PH model of the form of (2.1). This means that the treatment component only consists of one element: $\delta_b D_b$, whereby D_b is a dummy variable indicating that an individual is member of the treatment group. Thus, the estimated *baseline treatment effect* δ_b (not shown in the figure above) allows a

[17] See the descriptive analysis in section 2.3.1 and the first results table (Table 2.3) in the section 2.5.1 for a list of controlled observable characteristics.

[18] In the Appendix I provide, as a supplement, all the estimation results for the case of exit from unemployment in general. They would be especially useful for quantifying the impact of the program on duration in unemployment insurance. But this treatment effect is, net, zero as section 2.3.2 reports.

shift of the hazard rate from t_0 on until unemployment exit for all treated individuals. Note that this model is clearly more restrictive than the nonparametric one since it requires the hazard rate shift to be constant over time (which is not the case in the nonparametric analysis). Still it is useful to run this model just as a baseline benchmark. Note, moreover, that due to randomisation no issue of endogenous selection is involved here.

Next, the analysis progresses to the *main model with specific treatment effects for every treatment period.* This implies that the component $\sum_i \delta_i D_i(t_u)$ is used whereby $i \in \{a; c_1; c_2; c_3\}$ are the treatment effects by subsequent treatment period. Following the figure above, the treatment indicators in the hazard can be defined as follows: $D_a \equiv I(t_u \leq t_{c1})$, $D_{c1} \equiv I(t_{c1} < t_u \leq t_{c2})$, $D_{c2} \equiv I(t_{c2} < t_u \leq t_{c3})$, $D_{c3} \equiv I(t_{c3} < t_u)$, whereby all are conditioned on being in the treatment group.

Let us describe the content of the different treatment effects a bit more in detail: In the early stage of unemployment, from t_0 on, the *(gross) anticipation effect* δ_a is identified, due to the randomised treatment assignment at time t_0. δ_a measures potentially two effects: first and foremost the pre-intervention effect, coming from the fact that the individuals in the treatment group are informed about and assigned to the upcoming targeted coaching program during their first interview at the PES; second, a presumably small additional effect may come from the early-stage intense counseling. Therefore, to be more precise, this treatment effect ought to be described as a gross anticipation effect. δ_{c1} measures the effect of being in the coaching program, identified by allowing for a shift in the hazard at the time of entry into the program, t_{c1}. δ_{c2} measures the post-program effect of the coaching allowing for a further shift at time of program end, t_{c2}. Note that I define t_{c1} and t_{c2} as being being the start and the end of the coaching program plus 14 days each. The reason to do so is that there is a certain delay between having found a job and finally exiting. The 14 days' delay allows to take this into accout, such that successful job findings shortly before start or end of coaching are assigned to the right stage of the treatment. Allowing for more flexibility, I split the post-coaching effect into an earlier one, δ_{c2}, and a later one, δ_{c3}. The latter starts 180 days after end of coaching ($t_{c2} + 166$) and ends at unemployment exit (or censoring).

As a next step, the analysis aims at identifying possibilities of *potential policy improvements* by further targeting the new treatment plan to the *subpopulations where the interventions showed the best results.* This amounts to extending the treatment component $\sum_i \delta_i D_i(t_u)$ to allow for treatment effects for different subpopulations. The nonparametric analysis in section 2.3 showed that there are mainly two dimensions which happen to have a remarkable impact on the size of treatment effects – and are therefore of special interest for targeted policy design. The first dimension is the *timing of the coaching intervention.* As discussed in section 2.3.2, the impact on (early) outcomes changes considerably depending on when the individuals are supposed to enter the coaching program. In order to specifically identify and quantify the change of the anticipatory impact of the coaching announcement on the exit-to-job hazard, I allow the respective treatment effect to differ by time to entry into the program: The anticipation effect component $\delta_a D_a$ is therefore complemented by two incremental effects (interactions with D_a) which measure *early coaching intervention*, defined as time to coaching being smaller than 35 days (median: 19 days), and *late intervention*, which collects cases with time to coaching of 70+ days (median: 102 days).

The second policy improvement analysis looks at *age-dependency* of the treatment effects. Again, the nonparametric analysis in section 2.3.2 reports considerable differences in this dimension. Moreover, given the age-relatedness of the policy issue analysed in this paper, the age-dependency of treatment outcomes is of high interest per se. It is therefore worth to interact each of the subsequent treatment effects in $\sum_i \delta_i D_i(t_u)$ by an age dummy variable which indicates individuals aged 55+. This allows to estimate an increment to each period-specific treatment effect that captures differences in exit-to-job behavior of individuals aged 55+. The cumulation of the respective treatment effect and its 55+-increment (which is reported in the column 'transformations' of the respective estimation tables, see section 2.5.2) yields the treatment effects specific for the older participants.

It is important to point out that the definitions of the treatment effects in the models described above imply that the respective effects are identified by the population who *effectively participated* in the later stage treatment periods (from t_{c1} on). This makes sense here since we are interested in the *effective impact of intensified counseling and coaching on those who really followed it*. However, this makes the period-specific treatment effects subject to potential dynamic selection and endogenous non-compliance biases. Note, though, that the latter issue is very marginal here since only 3.2% of intentional non-compliance was found (see section 2.3.1). These two issues can be handled by introducing unobserved heterogeneity to the model (whereas a second equation to design later treatment entry is not necessary here, see section 2.4.2). This will be further discussed and then analysed in the next section and in section 2.4.4.

However, it can be, in addition, of policy interest how the *gross program effects* in different stages look like. Such an intention-to-treat (ITT) analysis uses in every stage all individuals remaining in unemployment who are *assigned* to the treatment – independently if they really were participating in the later treatment stages[19]. This reflects the total impact of the policy assigned at t_0, given that there is some non-participation. The vast majority of the non-participation is not due to intentional non-compliance, as section 2.3.1 demonstrates, but due to the announcement to have found a job (unemployment exit in some weeks or months) or a temporary subsidized job (remaining in unemployment but not subject to labor market policy during that time), thus due to normal reasons of dynamic selection which apply as well to the control group. This fact, combined

[19]Note that *all* individuals in the treatment group were informed at t_0 about the date for the upcoming coaching program. Thus, I dispose of the exact date of potential coaching entry for all treated individuals. This date is used to determine t_{c1}, t_{c2} and t_{c3} for treated individuals who finally didn't participate in the coaching. For further details, see footnote 5.

with randomisation and ex-ante timing of the treatment plan at t_0, alleviates the potential issue of bias due to endogenous selection. The ITT analysis is reported (following the same sequence of analyses as described above) in the Appendix in Table B3.

2.4.2 The Advantages of Randomisation in Timing-of-Events Models

The design of this program evaluation as a randomised experiment brings a series of advantages in terms of cleanness of the design, clarity of the interpretation and simplified identification of treatment effects effects. In particular, three advantages need to be pointed out: (i) clean identification of the treatment effect starting right at t_0; (ii) avoiding of the no-anticipation assumption due to perfect anticipation; (iii) avoiding of a separate modeling of the inflow into later treatment (coaching). This is discussed in the following.

First, randomisation at t_0 allows for a "clean" identification of the treatment effect that starts right at t_0. This is not possible for non-randomised studies since they cannot distinguish between endogenous selection and the real treatment effect in the first period from t_0 on (Abbring et al. 2005). In contrast, randomised treatment assignment leads to a balanced distribution of unobserved characteristics at t_0. This solves the selection issue at t_0 and allows therefore to identify, in particular, the *anticipation effect*[20] of a later treatment that starts at a $t > t_0$.

Second, randomisation combined with an exogenous timing of treatments and information (timing and characteristics of the treatment plan is revealed to the individuals at t_0) brings as well advantages – simplifications – for the identification of later treatment effects. In the standard case of the timing-of-events approach without randomisation Abbring and Van den Berg (2003) show that the identification of the effect of a treatment starting at $t_1 > t_0$, i.e. a hazard shift at t_1, requires the *no anticipation assumption* which basically implies that the counterfactual hazards (for TG and CG) must be equal up to t_1[21]. In the case here, however, of randomisation and full information at t_0 we encounter a situation of *perfect anticipation*. Since the sample is fully balanced at t_0 (between TG and CG)[22] and, in particular, the TG members have full information about the upcoming treatment periods, they can immediately and transparently act on this information – which is captured, without bias, by the anticipation effect δ_a, estimated over the period from t_0 to t_1 (or to t_{c1} in the specific case of this experiment). Thus, the no anticipation assumption

[20] Note that the pre-coaching-program effect here captures as well the impact of the intensified counseling treatment in the period of t_0 to t_{c1}. See last section.

[21] This could be expressed (in simplified notation) as $\theta^T(\tau_0|x, v_u) = \theta^C(\tau_0|x, v_u)$ where θ^T and θ^C are the counterfactual hazard rates a time $\tau_0 \in]t_0, t_1[$. Note, moreover, that the no anticipation assumption refers in fact to no probabilistic anticipation. Deterministic anticipation, i.e. acting on information which is available to everybody at t_0 (like general monitoring behavior of the PES or generally distributed information on a program etc.), does not break the assumption since this information is equally available for treatment and control group. See Arni et al. (2009) for a further discussion and example.

[22] This condition is necessary to identify effects from t_0, see first point above. For perfect anticipation, though, the presence of full information at t_0 is crucial.

is replaced by measurable *perfect anticipation*[23]. Finally, this full-information-argument carries over to the later treatment periods: Conditional on observables, unobservables, the previous treatment history and full (ex-ante) information about the treatment plan, the anticipation about the treatment in the next period is captured by the treatment effect in the ongoing period.

Third, a further advantage of randomisation and full information at t_0 is that these properties *make the separate modeling (by means of a further equation) of the inflow process into later stage treatment*[24] *unnecessary*. Thus, a control of unobserved heterogeneity is enough to cope with the ongoing dynamic selection. I.e., to cope with the fact that inflow into later treatment stages is not necessarily random any more, since – after the start of treatment at t_0 – the relative proportions of unobserved characteristics may change in a potentially different way in treatment and control group. The explanation for the redundancy of a separate modeling of later stage treatment inflow is the following: Due to randomisation and exogenous, ex-ante timing, the ongoing selection is *uncorrelated* to the propensity to enter the later treatment (coaching), *conditional* on the anticipation effect. In other words, the anticipation effect captures changes (related to early treatment) in the propensity to enter later treatment[25]. Again, this argument carries over to all the later stage treatment parts (D_{c1}, D_{c2}, D_{c3}). Moreover, by the same line of argumentation one can conclude that as well issues of potential non-compliance can be handled in the same, simplified way.

2.4.3 Modeling Post-Unemployment Employment Stability

An analog (M)PH model is set up to estimate the causal impact of the new policy on post-unemployment employment stability. This crucial dimension of post-unemployment jobs is assessed by modeling the recurrence propensity, i.e. the transition rate back into unemployment:

$$\theta_p(t_p|x, M_j, C_k, D_i, v_p) \quad = \quad \lambda_p(t_p) exp(x'\beta_p + \sum_{j=1}^{6} \tau_j M_j + \sum_{k=1}^{11} \gamma C_k + \delta_p D_p + v_p) \tag{2.3}$$

whereby t_p is defined as the duration from the time of transition from unemployment to a job to the time of reentry into unemployment. The transition (or non-censoring) indicator is therefore 1 if a reentry to unemployment is observed up to 1.5 years (540 days) after unemployment exit (exogenous censoring). As in model (2.1), the baseline hazard rate $\lambda_p(t_p)$ adopts the form of a piecewise-constant function[26]. D_p is a dummy variable indicating membership to the treatment

[23] So, more formally, the equality $\theta^T(\tau_0|x, v_u, D_a)\exp(\delta_a) = \theta^C(\tau_0|x, v_u, D_a)$ holds here and describes perfect anticipation – as compared to the no anticipation assumption in footnote 21 (using the same notation as there).

[24] This is the standard approach, as proposed in Abbring et al. (2003), for the timing-of-events model without randomisation.

[25] This means that for our main model (2.1) here the following orthogonality applies: $v_u \perp\!\!\!\perp D_{c1}|x, v_u, D_a$. If this independence is given, no further equation is necessary to model the relation between later treatment inflow and unobserved heterogeneity.

[26] Following the shape of the descriptive hazard, I estimate four intervals with splits at 210/390/480 days. Note,

group. This means that one constant treatment effect[27] is estimated for the post-unemployment period.

It is important to note that equation (2.3) above is estimated on the non-random subsample of individuals who found a job after unemployment. As a consequence, this further endogenous selection process can potentially bias the estimation results of (2.3). Therefore, I apply as well a model that simultaneously estimates (2.1) and (2.3), taking the potential correlation of v_u and v_p into account. This will be discussed in the next section.

2.4.4 Dynamic Selection and Unobserved Heterogeneity

Dynamic selection is a potential issue in the context of this study, even though it is designed as a field experiment. Initially, at t_0, randomisation indeed yields a balanced proportions of unobservable characteristics between treatment and control group at t_0. But as soon as treatment starts, here right after t_0, the balancing potentially gets compromised. This is the case if treatment causes dynamic selection to be *different* in the two groups (if balancing is equal, no problem arises for the identification of later treatment effects). This potential imbalance is taken into account in the timing-of-events models by allowing for *unobserved heterogeneity.* Moreover, section 2.4.2 shows that in our context of randomisation and full information at t_0, controlling for unobserved heterogeneity is sufficient to take into account potentially endogenous selections coming from take-up behavior of later treatment stages and intentional non-compliance.

In the following I will describe how I model unobserved heterogeneity in the case of one process (unemployment) and of two correlated processes (incl. post-unemployment). Then, I will discuss how I iteratively search for the best specification of unobserved heterogeneity by use of grid search and the non-parametric maximum likelihood estimator (NPMLE). Finally, I discuss the found results focusing on the question whether they improved the explanatory value of the models, as compared to their versions without unobserved heterogeneity.

I follow the standard non-parametric way of introducing unobserved heterogeneity which consists in modeling a *discrete mixture distribution for v_u and v_p* (as introduced by Heckman and Singer 1984). To start with, I choose the simplest possible design in that I allow v_u and v_p to have two points of support. This implies the estimation of following probabilities of mass point combinations:

$$p_n = P(v_u = v_u^n) \qquad \text{with} \quad n = 1, 2 \qquad \text{if only process } u \tag{2.4}$$

$$p_j = P(v_u = v_u^n, v_p = v_p^n) \qquad \text{with} \quad j = 1, \dots, 4 \qquad \text{if adding process } p \tag{2.5}$$

moreover, that I define a recurrence event as being at least 20 days out of initial unemployment before reentry. Therefore, the first interval starts at 20 days.

[27] As a sensitivity analysis, I implemented a more flexible specification which allows for a shift of the treatment effect after 270 days. The two estimated treatment effecs were not significantly different in size.

The above probabilities are designed in a logistic form, i.e. $p_n = \frac{exp(a_n)}{1+exp(a_1)}$ for the case (2.4) and $p_j = \frac{exp(a_j)}{1+exp(a_1)+exp(a_2)+exp(a_3)}$ for the case (2.5) (normalising one parameter to being 0). Thus, this implies the additional estimation of maximum two/four probability parameters a_n/a_j and of maximum two/four baseline hazard intercepts λ_0^n/λ_0^j in the 1/2 process/es model, respectively. By allowing for all possible mass points combinations in the latter case of two processes, I model the (potential) correlation of unobservables between the two processes, which is generated by the selective inflow into the post-unemployment employment status.

Combining the unobserved heterogeneity structure (2.4) from above with the main model (2.1) for the first process, I use an iterative procedure to find the optimal locations, proportions and numbers of mass points. This iterative estimation procedure largely follows the implementation of the NPMLE as proposed by Baker and Melino (2000). In the Appendix 2.A I provide a more detailed description of how I implemented the algorithm of grid search and step-wise estimation. The decision criterion to find the optimal model is the highest log likelihood, following the suggestions by Gaure et al. (2007).

This NPMLE procedure applied to (2.1) resulted in suggesting a 2-mass-points model as being the best choice[28]. Grid search for a third mass point (following the procedure by Gaure et al. 2007, see Appendix 2.A) did not provide any specification yielding a higher log likelihood. Estimation of the best 2-mass-points model delivers a log likelihood of -1536.16 – whereas the model *without* unobserved heterogeneity yields a log likelihood of -1455.45 (see Table 2.4). Therefore, the conclusion is that for our 1-process model there is *no gain in explanatory value by adding unobserved heterogeneity*. As a consequence, I report in section 2.5 the models without unobserved heterogeneity.

The same procedure was applied to the *2-processes model*, which combines equations (2.1) and (2.3) with the unobserved heterogeneity specification (2.5). The resulting best-choice-specification is reported as estimation 2 in Table 2.6. Two of the four possible mass point combinations turn out to be non-zero. But again, the log likelihood of -1987.05 is lower than the one resorting from estimation of the 2-processes model without unobserved heterogeneity (log lik of -1455.45+(-459.05)=-1914.5, see Tables 2.4 and 2.6, estimation 1). Thus, the conclusion for the 2-processes model is as well that *no gain in explanatory value by adding unobserved heterogeneity* can be achieved. (Estimation 2 is still reported for comparative reasons.)

Thus, the analysis of unobserved heterogeneity models reveals that the *size of imbalance in unobservables due to dynamic and endogenous selection is statistically not relevant here. Therefore, the models without unobserved heterogeneity can be interpreted causally.* There are different possible reasons for the non-importance of unobserved heterogeneity in the context of this study. First, the tight sampling criteria applied in the preselection into the sample may have avoided the generation of too big imbalances over the course of treatment: Individuals are in the same age

[28] Results of the grid search and unobserved heterogeneity estimations are available on request.

group, in the same labor market, comparable in terms of employability and in terms of skills. Second, the selection caused by the found treatment effects by period could be of a balanced nature: i.e., the individuals who found a job due to the program are not fundamentally different from the job finders in the control group. Finally, it is not completely excludable that the non-identification of further mass points may be due to the small sample size. However, this is not very probable since Monte Carlo simulations in Baker and Melino (2000) have shown that it is well possible to identify several mass points with 500 observations.

2.5 Results of the Econometric Model

This section aims at providing insights about the *specific impact patterns over time* caused by the new policy. Whereas the nonparametric analysis in section2.3 is very suitable to analyse the total or net effects of the whole policy, more econometric structure is needed to identify a series of dynamic treatment effects by treatment period and in the post-unemployment time. This is done by use of the models outlined in section 2.4. In the following, I report and discuss the results of the series of duration models which are described there. They follow three questions: (1) How does the outcome dynamics caused by the new policy look like by treatment period? (section 2.5.1) (2) How can the policy effect be improved by targeting the interventions on certain subpopulations? (section 2.5.2) (3) How did the quality of found jobs react on the treatment? Does the policy pay off for unemployment insurance? (section 2.5.3)

2.5.1 The Treatment Effects in Different Treatment Periods

At the start of this analysis of treatment effects by treatment period a glance shall be thrown on the *baseline model* which estimates the total effect of the program on duration to job finding (see section 2.4.1 for the model setups). This is, thus, the semi-parametric version of the non-parametric analysis of unemployment duration, and serves as a baseline benchmark. Table 2.3 reports the results. When only allowing for one constant, permanent treatment effect (δ_b), a zero effect of the treatment plan on the duration outcome is found. This zero effect clearly reflects the non-parametric result from the means and median comparisons between treatment group (TG) and control group (CG). Note, however, that results are not exactly comparable since this semi-parametric model presents a treatment effect averaged over time and puts therefore relatively more weight on early results (as the proportion of exits in the first 5 months is high, see Figure 2.1). The non-parametric survivor analysis, on the other hand, is more flexible in the sense that it exactly reports the survivor differences at every point in time. Therefore, the positive effect on job finding – which kicks in after some time – only gets visible in the survivor analysis (see Figure 2.5), but not in this baseline duration model. We need, thus, a split-up in treatment periods in order to get more specific insights.

Before doing so, let's complete the baseline picture by a short look at the role of the control variables and the fit of the baseline hazard estimation. The most prominent role among sociodemographic impact factors for job finding plays age. Not very surprisingly, the difference in the exit to job rate between individuals aged 45-49 and those aged above 55 is important. Moreover, female job seekers are relatively more successful (or quicker) in finding a job[29].

[29] Note that also the 15% significance level is reported in this paper. This is done because of the small sample size which generates relatively higher standard errors. Due to this fact, treatment effects must be of big size anyway in order to become significant at that sample size. Therefore, this further significance level seems justified.

Table 2.3: The total/net effect of the new policy on duration to job finding. (PH duration model)

	Destination: exit to job		
	coeff.	s.e.	transf.
Treatment effect			
Total effect (δ_t/in %)	-0.024	0.168	-0.024
Exit rate from unemployment			
$\lambda_b/exp(u_b)$, 1-50 days	-6.532***	0.442	6.44
$\lambda_1/exp(u_1)$, 51-100 days	0.823***	0.236	14.67
$\lambda_2/exp(u_2)$, 101-150 days	0.802***	0.250	14.37
$\lambda_3/exp(u_3)$, 151-250 days	0.214	0.260	7.98
$\lambda_4/exp(u_4)$, 251-400 days	0.162	0.283	7.57
$\lambda_5/exp(u_5)$, 401-550 days	-0.413	0.381	4.26
$\lambda_6/exp(u_6)$, 551+ days	-1.010°	0.633	2.35
Control variables			
UE duration in past 3 years	0.000	0.001	0.000
duration until availability	-0.001	0.003	-0.001
age: 50-54 (base: 45-49)	-0.336*	0.202	-0.286
age: 55-59	-0.657***	0.207	-0.482
age: 60+	-1.481***	0.354	-0.772
married (base: unmarried)	0.136	0.199	0.146
divorced	0.062	0.242	0.064
female	0.361°	0.243	0.434
non-Swiss	0.308	0.260	0.360
low employability (base: medium)	0.419°	0.289	0.521
semi-skilled (base: skilled)	-0.041	0.393	-0.040
unskilled	0.112	0.547	0.118
non-German-speaking	-0.012	0.340	-0.011
1 foreign language (base: 0)	-0.126	0.254	-0.118
2+ foreign languages	0.177	0.285	0.194
PES 2 (base: PES 1)	0.194	0.516	0.214
management (base: professionals)	-0.293	0.408	-0.254
support function	-0.076	0.546	-0.073
part-time (but above 50%)	0.246	0.232	0.279
occupations (base: office, accounting):			
Blue-collar manufacturing, construction	-0.298	0.277	-0.258
Engineers, technicians, Informatics	-0.429	0.333	-0.349
Enterpreneurs, marketing, banking, insurance	-0.536°	0.351	-0.415
Sales	0.166	0.332	0.180
Gastronomy, housekeeping, personal service	-0.117	0.364	-0.110
Science & arts, education, ealth occupations	0.061	0.326	0.063
Rest (mainly unskilled workers, helpers)	-0.345	0.398	-0.292
Month of entry in UE (base: Jan/Feb 2008):			
March/April 2008	-0.406	0.298	-0.334
May/June 2008	0.070	0.264	0.072
July/August 2008	-0.016	0.282	-0.016
Sept/Oct 2008	-0.019	0.267	-0.019
Nov/Dec 2008	-0.121	0.322	-0.114
Caseworker fixed effects (base: CW 1):			
CW 2	0.846**	0.415	1.329
CW 3	0.717*	0.418	1.049
CW 4	0.782**	0.393	1.186
CW 5	0.686°	0.424	0.985
CW 6	0.838**	0.376	1.311
CW 7	0.932**	0.417	1.539
CW 8	0.575°	0.391	0.777
CW 9	0.603	0.663	0.828
CW 10	0.338	0.751	0.403
CW: rest (smaller charges)	0.859*	0.478	1.360
Unobserved heterogeneity		No	
-Log-Likelihood		1468.01	
AIC		1517.01	
N		327	

Notes: Coefficients and their transformations are reported: Transformed treatment effects are changes in %. Transition rates are in % per month (for the respective piece of the hazard); note that λ_b is the intercept of the baseline hazard, the further steps are incremental; the transformations are calculated for an "average" individual: $u_j = \lambda_b + \lambda_j + \bar{x}'\beta_j + \sum_i \tau_i \bar{M}_i + \sum_k \gamma_k C_k$ where $j = 1, \ldots, 6$ ($\lambda_j = 0$ for first segment) and the bars are means, except for the past unemployment and the duration until availability where medians are used. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, $^\circ$ $p < 0.15$.
Source: Own estimations based on merged UIR-LZAR database.

Individuals of low employability[30] have, interestingly, a higher exit to job hazard rate. Moreover, caseworker fixed effects turn out to be of sizable importance: Since caseworkers are assigned by occupation (see section 2.2.3)[31], these effects reflect occupation-specific job chances – besides caseworker-specific differences in success in giving job finding support. The fact that not so many control variables are statistically significant may be partially explained by the relatively high homogeneity of the experimental population (similar age and employability, same labor market etc). Finally, when looking at the piecewise-constant baseline hazard rates for an "average" individual (see Notes of the Table 2.3 for the specific calculation) one may conclude that the estimation very appropriately fits the shape of the empirical hazard (see Figure 2.1). Over the different duration pieces, the monthly unemployment exit rate goes from 6.4% to about 15% and then down to 8% and less from 151 days on.

Table 2.4: Effects of the treatment plan on the exit to job rate. (PH duration model)

	Destination: exit to job		
	coeff.	s.e.	transf.
Treatment effects			
Anticipation effect (δ_a/in %)	-0.499**	0.236	-0.393
During coaching (δ_{c1}/in %)	-0.477°	0.309	-0.379
Post-coaching, 14-180 days (δ_{c2}/in %)	-0.023	0.250	-0.023
Post-Coaching, 181+ days (δ_{c3}/in %)	0.401	0.374	0.494
Control variables		Yes	
Unobserved heterogeneity		No	
-Log-Likelihood		1455.45	
AIC		1508.45	
N		327	

Notes: Coefficients and their transformations are reported: Transformed treatment effects are changes in %. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$.
Source: Own estimations based on merged UIR-LZAR database.

How do the *specific treatment effects by treatment period* look like? Table 2.4 reports these results which are based on model (2.1) with the same control variables. The dynamics of the treatment effects reveals indeed a pattern which was not yet visible in the nonparametric analysis (due to overlaps of treatment periods): The found zero effect on unemployment duration was, in fact, generated by the interplay of a period of lower exit rates, followed by one of higher exit rates. The *anticipation effect* (δ_a) is highly significantly negative. Treated individuals have an on average 37.6% ($= exp(\delta_a - 1)$ lower unemployment exit rate in the period between unemployment inflow and (potential) coaching entry. Thus, the prospect of being coached obviously results in a

[30]The employability rating is assessed by the PES employees at the time of registration. Here, the initial population only consists of individuals of employability medium and low; see section 2.2.3 for the sampling before randomisation.

[31]Note that this assignment rule implies that caseworker fixed effects and occupation dummies are quite highly correlated; this may explain why the latter are not significant. Further, note that I added a PES fixed effect since it is not fully collinear with the caseworker fixed effects. The reason is that the rest category of the latter contains individuals of both PES. Moreover, the PES fixed effect captures any potential differences originating from the fact that randomisation was done within each PES and that PES 2 entered the project later (June 2008).

smaller propensity to exit early to a job. The treated people seem to expect a positive outcome or at least some helpful support of the coaching program. Therefore, one may call this negative anticipation effect an *"attraction effect"* – as an opposite to the commonly found "threat effect" in the analysis of other kinds of programs (see e.g. Rosholm and Svarer 2008, and introduction of this paper). If this waiting behavior is rather driven by a smaller job search effort or by being more picky in accepting jobs, will be analysed in the companion paper Arni (2011) using the repeated survey data.

In the next treatment period, during coaching, a (slightly) significantly negative impact on exit rates is found as well. Thus, the commonly found *lock-in effect* is present here as well. Individuals participating in the coaching program do not exert the same job search effort than without coaching, presumably due to the high work load of the program. However, the effect is restricted to the short time span of the duration of the coaching (60 days in median) – right after, the treatment effect is already back to zero (δ_{c2}). Thus, the coaching design principle 'intense but short' turns out to be beneficial in restricting the lock-in effect.

Six months after the end of coaching, the treatment effect (for the coached individuals, δ_{c3}) reveals to be clearly positive but insignificant. The higher exit rate to a job of the coached reflects the insight of the nonparametric analysis that in later stages of the unemployment the positive impact of the new program kicks in. However, since the exits to job are quite dispersed over time (given the small sample) beyond 181+ days after coaching, the estimated δ_{c3} gets "averaged out" and therefore not that big – compared to the cross-sectionally measured significant effect on job finding proportions (see Table2.2). Note, in addition, that the standard error of δ_{c3} is comparably high due to the small sample size remaining at this late stage of unemployment. Moreover, it is interesting to consider as well the ITT analysis of the post-coaching effects. The ITT post-coaching effect beyond 180 days (δ_{c3}), reported in Table B3 in the Appendix, turns out to be higher than the specific one and to become significant. The ITT effects encompass the whole treatment group, thus as well the non-coached TG participants. These are individuals (except from the 3.2% non-compliers, see section 2.3.1) who announced in the period before coaching to have found a job or a temporary subsidised job[32]. So, they show by default (dynamic selection) a higher exit rate, but note that this kind of dynamic selection (and the availability of temporary subsidised jobs) is present as well in the control group. Thus, the interpretation of the higher post-coaching effect is that the intensified counseling led to additional job findings, beyond the coaching.

Finally, a glance at the results for the corresponding models for unemployment exit – see Tables B4 and B5 in the Appendix – shows that the treatment effects are very comparable to the exit-to-job analysis from before. The only salient difference is that the post-coaching effects are

[32]Going into a temporary subsidised job is not considered as an exit from unemployment. However, these kinds of jobs increase chances to find a non-subsidised employment (i.e. unemployment exit) thereafter, see e.g. Lalive et al. (2008) for the Swiss labor market.

weaker and always insignificant (treatment-specific and ITT). This reflects the result found in the nonparametric analysis (see section 2.3.2) that the treatment caused more individuals to exit to a job instead of exiting to non-employment (which is in these models here considered as an exit).

So, wrapping up, one can state that the nonparametric result of more job finding can be decomposed in this analysis into an *attraction effect and coaching lock-in which prolong unemployment duration, whereas in the post-coaching period exits to jobs increase, but in a dispersed (and therefore insignificant) way. Short: more treated individuals exit to a job, but they are not quicker in doing it, in terms of unemployment duration.*

2.5.2 How Can the Policy Design Be Further Improved?

In the following, I want to explore *how the positive impacts of the new policy can be improved by optimizing its design* – either through optimized timing of the interventions or through targeting to a subpopulation where the policy shows most effect. Two approaches will be analysed: First, can the unemployment exit behavior be optimized by intervening (even) earlier with the coaching intervention? Second, can the later treatment effects be improved by targeting the policy to some more specific age groups?

The answer to the first question has two aspects. With respect to avoiding the duration-prolonging attraction effect[33], early intervention is clearly successful. Table 2.5 reports that individuals who entered in median 30 days earlier into coaching show a hazard rate which is significantly higher than the negative anticipation effect of the average treatment group (in median 50 days to coaching). Thus, the *negative anticipation effect is significantly undone* by intervening earlier with coaching. Intervening later (subgroup 70+ days, median 102 days to coaching), in the opposite, does barely change the size of the attraction effect. This result is shown graphically as well in the hazard rate plots by anticipation groups in Figure 2.6b and 2.6c. Note that these hazard calculations are censored at the, real or potential, coaching entry – they thus only represent anticipation behavior. The figures reveal that beyond 20 days the exit rates increase in the control group, whereas they do not in the median and long anticipation duration subgroups of the TG. This generates the negative hazard differences as shown in Figure 2.6b. Finally, I perform a sensitivity analysis on potential endogeneity of prolonged anticipation durations[34]. It shows no impact of potential postponement behavior, thus the above-used anticipation variation can indeed

[33] This does not (forcefully) mean that the coaching is not attractive for individuals who ought to participate in the program very early. It simply means that the negative effect on the hazard due to program attractiveness has not yet been developed.

