How to Improve Labor Market Programs for Older Job-Seekers? Evidence from a Social Experiment

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Abstract: Older job seekers often face a higher longterm unemployment risk because their employability decreased over time. Therefore, targeted training is necessary to increase the older worker's core competencies, in particular their ability to better market themselves and to optimize their labor market success. I evaluate an new social experiment which implements a novel active labor market policy intervention in Switzerland that follows this idea. The evaluation is based on a unique dataset that merges register data with repeated surveys. I find that the new design principles of the intervention – earlier than normal, highly intense and clearly targeted – prove to be successful: Unlike the vast majority of training programs, this program doesn't yield a negative effect on unemployment duration. Moreover, it increases the stability of the first post-unemployment job; individuals who followed the new intervention showed a lower propensity to reenter unemployment. As a further novel result, I find a negative anticipation effect – an "attraction effect". Individuals are motivated to participate in the employability coaching – in contrast to many other programs where a "threat effect" is normally found. One policy conclusion of the experiment is that the costs of a training program can be reduced if it is taken up as early as possible and performed in a highly intense and targeted manner.

JEL Classification: J64, J65, J68, J14

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1 Introduction

Advanced working age and decreasing employability, going often together, are two major driving forces of long-term unemployment. It is defined as unemployment that exceeds the duration of one year. Long-term unemployment is considered as being especially harmful to the labour market prospects of concerned individuals. A longer absence from labour market implies most often a remarkable loss in human capital, employability and self-esteem. As a consequence, *avoiding* long-term unemployment (LU) – through reduction of LU *risk* – is a prominent issue for labour market policy. It gained and will gain even more importance due to the recent economic crisis and – in a longer run – due to the ongoing demographic change of the labor force. The latter observation is driven by the evidence that long-term unemployment is strongly age-dependent.

In the European OECD countries, as much as 36.8% of the unemployed individuals were concerned by long-term unemployment in the year 2008 (OECD 2009). The range goes 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 the long-term unemployed amounts to 10.6%. It is well conceivable that the generosity of the unemployment insurance benefits plays a role in determining this figure (though the low percentages in Scandinavian countries may speak against it). Beyond the institutional aspect, the prominent role of age in shaping this figure can clearly be shown. For the region under consideration in this social experiment (northern Switzerland), a highly age-related pattern arises. The proportion of individuals in unemployment insurance who face LU increases from 18.4% (age group 30–34) to 39.0% (age group 55–59) (AMOSA 2007). Thus, level and age-relatedness of long-term unemployment call for active labour market policy strategies that explicitly deal with the reduction of unemployment risk for *older workers*.

In this paper, I evaluate a new social experiment which implements a novel active labour market policy (ALMP) intervention in Switzerland that explicitly focusses on the mentioned risk group of individuals of age 45+ and lower employability. It features a fixed treatment plan which combines *individually targeted coaching* with *high-frequency counseling*. To increase effectiveness, the interventions happen *early* in the unemployment spell and at a high intensity. Thus, this treatment design takes up insights of empirical studies which show, on the one hand, that the best chances to reenter labor market exist in the first months of unemployment (e.g. AMOSA 2007) – and conclude, on the other hand, that optimal targeting of the programs on those who are most at risk would be an effective strategy (e.g. Huber et al. 2009 on German welfare-to-work programs). The social experiment was performed with 327 individuals of whom 60% were randomly assigned to the treatment group and 40% to the control group. The inflow period into the experiment lasted from December 2007 to December 2008. The individuals were observed until exit or until they reached the long-term unemployment threshold (12 months). In addition, a survey provides data about salaries etc. in the first job after exit.

This paper aims at making at least four contributions to the literature in the field. First, it evaluates the effects of a novel ALMP which follows a policy approach that is non-standard. 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 2008 and Hägglund 2006), this newly designed policy focusses more on *investing time* into the concerned individuals. This additional time per individual job seeker shall be invested into the development of labour market skills and improved search strategies. This feature distinguishes the here evaluated ALMP from the recently most common policies which combine monitoring and job search assistance (see e.g. Van den Berg et al. 2006, Graversen et al. 2008). Thus, the basic idea of the new policy is to combine the positive elements of monitoring and counseling with a more effective, targeted program to train employability.

The second main contribution of this project is its design as a social experiment. 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) 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 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 third main contribution of this paper lies in a novel combination of register data with repeated surveys. The project offered the unique opportunity to repeatedly survey all the participating job seekers as well as the caseworkers and the coach. This allows to go beyond the classical outcome analysis based on unemployment durations – as the repeated surveys can track what goes on in terms of behavioral reactions. The developments of motivation, job search behavior, reservation wages, health etc. can directly be observed. The use of such behavioral measures allows to analyse *why* certain effects on outcomes materialise. Such empirically founded behavioral explanations of ALMP effects are still broadly missing in the literature[1].

The fourth contribution of this paper lies in its focus on job seekers of older age. Whereas a small literature on active labor market programs specifically for the young unemployed exists (see Kluve et al. 2007), empirical evidence on ALMP which are focussed on older job seekers is largely missing. This is remarkable, considering the fact that the relative size of the older labor

[1] As a rare exception, one element of such an explanation is provided by Van den Berg et al. (2006) who point out that monitoring in unemployment insurance causes a shift from informal to formal job search. An alternative, more parametric approach to gain behavioral explanations is structural estimation, a recent example that analyses montoring in unemployment insurance being Van den Berg et al. (2010).

force is constantly growing.

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 (Kluve et al. 2007) – 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 two recent social experiments in Denmark and Sweden show (Graversen et al. 2008, Hägglund 2009). 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). The new ALMP considered in this paper was designed in order to combine the potentially effective elements like counseling and monitoring with a novel, highly targeted coaching program which aims at increasing (or restoring) employability and human capital. Thus, these elements of targeting and monitoring, together with the fact that the intervention is early and highly intense, should minimize the negative lock-in effect (i.e. prolongation of unemployment duration) which is normally found in human capital training programs.

The paper is organised as follows. In the next section, I will outline the different aspects of the performed social experiment: its treatment plan, its institutional background, its implementation and its potential effects from the viewpoint of job search theory. Section 3 provides the data description and descriptive analyses. Section 4 presents the econometric modeling of the treatment plan and its (intermediary) outcomes. Section 5 reports the estimation results and section 6 concludes.

2 The Experiment