[34] As discussed in section 2.3.1, the exogeneity of the coaching timing mechanism could be compromised by: duration to availability (i.e. being in cancellation period), a temporary subsidized job, calling in sick. By comparing real and potential coaching entry time (see footnote 5 for more on the latter), I identify 20 cases where they differ more than just a couple of days (natural break at ≤ 11 days; considered cases have delays of ≥ 45 days). Excluding them from the hazard calculation does barely change the mentioned hazard figures. Most probably, the delays are mainly due to administrative reasons (overbooking of the program, holidays from UI obligations).

Table 2.5: Change of the anticipation effect as a function of time to coaching intervention. And age-specific treatment effects: age 45-54 vs age 55+. (PH duration models)

	Destination: exit to job coeff.	s.e.	transf.
Anticipation effect by time to program			
Anticipation effect (δ_a/in %)	-0.582°	0.373	-0.441
... duration < 35 days [1)]	0.994*	0.571	0.510
... duration 70+ days [1)]	-0.104	0.472	-0.496
During coaching (δ_{c1}/in %)	-0.492°	0.311	-0.388
Post-coaching, 14-180 days (δ_{c2}/in %)	-0.026	0.251	-0.026
Post-Coaching, 181+ days (δ_{c3}/in %)	0.419	0.377	0.521
Control variables		Yes	
Unobserved heterogeneity		No	
-Log-Likelihood		1453.22	
N		327	
Age-specific treatment effects			
Anticipation effect (δ_a/in %)	-0.571**	0.288	-0.435
... for age 55+ [2)]	0.289	0.511	-0.246
During coaching (δ_{c1}/in %)	-0.783**	0.392	-0.543
... for age 55+ [2)]	1.060*	0.649	0.319
Post-coaching, 14-180 days (δ_{c2}/in %)	0.066	0.279	0.069
... for age 55+ [2)]	-0.381	0.559	-0.270
Post-Coaching, 181+ days (δ_{c3}/in %)	0.620°	0.416	0.859
... for age 55+ [2)]	-0.697	0.655	-0.074
Control variables		Yes	
Unobserved heterogeneity		No	
-Log-Likelihood		1452.12	
N		327	

Notes: Coefficients and their transformations are reported: Transformed treatment effects are changes in %. 1) Note that these anticipation sub-group coefficients are incremental to the main anticipation effect; the transformation into changes in %, though, contains the sum, i.e. $exp(\delta_a + \delta_{a,d}) - 1$ where $d \in \{< 35, 70+\}$. 2) Note that these age 55+-specific effects are incremental to the respective treatment effects above which apply to individuals aged 45-54; the transformation into changes in %, though, contains the sum, i.e. $exp(\delta_j + \delta_{j,55+}) - 1$ where $j \in \{a, c1, c2, c3\}$. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$.
Source: Own estimations based on merged UIR-LZAR database.

be considered as being exogenous.

The second aspect of the early intervention question is whether it reduces total unemployment duration. This has been discussed in section 2.3.2. The nonparametric results there show that a move from a median (long) anticipation duration policy to a short anticipation policy yield a reduction of unemployment by 9.2 (48.3) days, which is not significant. The detailed survivor analysis in Figure 2.6a reveals that earlier exit from coaching could obviously not be translated into earlier job finding (see section 2.3.2 for more details). Taking the two aspects of the early intervention question together the policy conclusion could thus be the following: *The earlier intervention strategy works in the sense that it eliminates the duration-prolonging aspects of the attraction effect. But in order to significantly reduce unemployment duration, additional policy* *measures would be necessary which are able to translate the earlier coaching exit into earlier job finding.* An option would be to even more intensify guidance around the end of coaching, e.g. through (more) intensified counseling and probably monitoring.

The second policy experiment focuses on further *age-targeting* of the new policy. Do individuals below and above age 55 react in the same way to the interventions? They do not, as the nonparametric analysis in section 2.3.2 already showed. Whereas the new policy causes a zero effect on the unemployment duration of individuals aged 45-55, the median unemployment duration of people aged 55+ significantly increases. This is analysed more in detail in the age-specific treatment effects model in Table 2.5. It reveals that for the individuals of age 55+ (i.e., adding the increments) the attraction effect is reduced to being insignificant, the during coaching lock-in effect vanishes, and the post-coaching effects never get positive. This behavioral pattern is consistent with the people of age 55+ believing less in the success of (this type of) coaching. This belief seemingly reflects in the anticipation and the missing lock-in behavior (less time investment in coaching); after coaching, the non-success belief seems to be realised. The age-specific analysis shows, on the other hand, that for the individuals aged 45-55 the positive effect of the policy beyond 180 days post coaching is higher and gets significant (at 15% treatment-specific, at 10% ITT, see Table B3 in Appendix). Therefore, the new policy is more suitable for the age group 45-55 than beyond.

Thus, if unemployment insurance has a restricted budget to invest in coaching and counseling programs of the form tested here, a further targeting of the new policy on the age group 45-55 is an option. However, this statement is conditional on the content of coaching and counseling. The coaching as performed here has set one focus, among others, on developing ideas on reorientations of job search (in other occupations, geographic regions etc.); possibly, individuals beyond 55 did not see any perspective of reorientation any more. More generally speaking, the content for supportive programs for people aged 55+ should be more directly targeted on job market issues regarding that age group.

2.5.3 What about the Quality of Found Jobs? Does the Program Pay Off?

What does the result that more treated individuals found a job mean for the quality of the found jobs? Did it go down? First nonparametric evidence on mean comparisons of gross salaries and recurrence to unemployment suggests a clear no. Monthly salaries' levels turn out te be equal in treatment and control group, the recurrence propensity in the treatment group is even lower (difference not significant). These results are based on the subsample of individuals who found a job at unemployment exit, thus this implies potentially endogenous selectivity. Therefore it is important to analyse these two dimensions of job quality under control for observables and unobservables.

First, I check whether the inclusion of the available observables into a (OLS) regression changes the result of no salary difference. This is not the case, the comparison of conditional means results as well in *no significant difference of monthly salaries realised after unemployment exit*[35]. Checking for unobservables is possible in the context of a duration model, thus for the recurrence dimension of job quality. This inclusion of unobserved heterogeneity has been done in the form of the 2-process model described in section 2.4, which simultaneously estimates the unemployment exit-to-job process and the recurrence to unemployment process. The results, discussed in section 2.4.4 and Table 2.6, showed that including unobserved heterogeneity does not increase the explanatory value of the model. Due to this insignificant importance of heterogeneity, the best choice is to use the specification without unobserved heterogeneity for the final analysis. For the sake of completeness, however, both versions of the model are reported in Table 2.6.

Table 2.6: Employment stability: Effect of new policy on reentry rate into unemployment (20–540 days after UE); sensitivity analysis: model with unobserved heterogeneity

	1: Employment stability			*2: Both processes: UE & post-UE*		
	coeff.	s.e.	transf.	coeff.	s.e.	transf.
Treatment effects						
Unemployment reentry (δ_p/in %)	**-0.590***	**0.341**	**-0.446**	**-0.629°**	**0.408**	**-0.467**
Anticipation effect (δ_a/in %)				-0.865**	0.353	-0.579
During coaching (δ_{c1}/in %)				-0.696°	0.431	-0.502
Post-coaching, 14-180 days (δ_{c2}/in %)				-0.222	0.325	-0.199
Post-Coaching, 181+ days (δ_{c3}/in %)				0.247	0.393	0.280
Reentry rate into unemployment						
$\lambda_{b,a}/exp(u_{b,a})$, 20-210 days	-6.112***	0.834	2.58	-7.344***	1.107	0.973
$\lambda_{b,b}/exp(u_{b,b})$				-5.859***	1.001	4.298
$\lambda_1/exp(u_{1,a})$, 211-390 days	-0.152	0.406	2.22	-0.094	0.417	0.886
$exp(u_{1,b})$						3.912
$\lambda_2/exp(u_{2,a})$, 391-480 days	-1.257°	0.798	0.73	-1.234	0.981	0.283
$exp(u_{2,b})$						1.252
$\lambda_3/exp(u_{3,a})$, 481+ days	-0.404	0.818	1.72	-0.438	1.020	0.628
$exp(u_{3,b})$						2.773
Probabilities:						
p_1 (type aa)				0.644	0.036	
p_4 (type bb)				0.356	–	
Unobserved heterogeneity		No			Yes	
All control variables UE process		–			Yes	
-Log-Likelihood		459.05			1987.05	
AIC		496.05			2080.05	
N UE/N post-UE		–/234			327/234	

Notes: Coefficients and their transformations are reported: Transformed coefficients are changes in %. Transition rates are in % per month (for the respective piece of the hazard). Note that λ_b is the intercept of the baseline hazards, the further steps are incremental; the transformations represent the monthly transition rate for an "average" individual: $u_{j,g} = \lambda_{b,g} + \lambda_j + \bar{x}'\beta_j + \sum_i \tau_i \bar{M}_i + \sum_k \gamma_k \bar{C}_k$ where $j = 1, \ldots, 6$ and $g \in \{a, b\}$ ($\lambda_j = 0$ for first segment) and the bars are means, except for the past unemployment and the duration until availability where medians are used. (post-)UE=(post-)unemployment. Probabilities: Model with 4 mass points whereby $p_2 = p_3 = 0$ is optimal; type aa=baseline hazards a in UE and post-UE, type bb=baseline hazards b in UE and post-UE. Note that in the post-UE process the occupation variables and the ones for non-German speaking and for support function are omitted (due to high collinearity to comparable variables) in order to avoid overparametrisation. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$.
Source: Own estimations based on merged UIR-LZAR database.

[35] Regression table is not reported but available on request.

The post-unemployment survivor analysis in Figure 2.7 and the means comparison of recurrence rates (see section 2.3.2) suggested a result of better employment stability in the treatment group over 1.5 years beyond unemployment. The means comparison did, however, not get significant. How does this picture change when controlling for observables and explicitly modeling post-unemployment duration? Table 2.6 (estimation 1) reveals that this results in a *significantly positive treatment effect on employment stability* over 1.5 years after unemployment[36]. This result will be further quantified (in terms of avoided unemployment) below. A glance on estimation 2 of Table 2.6, which features unobserved heterogeneity but has less explanatory value, shows that the result on employment stability does qualitatively not change; the treatment effect gets slightly stronger.

Using the estimation results in Table 2.6 it is possible to quantify the positive impact of the new policy on employment stability in terms of avoided future unemployment duration. This amounts to calculating the expected values of the post-unemployment duration t_p for the two counterfactuals. The difference between the two yields the average treatment effect on the treated (ATET) in terms of t_p, i.e. the not realised future unemployment (in days) due to the treatment (within 1.5 years after the original unemployment spell). Using the estimation model developed in section 2.4.3 and estimated in Table 2.6, estimation 1, I simulate the following equation which describes the density of post-unemployment employment durations:

$$f_p^D(t_p|x, v_p) = \theta_p^D(t_p|x, v_p) S_p^D(t_p|x, v_p)$$

whereby $D \in \{T, C\}$ indicates the treatment status, i.e. the two counterfactuals. θ_p represents the hazard derived in equation (2.3) in section 2.4.3 (whereby x comprises as well the inflow month- and caseworker dummies), S_p is the corresponding survivor function. Based on this density, the expected value of the employment duration can be calculated as

$$E(t_p|x, v, D_p) = \int_{20}^{\eta} t_p \, f_p^D(t_p|x, v_p) dt_p + \left[1 - \int_{20}^{\eta} f_p^D(t_p|x, v_p) dt_p\right] \cdot \eta \tag{2.6}$$

This equation takes into account that the employment durations are exogenously censored on March 31th 2010 (last data availability) or after 540 days (1.5 years)[37], this is described by the parameter η.

This simulation is run twice, for the two counterfactuals of being treated or not. It yields the $ATET = E(t_p|x, v, T) - E(t_p|x, v, C)$. The result of this calculation is that, on average, *treated individuals avoid future unemployment of 23.16 days.* These not incurred unemployment days

[36] I estimated as well a model which splits the treatment effect at 270 days after unemployment. This didn't yield statistically tractable differences in the effect size.

[37] For more details on the empirical issues with respect to θ_p (censoring, baseline hazard splits, 20 days threshold), see section 2.4.3 and footnote 26.

represent direct savings for the unemployment insurance (UI) accounts. Based on this quantification of the direct benefit of UI, I perform a cost-benefit accounting calculation in order to assess whether the investment in the new program pays off for the UI or not.

Table 2.7: Analysis of costs vs benefits of new policy for the UI accounts: avoided future unemployment vs. additional program cost; in CHF per job seeker in treatment group (TG)

Benefits		**Cost**	
		Additional cost of new program (compared to status quo):	
Average increase of duration until reentry into unemployment (up to 540 days after UE)	23.16 days	Coaching seminar instead of short job search assistance sequence	4500 CHF
... times average daily benefit rights	189.43 CHF	... times proportion of coaching participants in TG	53.80%
		Cost for additional counseling	115.38 CHF
Total savings for UI	**4387.01 CHF**	Total additional cost for UI	**2534.74 CHF**

Savings per job seeker due to avoided future unemployment: 1852.27 CHF

Notes: Average duration of avoided future unemployment is calculated by means of the simulation described in the respective section of the text. Average daily benefit rights are calculated according to the legal rules, based on the salary information in the survey. The calculation of the cost for additional counseling is based on the following data: Assume 100 cases per caseworker; median unemployment duration is 140 days; caseworkers in the new program got a reduction of the caseload by 20%; this results in a caseload reduction to 208 instead of 260 job seekers per year; this caseload reduction is multiplied by the average employment cost of a caseworker per year. 1 CHF=0.766 EUR. UI=unemployment insurance, UE=unemployment.
Source: Own calculations based on merged UIR-LZAR database.

Table 2.7 provides the details on this cost-benefit analysis for the UI accounts. Based on the data available and additional cost information by the PES administration, I can perform a detailed calculation of the additional cost of the new policy (as compared to the status quo[38]). The cost-benefit analysis yields a clearly positive result: *The avoided future unemployment pays the additional cost of the new program more than fully, specifically it covers 1.73 times the additional cost.*

Summing up the post unemployment results, one can draw a clearly positive conclusion: Due to the new policy, more individuals found a job, at the same salary level as the control group. In terms of employment stability, the quality of the jobs of the treated are better than the control group's: they show, on average, lower recurrence propensity into future unemployment. This constitutes savings for the unemployment insurance which more than pay off the additional program costs.

[38]The status quo for the control group during the first four months of unemployment (policy implementation span) is monthly counseling and a short, standard job search assistance workshop. For details see section 2.2.3.

2.6 Conclusion

This paper evaluates a new social experiment which implements a novel active labour market policy (ALMP) intervention in Switzerland that explicitly focuses on the job seekers of age 45+ and lower employability. This group faces (potentially) the highest risk of falling into the trap of long-term unemployment. The evaluated treatment plan is specifically targeted to this risk group and features two highly intense supportive treatments: high-frequency counseling (every second week, double intensity than normal) and an intense coaching program of 54 days in small groups that focuses on job search strategy, employability development, self-marketing and reorientation strategies. As a principle, the new policy intervenes early in the unemployment spell: High-frequency counseling starts right from the beginning on (and lasts four months), coaching on average after 50 days. The timing schedule of the treatment plan was fixed ex ante, which allows identification of detailed treatment effects. The evaluation relies on a unique database which combines rich register data with surveys of participants.

This new supportive labor market policy causes significantly positive treatment effects in the longer run and avoids too strong lock-ins in the shorter run. The results of the field experiment can be summarized in five main points. *First, the effect of the treatment plan on unemployment duration is zero.* Unlike the standard result found in evaluations of supportive labor market policy (training etc.), the lock-in effect (job seekers search less during the program due to high occupation) is not so dominant here such that it would prolong unemployment duration. The decomposition of the treatment effect shows countervailing tendencies: I find an *"attraction effect"* before coaching and the typical lock-in effect during coaching which prolong unemployment duration. The attraction effect is a phenomenon which has been rarely reported in the literature so far: It is the opposite of the more typical threat effect. Thereafter, more and more the positive impacts of coaching and intensified counseling on the job finding propensity kick in.

Second, significantly more individuals find a job in the treatment group. The job finding proportion is 9 percentage points higher in the treatment group. Thus, the procedure of job finding does not get accelerated by this new policy (due to coaching), but success is higher: The higher job finding proportion goes together with less exits to non-employment destinations in the treatment group. *Third, the more frequent job finding is not related to a job quality decrease* – first monthly salaries after unemployment are at the same levels, on average, for treated and controls. The new ALMP shows as well positive impacts in the longer run post unemployment period: *Fourth, employment stability is higher over the 1.5 years after unemployment exit* in the treatment group. A respective duration model finds a significantly lower recurrence rate to unemployment. *Fifth, the new policy pays off for unemployment insurance.* The counterfactual simulation of the mentioned model shows that, on average, the treated individuals generate *23 days less of future unemployment* (during the 1.5 years of post-unemployment observation period). This compensates more than 1.5 times the (high) additional program cost for, in particular, coaching and intensified counseling.

Moreover, I use the structure of the analysis to perform two policy experiments in order to assess potential improvements in policy design or through policy targeting. As a first test, the ex ante given timing schedule of coaching allows to assess whether (even) earlier coaching intervention would have further improved policy outcomes. I find that by starting coaching 30 days earlier the negative impact of the attraction effect on the exit hazard indeed vanishes, but this does not translate in further unemployment duration reduction through earlier job finding. For that, a further support (and/or monitoring) of earlier coached individuals would be required, presumably. The second policy experiment reveals that the subgroup of individuals aged 55+ does not show the above-mentioned distinct reaction pattern on the interventions; whereas the people aged 45-55 show a higher positive post-coaching effect. So, this form (content) of coaching and intensified counseling seems more suitable for the age group 45 to 55.

Some discussion of the external validity of this field experiment may be of interest. Generalizations to a larger population need to be made with care, given the relatively small sample size and the focus on one geographical region (Northern Switzerland), but they are possible given the right context. The region of the field experiment is a good representative of the Zurich labor market, which is the biggest (population: 3.7 million) in Switzerland; it represents one example of a strong, central high-productivity labor market in Europe. An exploratory matching exercise shows, moreover, high comparability of the drawn sample with neighboring (semi-urbanised) PES regions[39]. Most importantly, this field experiment is to be seen as a test of a new combination of labor market policy mechanisms. The main policy question of general interest is: Is it possible to design a supportive policy strategy, targeted on older job seekers, which improves their employability without prolonging unemployment?

Based on that question, mainly the following policy elements may be put forward as possible recommendations for targeted policy design: First, for supportive policy programs, the principle 'early and short but intense' seems beneficial. Given the result that it takes some time until such a coaching & counseling measure generates job finding success, early intervention makes sense. If the program is, in addition, attractive, early intervention helps reducing negative pre-program effects. The intense design of coaching (or training) helps restricting the lock-in effect. Second, it can pay off to invest in supportive policy measures for job seekers if they are targeted enough (in age, content) and strictly implemented. The above policy experiment suggests, however, that targeting the content to the age-specific issues is a key issue. It turns out, as well, that such a type of policy is not compatible with a policy aim that strictly focuses on unemployment duration reduction.

Finally, the positive impact of this coaching & counseling strategy on job finding raises the question about which behavioral elements have been driving the outcome. The companion paper,

[39] Details on the matching execise are available on request.

Arni (2011), analyses this issue using repeated survey evidence. Thus, the results of this field experiment call for more research in behavioral labor market policy design, in order to uncover behavioral mechanisms which can be targeted by precise and efficient policy interventions.

References

Abbring, Jaap H., and Gerard van den Berg (2005). "Social Experiments and Instrumental Variables with Duration Outcomes," *Tinbergen Institute Discussion Papers 05-047/3*, Tinbergen Institute.

Abbring, Jaap H., Van den Berg, Gerard J., and Jan C. van Ours (2005). "The Effect of Unemployment Insurance Sanctions on the Transition Rate from Unemployment to Employment." *Economic Journal*, 115, 602-630.

Abbring, Jaap H., and Gerard van den Berg (2003). "The Non-Parametric Identification of Treatment Effects in Duration Models." *Econometrica*, 71, 1491-1517.

AMOSA (2007). "Langzeitarbeitslosigkeit - Situation und Massnahmen", Arbeitsmarktbeobachtung Ostschweiz, Aargau und Zug (AMOSA), Zurich.

Arni, Patrick (2011). "What's in the Blackbox? A Field Experiment on the Impact of Labor Market Policy on Search Behavior and Beliefs", working paper, University of Lausanne.

Arni, Patrick, Lalive, Rafael, and Jan C. van Ours (2009). "How Effective Are Unemployment Benefit Sanctions? Looking Beyond Unemployment Exit", *IZA Discussion Paper 4509*.

Ashenfelter, Orley, Ashmore, David, and Olivier Deschênes (2005). "Do Unemployment Insurance Recipients Actively Seek Work? Randomized Trials in Four U.S. States." *Journal of Econometrics*, 125, 53-75.

Baker, Michael and Angelo Melino (2000). "Duration dependence and nonparametric heterogeneity: A Monte Carlo Study." *Journal of Econometrics*, 96, 357-393.

Black, Dan A., Smith, Jeffrey A., Berger, Mark C. and Brett J. Noel (2003). "Is the Threat of Training More Effective than Training Itself? Evidence from Random Assignments in the UI system." *American Economic Review*, 93, 1313-1327.

Card, David, Kluve, Jochen and Andrea Weber (2009). "Active Labor Market Policy Evaluations: A Meta-analysis", *Austrian Center for Labor and Welfare State Working Paper No 0902*, Linz.

Crépon, Bruno, Ferracci, Marc, Jolivet, Grégory and Van den Berg, Gerard J. (2010). "Analyzing the Anticipation of Treatments Using Data on Notification Dates," *IZA Discussion Paper 5265*, Institute for the Study of Labor (IZA).

Gaure, Simen, Røed, Knut and Tao Zhang (2007). "Time and Causality: A Monte Carlo Assessment of the Timing-of-events Approach", *Journal of Econometrics*, 141(2), 1159-1195.

Gerfin, Michael and Michael Lechner (2002). "A Microeconometric Evaluation of the Active Labour Market Policy in Switzerland", *Economic Journal*, 112(482), 854-893.

Graversen, Brian K., and Jan C. van Ours (2009). "How a Mandatory Activation Program Reduces Unemployment Durations: The Effects of Distance", *IZA Discussion Paper 4079.*

Graversen, Brian K., and Jan C. van Ours (2008). "How to Help Unemployed Find Jobs Quickly; Experimental Evidence from a Mandatory Activation Program", *Journal of Public Economics*, 92, 2020-2035.

Hägglund, Pathric (2009). "Experimental Evidence from Intensified Placement Efforts among Unemploymed in Sweden", *IFAU Working Paper 2009:16.*

Hägglund, Pathric (2006). "Are There Pre-Programme Effects of Swedish Active Labour Market Policies? Evidence from Three Randomised Experiments", *IFAU Working Paper 2006:2.*

Heckman, James and Burton Singer (1984). "A Method for Minimizing the Impact of Distributional Assumptions in Econometric Models for Duration Data", *Econometrica*, 52(2), 271-320.

Huber, Martin, Lechner, Michael, Wunsch, Conny and Thomas Walter (2009). "Do German Welfare-to-Work Programmes Reduce Welfare and Increase Work?", *IZA Discussion Paper 4090.*

Kluve, Jochen et al. (2007). "Active Labor Market Policies in Europe: Performance and Perspectives" Springer Verlag, Berlin.

Lalive Rafael, Van Ours, Jan C. and Josef Zweimüller (2008). "The Impact of Active Labor Market Programs on the Duration of Unemployment." *Economic Journal*, 118, 235-257.

Lalive, Rafael, Van Ours, Jan C., and Josef Zweimüller (2005). "The Effect of Benefit Sanctions on the Duration of Unemployment." *Journal of the European Economic Association*, 3 (6), 1386-1417.

Meyer, Bruce D. (1995). "Lessons from the U.S. Unemployment Insurance Experiments," *Journal of Economic Literature*, 33(1), 91-131.

OECD (2009). "OECD Employment Outlook 2009"; Statistical Appendix, OECD, Paris.

Rosholm, Michael, Michael Svarer and Johan Vikström (2010): "The Relative Effciency of Active Labor Market Policies: Evidence From a Social Experiment and Non-Parametric Methods", working paper, Aarhus School of Business.

Rosholm, Michael (2008). "Experimental Evidence on the Nature of the Danish Employment Miracle," *IZA Discussion Paper 3620.*

Michael Rosholm, Michael Svarer (2008). "The Threat Effect of Active Labour Market Programmes," *Scandinavian Journal of Economics*, 110(2), 385-401.

Van den Berg, Gerard J., and Bas van der Klaauw (2010). "Structural Empirical Evaluation of Job Search Monitoring", VU University Amsterdam.

Van den Berg, Gerard J. and Johan Vikström (2009). "Monitoring Job Offer Decisions, Punishments, Exit to Work, and Job Quality.” *IFAU Working Paper 2009:18*.

Van den Berg, Gerard J., and Bas van der Klaauw (2006). ”Counseling And Monitoring Of Unemployed Workers: Theory And Evidence From A Controlled Social Experiment,” *International Economic Review*, 47(3), 895-936.

Appendices

2.A Estimation of Unobserved Heterogeneity Mass Points by Grid Search

In this section of the Appendix I describe the systematic procedure I applied to search for unobserved heterogeneity in the context of the models developed in the sections 2.4.1, 2.4.3 and 2.4.4. Such a procedure amounts to searching for additional mass points in order to establish a discrete mixture distribution for v_u and v_p (described in section 2.4.4). Thus, the benchmark and starting point is the model with 1 mass point, i.e. with no unobserved heterogeneity in the baseline hazard profile. In the following, I demonstrate step-by-step the iterative procedure – an interplay between grid search and estimation – I use to establish a second mass point and then to search for further ones.

1. Use the results of the separate estimations of the two processes (unemployment and post-unemployment) without unobserved heterogeneity as starting values.

2. Start with an initial set of 2 mass points (per process), i.e. the aim is to estimate their probabilities and locations (=intercept of the transition rate/baseline hazard): p_1 and λ^a as well as p_2 and λ^b[40].

3. *Grid search (over the probabilities' space):* Run systematically through all possible combinations of probabilities, using a loop. I.e., pick a probability combination, fix it and estimate the corresponding location of the mass points. More specifically, I use a double loop:

   (a) Loop over the sign (i.e. 2 runs) of the difference between the two locations. Note that this loop is used to set the starting values for the location estimation: I.e., set $\lambda^b = \lambda^a \pm 3$, whereby λ^a is the location (intercept) of the baseline hazard of the model without unobserved heterogeneity[41].

   (b) Loop over the i increments (here of 0.01) of the probabilities which are to be grid-searched: $p_1 = 1 - i \cdot 0.01$, whereby $p_2 = 1 - p_1$. Choice criterion: Take the set (p_1^*, p_2^*) with the corresponding estimated $(\lambda^{a*}, \lambda^{b*})$ which yields the highest likelihood[42].

4. *Estimation of the probabilities:* Fix the location of the mass points at λ^{a*} and λ^{b*}. Use p_1^* and p_2^* to calculate the starting values for the parameters a_1 and a_2 (the probabilities

[40] Extension to two processes u and p implies four probabilities and location combinations: p_1 for type aa (i.e. λ_u^a and λ_p^a), p_2 for type ab (i.e. λ_u^a and λ_p^b), p_3 for type ba (i.e. λ_u^b and λ_p^a), p_4 for type ab (i.e. λ_u^b and λ_p^b).

[41] Note that the difference, 3, can be chosen arbitrarily. It should be sufficiently big in order to allow the estimation to distinguish the two locations.

[42] In the grid search performed for this paper, this criterion always corresponded to choosing the lowest AIC. See Gaure et al. (2007) for a discussion of choice criteria. They opt for the use of the likelihood.

are designed in a logistic form, see section 2.4.4). Estimate these parameters (i.e. the probabilities) in the model.

5. *Fully free estimation:* Un-fix the location of the mass points, and use them and the estimated probabilities as starting values for the fully free estimation. If this estimation yields a higher likelihood, continue with the next step; otherwise stop and choose the model without unobserved heterogeneity as the best one.

6. *Increase the set of mass points:* Add a third mass point to every process (this can be done gradually, following Gaure et al. 2007). Redo steps 3 to 5.

7. *Stopping rule:* After having performed step 6, check whether the chosen model with 3 mass points yields a higher likelihood. If no, stop and take the previous model as the best. If yes, continue by adding a fourth mass point... and so on.

2.B Additional Tables

Table B1: Balancing of observables between treatment group (TG) and control group (CG) for the populations of the entry and the final (caseworker) surveys and the subpopulation of respondents to salary questions

	At Entry		*At Exit*		*Salary Sample*	
	TG	CG	TG	CG	TG	CG
Gender: woman	43.60%	43.85%	45.24%	47.92%	51.02%	49.23%
Married (incl. separated)	56.40%*	46.15%*	53.17%	43.75%	61.22%*	47.69%*
Age	52.53	52.28	52.20	51.86	52.01	51.62
Nationality: CH	84.88%	85.38%	84.92%	85.42%	86.73%	81.00%
Qualification: (semi-)skilled	97.09%	96.15%	96.03%	94.79%	94.90%	95.38%
Employability: 4	21.51%	22.31%	22.22%	23.96%	19.39%	24.62%
At least 1 foreign language	58.72%	55.38%	60.32%	52.08%	54.08%	46.15%
Job $<$ 100%	17.44%	18.46%	20.63%	17.71%	23.47%	20.00%
PES 2	13.95%	10.77%	16.67%	12.50%	15.31%	15.38%
Observations	172	130	126	96	98	65
... in %	56.95%	43.05%	56.76%	43.24%	52.69%	46.10%
Response rate	92.47%	92.20%	78.75%	84.96%	68.06%	69.15%

Notes: All TG-CG differences are not significantly different from zero, except from those marked: *** 1%, ** 5%, * 10%, ° 15%
Source: LZAR database

Table B2: Determinants of coaching entry. Probit regression

	Coaching entry (treatment group) Coeff.	z-value
UE duration in past 3 years	-0.001	-1.04
duration until availability	-0.006*	-1.73
age: 50-54 (base: 45-49)	0.580**	2.10
age: 55-59	0.700**	2.14
age: 60+	0.975**	2.14
married (base: unmarried)	-0.478°	-1.50
divorced	-0.237	-0.63
female	-0.495°	-1.56
non-Swiss	-0.103	-0.26
low employability (base: medium)	0.224	0.47
semi-skilled (base: skilled)	-0.067	-0.15
unskilled	-0.115	-0.16
non-German-speaking	-0.895*	-1.76
1 foreign language (base: 0)	1.096**	2.33
2+ foreign languages	-0.830*	-1.72
PES 2 (base: PES 1)	-0.352	-0.43
management (base: professionals)	0.005	0.01
support function	-0.613	-0.90
part-time (but above 50%)	0.174	0.48
Occupations (base: office, accounting):		
Blue-collar manufacturing, construction	0.249	0.57
Engineers, technicians, Informatics	-0.066	-0.15
Enterpreneurs, marketing, banking, insurance	0.454	1.05
Sales	-0.204	-0.48
Gastronomy, housekeeping, personal service	1.054°	1.63
Science & arts, education, eealth occupations	-0.022	-0.05
Rest (mainly unskilled workers, helpers)	1.609***	2.74
Month of entry in UE (base: Jan/Feb 2008):		
March/April 2008	-0.403	-0.98
May/June 2008	0.299	0.71
July/August 2008	-0.408	-1.04
Sept/Oct 2008	-0.173	-0.45
Nov/Dec 2008	-2.061***	-3.50
Caseworker fixed effects (base: CW 1):		
CW 2	0.090	0.16
CW 3	0.512	0.93
CW 4	0.270	0.45
CW 5	-0.517	-0.81
CW 6	-0.996*	-1.72
CW 7	0.471	0.84
CW 8	-1.179*	-1.84
CW 9	0.430	0.46
CW 10	1.549°	1.52
CW: rest (smaller charges)	0.315	0.49
Constant	0.558	0.91
N	186	
Pseudo R^2	23.85	

Notes: Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$.
Source: Own estimations based on merged UIR-LZAR database.

Table B3: Effects of the treatment plan on the exit to job rate: by treatment periods; anticipation effect by time to coaching; age-specific treatment effects. ITT (intention-to-treat) models

	ITT		
	Destination: exit to job		
	coeff.	s.e.	transf.
Treatment effects by period			
Anticipation effect (δ_a/in %)	-0.472**	0.240	-0.376
During coaching (δ_{c1}/in %)	0.174	0.229	0.190
Post-coaching, 14-180 days (δ_{c2}/in %)	0.079	0.254	0.082
Post-Coaching, 181+ days (δ_{c3}/in %)	0.510°	0.360	0.666
Control variables		Yes	
Unobserved heterogeneity		No	
-Log-Likelihood		1463.48	
AIC		1515.48	
N		327	
Anticipation effect by time to program			
Anticipation effect (δ_a/in %)	-0.545°	0.370	-0.420
... duration < 35 days [1)]	0.954*	0.575	0.505
... duration 70+ days [1)]	-0.113	0.461	-0.482
During coaching (δ_{c1}/in %)	0.163	0.229	0.177
Post-coaching, 14-180 days (δ_{c2}/in %)	0.079	0.255	0.082
Post-Coaching, 181+ days (δ_{c3}/in %)	0.528°	0.362	0.696
Control variables		Yes	
Unobserved heterogeneity		No	
-Log-Likelihood		1461.38	
N		327	
Age-specific treatment effects			
Anticipation effect (δ_a/in %)	-0.516*	0.290	-0.403
... for age 55+ [2)]	0.190	0.506	-0.278
During coaching (δ_{c1}/in %)	0.121	0.269	0.128
... for age 55+ [2)]	0.285	0.487	0.500
Post-coaching, 14-180 days (δ_{c2}/in %)	0.188	0.289	0.206
... for age 55+ [2)]	-0.377	0.529	-0.172
Post-Coaching, 181+ days (δ_{c3}/in %)	0.743*	0.454	1.102
... for age 55+ [2)]	-0.703	0.622	0.041
Control variables		Yes	
Unobserved heterogeneity		No	
-Log-Likelihood		1461.62	
N		327	

Notes: Coefficients and their transformations are reported: Transformed treatment effects are changes in %. 1) Note that these anticipation sub-group coefficients are incremental to the main anticipation effect; the transformation into changes in %, though, contains the sum, i.e. $exp(\delta_a + \delta_{a,d}) - 1$ where $d \in \{< 35, 70+\}$. 2) Note that these age 55+-specific effects are incremental to the respective treatment effects above which apply to individuals aged 45-54; the transformation into changes in %, though, contains the sum, i.e. $exp(\delta_j + \delta_{j,55+}) - 1$ where $j \in \{a, c1, c2, c3\}$. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$.
Source: Own estimations based on merged UIR-LZAR database.

Table B4: The total/net effect of the new policy on unemployment duration. (PH duration model)

	Destination: exit from UE coeff.	s.e.	transf.
Treatment effect			
Total effect (δ_t/in %)	-0.050	0.157	-0.049
Exit rate from unemployment			
$\lambda_b/exp(u_b)$, 1-50 days	-6.055***	0.388	9.40
$\lambda_1/exp(u_1)$, 51-100 days	0.590***	0.206	16.96
$\lambda_2/exp(u_2)$, 101-150 days	0.628***	0.219	17.63
$\lambda_3/exp(u_3)$, 151-250 days	-0.036	0.233	9.07
$\lambda_4/exp(u_4)$, 251-400 days	-0.100	0.255	8.51
$\lambda_5/exp(u_5)$, 401-550 days	-0.431	0.318	6.11
$\lambda_6/exp(u_6)$, 551+ days	0.391	0.357	13.91
Control variables			
UE duration in past 3 years	0.000	0.001	0.000
duration until availability	-0.003	0.003	-0.003
age: 50-54 (base: 45-49)	-0.290°	0.190	-0.252
age: 55-59	-0.582***	0.199	-0.441
age: 60+	-1.170***	0.300	-0.690
married (base: unmarried)	0.085	0.175	0.089
divorced	0.155	0.226	0.168
female	0.240	0.226	0.271
non-Swiss	0.139	0.244	0.149
low employability (base: medium)	0.228	0.255	0.256
semi-skilled (base: skilled)	0.206	0.364	0.228
unskilled	0.155	0.462	0.167
non-German-speaking	-0.162	0.299	-0.149
1 foreign language (base: 0)	-0.125	0.244	-0.118
2+ foreign languages	0.197	0.269	0.218
PES 2 (base: PES 1)	0.114	0.508	0.121
management (base: professionals)	-0.314	0.371	-0.270
support function	0.139	0.526	0.149
part-time (but above 50%)	0.152	0.220	0.164
occupations (base: office, accounting):			
Blue-collar manufacturing, construction	-0.157	0.254	-0.146
Engineers, technicians, Informatics	-0.235	0.292	-0.209
Enterpreneurs, marketing, banking, insurance	-0.395	0.327	-0.326
Sales	0.168	0.320	0.183
Gastronomy, housekeeping, personal service	-0.108	0.337	-0.102
Science & arts, education, eealth occupations	0.100	0.302	0.105
Rest (mainly unskilled workers, helpers)	-0.271	0.353	-0.237
Month of entry in UE (base: Jan/Feb 2008):			
March/April 2008	-0.270	0.252	-0.236
May/June 2008	0.151	0.240	0.163
July/August 2008	-0.079	0.280	-0.076
Sept/Oct 2008	0.030	0.248	0.031
Nov/Dec 2008	-0.179	0.299	-0.164
Caseworker fixed effects (base: CW 1):			
CW 2	0.791*	0.448	1.207
CW 3	0.483	0.406	0.622
CW 4	0.459	0.340	0.582
CW 5	0.557	0.443	0.745
CW 6	0.638*	0.362	0.893
CW 7	0.645°	0.397	0.906
CW 8	0.548°	0.352	0.730
CW 9	0.569	0.642	0.766
CW 10	0.520	0.723	0.683
CW: rest (smaller charges)	0.741°	0.462	1.099
Unobserved heterogeneity		No	
-Log-Likelihood		1772.47	
AIC		1821.47	
N		327	

Notes: Coefficients and their transformations are reported: Transformed treatment effects are changes in %. Transition rates are in % per month (for the respective piece of the hazard); note that λ_b is the intercept of the baseline hazard, the further steps are incremental; the transformations are calculated for an "average" individual: $u_j = \lambda_b + \lambda_j + \bar{x}'\beta_j + \sum_i \tau_i \bar{M}_i + \sum_k \gamma_k C_k$ where $j = 1, \ldots, 6$ ($\lambda_j = 0$ for first segment) and the bars are means, except for the past unemployment and the duration until availability where medians are used. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, $^{\circ}$ $p < 0.15$. UE=unemployment
Source: Own estimations based on merged UIR-LZAR database.

Table B5: Effects of the treatment plan on unemployment exit rate: by treatment periods; anticipation effect by time to coaching; age-specific treatment effects. (PH duration models)

	treatment-specific *Destination: exit from UE*			*ITT* *Destination: exit from UE*		
	coeff.	s.e.	transf.	coeff.	s.e.	transf.
Treatment effects by treatment period						
Anticipation effect (δ_a/in %)	-0.542**	0.220	-0.418	-0.472**	0.220	-0.377
During coaching (δ_{c1}/in %)	-0.363	0.277	-0.304	0.163	0.217	0.177
Post-coaching, 14-180 days (δ_{c2}/in %)	-0.131	0.244	-0.123	-0.010	0.241	-0.010
Post-Coaching, 181+ days (δ_{c3}/in %)	0.378	0.324	0.459	0.341	0.308	0.407
Control variables		Yes			Yes	
Unobserved heterogeneity		No			No	
-Log-Likelihood		1760.12			1767.79	
AIC		1813.12			1819.79	
N		327			327	
Anticipation effect by time to program						
Anticipation effect (δ_a/in %)	-0.655*	0.340	-0.480	-0.597*	0.337	-0.449
... duration < 35 days [1)]	1.033**	0.477	0.460	1.102**	0.473	0.657
... duration 70+ days [1)]	-0.134	0.448	-0.545	-0.150	0.442	-0.526
During coaching (δ_{c1}/in %)	-0.385	0.278	-0.320	0.145	0.217	0.156
Post-coaching, 14-180 days (δ_{c2}/in %)	-0.139	0.244	-0.130	-0.014	0.241	-0.014
Post-Coaching, 181+ days (δ_{c3}/in %)	0.391	0.324	0.479	0.355	0.308	0.426
Control variables		Yes			Yes	
Unobserved heterogeneity		No			No	
-Log-Likelihood		1756.75			1763.70	
N		327			327	
Age-specific treatment effects						
Anticipation effect (δ_a/in %)	-0.595**	0.268	-0.448	-0.497*	0.266	-0.392
... for age 55+ [2)]	0.260	0.453	-0.284	0.157	0.450	-0.289
During coaching (δ_{c1}/in %)	-0.480	0.339	-0.381	0.193	0.258	0.212
... for age 55+ [2)]	0.537	0.600	0.059	-0.011	0.467	0.199
Post-coaching, 14-180 days (δ_{c2}/in %)	0.009	0.277	0.009	0.158	0.275	0.171
... for age 55+ [2)]	-0.524	0.545	-0.403	-0.579	0.505	-0.343
Post-Coaching, 181+ days (δ_{c3}/in %)	0.608°	0.408	0.838	0.597°	0.412	0.816
... for age 55+ [2)]	-0.632	0.553	-0.024	-0.679	0.546	-0.079
Control variables		Yes			Yes	
Unobserved heterogeneity		No			No	
-Log-Likelihood		1757.12			1765.61	
N		327			327	

Notes: Coefficients and their transformations are reported: Transformed treatment effects are changes in %. 1) Note that these anticipation sub-group coefficients are incremental to the main anticipation effect; the transformation into changes in %, though, contains the sum, i.e. $exp(\delta_a + \delta_{a,d}) - 1$ where $d \in \{< 35, 70+\}$. 2) Note that these age 55+-specific effects are incremental to the respective treatment effects above which apply to individuals aged 45-54; the transformation into changes in %, though, contains the sum, i.e. $exp(\delta_j + \delta_{j,55+}) - 1$ where $j \in \{a, c1, c2, c3\}$. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, $^\circ$ $p < 0.15$.
Source: Own estimations based on merged UIR-LZAR database.

Chapter 3

What's in the Blackbox? A Field Experiment on the Effect of Labor Market Policy on Search Behavior and Beliefs

Abstract[1]: Field evidence on the effect of labor market policy on job search behavior and beliefs is scarce. The paper analyses a field experiment on a labor market policy intervention, featuring bi-weekly counseling and an intense coaching program, where individuals are surveyed about search behavior and beliefs at different stages of the treatment plan. I find that the new policy increased the proportion of job finders by 9 percentage points. The policy did not increase job search intensity: individuals searched less before and during coaching, and not more thereafter. However, the program caused the reservation wages of the treated to adapt downwards – without lowering the realised salaries. Moreover, I find that the job seekers expect more job interviews than they realise – beliefs about job chances are clearly too optimistic. The intervention slightly dampens this upward bias. Simple regression analysis suggests that considered behavioral channels explain 5.7 of the 9 percentage points of the higher propensity of job finding; reduced reservation wages contribute about one half, lower search intensity and smaller bias in beliefs about one fourth each.

JEL Classification: J64, J65, J68, J14

Keywords: Social experiment, job search behavior, reservation wages, biased beliefs, labor market policy evaluation, difference-in-differences.

[1]I would like to thank Rafael Lalive, Jan van Ours, Lorenz Goette, Michael Rosholm, Gerard van den Berg,Josef Zweimüller, Jaap Abbring, Bas van der Klaauw, Geert Ridder, Arthur van Soest, Marco Caliendo, Brian Krogh Graversen, Konstantinos Tatsiramos, Matteo Picchio, Anna Giraldo and Olivier Deschênes for their valuable comments. Moreover, I thank the seminar participants at Tilburg University, Tinbergen Institute, Amsterdam, University of Lausanne and University of Zurich, the workshop participants in Berlin as well as the session participants at the EEA meeting in Glasgow and the IZA/IFAU conference in Uppsala for their valuable comments. I am grateful to Raphael Weisz and the Office of Economic Affairs and Labour (AWA) of Canton of Aargau, Switzerland, for providing the data and further support. I would like to thank as well the project teams in the regional PES. **Email: patrick.arni@unil.ch**

3.1 Introduction

One of the very core objectives of labor market policy is that they aim at *improving sub-optimal behavior* of job seekers. Causes can be institutional – labor market imperfections (like missing information or skills), the availability unemployment insurance benefits – or individual: Job seekers may search too less due to moral hazard or biased beliefs; or they may stick to too high reservation wages due to incomplete information about personal labor market chances. Unemployment insurance provides supportive labor market policy (training, coaching, job search assistance) to reduce shortcomings in information and skills, and restrictive labor market policy (monitoring, sanctions, deterrent programs) to reduce shirking and hiding of information. Thus, knowledge on the fundamental dimensions of job search behavior is key to be able to design and steer successful labor market policy.

This insight strongly collides with the fact that field evidence on the interplay between these behavioral dimensions and implemented labor market policy is still very scarce, even more experimental one. The classical program evaluation literature assesses the effects of labor market policy by directly focusing on final outcomes like unemployment duration, job finding rates and wages. This yields valuable results for the budget planners of unemployment insurance and for policy makers who are only interested in the net reduction of the stock of the unemployed. But as soon as questions arise on *how* labor market policy effectively generates its outcomes, researchers and policy makers have to almost uniquely rely on the predictions of theoretical models. This paper offers a first contribution to filling this empirical gap.

More specifically, this paper contributes to the literature by presenting a *labor market policy field experiment which was explicitly designed to assess behavioral dimensions described by job search theory.* The aim of the field experiment is to evaluate the impact of a supportive labor market policy on the *dynamics* – i.e. the evolution over the unemployment spell – of job search behavior and beliefs. In order to identify these dynamic treatment effects, it combines a randomised trial with a precisely timed plan of treatment interventions and detailed (register and survey) data. This allows to perform repeated difference-in-differences estimations which assess, for each treatment period, whether the implemented policy caused a change in the different behavior variables, as compared to the control group and to the initial level at unemployment entry.

This paper is, to my knowledge, the first that links experimental variation of labor market policy with repeated direct measurement of different dimensions of job search behavior. The unique data stem from a new social experiment which has been conducted in the years 2008 to 2010 in Switzerland. The policy intervention consists of an intense coaching program, combined with bi-weekly counseling. The only piece of existing experimental evidence[2] on a particular

[2]There is an older literature in the US which discusses labor market policy field experiments at the beginning of the 1990s, see e.g. Ashenfelter et al. (2005) and Johnson and Klepinger (1994). They purely focus on outcomes and do not provide empirics on search behavior.

indicator of job search that is reported in the literature is Van den Berg and Van der Klaauw (2006). They observe a shift from informal to formal job search as a consequence of increased monitoring on formal search channels in a field experiment in two Dutch cities. Schneider (2008) is one of the rare observational labor market policy evaluation studies which reports reservation wages, but only in a cross-section. She finds no effect of benefit sanctions on reservation wages in the German welfare system (unemployment benefits II) for 2005. Using a new observational dataset for Germany, Caliendo et al (2010) report that individuals with larger networks show a tendency to shift from formal to informal search and to have higher reservation wages. This study does, however, not observe impacts of labor market policy on these measures.

Classical job search theory provides two fundamental channels on how behavior is linked to labor market outcomes: the choices of *search effort* and (implicitly) of *reservation wage* (see e.g. Eckstein and Van den Berg 2007 for an overview on empirically applicable models). This view needs, however, to be extended. First, the reservation wage profiles are – due to labor market policy, information arrival, learning etc. – not stationary, as classical theory suggests. This is supported by evidence from real-time-search laboratory experiments (Brown, Flinn and Schotter 2011) as well as by nonstationary search theory (e.g. Van den Berg 1990). Second, search is a multidimensional concept in real life: Success in job finding is not only linked to the optimal quantitative level of search, but also to the optimal choice of search channels (variety and frequency of use) and of search strategy. As a consequence of these insights, I analyse in total six measures which track the three fundamental dimensions of job search behavior: The dimension of search is measured by *search effort* (job applications), *variety* and *frequency of use of search channels* and by a *strategy* indicator (extension of the scope of search). Moreover, direct measures of *reservation wages* and of the *bias in beliefs about job chances* (deviation between expected and realised job interviews) cover the other two fundamental dimensions.

The role of *beliefs* has only recently been introduced, as a third fundamental channel, into the context of job search behavior. This paper provides some first empirical insights into the dynamic interplay between beliefs about job chances and labor market policy intervention. The small emerging literature on beliefs and job search consists, so far, mainly of theoretical work, complemented by a lab experiment and some observational data analysis (see section ... for references and discussion). Labor market policy has not yet been introduced into that literature – a main reason being the lack of appropriate data. The social experiment analysed here provides such kind of data: They allow to construct a measure of beliefs about success of job search, and to relate it directly to different stages of the treatment. The notion of beliefs brings a new (potentially) important element into the discussion of labor market policy impacts. The existing theory shows that distorted beliefs can have negative influence on search intensity and success. This raises two questions: Do job seekers indeed have biased beliefs about job chances? And if yes, can supportive labor market policy, like coaching and counseling, reduce the bias in beliefs? These questions are addressed in this paper.

A natural final step in the analysis of labor market policy impacts on the three behavioral dimensions is to ask about the *relative importance of the different channels*. In the last part of this paper, I perform an exploratory analysis of this question. By means of a series of simple regressions, which relate behavioral channels and the treatment to the job finding outcome, I am able to provide a rough quantification of the contributions of the different channels to the treatment effect on the final outcome (higher proportion of job seekers who found a job). This delivers some interesting insights: notably that the role of reservation wages being adapted downwards was most important; and that (the reduction of) biased beliefs are indeed of importance as well; and finally the fact that reducing search intensity and channels (to a seemingly more efficient level) were of considerable relevance too. The restriction of this kind of analysis is given by the fact that this field experiment does not provide separate exogenous variation for every channel individually (but coevolution of all three as a reaction on treatment). So, the causal analysis of e.g. the isolated manipulation of beliefs on labor market outcomes is an issue for future research in the field.

The paper is structured as follows: The next section, 3.2, reports all the relevant information on the performed field experiment: the setup, institutional background and content of the treatment plan; the implementation of the experiment; the (register and survey) data, accompanied by some descriptive analysis; the main labor market outcomes of the experiment. Section 3.3 describes the construction of the six behavioral measures and presents hypotheses on how labor market policy may affect search behavior and beliefs. Section 3.4 sets up the econometric framework. The following Section 3.5 reports and discusses the estimation results about the causal effects of the policy intervention on job search behavior and beliefs. Section 3.6 assesses the relative importance of the different behavioral dimensions for the job finding outcome. Section 3.7 concludes.

3.2 The Experiment

In this section, I will first describe the interventions that constitute the treatment plan: their content and institutional background. Then, the specific implementation of the experiment (sampling and randomisation procedure) will be presented. Next, I will present the combined database which features register and survey data. I will discuss as well some descriptive analyses which assess the balancing of the surveys over time. Finally, I report the most important outcomes – i.e. the treatment effects on unemployment duration, job finding and quality – of the experiment, which have been evaluated in detail in the companion paper Arni (2011).

3.2.1 The Treatment Plan and its Context

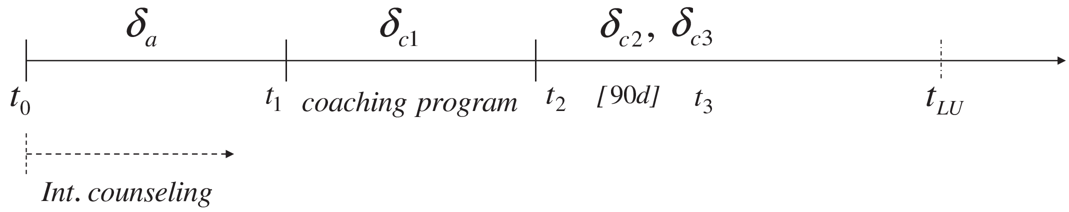
This experimental project was performed in two the public employment service (PES) offices in the Canton of Aargau in north-western Switzerland. The PES belong to a rather urbanised region in the agglomeration of Zurich. So, the region belongs to the "Greater Zurich Area" which features the biggest and economically strongest labour market in Switzerland (population: 3.7 million). 16 caseworkers were involved in the project, whereby 10 bore the main load of cases. The caseworkers were assigned to treatment and control group individuals. The assignment mechanism is exogenous to the treatment (by occupation). Caseworker and PES fixed effects will be taken into account in the estimations.

This social experiment for individuals aged 45+ was performed in the frame of the rules of the Swiss unemployment insurance (UI). The potential duration of unemployment benefits in the Swiss UI system is 2 years for individuals who meet the eligibility requirements. It is important to note that all the assignments to active labor market policy programs and the interview appointments – i.e. the described treatment plan of this experiment – are compulsory for job seekers. If they do not comply to these rules, they risk to be sanctioned (as well if they refuse suitable job offers or do not provide the amount of applications demanded by the caseworker). Sanctioning is comparably frequent in Switzerland (about every sixth job seeker is sanctioned, for details see Arni et al. (2009)). A broader discussion of the institutional backgrounds and the labor market situation in the time of the experiment can be found in the companion paper Arni (2011).

The *treatment plan* consists of two main measures and a specific timing of the interventions. The two main measures are high-frequency counseling by the caseworker at PES office and an intense external coaching program performed in small groups.

The *timing* of the interventions is highly relevant – mainly for two reasons. On one hand, *early intervention* is crucial for policy reasons. If the (intense) interventions start too late, the risk is high that the concerned job seeker is already on a vicious circle of being too long away from the labor market and therefore facing a decrease in employability – especially in the case of older job seekers who are often confronted with decreasing labor market attractiveness anyway. On the other hand, *to impose a clearly structured treatment order for which the timing is fixed ex-ante* is crucial for the identification of treatment effects. The fact that order and timing of the treatments are known from start on – which is the case here – makes this part of the treatment plan *exogenous.* I will use the fact of exogenous timing when discussing econometric modeling and identification, see section 3.4.

The timing of the treatment plan can be visualised in the following way:



So, we can distinguish four treatment periods: (i) the *anticipation period* (t_0 to t_1), i.e. before coaching entry (but already during intense counseling); (ii) the *during (potential*[3]*) coaching period* (t_1 to t_2); (iii) the *early post-coaching period* (t_2 to t_3), i.e. the first 90 days after (potential) coaching; (iv) the *later post-coaching period* (after t_3). The analysis by treatment period will always follow this schedule.

High-frequency counseling starts right from the beginning of the unemployment insurance spell, from the first interview on. Job seekers meet with their caseworkers every second week – thus in a double frequency compared to the normal monthly rhythm of interviews. Counseling goes on in high frequency for the treated during the first four months of the unemployment spell. Then, the frequency goes back to normal (monthly rhythm).

The basic idea behind increasing counseling frequency is that the caseworkers have *more time* available for the respective job seeker. This has as an effect that the job seeker is better known to the caseworker: counseling can therefore be more *targeted.* Moreover, more time remains in the interviews to go beyond administrative and monitoring tasks; this time is used to *counsel the job seeker in job search strategies.* Note, however, that this intensified support implies as well a certain tightening of monitoring and increased demands towards search effort of the job seeker.

The *coaching program*, the second main measure, starts in median after 50 days (48.5 days for those who really participate, 45 days until potential coaching entry for the others). Thus, the principle of early intervention is taken literally. The coaching was performed in small groups of

[3]Note that, due to the fact that the timing of the measures was fixed ex-ante, I can identify the *potential* coaching entry date for every person in the project, i.e. also for coaching non-participants and for the control group. The algorithm for identifying the potential coaching entry date is: next program start date which is $\geq$ (availability date + 5 days). The series of dates for coaching program starts was fixed with the coaching program provider before project start. Approximatively every 1.5th month a new coaching programs started; there were 9 in total over the year of inflow. This pre-fixing of the dates allowed the caseworkers to inform all individuals of the treatment group right at the first interview about the upcoming starting date of the coaching.

10-15 persons. An external, private-sector coaching firm was mandated to perform the coaching program. One coach ran all the coaching programs which took place during the year of inflow (December 2007 to December 2008; last program started in January 2009). The content and strategy of the coaching focussed on two points: (i) increasing the self-marketing skills for the labor market; (ii) improving self-assessment which should result in a better and more realistic self-profiling, which helps again for successful self-marketing. Thus, the coaching program features a strong element of human capital development (in terms of core competences and employability). The coaching program lasts 54 or 70 days (due to Christmas/New Year break). Job seekers were 3 to 4 full days per week in the program; in addition, homework had to be done as well. So the coaching program is highly intense and features a high work load (which results in a restriction of job search time).

The *content of the coaching* is crucial for understanding the treatment effects of this type of supportive labor market policy. In the following, I describe the five core elements that have been covered by the coaching program[4] over a net duration of 20 working days:

1. *Self-profiling and its consequence for optimizing search strategy:* Detailed collection and analysis of personal strengths and weaknesses; how to communicate them positively; putting the right ones on the CV; based on the clarified profile, how can *search strategy be optimized* (i.e. where to search, industries, geographical location, work shifts, types of contracts etc).

2. *Realistic self-assessment:* Contrast of self-perception and external perception; what is realistic to require/expect from potential jobs; realistic wage demands (in advanced age); what is still feasible in terms of educational updating; risk of long-term unemployment and benefits exhaustion.

3. *Improvement of job application skills:* Interview training & feedback; role plays; use and promotion of electronic applications, spontaneous applications by telephone (incl practical training).

4. *Job search efficiency:* Directed search; hints & lists where to search (focus on internet); general search coaching.

5. *Self-marketing:* How to sell oneself (incl practical training); do more self-marketing.

Note that the population of this field experiment consisted of job seekers aged 45 to 62. The skills update in these five dimensions was therefore targeted on issues for older job seekers.

The *control group* followed the 'status quo', i.e. was in the normal procedures and standard programs. This means in particular that they were interviewed only monthly and entry into

[4]This description of the core content is based on an interview with the coach plus written curricula of the coaching program (which were available on the internet during the time of the treatment).

active labor market programs (ALMP) normally started clearly later since the status quo doesn't feature an early intervention principle. A typical ALMP trajectory in the control group starts with participating in a short job search assistance sequence of 3 to 7 working days, roughly after 3 to 4 months of unemployment. Thus, this short program is normally the only ALMP activity in the control group that takes place during the period of intense intervention in the treatment group (first 4 months). After the four months (end of treatment) both groups follow status quo procedures (featuring monthly interviews and further ALMPs, dependent on individual needs). It is important to note that the individuals of the control group had no possibility to enter the coaching program. This newly designed program was exclusively open and assigned to the treatment group. As the treated, the control group was surveyed as well.

3.2.2 Implementation of the Experiment

The social experiment was implemented in two PES offices in north-western Switzerland. The treatment consisted in the two main measures and the timing strategy which are described in the last section 3.2.1. The members of the control group followed the status quo procedures.

Job seekers who were flowing into the two PES between December 2007 and December 2008 and met the participation eligibility conditions *were randomly assigned to treatment and control group at time* t_0, i.e. at registration before the first interview.

Thus, the assignment procedure, run separately for each of the two PES, consisted in three steps: First, the complete inflow of the respective PES was filtered with respect to the *eligibility conditions:* Age 45+, employability level medium or low, only full-time or part-time unemployed above 50%, enough (language) skills to follow the coaching, no top management and no job seekers who have found a longer-term temporary subsidised job (longer than a couple of days). Second, the remaining individuals were assigned to the caseworker pool. 16 caseworkers were involved in the project, whereby 10 bore the main load of cases. The *assignment mechanism* follows a fixed rule: assignment by occupation. It is therefore *exogenous* to the treatment (caseworkers took, thus, automatically cases in the treatment and the control group). Note, moreover, that caseworker and PES fixed effects will be taken into account in the estimations.

As a third step, the cases were *randomly assigned* to the treatment group (60%) and the control group (40%)[5], by use of a randomised list. Like that, the final sample amounts to *327 individuals* with 186/141 in the treatment/control group.

It is important to know which *information* was available for the treatment and control group at time t_0. In their first interview with the caseworker, the job seekers of both groups were informed

[5]In the first quarter of 2007, the random assignment ratio was 50%–50%. As a consequence of good economic conditions, inflow was lower than expected. We therefore decided to switch to a 60%–40% assignment rule. This explains why the treatment-control ration reported in the descriptive analysis in section 3.2.3 is in-between the two rules. This switch has no impact on the quality of randomisation.

in written form that they participate in a project for "quality control". This was necessary since both groups had to fill in repeated surveys over the duration of their unemployment spell (see section 3.2.3). On the other hand, the caseworkers were not allowed to use the terms 'long-term unemployment (risk group)' and 'randomisation'. The former was to avoid stigmatisation biases, the latter to prevent discussions which could potentially increase the risk of non-compliance.

Two further aspects of implementation are important to note. First, the initial survey of the job seeker has been performed *before* the first interview. This scheduling is crucial since it ensures that the initial survey data for the treatment group is not influenced by any information about the upcoming treatment. So, job seekers had been invited to the PES about 15 minutes before the start of the first interview with the caseworker, where the job seeker then got all the information (see above). Second, it is important to note that all the assignments to the treatment measures were compulsory (and could be sanctioned in the case of non-compliance, see last section). Still, non-compliance by the treated job seeker in terms of intentionally avoiding the coaching program can not be excluded with 100% certainty. But, as the non-compliance analysis in Arni (2011) shows, intentional non-compliance could only be observed in a negligibly small number of cases (3%).

3.2.3 Data & Descriptive Analysis

The evaluation of this social experiment is based on a unique combination of administrative records of the unemployment insurance (UI) and a series of repeated surveys on behavioral measures which cover the behavioral dynamics.

The *register data* are available for all job seekers who flow into registered unemployment between December 2007 and December 2008 in the region under consideration, the Canton of Aargau. The individuals are observed from start of their unemployment spell until the end of March 2010 (exogenous censoring date). Thus, all individuals are observed for at least 454 days and maximum 835 days. The censoring rate amounts to 16.5%. So, the vast majority of job seekers is observed at least up to benefit exhaustion (typically after two years). Note, moreover, that censoring is for the type of analysis performed here not an issue[6]. The register data include the common observable characteristics (see below). They track as well past unemployment histories up to three years before entry in the spell under consideration.

The comparison of observable characteristics between treatment and control group, see Table 3.1, shows that *randomisation worked very well*. No significant group differences can be detected for this sample of 327 job seekers (186 in treatment group, 141 in control group). Note that the

[6]The core analyses here are performed by treatment period, whereby the last treatment period (and thus survey measures) starts 90 days after (potential) coaching end (t_3), i.e. in median after 200 days of unemployment. See sections 3.2.1 and 3.4 for further details. In the analysis of job finding in section 3.6, censoring is appropriately taken into account.

Table 3.1: Comparison of characteristics of treatment vs control group

	Treatment Group	*Control Group*	*t-values*
Gender: Woman	44.1%	43.3%	0.15
Married (incl. separated)	56.4%	49.7%	1.22
Age	52.5	51.9	1.04
Nationality: CH	84.4%	85.1%	-0.17
Qualification: (semi-)skilled	96.2%	95.7%	0.22
Employability: 3/4	77.4% / 21.5%	78.0% / 21.3%	(-)0.05
At least 1 foreign language	55.4%	53.2%	0.39
Job < 100%	17.7%	17.7%	0.00
PES 2	14.5%	10.6%	1.04
Duation to availability (median, days)	11	13	-0.49
Past UE duration (median, days)	0	0	0.00
Observations	186	141	
... in %	56.9%	43.1%	

Notes: Frequency percentages for different observable characteristics by treatment and control group are reported. t-values are based on unpaired t-tests with equal variances.
Source: Own calculations based on merged UIR-LZAR database.

initial sampling according to the project eligibility criteria (see section 3.2.2) shapes the absolute values of the figures in Table 3.1. This explains, for example, the high proportion of skilled and of Swiss job seekers. Moreover, the project is focused to individuals of middle (3) and low (4) employability.

The median duration of unemployment history in the past three years is zero for both groups. 27.5% of the participants have a positive duration (median 113 days). Note that the PES 2 joined the experiment inflow later, from June 2008 on. This, combined with the slightly changed random assignment ratio over time (see footnote 5), mechanically explains the slightly higher percentage of random assignments to the treatment group. Since this was all fixed ex-ante, it doesn't affect randomisation. The median age of the participants in the social experiment is 52 years. The total age range of the participants lies between 45 and 62 years. 40% of the individuals in the sample are of age 45-49, 27.5% of age 50-54, 21.7% of age 55-59 and 10.7% of age 60-63. Note that none of this latter group had the possibility to pass to early retirement by means of unemployment insurance. The companion paper Arni (2011) reports some more details on the observed characteristics of the sample.

The *repeated surveys* were explicitly designed to track neatly the behavioral reactions of the job seekers on different elements and stages of the treatment. In particular, they cover measures of motivation (for job search, for coaching program), satisfaction, job search channels and the change of their use, reservation wage, job chances (expected job interviews) and health state. All the three perspectives of the project parties are represented: Caseworkers, job seekers and the coach are surveyed. The caseworker surveys are used here as an additional source to track issues of job search strategy & intensity (number of applications and their chances, changes in the scope of search) and reservation wages. The coach survey provides precise information about the decisions and conclusions with respect to job search strategy that arose from the coaching. The coach assesses as well the core competences of the participants.

The *timing of the repeated surveys* is dynamically adapted to the treatment plan. Thus, surveying is more frequent in the period of intense treatment, i.e. in the first four months. Specifically, the surveying rhythm is designed as follows: Entry survey before 1st interview, then subsequent surveys after 1/2/3/4/9/12 months of unemployment and at exit. If a job seeker is still in registered unemployment after 12 months – at the long-term unemployment threshold where the project stops – (s)he will get the final survey then. Thus, the final or exit survey is provided to all the participants. This last survey features as well questions about the first job, including salary, for the individuals who have exited to a job (they got the survey three months after exit).

The observed sample in the surveys is naturally subject to dynamic selection – individuals gradually leave unemployment for a job (or non-employment). Table C1 in the Appendix shows the dynamic development of the numbers of job seekers still present in unemployment at the mentioned points in time. These numbers provide the benchmark for a response rate of 100%. Of course, this response rate was not reached. The response rates are high in the earlier parts of unemployment, then they go down gradually, as the table shows. In the final survey, response rate is considerably higher again.

Note that the above-mentioned time structure of the surveys is then translated – using the exact date of each survey response – into a *timing structure relative to the treatment plan.*[7]. This structure allows the identification of treatment effects on the different outcomes by treatment period. The treatment periods are further described in the section 3.2.1 and visualised in the graphs on behavioral outcomes in the results sections.

Table C2 in the Appendix analyses the balancing of the observables, by treatment (TG) and control group (CG), over the periods of the treatment plan. All the cases which show significant differences in proportions of observable characteristics between treatment and control group are marked by the respective significance stars. The balancing characteristics for the job seeker surveys and for the caseworker surveys are reported.

The analysis yields the result that also in later stages of the treatment plan observables are only rarely not balanced between TG and CG. Only the nationality variable (proportion of

[7]Note that this relieves the problem of low response rates in the latest survey waves M9 and M13 (see Table C1 in the Appendix). The treatment stage 'later', as reported in the results, starts 90 days after (potential) coaching exit (t_3), thus it gathers survey information from M4, M9 and M13. If several surveys are available, the one nearest to 100 days after t_3 is chosen.

Swiss) and the indicator for PES 2 get more than once significant after t_0. Note that the latter divergence is mainly due to a mechanical issue (see above). This good balancedness also in later stages of the treatment plan can be interpreted as a positive sign that endogenous selection and attrition problems due to non-response are a only a minor issue in this study. Moreover, the difference-in-differences approach discussed in section 3.4 as well as the control for all the here mentioned observables will eliminate a greater part of the sources of potential bias.

It is, moreover, interesting to see that the values or proportions of the observed characteristics do barely change over time, as a combined inspection of the Tables 3.1 and C2 reveals. Thus, there is no strong pattern of general dynamic selection visible in the observables. The same statement applies to the across-surveys sample at time t_2 (last columns in Table C2) which is used for the analyses in section 3.6. The only slight time dynamics visible is that the average age slightly increases from the initial full sample to the latest treatment period. Overall, it can be thus concluded that the *dynamic selection processes in the here observed sample are weak* – in general over time and in terms of balancing of selection between treatment and control group (whereas the latter is crucial for an unbiased estimation of treatment effects per period).

3.2.4 The Main Outcomes of the Experiment

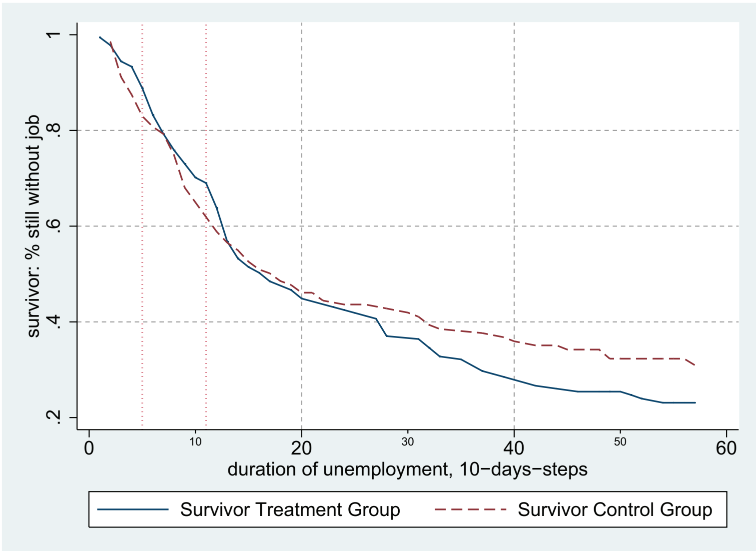
What are the results of the policy interventions performed in the field experiment on the labor market outcomes? This information about the treatment effects of the policy on unemployment duration, job finding proportions and quality of found jobs is key to the understanding of the behavioral results analysed in this paper. I therefore provide here a short summary of the main results, as found in the companion paper Arni (2011), in five points:

1. *The effect of the treatment plan on unemployment duration is zero.* So, no prolongation, like often the case for training programs, or shortening, like often the case for sanctions and monitoring. In fact, the zero effect on unemployment duration was generated by lower unemployment exit hazards of the treatment group in the anticipation period and during coaching (δ_a and δ_{c1} in the figure in section 3.2.1), which were counterbalanced by a higher propensity to find a job in later stages of unemployment (post-coaching). The survivors in Figure 3.1 demonstrate how *job finding success more and more kicks in over the course of unemployment*.

2. *Significantly more individuals found a job in the treatment group.* The proportion of job finders in the treatment group amounts to 72%, whereas 63% of the control group individuals found a job. This treatment effect of 9 percentage points will provide the base for the analyses in section 3.6.

3. *This more frequent job finding is not related to a job quality decrease in terms of salaries.* Even though, more of the treated individuals found a job, the realised salaries are not lower

Figure 3.1: Total treatment treatment effect to job finding, survivors treatment vs control group



than for the control group. The average monthly gross salary realised after unemployment exit is 5358 CHF for the treated and 5392 CHF for the controls[8] (difference insignificant).

4. *Employment stability is higher over the 1.5 years after unemployment exit.* The recurrence rate to unemployment over this time span is significantly lower for the treatment group. So, also in this dimension job quality of the treated did not decrease, but increase.

5. *The new policy pays off for unemployment insurance.* A counterfactual simulation shows that, on average, the treated individuals generate 23 days less of future unemployment (during the mentioned 1.5 years). This compensates more than 1.5 times the additional program cost of the new policy.

These results will be contrasted to the found behavioral results in the sections 3.5 and 3.6.

[8] 1 CHF = 0.78 EUR = 1.11 USD, i.e. the gross salaries are in the order of 4194 EUR or 5989 USD.

3.3 Measuring and Assessing Search Behavior & Beliefs

This section introduces how the three fundamental dimensions of behavior relevant for the search process are operationalized into empirical measures. In the total, I construct six behavioral measures, based on the repeated survey evidence. In a second part, I discuss for each of the six measures some possible hypotheses on how supportive labor market policy may impact on them. This discussion is based on insights from job search theory and empirical results from the program evaluation literature.

3.3.1 Measures for Search, Reservation Wages and Beliefs

As mentioned, I will consider 6 empirical measures to capture the 3 dimensions of job search behavior: search behavior, reservation wage, beliefs about job chances. I here briefly describe the survey items, on the base of which these 6 measures have been constructed, and the resulting design and units of measurement. I provide as well some information on averages and spreads of the measures.

As a first indicator of search, I construct the variable *job search effort*. The repeated surveys for the caseworkers always ask for reporting of the number of applications the job seeker has sent out in the last four weeks. Note that the job seekers must report all the applications to the caseworker, as an administrative rule (non-compliance can be sanctioned). Therefore, this information which is routinely protocoled by the caseworker should be of high reliability. On average over all treatment periods, job seekers send out 6.96 applications; the median is 6, the 25th percentile is 4, the 75th percentile is 9.

The second dimension of search is the *search channel variety*. The job seekers were asked in every survey which specific job search channel they used and how often. The following channels were proposed by the survey: PES-operated job offer database; newspapers; internet; private recruiters; job postings found in public spaces; network: strong ties (family, good friends; network: weak ties (colleagues at work, in sports and other associations, from hobbies, neighbors etc.); network: colleagues from school and other education programs; spontaneous applications by mail; spontaneous applications by telephone; other. To create the measure of channel variety, I counted all channels which have been used of the mentioned list, according to the respective survey. On average over all treatment periods, 6.83 channels have been used at least "monthly or rarer" (median 7, p25 5, p75 9).

A third element of search behavior is *search channel choice*. Based on the same block of items as above, I analyse for each of the mentioned channels their *frequency of use*. The frequency is measured on a 6-point-scale: 3 = "daily", 2.5 = "several times per week", 2 = "weekly", 1.5 = "several times per month", 1 = "monthly or rarer", 0 = "never". I assign the aforementioned values to the respective points of the frequency scale. This offers the big advantage that the frequency distribution can be characterised with common means and standard deviations. This, however, implies the assumption of the scale being approximatively metric. The facts that the frequency points are chosen in regular time steps and that the frequency distributions are not dominated by outliers suggest that this assumption can be justified[9]. These frequency measures allow two statements: First, how often is a certain channel used in the treatment and control group. Second, is there a shift to the more or less frequent use of some channels visible. The variety of frequency of use is, naturally, considerable between the different types of channels: Most frequent is the use of newspapers (mean 2.33, i.e. several times per week) and of internet (mean 2.24). Least frequent is the use of spontaneous written applications (mean 0.82, i.e. less than monthly) and of the contacts to former school mates and colleagues from education programs (mean 0.77).

The fourth aspect of search is *search strategy changes*. The caseworker and the coach have been asked whether they agreed with the job seeker on changing something in the search strategy. Specifically, they could indicate whether there was a change in: industry; occupation; place of work; kind of employer searched for; workload per week; permanent vs temporary jobs; working hours & shifts. The measure used here is a dummy variable which gets 1 if a change in at least one of these strategy dimension occurred. Detailed analysis revealed that the vast majority, more than 80%, of these changes were extensions (they could indicate extension/change/reduction) of the search scope, i.e. the new field was used for supplementary search while going on in the existing fields. The clarify the interpretation, I focus the indicator therefore on indicating search strategy *extensions*. In the periods before and after coaching, the probability of search strategy extensions is located at a mean of 0.20 (s.d. is 0.40), during coaching at a mean of 0.35 (s.d. 0.48). This differentiation is relevant, since the coaching program caused the strategy extensions more than to triple (see 3.5 for a discussion).

The second fundamental dimension of search behavior is *reservation wages*. They are surveyed by the classical question about the minimum (gross) wage the job seekers still would accept. They are finally reported by the caseworkers survey[10] and contain the minimal monthly gross salary (not wage) the job seekers would accept. Over all treatment periods, the median reservation salary amounts to 5200 CHF (mean 5417 CHF, p25 4200 CHF, p75 6500 CHF).

Finally, to construct the third dimension, *beliefs about job finding success*, I combined the above-discussed reports of applications with the questions posed to the job seeker about how

[9]The alternative approach to reduce the information to a probability of the frequency being above a certain value brings in more disadvantages (information loss).

[10]Note that the procedure was the following: The caseworker asked the job seeker the reservation wage question and reported his/her answer. The intention behind this kind of reporting is to reduce the risk of unreliable and wrong reportings. Given that the job seekers must communicate their reservation wage to the caseworker they cannot report any fantasy number as the caseworker will question the plausibility and ask further in unrealistic cases.

many interviews s/he expects (and s/he already has acquired) from the applications of the last four weeks. Moreover, I use the information in the surveys about realised interviews. Based on those items, I construct a measure which directly reports the *bias of beliefs about job chances*: The difference between the number of interviews expected and the number of interviews realised, divided by the number of applications sent out (within four weeks). On average over the treatment periods, the overestimated number of interviews per job application amounts to 0.26 (median 0.16, p25 0, p75 0.38). This means that 0.26 interviews more have been expected than effectively have been realised, per application sent.

3.3.2 How Labor Market Policy May Affect the Three Dimensions of Behavior

It may be of help for the understanding of the upcoming results to discuss some theoretical arguments and hypotheses on how supportive labor market policy may affect the three dimensions of job search behavior. This is done in the following, for each of the six behavioral measures (as constructed above).

Three key observations have to be stressed here, before starting the detailed discussion: First, the following discussion assumes, as a starting point, that the behavioral measures are *at suboptimal levels*. This is the common assumption (among others) which justifies the existence of labor market policy (see discussion in introduction). It is based on the observation that in real life missing information, not fully rational behavior and market imperfections play a crucial role. The second key observation is that most of the theoretical arguing below results in, potentially, *ambiguous effects on behavior*. This conclusion underlines the importance of an empirical evaluation to find out which behavioral mechanisms prevail. Third, it is important to note that the treatment effects of supportive labor market policy on behavior will crucially *depend on the content of the policy*. Thus, the following discussion is focused on potential treatment effects arising from the actions in the here considered type of coaching and counseling measures (see description in section 3.2.1).

Let us first discuss the potential treatment effects of the policy intervention on *search effort*. The typical issue linked to search effort is moral hazard, i.e. shirking behavior due to the presence of unemployment insurance benefits. Thus, the standard assumption would be that the job seekers search (quantitatively) too less. However, the fact that the field experiment here is performed with older job seekers arguably reduces the salience of this issue. The argument of lower importance of moral hazard can be underlined by observations from the reciprocity literature (e.g. Dohmen et al 2008, Bellemare et al 2007), which show that individuals above age 40 are more reciprocal and trustworthy, and by the benefit sanctions literature (e.g. Hofmann 2010), which reports that the effects of sanctions are weaker for older job seekers. A second argument which speaks against the hypothesis that the policy here increases search effort is the content of coaching. As discussed in section 3.2.1, the coaching features an element of search efficiency increase rather than an increase in effort. If we assume that search efficiency and effort predominantly act as substitutes, the hypothesis of a zero or negative effect on search effort of this kind of policy would be more realistic. Finally note that the companion paper Arni (2011) found an attraction effect in the anticipation period, i.e. a lower unemployment exit rate in the treatment group. This should reflect in lower search effort in the anticipation period.

Considering *search channel variety* it is sensible to assume that the variety is higher in early stages of unemployment – individuals try out different ways of search. Then, learning may come in – individuals find out what works best. A suitable hypothesis is that this kind of learning is reinforced by coaching & counseling. Thus, the policy intervention may result in a lower variety of search channels. A contradicting argument to that hypothesis is that counseling & coaching may provide additional inspiration for trying out further channels of search. In general, it seems sensible to conjecture that the effect of increasing the number of search channels on the job finding propensity follows an inverse U-shape – a certain variety is good, too much can be ineffective. So, if the policy aims at optimising job finding success and the initial level of variety is already high, the hypothesis of a reductive effect of coaching & counseling on channel variety seems again justified.

Hypotheses on the policy impact on *search channels choice and frequency of use* can be related to existing empirical literature. Van den Berg and van der Klaauw (2006) and Caliendo et al (2010) show that monitoring, on one hand, and the size of the personal network, on the other hand, matter for determining whether job seekers shift their search activities either towards formal or rather towards informal channels, respectively. Holzer (1988) and Weber and Mahringer (2008) demonstrate for unemployed youth in the US and for newly employed workers in Austria, respectively, that the channel choice is driven by relative costs, expected productivity and expected success in terms of getting good job and wage offers. Their results suggest that informal channels like asking friends or relatives or spontaneous applications (without referral) seem to be more productive in the mentioned sense. In short, this literature suggests that the shift to certain groups of channels can be promoted by incentives (cost reductions or increases) and credible information (recommendations for some channels, information on productivity of channels). Since the coaching here does not systematically target on the promotion of a specific type of channel (except from spontaneous applications by telephone, see section 3.2.1), one cannot expect to find substantial shifts in the channel portfolio. With respect to frequency of use, the hypothesis that coaching & counseling support learning on how to use channels in a directed way would suggest lower frequencies due to treatment. However, if the policy induces the use of additional channels, an increase in their frequency will obviously be observed.

It not only matters how much and through which channels an individual searches – but also *where* s/he searches. *Search strategy* improvement over the course of unemployment could imply the change or extension of search to other industries, other places of work, other occupations, other types of employers etc. Change of search strategy through extension of the search scope in (one or several of) the mentioned dimensions opens up a further range of potential job offers which – so the hypothesis – finally increases job finding rate. Such a hypothesis can become even more important in the context of coaching and counseling where potential search strategy changes are discussed extensively, like in the case here (see section 3.2.1). Therefore, the hypothesis that the policy here increases the probability of search strategy changes is straightforward.

How does supportive labor market policy affect *reservation wage setting*? The classical search models used as a base for empirics assume that the optimal reservation wage strategies are constant (see, e.g., the overview by Eckstein and Van den Berg 2007). This comes, among others, from the fact that these models assume no evolution (over the course of unemployment) of the encountered wage offer distribution; as well, the expected value of a future job is assumed to be the same, independently when in the spell the optimisation problem is faced. This is, of course, not the case in the real world. Nonstationarity has, thus, been introduced into search theory (e.g. Van den Berg 1990). Moreover, recent real-time-search laboratory experiments (Brown, Flinn and Schotter 2009) show a sharp decline in reservation wage over time. The authors explain this by the searchers experiencing non-stationary subjective costs of time spent searching. A further reason for changing reservation wage patterns are policy interventions which influence the value of continuing search. This is the case with coaching & counseling which is supposed to support learning on job search skills. Burdett and Vishwanath (1988) formulate such a model of *job search and learning*. Based on the reasonable assumption that workers do not have precise knowledge of the distribution of the prevailing wages, they show that the declining trend of reservation wages naturally arises from a learning and selection process.

Based on such an idea of a learning process, the hypothesis of a declining reservation wage profile due to the policy intervention can be put forward. A countervailing force on reservation wage setting could be that self-profiling and self-assessment elements of coaching may as well result in the job seekers *becoming more selective* with respect to job offer choice, i.e. keeping their reservation wage comparably high. If we assume that the initial reservation wage level is suboptimally high, as suggested by the model of Burdett and Vishwanath (1988), the learning process could be interpreted as a *disillusion process*. Under the same assumption, becoming more selective can be interpreted as *overconfidence* of the job seeker.

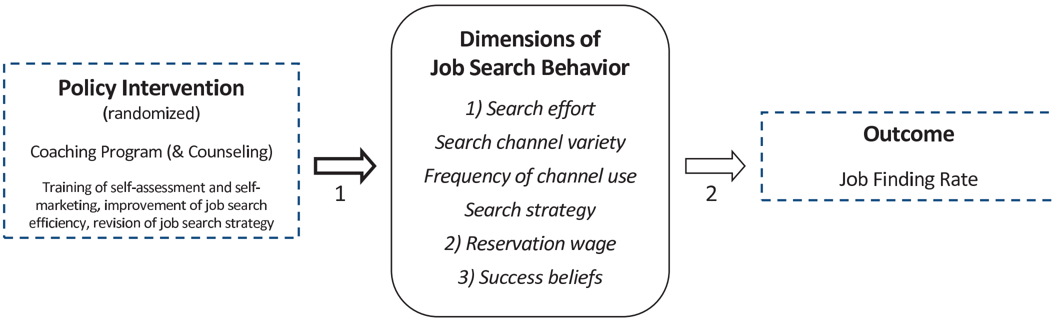
As a final dimension of fundamental behavior, I discuss *beliefs about success of job search*. A recently emerging behavioral search literature demonstrates that beliefs are important in shaping search outcomes and unemployment duration. Falk, Huffmann and Sunde (2006a) show in a lab experiment that job seekers are indeed uncertain about their job finding probability. Unsuccessful search induces individuals to revise their beliefs downwards; erosion of self-confidence decreases probability (or increases duration) of search, as they can show in the lab and in theory (Falk, Huffmann and Sunde 2006b). As a consequence, this suggests that the job finding rate for such low-confidence individuals – pessimists – is lower. Note that such a conclusion is intuitive in general for pessimism, i.e. also if already the initial beliefs (the priors) are biased downwards. The lab experiment finds as well that upward biased beliefs induce wrong amounts of search. Overly optimistic individuals overestimate their job finding probability and may, thus, search less than optimal and reduce their job finding rate. Such a behavior could alternatively be explained by hyperbolic discounting (see Paserman 2008). In short, this suggests the hypothesis that biased beliefs reduce the job finding rate. This can be tested in the data.

What exactly means "beliefs about success of job search"? In a neutral sense, this can be defined as an subjective expectation about the probability of finding a job. This can be specified by assuming that the subjective probability is mainly driven by the belief about the personal *ability* to find a job. This interpretation is put forward by the aforementioned authors as well as by the study of Spinnewijn (2009) who names these beliefs as *baseline beliefs*. Being the first derivative of the probability with respect to effort, the latter introduces as well so-called control beliefs. They correspond to the perceived efficiency how effort translates into the job finding probability. The data used here do not allow to directly measure control beliefs. An alternative concept of beliefs is put forward by Dubra (2004). He models them as (biased) expectations about the *job offer distribution*. This idea is directly related to the model of Burdett and Vishwanath (1988) and can thus be assessed by means of empirical reservation wage paths. The here analysed belief measure, however, tracks baseline beliefs.

I finalise the discussion about beliefs by stating two hypotheses: First, I conjecture that beliefs of job seekers about job finding success are indeed biased. Second, I hypothesise that the type of coaching & counseling analysed here has the potential to manipulate the bias of the baseline beliefs – either by reducing it, in the sense of disillusion (or even frustration if beliefs get over-corrected), or by increasing it, in the sense of overconfidence. The latter seems, however, less probable, given the content of the coaching.

3.4 Econometric Framework

In this section, I outline and discuss the econometric approach used to estimate the treatment effects of the experimentally implemented coaching & counseling intervention on the different dimensions of behavior. The setup of treatment relations can be visualised in the following way:



The econometric framework discussed below is modeled to causally estimate relation 1 above, i.e. the treatment effects of the policy intervention on the different dimensions of job search behavior. This amounts to estimating separate models for each of the six constructed behavioral measures and each treatment period. Identification is achieved by means of the randomised treatment assignment, the precise timing of the treatment periods and the applied difference-in-differences approach. Dynamic selection can potentially bias the causal estimates in late stages of unemployment. These points are further discussed below. Note that the relation 2, which amounts to a regression model that jointly relates the six behavioral measures to the job finding outcome, is analysed in section 3.6. This estimate is, however, not fully causal, due to a potential omitted variables bias.

The following econometric model is set up to analyse the impact of the experimental policy intervention on the evolution of the 6 considered measures of job search behavior. This is a dynamic problem due to the dynamic nature of the treatment. In fact, the policy intervention features a full treatment plan with different stages (see section 3.2), and every of those stages potentially influences the behavioral variables in a different way. I therefore estimate the impact of the treatment on the 6 behavioral variables for every stage of the intervention plan separately: *anticipation (t_0 to t_1), during coaching (t_1 to t_2), up to 90 days after coaching (t_2 to t_3), beyond 90 days after coaching* (after t_3). Note that the fact that this timing was fixed ex ante and communicated at t_0 provides the means to identify separate treatment effect by treatment period (Abbring et al. 2005). I refine this sequential strategy by the use of a *difference-in-differences (DiD)* approach following the implementation of Meyer (1995). Thus, I estimate regression models of the following type (omitting the individual subscript i):

$$y \quad = \quad \alpha + \gamma^{TG} D^{TG} + \gamma_t T_t + \delta_t D^{TG} T_t + x'\beta + \varepsilon \qquad \text{for } t = 1, \ldots, 4 \qquad (3.1)$$

whereby D^{TG} is a dummy variable for individuals in the treatment group, T_t a time indicator and x the set of the observed control variables. The time effect, γ_t, captures changes in levels of the behavioral variables over time which are common to the treatment and the control group. If, for instance, the reservation wage profile generally decreases over the time of the unemployment spell, γ_t will capture and measure the size of reduction of reservation wages.

The coefficient of key interest is the DiD parameter δ_t which measures the treatment effect in period t of the intervention on a certain behavioral outcome variable y. y represents the six mentioned behavioral measures. t indicates the four distinct periods of the treatment plan (see above). The sequential equations (3.1) above are estimated by OLS[11] or, in the case of reservation wages, by median (quantile) regression. Due to the skewed (typically approx. loglinear) distribution and broad range of wages, analysis of medians yields a more appropriate picture than of means, as they are not sensitive to outliers.

The benefits of using DiD in this context are twofold. First, DiD corrects for ex-ante differences in the behavioral outcomes. Even though groups are randomised at t_0 (and randomisation worked well in this experiment, see section 3.2.3) it can happen by chance that the initial levels of some behavioral variables are not fully balanced. DiD is a straightforward means to take this ex-ante difference into account; it will be captured by γ^{TG} in the model (3.1). Second, DiD does the same job with unbalanced unobservables which are constant over time. This is an important tool to reduce the impact of dynamic selection.

Is this setup appropriate to reach an estimate which can be interpreted causally (in the sense of the Rubin model)? The answer is yes, with some restrictions in the late periods of the treatment plan. To reach an unconfounded estimate of the treatment effect it is essential to rely on an exogenous treatment assignment mechanism. The best way to achieve this is to dispose of experimental variation. This is the case here, we dispose of a fully randomised social experiment. Thus, treatment assignment, and therefore D^{TG}, is fully exogenous. The randomised treatment assignment implies as well that omitted variables bias is not an issue here – any omitted variable is independent of D^{TG}.

However, unbalanced dynamic selection can be an issue of bias in the later stages of the treatment plan. The results of the estimation of the anticipation effect in the companion paper Arni (2011) indicate the direction of the potential selectivity: Individuals in the treatment group tend to exit less from unemployment in the anticipation period (attraction effect). As a consequence, more "high types" – e.g. in terms of ability and/or chances to find a job – remain in the treated group. If this selectivity issue can be solved by the use of DiD depends on nature of the impact of being high type on the intermediate outcome: If being high type influences the outcome in a constant extent over time, DiD will handle the issue, γ^{TG} will capture the unobservable. If the

[11] Note that I use OLS as well for the discrete measures of search strategy extension (dummy). I performed a sensitivity analysis using a probit model (table is available on request). The results are highly similar. Therefore, since there is no added value, I refrain from using probit and incurring the cost of imposing distributional assumptions.

influence changes over time, estimation of δ_t after coaching can potentially be biased. If there was a bias in the treatment effect in late stages, in which direction would it go? The treatment effect on reservation wages would be underestimated, if coaching acts in the theoretically predicted way. Thus, treatment would decrease reservation wages, whereas selection (more high types) would increase them – i.e., we observe an underestimation of the decrease. For the other dimensions, the direction of potential bias depends on which of the possible treatment impacts (see section 3.3.2) prevail.

It is important to note, however, that there are several empirical indications which suggest that the issue of unbalanced dynamic selection is of small size. First, the estimation of duration models featuring unobserved heterogeneity performed in the companion paper Arni (2011) show that such heterogeneity is statistically not relevant. Second, the descriptive analyses discussed in section 3.2.3 yield as a result that also in the later treatment periods almost no observables are imbalanced (see Table C2 in the Appendix). This suggests that the initial randomisation (plus the homogeneity of the initial sample) translated in a considerable degree to the later stages of treatment and unemployment.

3.5 Estimation Results

This section documents, in its first part, the results representing the dynamic treatment effects of the coaching & counseling policy intervention on the different behavioral dimensions. I will report them for each of the six behavioral measures. The second part of this section is dedicated to the discussion and interpretation of these results.

3.5.1 Treatment Effects of the Policy on Search Behavior & Beliefs

Was the content of coaching & counseling, as described in section 3.2.1, indeed implemented in practice? Did it find its way to the job seekers? The measure of *search strategy extensions* offers a direct opportunity to assess this question with respect to some elements of the coaching content: An important part of the latter is dedicated to discussing search strategy and search efficiency optimisations. The indicator analysed here becomes one if the respective individual agreed with the coach (and/or caseworker) to extend the scope of search in at least one of the following seven dimensions: change of industry, of occupation, of geographical place of work, kind of employer, workload searched for, permanent vs temporary job, work hours & shifts (see section 3.3.1 for some descriptives on the indicator).

Figure 3.2: Probability of search strategy change: extension

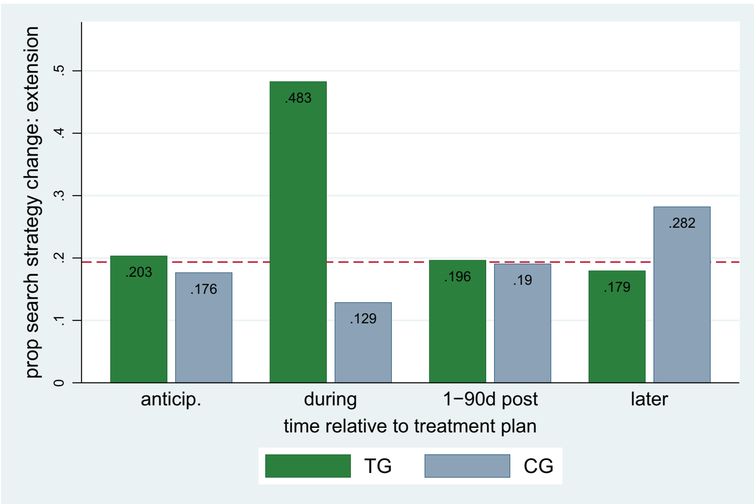


Figure 3.2 and Table 3.2 show a very distinctly shaped picture: Whereas the propensity to extend the scope of search is around 0.2 for the treatment group (TG) and the control group (CG) in the anticipation period as well as after coaching, the amount of strategy extension massively increases for the treated during coaching: 48% of them extend search strategy as a consequence of the treatment, whereas only 18% of the CG members extend strategy during the same period.

Table 3.2: Treatment effect on search strategy extension: OLS regressions

	(1) *anticipation* coef	se	(2) *during coaching* coef	se	(3) *1-90d post-coa.* coef	se	(4) *91+d post-coa.* coef	se
treatment (D^{TG})	**0.045**	(0.096)	**0.424*****	(0.071)	**0.042**	(0.105)	**0.081**	(0.149)
UE duration in past 3 years	0.000	(0.000)	0.000	(0.000)	-0.001	(0.001)	0.000	(0.000)
duration until availability	0.000	(0.001)	-0.000	(0.001)	0.000	(0.001)	0.003*	(0.002)
age: 50-54 (base: 45-49)	0.123	(0.150)	-0.075	(0.090)	0.075	(0.121)	0.246	(0.183)
age: 55-59	-0.131	(0.114)	-0.006	(0.091)	0.059	(0.127)	0.058	(0.153)
age: 60+	0.052	(0.119)	-0.040	(0.113)	-0.061	(0.126)	0.026	(0.204)
married (base: unmarried)	0.404***	(0.122)	-0.024	(0.100)	-0.026	(0.093)	0.191	(0.132)
divorced	0.206	(0.165)	0.013	(0.104)	-0.119	(0.124)	0.166	(0.171)
female	-0.091	(0.104)	0.058	(0.094)	-0.084	(0.117)	0.168	(0.188)
non-Swiss	0.180	(0.153)	-0.108	(0.125)	0.057	(0.149)	0.075	(0.184)
low employability (base: medium)	0.258°	(0.169)	0.026	(0.128)	0.137	(0.160)	0.076	(0.197)
semi-skilled (base: skilled)	-0.471**	(0.210)	-0.095	(0.150)	-0.080	(0.195)	-0.366	(0.283)
unskilled	-0.309°	(0.196)	0.096	(0.244)	0.280	(0.268)	0.033	(0.303)
non-German-speaking	0.064	(0.250)	-0.002	(0.198)	0.019	(0.218)	-0.064	(0.242)
1 foreign language (base: 0)	0.144	(0.195)	0.068	(0.112)	0.034	(0.118)	-0.235°	(0.150)
2+ foreign languages	-0.051	(0.194)	-0.057	(0.104)	-0.056	(0.134)	0.232°	(0.152)
PES 2 (base: PES 1)	-0.730*	(0.375)	0.264	(0.239)	-0.226	(0.486)	0.284	(0.266)
management (base: professionals)	-0.325**	(0.157)	-0.016	(0.149)	-0.219°	(0.138)	-0.250	(0.229)
support function	0.228	(0.282)	-0.147	(0.219)	0.028	(0.200)	0.219	(0.239)
part-time (> 50%)	0.023	(0.121)	-0.111	(0.104)	-0.061	(0.127)	-0.311**	(0.150)
caseworker FE: CW 2	-0.210	(0.149)	-0.150	(0.168)	-0.050	(0.195)	-0.266	(0.323)
CW 3	0.120	(0.175)	-0.113	(0.148)	0.106	(0.221)	-0.106	(0.280)
CW 4	0.127	(0.158)	0.247°	(0.149)	0.407*	(0.221)	0.042	(0.218)
CW 5	-0.206	(0.215)	-0.128	(0.176)	-0.347**	(0.162)	-0.449*	(0.232)
CW 6	0.654***	(0.198)	0.356°	(0.228)	0.751***	(0.158)	0.282	(0.379)
CW 7	-0.209°	(0.133)	-0.063	(0.165)	-0.306**	(0.129)	-0.270	(0.291)
CW 8	-0.292	(0.215)	-0.028	(0.165)	-0.203	(0.201)	-0.210	(0.266)
CW 9	0.874***	(0.167)	-0.393°	(0.260)	-0.079	(0.541)	-0.752**	(0.321)
CW 10	0.819*	(0.455)	-0.4470	(0.281)	0.011	(0.520)	-0.355	(0.418)
CW: rest (small charges)	-0.014	(0.259)	-0.153	(0.216)	0.183	(0.482)	-0.458°	(0.290)
Constant	-0.217	(0.184)	0.138	(0.136)	0.267°	(0.162)	0.056	(0.206)
Observations	93		186		98		78	
R^2	0.442		0.249		0.306		0.453	

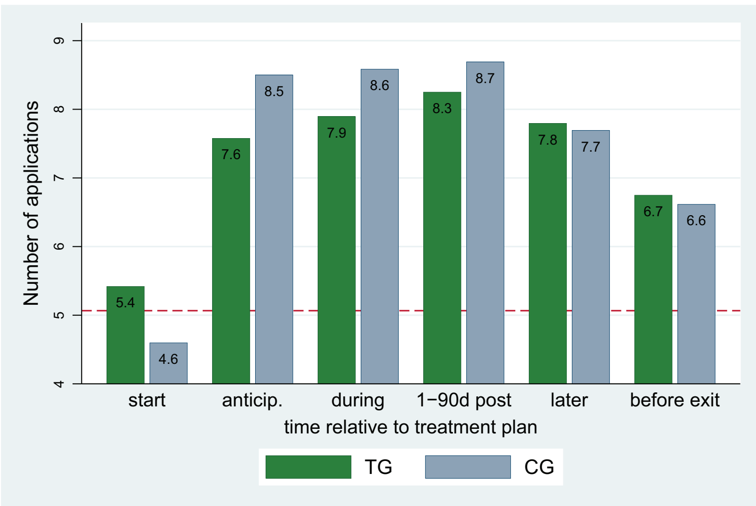
Note: Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ ° $p < 0.15$; available observations at t_0: 0.
Source: LZAR database

This is reflected in the regression estimates of the treatment effect by treatment period. Table 3.2 reports a *massive and highly significant increase of the propensity to extend the scope of search* by 42.4 percentage points[12]. So, the initial question about the implementation of respective contents can clearly answered by yes. It is interesting, however, to remark that this strategy extension behavior is solely shown during coaching This strongly suggests that this kind of behavior is causally linked to the presence in the coaching program[13] – high-frequency counseling plays here a minor role as there is no tendency to strategy extensions visible in the pre- and post-coaching times.

Is there a pattern with respect to *who* was coached towards a strategy revision? Table C3 in the Appendix reports the respective regression to analyse the determinants of search strategy changes due to coaching. A prototype of a strategy changer would be a married woman aged below 55 and skilled. When adding data about key qualifications (reported by the coach) of the job seekers to the model, I find that individuals with insufficient competence (with respect to the demands of the searched job) in systematic-analytic thinking have a highly reduced propensity to change search strategy.

A second aspect of the fundamental dimension of search is the pure *search effort*. The most striking result is that the coached & counseled individuals in the treatment group *never searched more than the control group*, in all the periods potentially affected by the treatment (from anticipation until exit).

Figure 3.3: Search effort: number of applications sent out (in 4 weeks)



[12]Note that, due to the fact that at t_0 no strategy changes are possible yet, this regression is not DiD as modeled in section 3.4. But given the zero level of the outcome at t_0, the direct regression per period is equivalent to DiD.

[13]This is indeed the case as a detailed analysis of the survey data behind the indicator reveals: The vast majority of strategy extensions in the second period was reported (and recommended) by the coach, not by the caseworker.

Table 3.3: Treatment effect on search effort (number of applications): OLS regressions, difference-in-differences

	(1) *anticipation* coef	se	(2) *during coaching* coef	se	(3) *1-90d post-coa.* coef	se	(4) *91+d post-coa.* coef	se
time (T_t)	4.106***	(0.995)	4.127***	(0.703)	4.405***	(0.923)	3.407***	(0.761)
treatment (D^{TG})	0.8160	(0.509)	0.877*	(0.505)	0.8190	(0.514)	0.715	(0.515)
DiD ($D^{TG}T_t$)	**-1.705**	(1.194)	**-1.642***	(0.878)	**-1.947***	(1.160)	**-0.704**	(1.063)
UE duration in past 3 years	-0.001	(0.002)	0.001	(0.001)	0.001	(0.002)	0.000	(0.002)
duration until availability	-0.009°	(0.006)	-0.007	(0.006)	-0.004	(0.006)	-0.001	(0.006)
age: 50-54 (base: 45-49)	0.434	(0.531)	-0.334	(0.555)	0.132	(0.578)	0.485	(0.595)
age: 55-59	0.615	(0.580)	0.185	(0.546)	0.414	(0.574)	0.245	(0.550)
age: 60+	-0.978°	(0.676)	-0.613	(0.606)	-1.089°	(0.707)	-1.157*	(0.637)
married (base: unmarried)	-0.170	(0.625)	-0.301	(0.580)	0.184	(0.629)	-0.033	(0.608)
divorced	-0.112	(0.685)	0.393	(0.651)	0.302	(0.602)	-0.178	(0.615)
female	-0.324	(0.661)	-0.181	(0.610)	-0.588	(0.609)	-0.219	(0.628)
non-Swiss	1.088	(0.756)	0.628	(0.669)	0.854	(0.863)	1.052	(0.764)
low employability (base: medium)	-0.883	(0.880)	-1.501*	(0.781)	-0.804	(0.850)	-0.739	(0.875)
semi-skilled (base: skilled)	0.110	(0.919)	0.308	(0.772)	0.499	(0.851)	0.470	(0.941)
unskilled	0.636	(1.221)	0.733	(1.116)	1.604	(1.117)	0.197	(1.427)
non-German-speaking	0.796	(1.371)	-0.299	(0.870)	-0.069	(0.919)	0.423	(1.008)
1 foreign language (base: 0)	0.342	(0.649)	0.165	(0.560)	0.053	(0.641)	0.327	(0.682)
2+ foreign languages	0.810	(0.644)	0.661	(0.560)	0.714	(0.630)	0.612	(0.692)
PES 2 (base: PES 1)	3.027**	(1.220)	2.259**	(1.068)	2.870**	(1.301)	2.849**	(1.371)
management (base: professionals)	0.320	(1.198)	1.013	(1.046)	0.302	(1.190)	0.960	(1.368)
support function	-1.275	(1.073)	-1.953**	(0.874)	-1.452	(1.085)	-1.293	(1.074)
part-time (> 50%)	-1.098*	(0.615)	-1.251**	(0.566)	-1.451**	(0.599)	-1.282**	(0.614)
caseworker FE: CW 2	2.562°	(1.643)	2.424°	(1.673)	2.112	(1.565)	1.351	(1.831)
CW 3	-0.258	(0.856)	0.242	(0.821)	0.849	(0.852)	0.153	(0.952)
CW 4	0.983	(0.816)	0.270	(0.704)	1.362*	(0.796)	0.844	(0.802)
CW 5	1.063	(1.093)	0.405	(0.977)	0.446	(0.895)	0.742	(0.922)
CW 6	0.850	(0.920)	1.900*	(0.987)	1.635°	(1.062)	2.101**	(0.948)
CW 7	-1.227°	(0.798)	-0.462	(0.702)	0.320	(1.036)	-0.649	(0.808)
CW 8	0.828	(1.120)	1.245	(1.055)	1.194	(1.052)	0.877	(1.089)
CW 9	-2.668*	(1.486)	-1.983	(1.382)	-2.036	(1.573)	-2.331	(1.675)
CW 10	-1.687	(2.022)	0.341	(1.926)	-1.448	(2.034)	-0.069	(2.559)
CW: rest (small charges)	-0.918	(0.969)	-0.560	(0.983)	-0.897	(1.184)	-0.449	(1.221)
Constant	4.088***	(0.881)	4.312***	(0.837)	3.806***	(0.848)	3.795***	(0.839)
Observations	394		473		399		379	
Pseudo R^2	0.185		0.205		0.181		0.177	

Note: Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ ° $p < 0.15$; available observations at t_0: 301.
Source: LZAR database

Table 3.3 shows that *during coaching and in the first 3 months after coaching treated individuals sent out significantly less applications* (-1.64/-1.95). The difference in the anticipation period is of the same size, but it doesn't become significant. Beyond three months after (potential) coaching, the negative impact of the treatment on the quantitative level of search effort tends to vanish, as the Table 3.3 and the Figure 3.3 show.

A look at the control variables in Table 3.3 reveals that mainly individuals beyond age 60 exert less search effort (about 1 application less) than younger job seekers[14]. In particular in the first stages of unemployment (until end of coaching period) people with very low employability show significantly lower search effort. The constantly significant dummy for PES 2 shows that this PES permanently requires about 3 applications more per month. Due to the high federalism of the organisation of unemployment insurance, such differences in policy implementation by PES are common in Switzerland. Finally, the significance of some of the caseworker dummies indicates that the requirements on job search effort posed by the caseworkers may differ by industry (since caseworker assignment is by industry). Of course, the caseworker fixed effects cover as well other differences in caseworker behavior.

I shortly want to discuss here, at the beginning, the interpretation of the other two coefficients which come together with the DiD coefficient (δ_t, see equation (3.1)). The coefficient called 'time' (γ_t in (3.1)) captures the effect of the ongoing duration of unemployment, as compared to t_0. It's size of about 4 in Table 3.3 reflects the fact that individuals sent out a smaller amount of applications before the initial meeting since they mostly haven't been on job search for already 4 weeks. γ^{TG}, the coefficient on the treatment dummy D^{TG} captures the initial difference in levels of search. By coincidence (generated by the randomisation[15]), the initial levels are not that well balanced for this measure. Note that the coefficient γ^{TG} also partially captures the unobserved influence of dynamic selection on balancing, if it acts in a constant way over time (see discussion in section 3.4). If this unobserved influence changes over time, the treatment effect on search effort could be slightly biased in the post-coaching periods. But the amount of potential bias is low, given the result in Arni (2011) that introduction of unobserved heterogeneity turned out not to be statistically relevant and only slightly changed the sizes of late treatment effects.

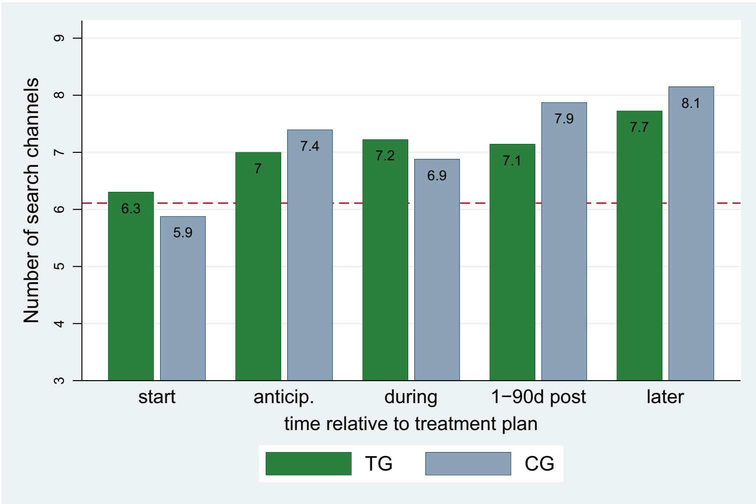
Let's have a look, next, at the effect of the policy intervention on the *variety of used channels of search*. In parallel to the result on search effort, I find here as well that *the treated never increased channel variety, but some time reduced it*, as compared to the control group. Figure 3.4 and mainly Table 3.4 reveal that the treated individuals used a *significantly lower variety of channels (-1.2*

[14]Note that the reduction is significant at the level of 15% error probability. The small sample size sets the threshold of significant high, such that only very remarkable changes (in size) become significant. To take this into account to a certain degree, I allow as well for the 15% significance level.

[15]Note that the descriptive analysis, see section 3.2.3, shows that the randomisation worked really well. I found as well in further descriptive analyses no indication of a systematic bias in reporting of search effort. The initial difference can, therefore, be interpreted as an (exogenous) random event generated by the randomisation (combined with the fact of the comparably small sample size).

channels) in the first three months after coaching. Smaller (and insignificant) reductions in the channel variety are found as well thereafter, and in the anticipation period. An interesting side observation is that women used a significantly lower channel variation (about -1 channel) as well as individuals aged 55+.

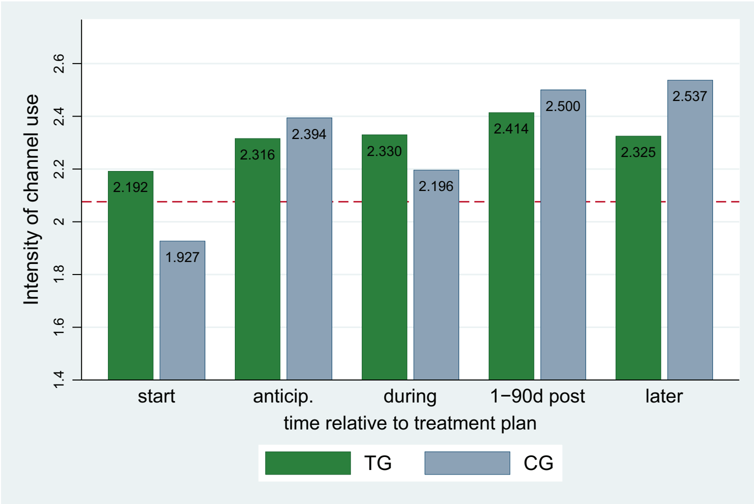
Figure 3.4: Number of search channels used



How did the treatment affect *channel choice* and the *frequency of channel use*? The available data allow the analysis of these questions by looking at the results for each channel of search separately. This is done in Table 3.5, where I report the six most important search channels. A first observation is that the negative signs on the DiD coefficients clearly prevail. Thus, as observed for the effort and channel variety dimensions of search, *frequencies of use are in tendency reduced and not increased*, too. I distinguish three formal channels – newspapers, internet and private recruiters – and three informal channels – network (weak ties) and spontaneous applications by telephone or by mail.

The most prominent result is that *the treatment caused significant reductions of frequencies of use of formal channels after coaching.* The negative treatment effect for newspapers gets significant beyond 3 months after coaching, the one for the internet in both post-coaching periods and the one for the reduced use of private recruiters in the first 3 months after coaching. Note that there is as well a tendency for reduced frequencies of formal channels in the anticipation period (which becomes significant for the case of newspapers). Figure 3.5 graphically illustrates the example of the frequencies of use of the internet. This figure and the analysis of the general time trend T_t (which is identified by the control group behavior) reveal that the found negative treatment effect in later stages is due to the fact that the CG individuals increased the use of internet (over time) more than the TG people.

Figure 3.5: Frequency of search channels use: internet



On the side of the informal channels, however, there is almost no significantly negative treatment effect visible. *The impact of the new policy on the use of personal networks is zero.* A highly significant and quantitatively important (plus 44.7 percentage points) upward move is found for spontaneous applications by telephone during the coaching period. This has to be linked to the fact that the coach explicitly promoted this type of spontaneous acquisitions. On the opposite, a significant reduction of spontaneous written applications can be observed right after the end of coaching. This may point to a substitution behavior. There is, however, as well a difference in the time dynamics of use between the two types of spontaneous applications. The general trend of use (T_t) for telephone applications only goes up in later stages of unemployment (of the control group), whereas the use of spontaneous written applications already (significantly) increases in earlier stages. So, coaching launched the trend of using more telephone applications earlier than in the default case of the control group (this arises as well from the, not reported, corresponding figure).

Table 3.4: Treatment effect on number of used search channels: OLS regressions, difference-in-differences

	(1) *anticipation*		(2) *during coaching*		(3) *1-90d post-coa.*		(4) *91+d post-coa.*	
	coef	se	coef	se	coef	se	coef	se
time (T_t)	1.443***	(0.429)	1.014***	(0.352)	2.129***	(0.392)	2.260***	(0.408)
treatment (D^{TG})	0.520*	(0.280)	0.513*	(0.278)	0.536*	(0.279)	0.531*	(0.285)
DiD ($\boldsymbol{D^{TG}T_t}$)	**-0.622**	(0.527)	**-0.000**	(0.463)	**-1.213****	(0.499)	**-0.485**	(0.581)
UE duration in past 3 years	0.002**	(0.001)	0.002*	(0.001)	0.003***	(0.001)	0.002**	(0.001)
duration until availability	-0.003	(0.003)	-0.006*	(0.003)	-0.005°	(0.004)	-0.004	(0.004)
age: 50-54 (base: 45-49)	-0.438	(0.304)	-0.685**	(0.279)	-0.336	(0.293)	-0.688**	(0.317)
age: 55-59	-0.869***	(0.296)	-0.649**	(0.297)	-0.655**	(0.291)	-0.850***	(0.307)
age: 60+	-1.819***	(0.424)	-1.478***	(0.411)	-1.512***	(0.427)	-1.830***	(0.445)
married (base: unmarried)	0.008	(0.293)	-0.079	(0.286)	-0.121	(0.279)	-0.030	(0.298)
divorced	0.174	(0.364)	0.222	(0.333)	0.083	(0.341)	0.270	(0.352)
female	-1.049***	(0.283)	-0.833***	(0.276)	-0.817***	(0.276)	-1.044***	(0.309)
non-Swiss	-0.284	(0.341)	-0.092	(0.332)	-0.027	(0.342)	-0.261	(0.363)
low employability (base: medium)	0.924**	(0.447)	0.707*	(0.377)	0.381	(0.430)	0.728*	(0.434)
semi-skilled (base: skilled)	0.572	(0.550)	0.151	(0.483)	0.337	(0.521)	0.522	(0.573)
unskilled	-0.461	(0.558)	-0.347	(0.520)	-0.223	(0.598)	-0.217	(0.574)
non-German-speaking	-1.087*	(0.592)	-0.451	(0.511)	-0.8270	(0.538)	-0.482	(0.594)
1 foreign language (base: 0)	0.831**	(0.354)	0.903***	(0.332)	0.872***	(0.332)	0.717**	(0.345)
2+ foreign languages	-0.286	(0.359)	-0.629*	(0.335)	-0.469	(0.333)	-0.181	(0.354)
PES 2 (base: PES 1)	1.106°	(0.762)	-0.089	(0.737)	0.576	(0.922)	0.649	(0.840)
management (base: professionals)	-0.280	(0.387)	-0.030	(0.370)	-0.495	(0.415)	-0.346	(0.436)
support function	-0.382	(0.608)	-0.534	(0.618)	-0.315	(0.548)	-0.363	(0.614)
part-time (> 50%)	-0.012	(0.334)	0.042	(0.321)	-0.163	(0.334)	0.027	(0.357)
caseworker FE: CW 2	-0.370	(0.685)	-0.883	(0.709)	-0.953°	(0.639)	-1.640*	(0.878)
CW 3	-0.519	(0.433)	-0.654°	(0.436)	-0.563	(0.453)	-0.330	(0.472)
CW 4	0.216	(0.454)	0.126	(0.449)	0.218	(0.449)	0.145	(0.482)
CW 5	-0.898	(0.630)	-1.054*	(0.546)	-0.557	(0.568)	-0.902°	(0.582)
CW 6	0.006	(0.454)	0.358	(0.501)	0.383	(0.512)	0.191	(0.513)
CW 7	0.124	(0.567)	0.304	(0.479)	0.091	(0.563)	-0.239	(0.624)
CW 8	-1.283**	(0.548)	-1.283**	(0.498)	-0.890*	(0.525)	-1.124**	(0.533)
CW 9	-2.120**	(0.980)	-0.819	(0.964)	-1.496	(1.065)	-1.241	(1.007)
CW 10	-1.620*	(0.939)	-0.356	(0.885)	-1.050	(1.060)	-1.248	(0.988)
CW: rest (small charges)	-0.911	(0.661)	-0.200	(0.696)	-0.520	(0.857)	-0.538	(0.798)
Constant	6.741***	(0.464)	6.858***	(0.450)	6.708***	(0.450)	6.825***	(0.474)
Observations	386		464		407		363	
Pseudo R^2	0.201		0.166		0.204		0.237	

Note: Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ ° $p < 0.15$; available observations at t_0: 296.
Source: LZAR database

Table 3.5: Treatment effect on the frequency of use of different search channels: OLS regressions, difference-in-differences

	(1) *anticipation* coef	se	(2) *during coaching* coef	se	(3) *1-90d post-coa.* coef	se	(4) *91+d post-coa.* coef	se
Formal channels								
newspapers								
time (T_t)	0.231°	(0.148)	0.098	(0.123)	0.146	(0.135)	0.185	(0.143)
treatment (D^{TG})	0.155°	(0.104)	0.144	(0.103)	0.128	(0.104)	0.145	(0.105)
DiD ($D^{TG}T_t$)	**-0.295°**	(0.187)	**-0.025**	(0.156)	**-0.157**	(0.174)	**-0.417****	(0.202)
internet								
time (T_t)	0.419**	(0.204)	0.253°	(0.163)	0.565***	(0.174)	0.617***	(0.190)
treatment (D^{TG})	0.328**	(0.136)	0.325**	(0.136)	0.329**	(0.137)	0.347**	(0.137)
DiD ($D^{TG}T_t$)	**-0.325**	(0.248)	**-0.131**	(0.207)	**-0.335°**	(0.228)	**-0.443***	(0.255)
private recruiters								
time (T_t)	0.531**	(0.223)	0.334**	(0.149)	0.735***	(0.165)	0.366*	(0.189)
treatment (D^{TG})	0.198°	(0.126)	0.177	(0.124)	0.221*	(0.125)	0.216*	(0.126)
DiD ($D^{TG}T_t$)	**-0.298**	(0.267)	**-0.188**	(0.203)	**-0.510****	(0.220)	**-0.120**	(0.266)
Informal channels								
network								
time (T_t)	0.287*	(0.170)	0.042	(0.135)	0.067	(0.145)	-0.166	(0.163)
treatment (D^{TG})	-0.026	(0.117)	-0.034	(0.116)	-0.031	(0.118)	-0.036	(0.117)
DiD ($D^{TG}T_t$)	**-0.139**	(0.216)	**0.116**	(0.181)	**-0.015**	(0.198)	**0.066**	(0.221)
spontaneous appl.: by tel.								
time (T_t)	0.023	(0.186)	-0.097	(0.127)	0.236°	(0.150)	0.404**	(0.190)
treatment (D^{TG})	-0.100	(0.113)	-0.084	(0.110)	-0.094	(0.111)	-0.093	(0.113)
DiD ($D^{TG}T_t$)	**0.073**	(0.230)	**0.447*****	(0.169)	**0.064**	(0.199)	**-0.008**	(0.245)
spontaneous appl.: written								
time (T_t)	0.325*	(0.184)	0.122	(0.136)	0.355**	(0.158)	0.264°	(0.178)
treatment (D^{TG})	0.066	(0.109)	0.038	(0.108)	0.053	(0.108)	0.051	(0.108)
DiD ($D^{TG}T_t$)	**-0.278**	(0.230)	**-0.020**	(0.173)	**-0.314°**	(0.200)	**-0.158**	(0.223)
Observables	Yes		Yes		Yes		Yes	
N Obs	387		465		408		364	

Note: Frequency of channel use, the dependent variable, is measured on a 6 point scale: 3 = daily, 2.5 = several times per week, 2 = weekly, 1.5 = several times per month, 1 = monthly or less often, 0 = never. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$; available observations at t_0: 296.
Source: LZAR database

As a second fundamental dimension of job search behavior, I analyse the evolution of *reservation wages*. Note that, in fact, the empirical measure reports reservation salaries (i.e. minimal monthly gross earnings that still would be accepted by the job seeker). Figure 3.6, supported by the estimations in Table 3.6, reveal a remarkable pattern: *Reservation wages of the treated are reduced over time (after the anticipation period), whereas the control group keeps reservation wages at the intial level.* Median reservation wages of the treated are kept significantly higher in the anticipation period, as compared to the control group[16]. Then, the opposite trend kicks in: Reservation wages of the TG are significantly lower in the TG from the during coaching period on (whereas the latest period is not significant any more).

Figure 3.6: Reservation wages by periods of the treatment plan

It is important to note here the corresponding labor market outcomes, i.e. that in the treatment group more people finally find a job, which pays on average the same salary than in the control group. This interesting *combination of lower reservation wages with higher job finding proportions at the same salary level* will be further discussed and put in a theoretical context in the next subsection. Specifically, the pre- and post-unemployment (gross) salaries are the following (see Arni 2011 for details): The pre-unemployment median salary is 5500 CHF (1 CHF=0.78 EUR=1.11 USD) for the treatment and for the control group. The realised median salaries after unemployment are 5470/5350 CHF in the treatment/control group. Thus, the (gross) reservation wage of job seekers at t_0 is of equal level as the pre-unemployment salary.

[16]Note that the control group shows a temporary reduction in reservation wages in the early stage of unemployment (see effect of T_t in anticipation and Figure 3.6), then they go back to the initial level. This pattern is consistent with the typical unemployment exit rate profile over time: the exit rate peaks in the first months (i.e. during the time of lower reservation wages) and then goes down (when reservation wages go up again). Thus, there seems to be a certain initial motivation to accept more jobs in order to early exit from unemployment, which then fades away.

Table 3.6: Treatment effect on reservation wages: median regressions, difference-in-differences

	(1) *anticipation* coef	se	(2) *during coaching* coef	se	(3) *1-90d post-coa.* coef	se	(4) *91+d post-coa.* coef	se
time (T_t)	-361.626*	(187.059)	61.202	(148.797)	108.149	(202.794)	-112.909	(318.122)
treatment (D^{TG})	-86.037	(170.086)	-107.691	(188.387)	-53.748	(182.737)	-36.178	(195.572)
DiD ($\boldsymbol{D^{TG}T_t}$)	**408.578***	(223.116)	**-342.517***	(204.532)	**-446.395***	(262.093)	**-183.053**	(354.011)
UE duration in past 3 years	-0.654	(0.679)	-0.486	(0.598)	-0.166	(0.780)	-1.030	(0.749)
duration until availability	-2.959	(2.690)	-4.091*	(2.282)	-3.594	(2.421)	-2.936	(2.989)
age: 50-54 (base: 45-49)	-184.051	(206.708)	-257.790	(187.538)	-368.391	(258.830)	89.359	(254.331)
age: 55-59	-161.354	(201.964)	-83.964	(183.346)	-97.701	(188.778)	-36.841	(244.650)
age: 60+	-229.945	(329.329)	12.152	(326.958)	118.829	(350.973)	205.265	(404.597)
married (base: unmarried)	716.514***	(248.180)	743.258***	(147.240)	421.156*	(232.894)	540.691**	(224.948)
divorced	503.407**	(244.381)	404.824**	(190.390)	290.687	(280.216)	224.326	(256.204)
female	-1,312.032***	(228.466)	-1,278.719***	(223.510)	-1,437.748***	(242.367)	-1,256.324***	(292.453)
non-Swiss	-68.131	(389.751)	-189.388	(246.205)	-23.726	(282.544)	-23.223	(385.089)
low employability (base: medium)	-820.871**	(356.708)	-258.789	(337.728)	-729.700*	(372.985)	-414.919	(383.558)
semi-skilled (base: skilled)	-216.509	(387.047)	-264.302	(272.572)	-314.643	(350.608)	63.919	(381.960)
unskilled	70.456	(697.978)	-500.184	(497.377)	-117.085	(599.885)	-597.123	(737.199)
non-German-speaking	580.085	(524.505)	404.093	(372.229)	483.469	(439.203)	100.607	(546.596)
1 foreign language (base: 0)	565.215*	(341.462)	-29.239	(280.697)	459.009	(318.841)	134.941	(338.183)
2+ foreign languages	88.897	(342.609)	575.435**	(273.518)	144.117	(324.612)	469.398	(312.995)
PES 2 (base: PES 1)	-219.727	(566.328)	464.253	(577.721)	15.293	(551.765)	409.423	(805.277)
management (base: professionals)	854.493*	(453.476)	829.462**	(324.579)	793.992*	(421.527)	648.937	(451.069)
support function	-462.550	(614.236)	-32.784	(441.062)	-220.693	(648.340)	-0.575	(713.556)
part-time (> 50%)	-1,708.556***	(241.228)	-1,647.762***	(211.468)	-1,620.020***	(244.558)	-1,630.767***	(290.856)
caseworker FE: CW 2	206.311	(388.538)	211.033	(486.274)	197.499	(528.216)	56.197	(545.525)
CW 3	-628.227**	(318.443)	-632.736**	(270.552)	-769.382**	(361.983)	-890.177**	(363.174)
CW 4	-1,078.488***	(266.134)	-1,258.412***	(287.396)	-1,434.001***	(355.592)	-1,497.469***	(337.363)
CW 5	-639.583**	(315.046)	-721.727*	(399.274)	-586.250	(365.412)	-508.212	(438.261)
CW 6	-67.693	(319.979)	-15.735	(379.219)	-193.194	(350.169)	-43.092	(423.895)
CW 7	-1,227.131***	(443.410)	-1,120.594**	(508.489)	-762.528	(665.189)	-1,341.871*	(714.331)
CW 8	-250.576	(446.057)	-794.594	(503.263)	-536.449	(491.094)	-757.132	(533.044)
CW 9	-1,422.069**	(586.607)	-2,052.351***	(666.668)	-1,373.552**	(612.790)	-1,956.943**	(869.438)
CW 10	-811.235	(1,070.086)	-747.058	(817.128)	-636.087	(809.248)	-590.758	(1,231.977)
CW: rest (small charges)	152.035	(413.581)	-790.623	(520.373)	-454.475	(587.078)	-557.739	(788.686)
Constant	6,225.614***	(376.689)	6,350.461***	(337.546)	6,533.486***	(435.961)	6,354.970***	(444.058)
Observations	358		435		363		342	
Pseudo R^2	0.3882		0.3747		0.3499		0.3193	

Note: Standard errors in parentheses (bootstrapped, 100 replications); *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; available observations at t_0: 265
Source: LZAR database

This zero difference suggests that individuals do not (yet) take into account that unemployment is often linked with human capital and wage loss, in particular for individuals of age 45+. Moreover, intentional overreporting could be another explanation for the high initial level of reservation wages. Note that, due to randomisation, overreporting behavior at t_0 should be balanced. The comparison of the last reported reservation wages with the realised salaries after unemployment reveals that *the control group's last reported reservation wages are above the median realised salaries whereas the treated' reservation wages are below.*

Finally, the analysis focuses on the third fundamental determinant of job search behavior: *beliefs about job market chances.* Figure 3.7 supports the first hypothesis proposed in section 3.3.2: *Job seekers indeed show biased beliefs – they overestimate the chances to acquire job interviews.* On average over the treatment periods, 0.26 interviews more have been expected than effectively have been realised, per application sent (value significantly different from 0). The time trend T_t in Table 3.7 demonstrates that the bias in beliefs significantly increases, compared to the beliefs at t_0. The initial beliefs show, by coincidence[17], some imbalance; this is captured by D^{TG}. Interestingly, women have a significantly lower distortion in their labor market beliefs than men do.

Figure 3.7: Biased beliefs: Overestimated number of interviews (expected−realised interviews), per job application

How does coaching & counseling affect the bias in beliefs about job market chances? The second suggested hypothesis is supported as well by the data: The here implemented typo of supportive labor market policy is able to manipulate the bias of beliefs. More specifically, the *coaching & counseling treatment significantly reduces the bias of beliefs during (and after[18]) coaching.*

[17]See footnote 15 for a discussion.

[18]Note that this coefficient only gets significant if we jointly consider the during and post coaching period (regression 3). Post coaching alone (4) is not significant. Model 3 was added to assess the sensitivity of the significance with respect to (small) sample size.

Table 3.7: Treatment effect on (biased) beliefs: OLS regressions, difference-in-differences

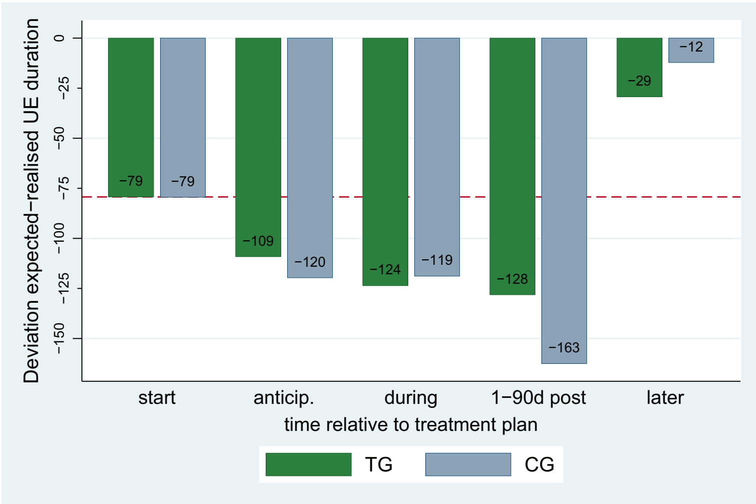
	(1) *anticipation*		(2) *during coaching*		(3) *during & post coa.*		(4) *post coa.*	
	coef	se	coef	se	coef	se	coef	se
time (T_t)	0.067	(0.074)	0.167°	(0.102)	0.180*	(0.094)	0.204°	(0.140)
treatment (D^{TG})	0.113*	(0.059)	0.131**	(0.063)	0.110*	(0.063)	0.099°	(0.061)
DiD ($\boldsymbol{D^{TG}T_t}$)	**-0.067**	(0.117)	**-0.200°**	(0.125)	**-0.180°**	(0.118)	**-0.133**	(0.173)
UE duration in past 3 years	0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)
duration until availability	-0.001°	(0.001)	-0.002**	(0.001)	-0.001	(0.001)	0.000	(0.001)
age: 50-54 (base: 45-49)	0.015	(0.064)	0.029	(0.060)	-0.047	(0.077)	-0.057	(0.096)
age: 55-59	0.065	(0.072)	0.144*	(0.084)	0.011	(0.088)	-0.061	(0.099)
age: 60+	-0.059	(0.104)	0.018	(0.095)	0.026	(0.103)	0.009	(0.142)
married (base: unmarried)	0.173**	(0.068)	0.107	(0.080)	0.072	(0.074)	0.123°	(0.081)
divorced	0.192**	(0.088)	0.049	(0.085)	0.033	(0.089)	0.167°	(0.110)
female	-0.136**	(0.068)	-0.134**	(0.066)	-0.117*	(0.066)	-0.076	(0.077)
non-Swiss	0.129°	(0.083)	0.123°	(0.079)	0.232*	(0.120)	0.276*	(0.152)
low employability (base: medium)	0.008	(0.098)	-0.003	(0.089)	0.053	(0.103)	0.046	(0.129)
semi-skilled (base: skilled)	-0.200**	(0.094)	-0.261**	(0.115)	-0.201*	(0.116)	-0.147	(0.129)
unskilled	-0.141	(0.142)	0.209	(0.239)	0.201	(0.219)	-0.156	(0.152)
non-German-speaking	0.055	(0.100)	0.169*	(0.098)	0.066	(0.121)	-0.042	(0.140)
1 foreign language (base: 0)	-0.001	(0.091)	-0.064	(0.098)	-0.095	(0.084)	-0.091	(0.095)
2+ foreign languages	-0.061	(0.082)	0.016	(0.089)	0.121	(0.089)	0.117	(0.102)
PES 2 (base: PES 1)	-0.011	(0.195)	-0.260	(0.281)	-0.150	(0.240)	0.044	(0.248)
management (base: professionals)	-0.115	(0.094)	-0.142*	(0.081)	-0.081	(0.085)	-0.070	(0.103)
support function	-0.036	(0.092)	0.410	(0.302)	0.355	(0.250)	0.018	(0.121)
part-time (> 50%)	-0.120°	(0.073)	-0.150*	(0.089)	-0.060	(0.085)	-0.087	(0.084)
caseworker FE: CW 2	-0.160°	(0.106)	-0.130	(0.100)	-0.114	(0.106)	-0.139	(0.130)
CW 3	-0.005	(0.123)	0.008	(0.117)	-0.034	(0.110)	-0.073	(0.130)
CW 4	0.020	(0.083)	0.144	(0.119)	0.059	(0.112)	0.002	(0.125)
CW 5	-0.124	(0.138)	-0.075	(0.117)	0.073	(0.167)	0.078	(0.208)
CW 6	0.267**	(0.103)	0.242**	(0.105)	0.233**	(0.113)	0.267**	(0.129)
CW 7	0.213	(0.194)	0.109	(0.138)	0.141	(0.126)	0.217	(0.164)
CW 8	-0.069	(0.115)	-0.067	(0.112)	-0.064	(0.123)	0.006	(0.148)
CW 9	0.211	(0.224)	0.465	(0.325)	0.346	(0.280)	0.052	(0.248)
CW 10	0.042	(0.219)	0.310	(0.298)	0.120	(0.260)	-0.059	(0.277)
CW: rest (small charges)	0.120	(0.175)	0.316	(0.268)	0.217	(0.228)	0.120	(0.249)
Constant	0.112	(0.109)	0.136	(0.117)	0.133	(0.118)	0.068	(0.134)
Observations	207		239		292		214	
R^2	0.203		0.189		0.140		0.157	

Note: Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ ° $p < 0.15$; available observations at t_0: 161.
Source: LZAR database

However, the significance of the impact is rather weak. The results on the two mentioned hypotheses shall be further discussed in the next subsection.

A final interesting result about beliefs – which has not yet been reported in the literature, to my knowledge – is illustrated in Figure 3.8. Not only the job seekers report biased beliefs, but as well the caseworkers: The *caseworkers systematically underestimate the duration of unemployment.* Caseworkers have been asked to predict how long a respective job seeker will remain unemployed. They underestimate unemployment duration by 80 up to 160 days (sign. different from 0) – with increasing tendency over the course of unemployment (except the last period which is mostly near to exit). It is up to future investigations to find out why caseworkers are permanently too optimistic.

Figure 3.8: Biased beliefs of caseworkers: Deviation of expectation about unemployment duration (expected−realised)



3.5.2 Discussion & Interpretation of the Results

The results found and reported in the last subsection shall be linked now to the discussion on possible theoretical explanations, as done in section 3.3.2, and the main labor market outcomes, as reported in section 3.2.4.

A first general observation is that coaching & counseling *indeed managed to manipulate the behavior of individuals, most often in the direction intended by the content.* One striking result is, as reported above, the massive increase in search strategy extensions as a reaction to coaching. A second result supporting this observation is the remarkable increase of the use of spontaneous applications by telephone during coaching; this channel was explicitly promoted in the coaching. However, these two changes were not very sustainable in the post coaching period. Two further elements of the content of the measures (see section 3.2.1) that, arguably, have been taken up are the promotion of more efficient search and the setting of more realistic demands towards potential future jobs. This two elements will be further discussed below.

An outcome which was not intended or promoted by the coaching & counseling but still realised are the behavioral reactions that are linked to the *attraction effect* and the *lock-in effect*. These two effects have been found in the companion paper Arni (2011): The job seekers reduce their unemployment exit rates during the anticipation period and during coaching. The behavioral hypothesis behind these effects is that individuals intendedly search less than they would do without the program – because they expect some utility from the upcoming coaching, in the anticipation period, and because they are charged by the workload, during coaching. There are several indications in the results which underline that the attraction and lock-in effect are indeed driven by reduction of search activity in some dimension: The search effort is clearly lower in the pre- and during coaching period (-1.7 applications, significant during coaching). The channel variety of the treated is as well reduced during the anticipation period, though insignificant. The frequency of newspaper use is significantly reduced in the anticipation period, compared to the control group. Also, the use of internet, private recruiters and written spontaneous applications are reduced in the same size, but insignificant, however. Note that these early reductions hardly can be explained by the counseling part of the treatment (which starts at t_0): It takes some time until the double frequency of counseling makes a difference to the status quo (monthly counseling), and until a learning process is realised – whereas the anticipation period is in median only 50 days long.

A hypothesis from section 3.3.2 which is supported by the data is the fact that the *coached & counseled individuals did not search more. Whereas they still found in total more jobs.* This suggests two insights: First, the *relation between effort and job finding is not monotonically increasing*; the marginal benefits of additional effort may get too low. The learning process induced by coaching & counseling may have fostered this insight. A second conclusion may be that it can be more successful (in terms of job finding) to increase *search efficiency* (or productivity) than pure effort quantity. This may be especially the case for older job seekers whose job finding problems are, arguably, less caused by moral hazard behavior (see section 3.3.2) than by insufficient or outdated search skills. It seems that the focus of the coaching on search efficiency has had its impact on the outcomes.

Consistent with this notion of increased search efficiency due to the treatment are the results that the *variety of used channels and the frequency of the use of formal channels (newspapers, internet, private recruiters) are lower in the treatment group after coaching.* One can conjecture that the updated search skills in the program induced a learning process which led to a *more directed way of search*: Individuals disposed of more information and knowledge of search, such that they knew more precisely where to search.

Considering the *choice of search channel types* the results revealed that it was *predominantly the formal channels where frequency of use reduced after coaching.* This can be well understood in the context of the above-discussed interpretation: If individuals indeed search in a more efficient and directed way it is natural that mostly the frequency of the formal channels like newspapers and internet reduce – since they are used most often and in the broadest way; so efficiency gains are highest there. This argument of search efficiency (or productivity) gains is supported by existing literature (see section 3.3.2). A final insight with respect to search channel choice that may be deduced from the found results is that *the use of informal channels only increases if the respective channels are explicitly promoted by the labor market policy.* In the case of the coaching here, the use of spontaneous applications by telephone significantly increased, whereas the use of personal networks did not. This is consistent with the fact that the former was explicitly trained and promoted by the coach[19], whereas the latter was not.

A particularly interesting and relevant behavioral change as a result of the policy intervention materialised in the evolution of reservation wages over the course of unemployment: *The treated reduced reservation wages over the course of unemployment, whereas the control group did not. In parallel, the treated did not realise lower salaries after unemployment than the control group.* This evidence is highly consistent with the model proposed by Burdett and Vishwanath (1988): They show that *declining reservation wages over the spell can be explained by a process of learning* (see as well section 3.3.2). This implies that the job seekers initially do not have precise knowledge on the job offer distribution and the offered wages. Learning means thus the gathering and application of such information. The found evidence strongly supports this model: At t_0 the median reservation wages for both groups are 5500 CHF (1 CHF=0.78 EUR=1.11 USD); the median pre-unemployment salary is as well 5500 CHF. The median salaries realised after unemployment are 5470/5350 CHF for the treatment/control group. The reservation wages reported in the post-coaching periods, however, amount to 5500 CHF for the control group vs. 4750 CHF for the treatment group.

The combination of this evidence and the described model suggests, thus, that the control group people remained at an "uninformed" level of reservation wage, whereas the treatment group members engaged in a learning process, induced by coaching & counseling. This learning resulted in a downward update of reservation wages. Learning means here information gathering in the sense of knowing better which job and wages offers are still realistic to achieve for unemployed job seekers in the age group 45+. *This more informed and more realistic job search and job acceptance behavior seemingly resulted in a increased amount of job offers and finally found jobs.* Note that a job at the level of 5400 CHF would have been accepted by a TG member – but not by a CG member, following the reservation wage rule. Thus, the acceptance of such jobs may explain the higher job finding proportion in the TG at the same level of accepted salaries. This learning

[19]Note that this information was directly gathered by an interview with the coach (and is as well part of the written announcement documents for the coaching program).

process explanation could be summarized by the notion of *disillusion*, as proposed in section 3.3.2.

Finally, let's contrast the found evidence on *beliefs* about job chances with some theoretical reflections. The first result that the *job seekers significantly and permanently (over time) overestimate their chance to get a job interview* is fully consistent with the (very) scarce existing literature (see section 3.3.2). The insight that *supportive labor market policy is in principle able to reduce the positive bias of beliefs* is new. Based on the result of the theoretical literature that biased beliefs induce suboptimal levels of search (and presumably of reservation wages as well), it is attractive for unemployment insurance to develop labor market policy designs which focus on the reduction of biased beliefs.

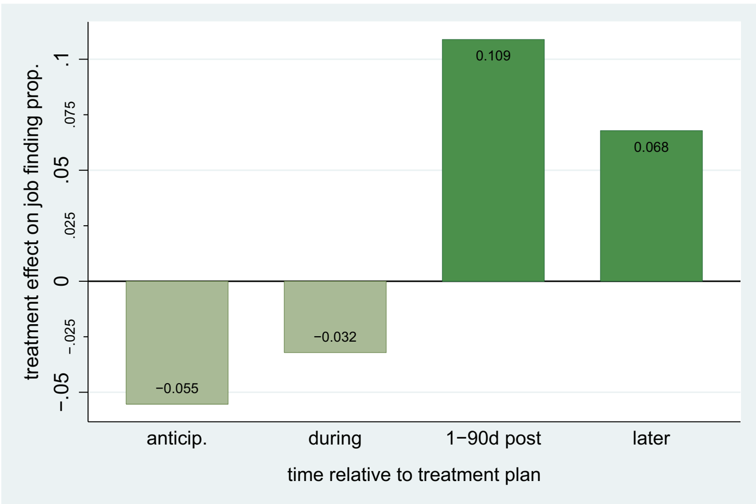
The observation of upward updating in the – overestimated – beliefs in the control group rather contradicts theoretical predictions: Being already overly optimistic at t_0 increases the risk of being frustrated by the number or quality of arriving offers which is below the beliefs. This creates a potential for downward updating. A possible explanation of the upward tendency could be selection. But there is no specific subgroup visible in the data which shows both, a comparably higher bias in beliefs and a comparably longer unemployment duration. However, a hypothesis concerning upward updating which is supported by the data is the following: The beliefs of the job seekers could be influenced by the beliefs of the caseworkers, which are remarkably upward biased too, as the results showed.

3.6 The Importance of Search Behavior & Beliefs for Job Finding

So far, the analysis focused on the causal estimation of the treatment effects of the policy intervention on the different dimensions of behavior. As a consequence of the found results, the natural question arises how those are related to the final labor market outcome, the job finding. How important are the different behavioral channels? This issue shall be analysed in the following. First, I present detailed evidence on the dynamics of the job finding proportion by treatment period. The evolution of job finding can then be contrasted to the found behavioral evolutions. Second, I estimate and discuss a series of simple regressions which allow to quantify the relative importance of the three behavioral dimensions in an exploratory manner.

3.6.1 Assessing the Coevolution of Behavioral Channels and Job Finding

Figure 3.9: Treatment effect on job finding proportion, per treatment period



One of the fundamental results found in the evaluation of the here analysed field experiment was that *the treatment increased the proportion of job finders by 9 percentage points, from 63 to 72%*. In order to contrast that positive outcome of job finding with the different behavioral outcomes, the job finding proportion has to be decomposed in the same way – by treatment period – as the behavioral measures. This is done in Table 3.8 and Figure 3.9. The disaggregation shows a distinct pattern which fully corresponds to the main labor market outcomes of the experiment as described in section 3.2.4. In the anticipation period, the proportion of job finders is 5.5 percentage points lower in the treatment group, in the period during (potential) coaching 3.2 percentage points. When going into the post-coaching periods, the treatment effect remarkably changes into positive: The job finding proportion of the treated is 10.9 percentage points higher

Table 3.8: Job finding proportions: total; disaggregation by treatment period or by pre-vs-post coaching; across surveys sample at t_2 (used for job finding regressions)

			disaggregation by treatment period			
	obs.	*total*	*anticipation*	*during coa.*	*1-90d post-coa.*	*91+d post-coa.*
CG	141	0.631	0.184	0.177	0.128	0.142
TG	186	0.720	0.129	0.145	0.237	0.210
diff. (TE)	327	**0.089***	**-0.055**	**-0.032**	**0.109****	**0.068°**
t-val		1.718	-1.378	-0.786	2.504	1.581

			disaggr. pre-vs-post-coaching		*sample of surveys @ t_2*	
	obs.	*total*	*ant. & during*	*post-coa.*	obs.	*from t_2 on*
CG	141	0.631	0.362	0.270	65	0.538
TG	186	0.720	0.274	0.446	84	0.690
diff. (TE)	327	**0.089***	**-0.088***	**0.177*****	149	**0.152***
t-val		1.718	-1.694	3.323		1.910

Note: Significance (t-test): *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$.
Source: LZAR database

in the first 3 months, then 6.8 percentage points. These four job finding treatment effects by period add up to the total effect on job finding of 8.9%. The two positive treatment effects in the post-coaching periods get significant seperately, the two smaller negative effects before not. However, if the effects before & during coaching and those thereafter are aggregated, respectively, the first effect amounts to significant -8.8%, the second to significant +17.7%.

This dynamics of the treatment-related job finding process reflects the facts that in the anticipation and the during coaching period the attraction effect and the lock-in effect dominates, which lowered unemployment exit in the TG (see section 3.2.4). More and more, the positive impact of the treatment plan kicks in: the job finding proportion in the treatment group becomes significantly higher than the one of the CG. What is visible if we put that job finding evolution pattern besides the evolutions of the behavioral measures? In the following, I present and interpret those coevolution patterns.

The coevolution of search effort (Figure 3.3) and job finding suggests that, in the anticipation and during coaching period, the attraction- and lock-in-effect arguments prevail: Individuals exert less search effort due to the attractiveness and, then, the workload of the coaching. As a consequence, less jobs are found. However, once the learning process of coaching & counseling is becoming prevalent, the motive of search efficiency (see last section) can cause a negative relation between realised search effort and upcoming job finding. The analog interpretation can be applied when considering the coevolutions of job finding with channel variety (Figure 3.4) and with the frequencies of use of formal search channels (see Table 3.5).

An analog interpretation can be put forward as well for the case of reservation wage evolution (Figure 3.6). During anticipation, reservation wages have been kept high by the job seekers – which would provide an explanation for the initially negative treatment effect on job finding. Later, when learning according to the Burdett et al. (1988) model has diminished the reservation wage level in the TG, more jobs are found, as a consequence.

Biased beliefs, finally, are, according to theory, harmful for job finding. The fact that beliefs about chances to acquire job interviews are systematically upward biased calls for a, in general, dampening impact on job finding propensity. The more the treatment is able to reduce the upward bias of beliefs, the more this kind of disillusion should positively contribute to the treatment effect on job finding towards and after the end of coaching.

3.6.2 The Relative Importance of Behavioral Channels to Explain Job Finding

In the following, I want to analyse the relation between behavioral changes and the subsequent job finding outcome. Estimating such a relation allows to obtain parameters on the size of the correlation of a respective behavioral change to job finding thereafter. These parameters provide a base to quantify, finally, the *relative importance of the behavioral impacts found in section 3.5 for the job finding treatment effect*. Moreover, one can quantify as well to which amount the treatment effect on job finding can be explained by the behavioral channels.

I set up and estimate the following model (i subscripts omitted):

$$y = \alpha + \delta D^{TG} + \Delta z'\phi + x'\beta + \varepsilon \qquad \text{for } t = 1, \ldots, 4 \tag{3.2}$$

whereby D^{TG} is again the dummy variable indicating TG membership and x are the control variables. The vector Δz contains the behavioral changes over time, for each of the considered behavioral measures. How is the timing of behavioral changes and the job finding outcome best chosen? The reasonings deduced from the coevolution analysis above provide a natural division of the behavioral and job finding evolutions in two periods: From t_0 up to the end of the (potential) coaching, t_2, vs. the period thereafter. The first period can be used to track the behavioral changes generated by the treatment, up to the end of coaching. The second period is then used to measure the job finding outcome.

This implies the use of a sample which contains all the individuals who are still present in unemployment by t_2 and who have filled out the survey timed shortly before (potential) coaching end. This generates a subsample of 149 individuals. This sample features a positive treatment effect on job finding (in the period after the survey) of +15.2%, as Table 3.8 shows. This figure is mostly comparable with the job finding effect for the whole post-coaching period of +17.7%. The reason for the slightly smaller size of the treatment effect in the t_2 survey sample is some mechanistic selection: Those individuals who are shortly before exiting to a job do not have to show up any more in the PES; they are, thus, missing in the survey. Note, however, that this mechanistic selection effect applies in the same amount to the treatment and to the control group. The treatment effect is thus not biased. Table C2 in the Appendix analyses the balancing of the observable characteristics in this t_2 survey sample. Except from nationality, none of the observed characteristics is statistically out of balance. The ratio of treated to controls neatly corresponds to the initial ratio.

The split and sampling at t_2 provides some natural causality from the behavioral changes on the subsequent job finding outcome. However, such a regression model *cannot* be causally interpreted without further – rather strong – restrictions. Note that we do here not dispose of experimental (or, more generally, exogenous) variation for each measure of behavior separately. This would be necessary to fully solve the endogeneity problem, which arises here from a potential omitted variables bias. As soon as there are unobserved variables which are correlated with some measures of behavior and the job finding outcome, the estimation will incur a bias. One can figure out plausible examples of such unobservables, e.g. motivation could be one candidate. Thus, this fact that ϕ could potentially be biased needs to be kept in mind when interpreting the results.

Table 3.9 provides a series of subsequent (OLS) regression models which are based on model (3.2) above. I stepwise introduce the control variables x and the behavioral changes in the different channels Δz. This stepwise inclusion allows to analyse the correlation between the treatment and the mentioned variables by looking at the changes in the estimated treatment impact δ.

The baseline specification (1) in Table 3.9 estimates, naturally, the same (significant) treatment effect as the one reported in the means comparison in Table 3.8 above, i.e. 15.2%. First, I introduce now all the x variables except the caseworker fixed effects. The treatment effect on job finding reduces to 11.4%. This means that 3.8 percentage points of the treatment effect can be explained by observational characteristics. It turns out that being of age 55+ is the crucial characteristic which is negatively correlated with the treatment success[20].

Next, I introduce five of the six behavioral measures[21]. The corresponding specification (3) shows interesting results: The five behavioral measures explain in total 7.4 percentage points of the treatment effect (not yet conditioned on caseworker fixed effects). Reducing the reservation wage and the upward bias in beliefs from t_0 to t_2 both significantly increase the propensity to find a job. In the dimension of search, reducing the channel variety yields as well a significant increase in job finding. Exerting a strategy extension provides an effect which is of comparable quantitative size, but insignificant. Finally, reducing search effort is positively related to job finding, but insignificant either.

[20]This can be deduced from sequentially introducing the x variables and from a corresponding result in the companion paper Arni (2011).

[21]I do not add here the separate frequency variables for the six considered job search channels in order to avoid too much collinearity between them and the other behavioral measures. This would harm the precision of the estimation of the other ϕ.

Table 3.9: Explaining the job finding treatment effect: regression (OLS) of job finding on treatment, behavioral dimensions and observables

	(1) coef	se	(2) coef	se	(3) coef	se	(4) coef	se	(5) coef	se
treatment	**0.152***	(0.080)	**0.114**	(0.086)	**0.040**	(0.100)	**-0.012**	(0.110)	**0.018**	(0.107)
reservation wage					**-0.082***	(0.050)	**-0.077***	(0.045)	**-0.063°**	(0.043)
(upward) biased belief					**-0.072***	(0.042)	**-0.055**	(0.045)	**-0.044**	(0.045)
search effort: applications					**-0.011**	(0.008)	**-0.013°**	(0.009)	**-0.010**	(0.009)
search strategy: extension					**0.035**	(0.100)	**-0.003**	(0.105)	**-0.020**	(0.103)
search channels: number					**-0.033***	(0.019)	**-0.044****	(0.019)	**-0.048***	(0.025)
channel use: newspapers									0.076*	(0.043)
channel use: internet									0.019	(0.036)
channel use: private recruiters									0.020	(0.036)
channel use: network									0.069*	(0.042)
channel use: spont. appl. by tel.									0.008	(0.048)
channel use: spont. written appl.									-0.114**	(0.053)
UE duration in past 3 years			-0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)
duration until availability			0.000	(0.002)	-0.001	(0.002)	-0.001	(0.001)	-0.001	(0.001)
age: 50-54 (base: 45-49)			-0.137	(0.095)	-0.184**	(0.092)	-0.173*	(0.096)	-0.239**	(0.100)
age: 55-59			-0.367***	(0.121)	-0.410***	(0.112)	-0.417***	(0.104)	-0.396***	(0.104)
age: 60+			-0.356***	(0.135)	-0.355***	(0.133)	-0.375***	(0.131)	-0.424***	(0.129)
married (base: unmarried)			0.076	(0.119)	0.067	(0.110)	0.142	(0.113)	0.184°	(0.113)
divorced			0.007	(0.120)	0.018	(0.115)	0.066	(0.112)	0.075	(0.109)
female			0.113	(0.101)	0.141	(0.107)	0.139	(0.102)	0.226*	(0.118)
non-Swiss			0.089	(0.134)	0.104	(0.135)	0.143	(0.136)	0.139	(0.131)
low employability (base: medium)			0.174°	(0.111)	0.184°	(0.120)	0.294**	(0.130)	0.292**	(0.121)
semi-skilled (base: skilled)			-0.053	(0.144)	-0.058	(0.139)	-0.073	(0.148)	-0.048	(0.147)
unskilled			0.132	(0.185)	0.230	(0.201)	0.155	(0.249)	0.158	(0.257)
non-German-speaking			-0.022	(0.193)	-0.001	(0.202)	0.013	(0.202)	0.045	(0.177)
1 foreign language (base: 0)			-0.078	(0.131)	-0.070	(0.135)	-0.104	(0.141)	-0.082	(0.130)
2+ foreign languages			0.095	(0.128)	0.085	(0.129)	0.084	(0.132)	0.024	(0.126)
PES 2 (base: PES 1)			0.056	(0.101)	0.059	(0.096)	-0.174	(0.129)	-0.252*	(0.139)
management (base: professionals)			-0.087	(0.144)	0.013	(0.179)	-0.000	(0.173)	0.040	(0.175)
part-time (> 50%)			0.154	(0.112)	0.203*	(0.103)	0.200*	(0.107)	0.207*	(0.110)
constant	0.538***	(0.062)	0.562***	(0.134)	0.678***	(0.124)	0.546***	(0.155)	0.475***	(0.153)
caseworker FE	No		No		No		Yes		Yes	
Observations	149		149		149		149		149	
R^2	0.024		0.171		0.232		0.317		0.382	

Note: Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$. UE=unemployment; spont. appl.=spontaneous application.
Source: LZAR database

To get the final, preferred specification (4), I introduce caseworker fixed effects. They control for differences in caseworker efficiency (or, more generally, behavior) as well as for impact differences by industry (since the exogenous caseworker assignement rule is by industry, see section 3.2.2). Comparing the treatment indicator between (3) and (4) reveals that caseworker differences account for at least 4 percentage points. This value does, however, not yet take into account the correlation of caseworker fixed effects and the behavioral variables. So, the former could explain even more of the treatment effect, which is the case as the final quantification below will show.

In this preferred specification, the positive effect of reducing reservation wage remains prominent and significant: Reducing the reservation wage by 1000 CHF (from t_0 to t_2) would imply an increase of the job finding propensity by 7.7 percentage points[22]. The bias in beliefs is somewhat correlated to caseworker behavior: the size of the parameter is slightly reduced and falls below the significance threshold. However, the size of the effect remains remarkable: reducing the bias in beliefs by the overall mean value of 0.26 goes together with an increase in job finding by 1.4 percentage points. Once controlled for caseworker fixed effects, the negative impact of search effort gets significant. The parameter on strategy extensions goes to zero. The positive impact of a reduction in search channel variety, however, increases and is highly significant.

As a sensitivity analysis, I add the measures for frequency of use by channel. They quantitatively barely change the results for the other five measures. Interestingly, increasing the frequency of use of newspapers and of the personal network (of weak ties) from t_0 to t_2 are both favorably related to job finding. So, the observed treatment effect of the policy to lower newspaper use frequency did therefore not contribute to more job finding. On the other hand, an explicit promotion and training of network use by coaching & counseling would have been beneficial. Finally, writing more spontaneous applications is clearly negatively related to job finding. This last result has to be taken with care, however, since the use of this type of applications was in general a rare event.

Using the parameters from the preferred specification (4), I quantify the importance of the causal treatment effects of policy on behavior found in section 3.5. Specifically, I compute thus $\Delta z_m * \cdot \hat{\phi}_m$ for each of the five behavioral measures (m). Δz_m^* are the respective treatment effects found for the period during (potential) coaching. Table 3.10 reports these quantifications. They show that *the five behavioral measures together may explain 5.7 percentage points of the positive treatment effect on job finding*[23].

Of these 5.7 percentage points of explained job finding treatment effect, 2.6 points – or 46% – are related to reduction in reservation wages. The treatment-caused reduction in biased beliefs contributes 1.1 percentage points. Search effort reduction accounts for 2.1 points. The other two aspects of search yield zero contribution[24]. So, the relative importance of the reservation wage

[22]Note that the reservation salaries are measured in 1000 CHF in this regression (for reasons of scaling).

[23]Note that doing the same calculation for specification (3) amounts to 7.5 percentage points explained job finding treatment effect. This difference represents the correlation between caseworker behavior and behavioral channels.

[24]Note that the zero contribution of reduced search channel variety is due to the fact that the effect of treatment

Table 3.10: The quantitative contributions of the behavioral treatment effects to the treatment effect on job finding

	coeff.	behavioral TE	contribution to job finding TE	... in %
reservation wage	-0.077	-0.343	**0.0263**	46.2%
(upward) biased belief	-0.055	-0.200	**0.0110**	19.4%
search effort: applications	-0.013	-1.642	**0.0207**	36.4%
search strategy: extension	-0.003	0.424	**-0.0011**	-2.0%
search channels: number	-0.044	-0.000	**0.0000**	0%
sum (explained job finding TE)			**0.0569**	

Note: TE = treatment effect; coeff.=coefficients from Table 3.9; the behavioral TE were estimated in section 3.5.
Source: LZAR database

reduction is highest, followed by the impact of search effort reduction and of belief bias reduction. Note that this quantification has to be considered with care, since only the Δz_m^* is causally estimated, whereas the $\hat{\phi}_m$ is only unbiased under the assumption of no relevant correlated omitted variable. However, the general conclusion that *manipulation of search behavior and beliefs by labor market policy is possible and quantitatively relevant for job finding* can be drawn nonetheless from this field experiment.

on variety was zero in the during coaching period. However, would one take the variety-reducing treatment effect which materialised in the three months after coaching, a contribution to the job finding treatment effect of 5.4 percentage points would result.

3.7 Conclusion

This paper evaluates a new *field experiment which allows to assess the dynamic impacts of labor market policy on three fundamental dimensions of job search behavior – i.e. on reservation wages, on (biased) beliefs about labor market chances and on different aspects of search behavior*. Empirical evidence on effects of labor market policy interventions on the behavioral variables of job search theory is still very scarce, even more experimental evidence. Moreover, the data allow to empirically measure beliefs about job finding success; the analysis of labor market policy impacts on beliefs is new in the literature.

The field experiment, performed in northern Switzerland from 2008 to 2010, implemented a newly designed supportive labor market policy which features an intense coaching program (20 working days over 54 days) and high-frequency counseling (every second week, during the first 4 months of unemployment). The experiment is accompanied by a unique dataset which combines rich register data with repeated surveys. This, combined with an ex ante fixed (and known) timing schedule allows the identification of treatment effects by periods of the treatment plan. Based on that structure, I use a difference-in-differences approach to estimate the behavioral treatment effects by period. The main labor market outcome of the treatment is that it increased the proportion of job finders by 9 percentage points – without harming the salary level and stability of the found jobs.

The findings can be summarised as follows: (i) The coaching & counseling strategy (mostly) managed to manipulate the job seeker's behavior according to some main intentions of the content: First, search strategy was changed considerably more often. Second, the goal to improve search efficiency seems to have influenced job seekers: search effort, search channel variety and the frequency of use of formal channels was reduced during coaching and/or in the three months after coaching – whereas the treated job seekers parallely found more jobs. Third, the treated job seekers increased the use of the informal search channel which was explicitly trained in the coaching (spontaneous applications by telephone). (ii) Reservation wages in the treatment group are reduced over the spell of unemployment, whereas the control group kept them high. This is consistent with a model on learning about the available distribution of job and wage offers (Burdett et al. 1988). Coaching & counseling seems to have induced the learning about such information, which resulted in a downward adjustment of reservation wages. This could be framed as a disillusion effect.

(iii) Individuals never search more (number of applications) in the treatment group – while finding more jobs at the end. During coaching, the treated even search less. This illustrates the non-monotonicity of the relation between search effort and job finding. Moreover, it points to the importance of search efficiency (productivity). (iv) All the job seekers show (upward) biased beliefs: they overestimate their chances for job interviews, and the overestimation even increases, in tendency, over the course of unemployment. Caseworkers show as well upward biased beliefs (they systematically underestimate the job seeker's unemployment duration). Coaching & counseling slightly decreases the upward bias in beliefs.

How important is the impact of the different dimensions of behavior on the job finding outcome? A series of simple (non-causal) regressions allows to quantify the contributions: Of the job finding treatment effect of 9 percentage points 5.7 percentage points can be explained by the above-mentioned behavioral treatment effects (up to the end of coaching). This shows that the behavioral changes induced by this supportive labor market policy are indeed relevant. The importance of reduced reservation wages is highest (46%), followed by reduced search effort (35%) and reduced bias in beliefs (19%).

Which insights can be gained for policy design? The first and main insight is the following: *This field experiment demonstrates that it is possible to design a types of supportive labor market policy (coaching & counseling with specific content) which is able to change behavior in intended ways.* This shows that it can be attractive for unemployment insurance managers to design targeted labor market policies which explicitly focus on the manipulation of some aspects of fundamental behavior. The results of this experiment suggest that the following policy design elements may be successful in terms of job finding: Training of search efficiency; explicit training of the use of some specific search channels; focus on information and disillusion strategies in terms of expectations towards future jobs. The results suggest as well that simple discussion of such search strategies with job seekers is not enough, intense training and application of them seems necessary – in order to induce learning.

These issues raised in this paper call for a future research agenda in the behavioral labor market policy evaluation. First, more – empirical and theoretical – research is necessary to understand the interplay of biased beliefs, policy assignments, and policy impacts. Second, more research based on a combination of survey and register data – optimally combined with a field experiment – is necessary to answer questions which relate to the behavioral blackbox in labor market activity. Thus, the willingness of policy makers to occasionally or systematically add survey elements to register data is necessary. Third and last, an exciting field of research would be the development and test (in the lab and in the field) of targeted policy/incentive mechanisms which can be focused on specifically incentivising certain elements of the results discussed above.

References

Abbring, Jaap H., and Gerard van den Berg (2005). "Social Experiments and Instrumental Variables with Duration Outcomes," *Tinbergen Institute Discussion Papers 05-047/3.*

Abbring, Jaap H., Van den Berg, Gerard J., and Jan C. van Ours (2005). "The Effect of Unemployment Insurance Sanctions on the Transition Rate from Unemployment to Employment." *Economic Journal*, 115, 602-630.

Arni, Patrick (2011). "How to Improve Labor Market Programs for Older Job-Seekers? A Field Experiment", working paper, University of Lausanne.

Arni, Patrick, Lalive, Rafael, and Jan C. van Ours (2009). "How Effective Are Unemployment Benefit Sanctions? Looking Beyond Unemployment Exit", *IZA Discussion Paper 4509.*

Ashenfelter, Orley, Ashmore, David, and Olivier Deschênes (2005). "Do Unemployment Insurance Recipients Actively Seek Work? Randomized Trials in Four U.S. States." *Journal of Econometrics*, 125, 53-75.

Bellemare, Charles and Kröger, Sabine (2007). "On Representative Social Capital," *European Economic Review*, 51(1), 183-202.

Black, Dan A., Smith, Jeffrey A., Berger, Mark C. and Brett J. Noel (2003). "Is the Threat of Training More Effective than Training Itself? Evidence from Random Assignments in the UI system." *American Economic Review*, 93, 1313-1327.

Blanchflower, David G., Andrew J. Oswald (2004). "Well-being over time in Britain and the USA". *Journal of Public Economics*, 88, 1359-1386.

Brown, Meta, Flinn, Christopher J., Andrew Schotter (2011). "Real-Time Search in the Laboratory and the Market," *American Economic Review*, 101 (2), 948-74.

Burdett, Kenneth, Vishwanath, Tara (1988). "Declining Reservation Wages and Learning," *Review of Economic Studies*, vol. 55(4), 655-65.

Caliendo, Marco, Schmidl, Ricarda, Uhlendorff, Arne (2010). "Social Networks, Job Search Methods and Reservation Wages: Evidence for Germany," *IZA Discussion Paper 5165*, Institute for the Study of Labor (IZA).

Dohmen, Thomas, Falk, Armin, Huffman, David and Uwe Sunde (2008). "Representative Trust And Reciprocity: Prevalence And Determinants," *Economic Inquiry*, 46(1), 84-90.

Dubra, Juan (2004). "Optimism and Overconfidence in Search," *Review of Economic Dynamics*, 7(1), 198-218.

Eckstein, Zvi, Van den Berg, Gerard J., (2007). "Empirical labor search: A survey," *Journal of Econometrics*, Elsevier, vol. 136(2), 531-564.

Falk, Armin, David Huffman, Uwe Sunde (2006a). "Self-Confidence and Search," *IZA Discussion Paper 2525*, Institute for the Study of Labor (IZA).

Falk, Armin, David Huffman, Uwe Sunde (2006b). "Do I Have What It Takes? Equilibrium Search with Type Uncertainty and Non-Participation," *IZA Discussion Paper 2531*, Institute for the Study of Labor (IZA).

Graversen, Brian K., and Jan C. van Ours (2009). "How a Mandatory Activation Program Reduces Unemployment Durations: The Effects of Distance", *IZA Discussion Paper 4079.*

Hofmann, Barbara (2008). "Work Incentives? Ex-post Effects of Unemployment Insurance Sanctions. Evidence from West Germany," *IAB Discussion Paper 43/2008.*

Holzer, H. (1988): "Search Method Use by Unemployed Youth". *Journal of Labor Economics*, 1, 1-20.

Johnson, Terry R. and Daniel H. Klepinger (1994). "Experimental Evidence on Unemployment Insurance Work-Search Policies". *Journal of Human Resources*, 29(3), 695-717.

Meyer, Bruce D (1995). "Natural and Quasi-experiments in Economics", *Journal of Business & Economic Statistics*, American Statistical Association, 13(2), 151-61.

Mortensen, Dale T. (1986). Job Search and Labor Market Analysis. In: O. Ashenfelter and R. Layard (eds.), *Handbook of Labor Economics*, 849-920. Amsterdam: North Holland.

Paserman, M. Daniele (2008). "Job Search and Hyperbolic Discounting: Structural Estimation and Policy Evaluation," *Economic Journal*, 118(531), 1418-1452.

Pissarides, Christopher A. (1990). "Equilibrium Unemployment Theory", Blackwell, Oxford.

Schneider, Julia (2008). "The Effect of Unemployment Benefit II Sanctions on Reservation Wages." *IAB Discussion Paper 19/2008.*

Spinnewijn, Johannes (2009). "Unemployed but Optimistic: Optimal Insurance Design with Biased Beliefs", working paper, London School of Economics.

Van den Berg, Gerard J., and Bas van der Klaauw (2006). "Counseling And Monitoring Of Unemployed Workers: Theory And Evidence From A Controlled Social Experiment," *International Economic Review*, 47(3), 895-936.

Van den Berg, Gerard J, (1990). "Nonstationarity in Job Search Theory," *Review of Economic Studies*, 57(2), 255-77.

Weber, A., and H. Mahringer (2008). "Choice and Success of Job Search Methods". *Empirical Economics*, 35, 153-178.

Winkelmann, Liliana, Winkelmann, Rainer, 1998. "Why Are the Unemployed So Unhappy? Evidence from Panel Data," *Economica*, 65(257), 1-15.

Appendices

3.A Dimensions of Job Search Behavior and How Policy May Affect them

The common strands of job search theory base their models on two fundamental variables which define the optimizing behavior of the individuals: search effort and reservation wages (see e.g. Eckstein and Van den Berg 2007 for an overview on empirically applicable models). Beyond that, empirically oriented literature discusses the choice of different search channels as a further dimension of search behavior. Finally, a small, recently emerging behavioral literature introduced the notion of (biased) beliefs into the framework of job search models. The aim of this section is to integrate these perspectives – and to enrich them by a fifth variable: search strategy choice – in order to discuss them in a common context. For the sake of illustration, I show how these 5 fundamental variables could be integrated in a common job search model, in the context of the policy intervention which is discussed here (coaching). The setup of such a model helps structuring the reasoning about hypotheses concerning the impact of the policy on different dimensions of behavior.

3.A.1 An Illustrative Model

The point of departure for the development of this illustrative model is a basic job search model with endogenous search effort, as presented, e.g., by Mortensen (1986). The unemployed individual searches sequentially for a job with *effort* e, typically (and here) measured by the number of applications in a certain period. This effort is relevant for (co-)determining the job offer arrival rate $\lambda[\cdot]$ and the search costs $c[\cdot]$. In principle, more effort should result in more offers arriving ($\partial\lambda[\cdot]/\partial e > 0$) and in increased cost ($\partial c[\cdot]/\partial e > 0$). But when considering the quality of arrived offers, more is maybe not always better. To take this idea into account I introduce in the model below an efficiency parameter which describes the translation of effort into job finding success (see below).

Though, practice and a small body of empirical literature show that search is not just driven by one-dimensional behavior (effort choice). Considerable attention in the literature has been given to the fact that individuals use very different *search channels* (from newspapers and internet to informal contacts via friends to direct acquisition etc.) – with different efficiencies and different coverage of distinct fields of the labor market. Van den Berg and van der Klaauw (2006) and Caliendo et al (2010) show that monitoring, on one hand, and the size of the personal network, on the other hand, matter for determining whether job seekers shift their search activities either towards formal or rather towards informal channels, respectively. Holzer (1988) and Weber and Mahringer (2008) demonstrate for unemployed youth in the US and for newly employed workers in Austria, respectively, that the channel choice is driven by relative costs, expected productivity and expected success in terms of getting good job and wage offers. Their results suggest that informal channels like asking friends or relatives or direct applications (without referral) seem to be more productive in the mentioned sense.

Relying on this idea that effective channel choice rather focusses on a collection of productive channels than on maximisation of channel variety, I construct an indicator n that measures channel variety. The hypothesis is, thus, that a directed choice of channels – a certain number of channels in the middle quantiles of the distribution – should be most effective. The coaching (policy intervention) could boost the directedness of the choice.

As a third aspect of search behavior that is important but mostly neglected is *search strategy*. It not only matters how much and through which channels an individual searches – but also *where* s/he searches. Strategy improvement over the course of unemployment could imply the change or extension of search to other industries, other places of work, other occupations, other types of employers etc. Change of search strategy through extension of the search scope in (one or several of) the mentioned dimensions opens up a further range of potential job offers which – so the hypothesis – finally increases job finding rate. Such a hypothesis can become even more important in a context of intense coaching, counseling or job search assistance as a policy intervention, like in the case of the field experiment here. Therefore, I introduce search strategy change a – here in the form of the extension of the scope of search[25] – as a further dimension of search behavior. The three dimensions e, n and a are defined as being the determinants of the search function $s(e, n, a)$ which, in turn, is the main determinant of the cost and job offer arrival functions (see below).

Beyond these three direct measures of search behavior, the optimal strategy of the individual is to be characterised by a *reservation wage* w^r. It is defined, in theory and in the data used here, as being the minimal acceptable wage offer. The classical search models used as a base for empirics assume that the optimal reservation wage strategies are constant (see, e.g., Eckstein and Van den Berg 2007). This comes, among others, from the fact that these models assume no evolution (over the course of unemployment) of the encountered wage offer distribution; as well, the expected value of a future job is assumed to be the same, independently when in the spell the optimisation problem is faced. This is, of course, not the case in the real world. Nonstationarity has, thus, been introduced into search theory (e.g. Van den Berg 1990). Moreover, recent real-time-search laboratory experiments (Brown, Flinn and Schotter 2009) show a sharp decline in reservation wage over time. The authors explain this by the searchers experiencing non-stationary subjective costs of time spent searching. A further reason for changing reservation wage patterns are policy interventions which influence the value of continuing search. This is the case with coaching which

[25] Analysis of the data of this field experiment shows that coaching indeed led to a big increase in search strategy adaptations. Within these adaptations, the fact of extending the scope of search in at least one dimension prevailed. Therefore, I use an indicator "search strategy: extension" in the empirical analysis. See also section 3.3.1 for further data description.

potentially renders individuals more effective and more realistic in job search. Accordingly, I distinguish periods before and after coaching to allow for different levels of reservation wage choice.

As a final ingredient of the model to be analysed in this paper, I introduce *beliefs about success of job search*, $\tilde{p}$ in our model. A recently emerging behavioral search literature demonstrates that beliefs are important in shaping search outcomes and unemployment duration. Falk, Huffmann and Sunde (2006a) show in a lab experiment that job seekers are indeed uncertain about their job finding probability. Unsuccessful search induces individuals to revise their beliefs downwards; erosion of self-confidence decreases probability (or increases duration) of search, as they can show in the lab and in theory (Falk, Huffmann and Sunde 2006b). As a consequence, this suggests that the job finding rate for such low-confidence individuals – pessimists – is lower. Note that such a conclusion is intuitive in general for pessimism, i.e. also if already the starting beliefs (or the priors) are biased downwards. The lab experiment finds as well that upward biased beliefs induce wrong amounts of search. Overly optimistic individuals overestimate their job finding probability and may, thus, search less than optimal and prolong their unemployment spell, i.e. reduce their job finding rate. Such a behavior could alternatively be explained by hyperbolic discounting (see Paserman 2008). In short, this suggests the hypothesis that biased beliefs reduce the job finding rate. This can be directly tested in the data.

What exactly means "beliefs about success of job search"? In a neutral sense, this can be defined as an subjective expectation about the probability of finding a job. This definition is used here for $\tilde{p}$ as well as in Falk, Huffmann and Sunde (2006b). This can be specified by assuming that the subjective probability is mainly driven by the belief about the personal *ability* to find a job. This interpretation is put forward by the aforementioned authors as well as by the study of Spinnewijn (2009) who names these beliefs as baseline beliefs. Being the first derivative of the probability with respect to effort, the latter introduces as well so-called control beliefs. They correspond to the perceived efficiency how effort translates into the job finding probability. The model and the data in this study allow to estimate this parameter (γ below), but not to directly measure it. An alternative concept of beliefs is put forward by Dubra (2004). He models them as (biased) expectations about the *job offer distribution*. The interpretation being different, the hypothesised effects of biased beliefs on job finding rates (and unemployment duration) are the same. It is interesting, though, to mention that such an interpretation of evolving beliefs directly implies non-stationary reservation wage paths (Burdett and Vishwanath 1988).

Finally, an important aspect of beliefs in job search models is that the impacts of labor market policy interventions on the evolution of beliefs has not yet been studied, neither empirically nor theoretically. This paper aims at giving a first empirical insight into that question.

I integrate now the five above-mentioned dimensions of job search behavior into a basic job search model. This is done for illustrative reasons, to structure the thoughts about the behavioral mechanisms induced by the experimental policy intervention in form of a coaching program. Thus, solving such a model is not in the scope of this paper. I focus therefore on presenting and discussing the two crucial Bellman equations which define the optimisation problem of the job seekers in two distinct states of the treatment plan. The models follows the familiar structure of asset flow value equations, as presented, e.g., in Pissarides (1990). According to the sequential treatment plan (details see next section), I distinguish two states: the state of *anticipation of the coaching*, and the *post-coaching* state. It is crucial to note that the value functions are different. In the anticipation state, which starts at unemployment entry (t_0), individuals take into account the costs and benefits of the upcoming coaching – pre-coaching behavior is influenced by the anticipation of the value of coaching. This element is, obviously, not present any more in the post-coaching state, which start after coaching exit (t_2). Note, though, that the coaching effect may enter through the change of various efficiency parameters (see discussion below). Following the implementations of Boone and Van Ours (2004) and Abbring et al (2005) in modeling job search with ALMP (active labor market policy), I model the two Bellman equations as follows:

$$\begin{aligned}\rho V_{u0} &= \max_{e_0,n_0,a_0} \Big\{ b - c\big[s(e_0,n_0,a_0)\big] + \lambda\big[\mu, s(e_0,n_0,a_0), \tilde{p}_0\big] \int_{w_0^r}^{\infty} (\frac{w_0}{\rho} - V_{u0}) dF(w_0) \\ &\qquad + \varphi\big[\tilde{p}_0, s(e_0,n_0,a_0), \varepsilon\big](V_{u2} - V_{u0}) \Big\} \qquad (3.3)\\ \rho V_{u2} &= \max_{e_2,n_2,a_2} \Big\{ b - c\big[s(e_2,n_2,a_2)\big] + \lambda\big[\mu, s(e_2,n_2,a_2), \tilde{p}_2\big] \int_{w_2^r}^{\infty} (\frac{w_2}{\rho} - V_{u2}) dF(w_2) \Big\} \qquad (3.4)\end{aligned}$$

Optimal reservation wages imply $w_0^r = \rho V_{u0} = \rho V_{e0}(w_0^r)$ and $w_2^r = \rho V_{u2} = \rho V_{e2}(w_2^r)$. The job finding rates for optimising individuals can be represented as $\theta_{u0} = \lambda\big[\mu, s(e_0^*, n_0^*, a_0^*), \tilde{p}_0\big]\big[1 - F(w_0^r)\big]$ for the anticipation period and $\theta_{u2} = \lambda\big[\mu, s(e_2^*, n_2^*, a_2^*), \tilde{p}_2\big]\big[1 - F(w_2^r)\big]$ for the post-coaching period.

The optimisation problem before coaching, (3.3), consists of three elements: (i) The flow of benefits (b) net of search costs; the search costs are determined by three dimensions of search behavior: effort, channel choice, strategy choice. (ii) The *perceived* job offer arrival rate times the expected gain of finding a job over staying unemployed. Here, beliefs (potentially) affect the determination of the job offer arrival rate, besides other elements like the labor market thightness μ and the search function. Thus, the theoretical idea is that overly pessimistic or optimistic individuals under- or overestimate, respectively, the arrival rate of job offers, which distorts the expected value of finding a job. (iii) The transition rate to entering the post-coaching period as an unemployed times the differential value of being unemployed and coached as compared to being unemployed in early stages (before coaching). The transition rate is dependent on search activity, the subjective probability (not) to find a job and the compliance rate (ε: probability to intendedly non-comply[26]). The value differential captures as well the net expected utility of coaching. If a threat effect prevailed (e.g. Graversen and Van Ours 2009, Black et al 2003), this utility would be negative. This is not the case, as the companion paper Arni (2011) shows: coaching exerts a significant attraction effect, thus a positive expected utility.

In the post-coaching period, (3.4), the individual optimises the expected value only among the elements (i) and (ii). Note that in this stage several factors of the optimisation problem could have been implicitly changed due to the coaching. In particular, coaching could have changed *effectiveness* and directedness of search, beliefs have been updated, and the considered job offers – reflected in the wage distribution – could have changed due to an extension of search strategy. Put more formally, the first derivatives of the job finding rate with respect to the five dimensions of job search behavior can be affected by the policy intervention and the course of unemployment. I.e., the derivatives $\frac{\partial \theta_{u2}}{\partial e_2} = \frac{\partial \left\{ \lambda \left[\mu, s(e_2^*, n_2^*, a_2^*), \tilde{p}_2 \right] \left[1 - F(w_2^r) \right] \right\}}{\partial e_2}$, $\frac{\partial \theta_{u2}}{\partial n_2}$, $\frac{\partial \theta_{u2}}{\partial a_2}$, $\frac{\partial \theta_{u2}}{\partial \tilde{p}_2}$ and $\frac{\partial \theta_{u2}}{\partial w_2^r}$ contain *efficiency parameters* of the five behavioral variables. Let them be $\gamma_2, \nu_2, \alpha_2, \beta_2, \phi_2$.

These five efficiency parameters determine how each of the 5 behavioral elements translates into the change of the job finding rate. They describe, thus, the *impacts* of different behaviors concerning job search effort, channel and strategy choice as well as success beliefs and reservation wages. $\gamma_2, \nu_2, \alpha_2, \beta_2, \phi_2$ are the theoretical equivalents to the empirical impact estimates for different behavioral dimensions – i.e., one could in principle decompose the treatment effects of those dimensions: Such a decomposition would allow to differentiate the treatment effects into effects concerning behavioral changes of $e, n, a, \tilde{p}, w^r$ and into changes of the impact size, i.e. of $\gamma, \nu, \alpha, \beta, \phi$.

Note that $\gamma, \nu, \alpha, \beta, \phi$ are assumed to be exogenous in the context of the model presented above. For the post-coaching period, the idea is that one of the main results of coaching is a change of the efficiency parameters $\gamma_2, \nu_2, \alpha_2, \beta_2, \phi_2$. These changed parameters are then taken as exogenously given for the optimisation problem (3.4). Similarly, at the beginning of the anticipation period, the efficiency parameters $\gamma_0, \nu_0, \alpha_0, \beta_0, \phi_0$ are pre-determined by the individuals: They anticipate the expected utility of the upcoming coaching and adapt their efficiency parameters accordingly; they could, e.g., reduce γ_0 to "avoid" finding a job already before coaching starts. Having set these parameters, individuals go into solving optimisation problem (3.3).[27]

[26] The non-compliance rate in the social experiment is around 3%, i.e. very small, as direct surveying showed (see Arni 2011).

[27] Technically, the setting and use of such pre-determined parameters could be thought of as a problem of constrained optimisation: Thus, the parameters defined above could be seen as restrictions under which optimisation problem (3.4) has to be solved (analog case for problem (3.3)). An alternative modeling approach would be to integrate $\gamma, \nu, \alpha, \beta, \phi$ directly into the search and acceptance rate functions. Since it is not the aim of this paper to solve the outlined model, such issues are not further detailed here.

3.A.2 Generating Hypotheses from the Model

Now, as a next step, hypotheses can be made on the evolution of the 5 behavioral elements $e, n, a, \tilde{p}, w^r$ and their corresponding efficiency/impact parameters $\gamma, \nu, \alpha, \beta, \phi$ as an effect of the different stages of the treatment plan. Based on the fact that the coaching treatment generated an attraction effect in the anticipation period (Arni 2011), the hypothesis of a reduced search effort e and/or efficiency γ seems sensible. The positive utility of coaching increases value of staying unemployed, it is thus behaviorally optimal in this period not to be too successful in search[28]. The variety of search channels n is in this early period presumably increasing – individuals try out different ways of search –, whereas efficiency of channel use ν is presumably rather low, before the learning process through coaching and counseling starts having impact. It can be hypothesised that the effect of increasing the number of search channels on the job finding rate follows an inverse U-shape – a certain variety is good, to much can be ineffective. Issues of search strategy change, i.e. a and α, do typically not yet play a role in very early stages of unemployment, they are therefore marginal in the anticipation period (and not estimated in the empirical model).

How do beliefs react on the expected policy interventions in the anticipation period? As discussed above, biased beliefs in both directions reduce job finding probabilities, from a theoretical and empirical point of view. For pessimists, this tendency could be *reinforced* by the anticipation of coaching: a low $\tilde{p}$ reduces the expected value of future employment, whereas the coaching utility increases the value of staying unemployed – early job finding gets even less attractive. For overly optimistic beliefs, things are ambiguous. Optimism could have a multiplier effect in the sense that it positively boosts search behavior $s(\dot{})$, thus the increase of the subjective job offer arrival rate would dominate and therefore improve the value of future employment and the job finding rate. On the other hand, optimists could behave like hyperbolic discounters and postpone search; moreover, they could be tempted to keep reservation wage w_0^r high; this would result in a negative effect on the job finding rate. Thus, it is up to empirics to evaluate which effect dominates. How anticipation of the coaching influences the impact size β_0 of (biased) beliefs could be driven by the signal the referral to coaching sends to the concerned job seeker. The referral could be interpreted by the job seeker that he needs support in self-assessment or in self-confidence. As a consequence, he may rather impair the importance of his own beliefs.

One hypothesis on the indirect reaction of reservation wages in interaction with beliefs has yet been mentioned. A further, direct argument to keep reservation wages high in the anticipation period is the same as used for search effort: individuals appreciate the upcoming coaching, this utility adds to the attractiveness of staying unemployed. Moreover, assessment of the own chances on the labor market may be rather noisy at the beginning of unemployment (if the individual is not a repeated job changer). If the job seeker is aware of that she would rather show

[28] To avoid the risk of a benefit sanction due to too low search effort, a strategy of rather reducing γ than e could be more promising from the point of view of the job seekers.

tendency to lower the importance ϕ_0 of reservation wage in determining total behavior. Combining that with the above-mentioned signaling argument results in the hypothesis that the coaching anticipation/referral induces a lower impact size of reservation wage behavior.

The following hypotheses on *post-coaching* behavior are based on the assumption that the coaching achieved its goal of improving self-assessment and search efficiency. If that was empirically not the case, the hypothesised effects would not materialise or their signs could even revert. Note that such hypotheses of post-coaching behavioral effects need empirically be tested in a two-step procedure: first, one needs to evaluate whether the treatment indeed had an impact on some dimensions of search behavior, then, this impact has to be related to the job finding rate (see section 3.4 for the empirical implementation). A first hypothesis directly results from the mentioned assumption of coaching effectiveness: Due to coaching, the efficiencies of search effort and channel use, γ_2 and ν_2, should increase, which is positive for the job finding rate[29]. The impact on α seems more ambiguous: Coaching should as well improve the efficiency of the implementation of search strategy changes; on the other hand, the threshold (or the pressure) to do a strategy change is lower (higher) in the context of a coaching, so in tendency also less effective strategy changes are being executed, with negative impact on efficiency. How do the levels of e, n and a change as a consequence of coaching? Under the assumption of coaching effectiveness, it is pretty obvious to conjecture that more strategy changes have been done and that this is in tendency positive for the job offer arrival rate (in particular if a change mostly means an extension of the scope of search, as it is the case here, see section 3.2). For e and n the above-mentioned hypothesis of an inverse U-shaped effect can be adopted: coaching could lead to focussing the individuals on an optimal, rather than a maximal, level of e and n.

The possible effect of the treatment on reservation wages can be summarized in three hypotheses: *over-confidence, disillusion, frustration*. The first hypothesis implies that the training of self-assessment and self-marketing skills in the coaching program distorted confidence upwards; individuals overestimate their labor market chances and therefore set a reservation wage which is too high; a lower job finding probability is the result. If training of self-assessment resulted in a *realistic* picture of the individual's labor market chances, reservation wages are revised in an optimal amount downwards – this is the case of disillusion. The study of Spinnewijn (2009) as well as this here (see section 3.2) show that job seekers ex-ante overestimate (or at least overreport) their job market chances; being realistic means therefore a downward revision. The disillusion scenario would yield an improvement in the job finding rate. The final case of frustration represents the opposite of the over-confidence case. Setting a too low reservation wage means accepting more low-quality jobs; this would become visible in the empirical results by a lower stability of future jobs. Note that these three cases narrowly relate reservation wage discussion with beliefs about

[29] An opposite effect consists in the fact that higher efficiencies reduce search cost $c[\cdot]$. This improves the value of staying unemployed; but it could also act as an incentive to search more. Thus, in total, the net impact of such effects on search costs seem to be smaller than the above-mentioned direct efficiency gain in search.

labor market conditions, proxied by the wage offer distribution. Accordingly, these hypotheses are linked to the models of Burdett and Vishwanath (1988) and Dubra (1999). A further argument from stationary search theory is that job seekers want to profit from the efficiency gain in search, due to coaching, through becoming more selective in accepting job offers, i.e. increasing w_2^r. Such a behavior would (partially) counterbalance the positive effect of search efficiency on the job finding rate. Finally, the impact size phi_2 of reservation wages on job finding should increase due to coaching and counseling, if we apply the same argument as above (less noisy assessment of job market chances).

In which way does coaching influence the updating of beliefs $\tilde{p}_2$? The three cases of over-confidence, disillusion and frustration can also be applied to this question. Coaching could accelerate downward updating and make job seekers more pessimistic. If they haven't been optimistic ex-ante, this would result in a worse job finding rate. On the other extreme, coaching could support too much upward updating, and over-confidence ends up as well in a lower job finding rate (due to too less search). Alternatively, over-confidence could support the above-mentioned positive multiplier effect on search, which would improve the job finding rate – the net effect of over-confidence is theoretically not clear, though. Disillusion, finally, would mean that coaching resulted in adjusting the beliefs to a realistic level. Being not biased any more, beliefs would not have the potential to negatively influence job finding rate. Last, the importance β_2 of beliefs for shaping the job finding rate could increase due to coaching. If coaching strengthens the job seeker's perception that she improved in assessing the own profile and competences, then she will believe more in her own beliefs.

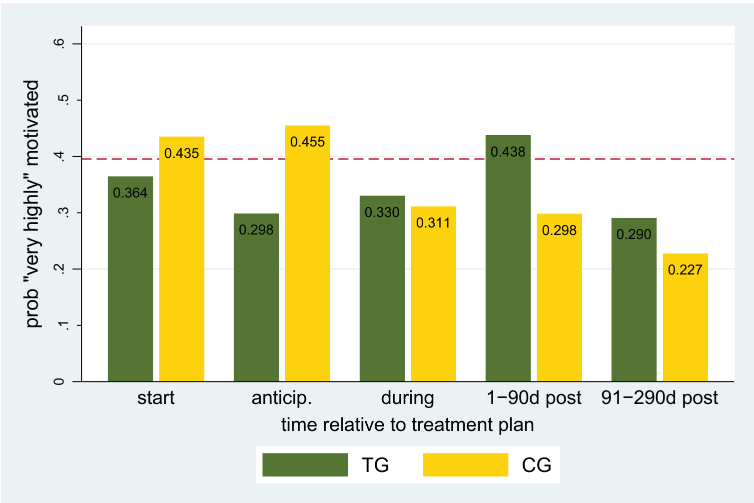
The theoretical analysis shows that for a series of behavioral impacts of the treatment several hypotheses with opposing directions of effect can be made. In order to assess which of these hypotheses dominates, empirical analysis is necessary. In the following, I develop and discuss such an analysis. A crucial feature of the empirical framework is that I introduce, in its last step, all 5 behavioral dimensions and their impact sizes into the same model. This allows for analyses of the effect of one behavioral channel conditional on fixing the impact of the others – which is a basic condition for disentangling overlapping behavioral effects.

3.B How does the Policy Intervention Affect Motivation and Happiness?

This field experiment allows as well to make some statements on how intense support of the unemployed affects happiness, measured with the standard life satisfaction question. Whereas there is nowadays a large literature on the relation of unemployment and unhappiness (see, e.g., early studies of Clark and Oswald 1994, Winkelmann and Winkelmann 1998), the direct impact of labor market policy on the evolution of happiness has not been analysed yet, to my knowledge. This raises the interesting question whether the harm of individual well-being caused by unemployment can be alleviated by supportive labor market policy. It turns out that this is, to a certain degree, the case – with a sustaining positive effect even after unemployment.

Thus, I want to shed a light on the question how the policy intervention – and its behavioral mechanisms behind – causes non-standard outcomes to react: In particular, how does the policy intervention affect motivation and happiness? Job search motivation can be considered as intermediary behavioral outcome which may be the base for economic action thereafter. Happiness is seen as an alternative outcome indicator which measures utility in a broader sense (see e.g. Blanchflower and Oswald 2004).

Figure 3.10: Motivation for job search by periods of the treatment plan



The analysis of motivation and happiness follows the same experimental difference-in-differences estimation approach as described in section 3.4. One can consider basically two roles of *job search motivation*: First, it can act as a subjective predictor of upcoming job search activity. Second, it may be interpreted as a more psychological indicator that tracks intrinsic motivation (for professional activity) of the individuals. This second interpretation would point to a more idiosyncratic motion of the indicator, rather independent of directly search-oriented behavior.

The first interpretation, on the other hand, would let us expect a clear correlation between job search motivation and, e.g., job search effort. I will perform some simple correlation analyses to descriptively test that.

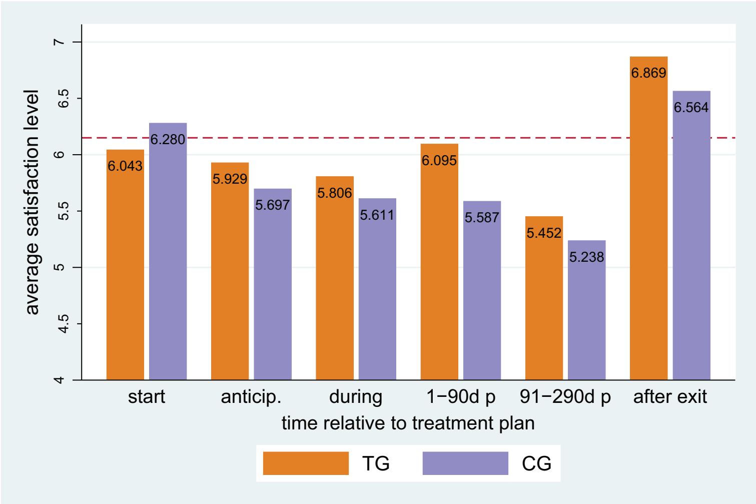
How is job search motivation affected by the counseling and coaching intervention? Figure 3.10 and Table C4 provide an answer. In Figure 3.10, mainly two observations are striking: Job search motivation of the treated plummets in the anticipation period and then starts resurging. The first decrease is consistent with the attraction effect behavior, observed in other indicators above: The prospect of being coached seems to reduce motivation to proactively search for job offers in the anticipation period. The DiD coefficient in Table C4 shows, though, that the size of the reductive effect does not get significant. The second observation in the figure concerns the different time evolution of motivation in control and treatment group. Whereas in the first case motivation gradually falls over time, the treatment group motivation re-ascends after anticipation. It reaches a *significantly higher motivation niveau in the first three months after coaching*. Then, motivation remains higher than in the control group, but difference becomes insignificant. So, the main result of this analysis is that *coaching had a significantly positive influence on job search motivation in the shorter run (up to 3 months thereafter)*. In the anticipation period, it shows some indication of the attraction effect.

The Table C6 presents some descriptive evidence on the correlation of job search motivation and search effort – first on contemporaneous correlations, then time-lagged ones. A main insight is that *job search motivation and effort are indeed significantly correlated*. This rather points to the economic interpretation of job search motivation as being a proxy for subjectively predicted – or yet realised – job search activity. As I find correlations in the contemporaneous case (1) and in the time-lagged case (2), I cannot make a certain statement about which time relation prevails between search motivation and effort, based in this descriptive evidence. It is interesting to see that in the anticipation period the correlation between motivation and effort relatively increases[30]. The fact of searching less efficiently (see last subsection) is corroborated by low motivation. This higher correlation disappears during coaching: Whereas search effort is low due to the lock-in effect, motivation remains stable. After coaching, the correlation is higher in the treatment than in the control group again, though correlations fall below significance level. The fact that coaching boosted job search motivation is, in tendency, correlated with the slight resurge of search effort after the lock-in period. In general, correlation between search motivation and effort gets *weaker* in the later stages of unemployment.

As a final step, I move to the analysis of general life satisfaction (happiness) as an alternative outcome measure. Figure 3.11 shows a distinct picture: Happiness clearly decreases over the course of unemployment – a finding which is not new (see section 3.A). Three months after exit from

[30]Note that we observe here a certain unbalancedness of starting levels at t_0. This unbalancedness is imported through the initial differences in search effort found earlier, which were caused by coincidence in the randomisation. Therefore, interpretation needs to be made relative to the initial values.

Figure 3.11: Life satisfaction by periods of the treatment plan and 3 months after unemployment exit



unemployment insurance, happiness is on a clearly higher level again. A look into the estimations by treatment period, see Table C5, reveals interesting differences in the evolution of life satisfaction: Unlike motivation, *happiness does not go down for the treated in the anticipation period.* This corroborates the interpretation of the found negative anticipation effect as being an *attraction* effect. Was the prospect of being coached not considered as being pleasant, the happiness would have gone down by the extent of the control group's. *In the first three months after coaching, I find a significantly higher happiness level in the treatment group* than in the control group. Thereafter, the positive effect gets somewhat smaller an insignificant. So, in these periods, happiness and motivation evolutions are quite parallel. Note that the presented correlation Table C6 reports as well a substantial correlation between motivation and happiness[31]. Finally, an important result is found for the post-unemployment situation. Three months after unemployment exit the happiness increase caused by coaching still sustains: *treated individuals remain significantly happier beyond unemployment exit.* To wrap up, the experimental policy intervention, with the coaching program as its main measure, clearly caused higher life satisfaction for the concerned job seekers, which sustained also after unemployment exit.

[31] An exception is the anticipation period/early unemployment: There, correlation disappears for both groups. This makes sense for the treatment group, in the context of the above-mentioned attraction argument. The negative correlation in the control group reflects diverging evolutions: job seekers are still highly motivated to search, but the unsatisfactory situation of being unemployed begins to reflect in lower happiness values.

3.C Additional Tables

Table C1: Repeated surveys: Filled questionnaires and response rate by time of survey

Job seeker surveys

	Entry	*M2*	*M3*	*M4*	*M9*	*M13*	*Exit*
Registered job seekers	327	258	210	182	112	87	273
Questionnaires	298	198	137	106	42	31	154
Response rate	91.1%	76.7%	65.2%	58.2%	37.5%	35.6%	56.4%

Caseworker surveys

	Entry	*M2*	*M3*	*M4*	*M9*	*M13*	*Exit*
Registered job seekers	327	258	210	182	112	87	273
Questionnaires	302	213	141	114	48	42	222
Response rate	92.4%	82.6%	67.1%	62.6%	42.9%	48.3%	81.3%

Notes: See section 3.2.3 for a description of the survey timing and an exact definition of the *Entry, M2, ... Exit* dates.

Source: LZAR database.

Table C2: Repeated surveys: Balancing of observables, by treatment (TG) and control group (CG) and periods of the treatment plan

	Start		Anticipation		During Coaching		1-90d post Coaching		Later		Both Surveys @ t_2	
Job seeker surveys	TG	CG	TG	CG	TG	CG	TG	CG	TG	CG	TG	CG
Gender: woman	44.05%	43.08%	45.61%	42.42%	46.81%	44.59%	43.75%	36.17%	47.50%	37.04%	44.05%	46.15%
Married (incl. Separated)	55.95%	47.69%	66.67%	51.52%	57.45%	48.65%	64.06%	51.06%	50.00%	37.04%	57.14%	50.77%
Age	52.45	52.38	53.23	52.85	52.51	52.64	52.98	53.19	54.65	53.48	52.71	52.77
Nationality: CH	85.12%	86.15%	87.72%	78.79%	80.85%**	93.24%**	84.38%	85.11%	87.50%	88.89%	80.95%**	93.85%**
Qualification: (semi-)skilled	97.02%	96.15%	98.25%	100.00%	96.81%	95.95%	96.88%	95.74%	100%*	92.59%*	97.62%	96.92%
Employability: 4	20.83%	21.54%	15.79%	15.15%	21.28%	17.57%	18.75%	21.28%	22.50%	14.81%	22.62%	20.00%
At least 1 foreign language	58.33%	55.38%	66.67%	63.64%	57.45%	59.46%	60.94%	59.57%	55.00%	66.67%	55.95%	55.38%
Job < 100%	17.26%	17.69%	21.05%	18.18%	15.96%	21.62%	15.63%	19.15%	22.50%	11.11%	15.48%	23.08%
PES 2	14.29%	10.77%	10.53%	6.06%	14.89%	12.16%	20.31%**	6.38%**	12.50%	7.41%	15.48%	10.77%
Observations	168	130	57	33	94	74	64	47	40	27	84	65
... in %	56.38%	43.62%	63.33%	36.67%	55.95%	44.05%	57.66%	42.34%	59.70%	40.30%	56.38%	43.62%
Caseworker surveys												
Gender: woman	43.60%	43.85%	45.00%	44.12%	45.45%	45.68%	41.38%	34.88%	43.90%	35.00%		
Married (incl. Separated)	56.40%*	46.15%*	63.33%	55.88%	55.56%	49.38%	63.79%	48.84%	48.78%	42.50%		
Age	52.53	52.28	52.97	52.71	52.58	52.53	53.12	53.58	54.63	53.98		
Nationality: CH	84.88%	85.38%	91.67%*	79.41%*	81.82%**	93.83%**	82.76%	86.05%	85.37%	85.00%		
Qualification: (semi-)skilled	97.09%	96.15%	96.67%	97.06%	97.98%	96.30%	96.55%	95.35%	100%	97.50%		
Employability: 4	21.51%	22.31%	18.33%	17.65%	23.23%	17.28%	17.24%	27.91%	21.95%	17.50%		
At least 1 foreign language	58.72%	55.38%	60.00%	61.76%	54.55%	60.49%	58.62%	53.49%	51.22%	57.50%		
Job < 100%	17.44%	18.46%	23.33%	14.71%	15.15%	20.99%	17.24%	18.60%	21.95%	15.00%		
PES 2	13.95%	10.77%	8.33%	5.88%	15.15%	11.11%	22.41%**	4.65%**	9.76%	2.50%		
Observations	172	130	60	34	99	81	58	43	41	40		
... in %	56.95%	43.05%	63.83%	36.17%	55.00%	45.00%	57.43%	42.57%	50.62%	49.38%		

Notes: All TG-CG differences are not significantly different from zero, except from those marked: *** 1%, ** 5%, * 10%
Source: LZAR database.

Table C3: Determinants of search strategy change recommendation (by coach). Probit regression

	Search str. change (coached individuals)		*Search str. change* (coached individuals)	
	Coeff.	z-value	Coeff.	z-value
UE duration in past 3 years	0.004	1.18	0.003	0.82
duration until availability	0.010	1.31	0.009	1.02
age: 50-54 (base: 45-49)	-0.331	-0.59	-0.239	-0.40
age: 55-59	-0.849	-1.43	-1.160*	-1.69
age: 60+	-0.718	-0.91	-0.915	-1.05
married (base: unmarried)	1.039*	1.89	1.360**	2.06
divorced	1.200*	1.74	1.763**	2.16
female	1.708***	2.82	2.043***	2.88
non-Swiss	0.551	0.75	0.650	0.85
low employability (base: medium)	-1.016	-1.35	-0.902	-1.08
semi-skilled (base: skilled)	-0.915	-1.19	-1.413*	-1.65
unskilled	-2.545*	-1.77	-2.959*	-1.70
non-German-speaking	-0.225	-0.24	-0.197	-0.20
1 foreign language (base: 0)	1.539**	1.99	1.640*	1.89
2+ foreign languages	-1.352*	-1.92	-1.216°	-1.52
PES 2 (base: PES 1)	0.700	0.64	0.647	0.51
part-time (but above 50%)	-0.484	-0.81	-0.416	-0.63
Month of entry in UE (base: Jan/Feb 2008):				
March/April 2008	1.758**	2.32	2.346***	2.65
May/June 2008	0.269	0.41	0.026	0.04
July/August 2008	1.790**	2.36	1.972**	2.38
Sept/Oct 2008	0.832	1.27	0.823	1.20
Nov/Dec 2008	0.719	0.64	0.436	0.26
Caseworker fixed effects (base: CW 1):				
CW 2	0.097	0.10	-0.124	-0.12
CW 3	-1.241°	-1.46	-0.828	-0.81
CW 4	-0.367	-0.47	-0.383	-0.45
CW 5	2.107*	1.89	2.441*	1.95
CW 6	1.688°	1.51	1.767°	1.57
CW 7	0.429	0.52	-0.020	-0.02
CW 8	3.062***	2.61	3.134**	2.45
CW 9	-1.588	-1.03	-1.815	-0.98
CW 10	-2.101°	-1.54	-2.331°	-1.47
CW: rest (smaller charges)	0.565	0.62	0.804	0.80
duration until coaching entry			-0.001	-0.27
application knowhow not good			0.620	0.82
insufficient key qualifications[1)]:				
ability to solve problems			0.554	0.42
systematic-analytic thinking			-3.163***	-2.66
Constant	-2.514**	-1.96	-2.938**	-2.15
N	100		100	
Pseudo R^2	39.70		46.28	

Notes: 1) Survey item 'insufficient key qualification' (assessed by coach): mentioned key qualification is at a lower level than it is demanded in the field where the job seeker searches. Note that the function and occupation variables were not used in this regression due to multicollinearity issues. Analyses of similar regressions show that these variables are not relevant (significant) for the probability of getting a search strategy change recommended. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, ° $p < 0.15$.
Source: Own estimations based on merged UIR-LZAR database.

Table C4: Treatment effect on job search motivation (proportion "very highly" motivated) : OLS regressions, difference-in-differences, ITT

	(1) *anticipation*		(2) *during coaching*		(3) *1-90d post-coa.*		(4) *91+d post-coa.*	
	coef	se	coef	se	coef	se	coef	se
time (T_t)	0.025	(0.100)	-0.111	(0.068)	-0.127	(0.078)	-0.196*	(0.103)
treatment (D^{TG})	-0.073	(0.058)	-0.082	(0.057)	-0.081	(0.058)	-0.082	(0.059)
DiD ($\boldsymbol{D^{TG}T_t}$)	**-0.094**	(0.122)	**0.066**	(0.092)	**0.185***	(0.108)	**0.147**	(0.142)
UE duration in past 3 years	0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)
duration until availability	-0.001	(0.001)	-0.001	(0.001)	-0.001	(0.001)	0.000	(0.001)
age: 50-54 (base: 45-49)	-0.206***	(0.063)	-0.109*	(0.056)	-0.160**	(0.063)	-0.181***	(0.067)
age: 55-59	-0.089	(0.069)	-0.058	(0.065)	-0.146**	(0.067)	-0.146**	(0.071)
age: 60+	-0.194**	(0.084)	-0.119	(0.080)	-0.209**	(0.086)	-0.183*	(0.095)
married (base: unmarried)	0.103	(0.067)	0.201***	(0.060)	0.178***	(0.066)	0.174**	(0.068)
divorced	0.056	(0.079)	0.100	(0.068)	0.091	(0.076)	0.100	(0.080)
female	0.024	(0.067)	-0.013	(0.063)	0.016	(0.066)	-0.038	(0.072)
non-Swiss	-0.038	(0.085)	0.049	(0.082)	0.037	(0.084)	-0.086	(0.094)
low employability (base: medium)	0.134	(0.096)	0.059	(0.079)	0.069	(0.091)	0.100	(0.099)
semi-skilled (base: skilled)	-0.084	(0.119)	-0.147	(0.105)	-0.126	(0.121)	-0.152	(0.123)
unskilled	-0.174	(0.117)	-0.183**	(0.089)	-0.086	(0.116)	-0.067	(0.113)
non-German-speaking	0.197*	(0.118)	0.139	(0.102)	0.128	(0.113)	0.267**	(0.113)
1 foreign language (base: 0)	0.096	(0.085)	0.072	(0.077)	0.094	(0.084)	0.121	(0.091)
2+ foreign languages	-0.026	(0.086)	-0.055	(0.078)	-0.093	(0.086)	-0.060	(0.091)
PES 2 (base: PES 1)	0.139	(0.174)	0.131	(0.165)	0.228	(0.186)	0.118	(0.203)
management (base: professionals)	0.180	(0.112)	0.190*	(0.109)	0.211*	(0.113)	0.204	(0.125)
support function	-0.169	(0.106)	-0.173*	(0.090)	-0.137	(0.123)	-0.078	(0.121)
part-time (> 50%)	-0.111	(0.073)	-0.084	(0.067)	-0.067	(0.071)	-0.091	(0.077)
caseworker FE: CW 2	0.011	(0.123)	0.033	(0.119)	-0.003	(0.110)	-0.035	(0.138)
CW 3	-0.092	(0.101)	-0.001	(0.092)	-0.047	(0.100)	-0.024	(0.105)
CW 4	-0.061	(0.100)	-0.003	(0.095)	-0.025	(0.103)	-0.034	(0.106)
CW 5	-0.151	(0.126)	-0.118	(0.108)	-0.029	(0.121)	-0.146	(0.120)
CW 6	-0.218*	(0.111)	-0.106	(0.117)	-0.079	(0.124)	-0.111	(0.134)
CW 7	0.013	(0.114)	0.018	(0.099)	0.100	(0.111)	0.081	(0.115)
CW 8	-0.185	(0.116)	-0.161*	(0.098)	-0.127	(0.108)	-0.164	(0.117)
CW 9	-0.176	(0.211)	-0.208	(0.197)	-0.222	(0.220)	-0.187	(0.237)
CW 10	-0.129	(0.223)	-0.134	(0.209)	-0.200	(0.226)	-0.182	(0.244)
CW: rest (small charges)	-0.084	(0.147)	-0.064	(0.152)	-0.142	(0.176)	-0.055	(0.187)
Constant	0.483***	(0.107)	0.425***	(0.098)	0.440***	(0.103)	0.446***	(0.109)
Observations	388		466		409		351	
R^2	0.118		0.105		0.104		0.121	

Note: Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; available observations at t_0: 298.
Source: LZAR database

Table C5: Treatment effect on general life satisfaction (happiness), scale 1 to 8 (very happy): OLS regressions, difference-in-differences, ITT

	(1) *anticipation*		(2) *during coaching*		(3) *1-90d post-coa.*		(4) *91+d post-coa.*		(5) *post-ue*	
	coef	se	coef	se	coef	se	coef	se	coef	se
time (T_t)	-0.671**	(0.324)	-0.626***	(0.216)	-0.699**	(0.275)	-0.990**	(0.434)	0.336	(0.237)
treatment (D^{TG})	-0.194	(0.170)	-0.264°	(0.168)	-0.263°	(0.169)	-0.253°	(0.171)	-0.222	(0.167)
DiD ($\boldsymbol{D^{TG}T_t}$)	**0.438**	(0.392)	**0.366**	(0.285)	**0.718****	(0.340)	**0.433**	(0.515)	**0.509***	(0.300)
UE duration in past 3 years	0.000	(0.000)	0.000	(0.001)	0.000	(0.001)	0.000	(0.000)	0.000	(0.001)
duration until availability	0.001	(0.002)	0.002	(0.002)	0.001	(0.002)	0.001	(0.003)	0.001	(0.002)
age: 50-54 (base: 45-49)	-0.056	(0.210)	-0.128	(0.185)	-0.000	(0.187)	0.025	(0.211)	-0.052	(0.191)
age: 55-59	0.175	(0.195)	-0.221	(0.185)	-0.191	(0.208)	-0.213	(0.218)	0.066	(0.188)
age: 60+	0.109	(0.284)	0.362°	(0.227)	0.252	(0.258)	0.420°	(0.289)	0.333	(0.274)
married (base: unmarried)	0.506**	(0.204)	0.660***	(0.199)	0.664***	(0.199)	0.741***	(0.222)	0.584***	(0.193)
divorced	0.420*	(0.223)	0.501**	(0.208)	0.463**	(0.227)	0.537**	(0.251)	0.293	(0.212)
female	0.194	(0.174)	0.259°	(0.170)	0.231	(0.179)	0.123	(0.191)	0.180	(0.168)
non-Swiss	0.053	(0.280)	0.090	(0.266)	0.081	(0.276)	0.022	(0.318)	0.016	(0.265)
low employability (base: medium)	0.005	(0.295)	-0.091	(0.266)	0.015	(0.279)	0.047	(0.332)	-0.002	(0.310)
semi-skilled (base: skilled)	-0.837**	(0.385)	-0.739**	(0.353)	-0.817*	(0.416)	-0.706*	(0.417)	-0.777**	(0.366)
unskilled	0.120	(0.410)	-0.746	(0.529)	-0.094	(0.371)	0.090	(0.408)	0.065	(0.328)
non-German-speaking	0.226	(0.427)	0.015	(0.432)	0.083	(0.431)	0.046	(0.452)	0.217	(0.405)
1 foreign language (base: 0)	0.017	(0.290)	0.283	(0.249)	0.279	(0.260)	0.114	(0.274)	-0.015	(0.255)
2+ foreign languages	-0.103	(0.286)	-0.306	(0.244)	-0.234	(0.258)	-0.096	(0.269)	-0.014	(0.256)
PES 2 (base: PES 1)	-0.190	(0.518)	0.242	(0.415)	0.157	(0.475)	0.093	(0.506)	-0.041	(0.390)
management (base: professionals)	-0.175	(0.386)	0.180	(0.335)	0.170	(0.292)	0.231	(0.370)	-0.184	(0.327)
support function	-0.341	(0.501)	-0.110	(0.442)	-0.337	(0.448)	-0.255	(0.535)	-0.365	(0.418)
part-time (> 50%)	-0.028	(0.247)	-0.099	(0.215)	0.128	(0.220)	0.045	(0.242)	-0.145	(0.214)
caseworker FE: CW 2	0.256	(0.347)	0.348	(0.313)	0.331	(0.345)	-0.090	(0.348)	0.125	(0.333)
CW 3	-0.242	(0.271)	-0.145	(0.241)	0.029	(0.263)	-0.183	(0.279)	-0.084	(0.235)
CW 4	-0.248	(0.307)	-0.184	(0.278)	-0.122	(0.307)	-0.453	(0.325)	-0.301	(0.285)
CW 5	-0.110	(0.430)	0.251	(0.364)	0.105	(0.402)	-0.426	(0.466)	0.007	(0.407)
CW 6	0.142	(0.345)	0.257	(0.388)	0.233	(0.402)	-0.258	(0.517)	0.109	(0.374)
CW 7	0.059	(0.319)	0.598**	(0.274)	0.221	(0.301)	0.322	(0.335)	0.005	(0.287)
CW 8	-0.273	(0.349)	-0.019	(0.311)	-0.062	(0.340)	-0.080	(0.389)	-0.236	(0.348)
CW 9	0.097	(0.627)	0.094	(0.514)	0.034	(0.596)	0.049	(0.632)	0.033	(0.516)
CW 10	-0.375	(0.729)	-1.145*	(0.676)	-0.574	(0.651)	-0.768	(0.755)	-0.764	(0.597)
CW: rest (small charges)	-0.250	(0.405)	-0.093	(0.370)	0.037	(0.462)	-0.202	(0.471)	-0.059	(0.368)
Constant	5.963***	(0.298)	5.729***	(0.290)	5.627***	(0.319)	5.769***	(0.323)	5.960***	(0.273)
Observations	388		466		409		351		404	
R^2	0.0817		0.124		0.0954		0.134		0.119	

Note: Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ ° $p < 0.15$; available observations at t_0: 298. ue=unemployment.
Source: LZAR database

Table C6: Correlations between job search motivation, search effort and happiness, by periods of the treatment plan and by treatment group (TG) and control group (CG)

		total	t_0	anticip.	during coa.	1-90d post	91+d post
(1) motivation<>search effort	TG	0.123**	0.136*	0.384***	-0.011	0.086	0.215
	CG	0.189***	0.270***	0.364**	0.307**	0.045	0.032
	total	0.159***	0.194***	0.377***	0.179**	0.056	0.122
(2) motivation>search effort at $t+1$	TG	0.030	0.008	0.284°	0.011	0.222	
	CG	0.273***	0.299*	0.671***	0.166	0.091	
	total	0.148**	0.156°	0.522***	0.084	0.141	
(3) motivation <> happiness	TG	0.199***	0.156**	0.155	0.319***	-0.141	0.384**
	CG	0.292***	0.236***	-0.090	0.542***	0.294**	-0.022
	total	0.245***	0.193***	0.053	0.447***	0.137°	0.241**
Observations (1)		678	293	82	154	92	57
Observations (2)		295	90	43	76	53	
Observations (3)		734	298	90	168	111	67

Note: Pairwise correlations; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ ° $p < 0.15$.
Source: LZAR database

Conclusion

The first two chapters of this dissertation provide a series of results and insights on how labor market policies in unemployment insurance shape (in particular) post-unemployment outcomes. The presented evaluations cover two essential, opposite types of labor market policies which are at disposal of policy makers: Supportive measures like training, coaching and job search assistance – and sanctioning measures like benefit sanctions systems and monitoring. Whereas the first analysed policy (a combination of coaching & counseling) increases employment stability and avoids therefore future unemployment, the second type of policy turns out to be harmful for post-unemployment development. Considering the net effects – which trade off unemployment and post-unemployment outcomes –, the first policy results in a positive effect (bigger employment stability without prolongation of unemployment before). The second policy type, on the other hand, causes a reduction in earnings generated in the two years after unemployment which is clearly bigger than the gain of additional earnings by re-starting employment earlier. There are many more results in these two studies – in particular a series of interesting findings on anticipation-, warnings- and ex-ante effects. They can be found in the respective sections of the first two chapters.

In the third chapter, I adopt a behavioral focus: What happens in the 'blackbox' of job search behavior when individuals get challenged by an intense supportive policy intervention (coaching) that aims at improving self-assessment and job search skills? Using this social experiment, repeated surveys and register data, I find distinct reactions on the policy intervention: In the anticipation period and during coaching, the individuals reduce search effort and the frequency of use of formal search channels like newspapers and internet. Moreover, individuals keep reservation wages high during anticipation. At the end and after coaching, in contrast, the job seekers apply the learned skills and search more efficiently, mainly more directedly. I observe as well that treated individuals reduce their reservation wages. This could be shaped as a disillusion effect: The treated individuals get more realistic about their requirement towards a future job. When comparing expectations and realisations of job interviews, the data show that unbiased beliefs are more beneficial for getting a job. However, the beliefs of the job seekers are clearly positively biased – they overestimate their job chances. This bias in beliefs is slightly dampened by the coaching and counseling intervention. It is an interesting insight for policy design that this type of labor market policy is able to significantly change reservation wages and biased beliefs – in the direction of a more realistic assessment and, as a consequence, more successful job finding. By the way: the individuals get as well happier due to the coaching & counseling intervention, and they remain happier even 3 months after unemployment exit. More results can be detected in chapter three of the dissertation.

From a methodological point of view, the arch of this dissertation goes from the advanced use of methods of duration modeling and controlling for correlated unobserved heterogeneity in the context of endogenous treatment assignments, on the one hand, up to fully randomised experimental approaches, on the other hand. Thus, it was a further aim of this dissertation to learn and explore the broad range of state-of-the-art treatment evaluation approaches from fully non-experimental to fully experimental. So, the toolkit for development of future research is at disposal.

What are the implications of the results found in this dissertation for the future research agenda and for policy design? The second question has been answered in detail in the conclusion sections of the three chapters. I want therefore focus here on more general conclusions for policy design which can be made based on the results of the three studies. First, it is recommendable that policy makers in labor market policy and related fields *extend their policy function*: The goals with respect to which policy gets optimised should adopt a more holistic perspective. As the results on post-unemployment, behavior, beliefs and happiness demonstrate – the impacts of labor market policy go beyond the question of shorter vs. longer unemployment duration. The policies influence, through post-unemployment impacts, as well equilibrium unemployment and the aggregate of economic value generated. Moreover, policy-induced changes of biased beliefs, behavior and happiness can become relevant for future economic activity and success on the labor market. Thus, the goals what labor market policy should achieve and what not, should be specified beyond the standard statement of reducing unemployment in the short run. Once the policy aims are specified in such a way, then policy can – and should, this is the second recommendation – be *stronger targeted*. The found results suggest that there are subpopulations – defined, e.g., by age or also by a certain behavioral pattern – which best profit from a certain type of policy. Certain subgroups need coaching of some skills, others need disillusion, for others a focus on monitoring can prove useful, etc. The here presented combination of behavioral data and specific evaluation procedures can provide models which allow to better *profile* the individuals – which is the base for more targeting.

A third recommendation for general lines of policy design is *early and intense intervention*. The result found in the second chapter that early and intense intervention could avoid the cost of prolonged unemployment duration can be generalised, to some degree. The phenomenon of the attraction effect can in principle appear for the whole group of supportive labor market policy, like training courses, coaching and job search assistance. They all are potentially attractive, such that job seekers may show tendency to wait, in order to be able to participate in these measures later. Starting such labor market policy measures early in the unemployment spell is a simple measure to reduce negative effects on unemployment duration. Similar, it is recommendable to perform policy measures like training and coaching in a short and highly intense manner, instead of spreading them out over a longer time. Like that, the negative lock-in effect (less search during the program) can be reduced while maintaining the total amount of training content. Thus, there is optimisation potential in the unemployment insurance system through cleverly timing the labor market policy.

Fourth and finally, the results of the final chapter of the dissertation suggest that more should be invested in the design of policies which are able to *specifically affect certain kinds of behavior*. Thus, for example, there should be mechanisms designed which act against the bias in the beliefs of job seekers and of caseworkers. Results show that biased beliefs harm job finding success, so reducing this bias would be of direct use for the outcome. The same applies to potentially too high reservation wages. Another example would be the issue to design trainings which directly support the efficiency of search and channel use; efficiency can be as important as quantity. It is obvious that such targeted mechanism design is nontrivial and necessitates more research in behavioral job search approaches. So, a part of a future research agenda consists in investing in this kind of research – through, e.g., theoretical models about the interaction of beliefs and policy interventions, and through lab and field experiments to test new policy designs.

Two further elements of a potential future research agenda are the exploration of two kinds of interaction effects: First, *interaction between different types of labor market policies*. Sanctioning and supportive policies are normally both present in parallel. Evaluations up to now typically assume that the effects of such policies are mutually independent. This assumption is not really plausible. The fact of being in (or anticipating) a training program, e.g., may influence the threat and risk of being sanctioned, and vice versa. Thus, it is an interesting question for further research (which we already started) to assess impacts of policy mixes jointly. Moreover, note that the insights of such a research could further specify how the negative net impact of sanctioning on earnings could be reduced: maybe through a more optimal mix of supportive and sanctioning policies. Finally, a second type of interaction of high interest is the *interplay between different social insurances*. The proportion of individuals who switched from unemployment into disability insurance increased in the last years in Europe. Another issue is the potential efficiency losses in treating optimally individuals who switch from unemployment insurance to social assistance. Or a further issue is the interaction of unemployment insurance and early retirement. Here again, a more holistic perspective on social policy evaluation may prove useful for the future.

Thus, there are more than enough topics and discussion issues for future research. Research that has – in order to remain inspiring – to get off the beaten tracks and to pave new tracks.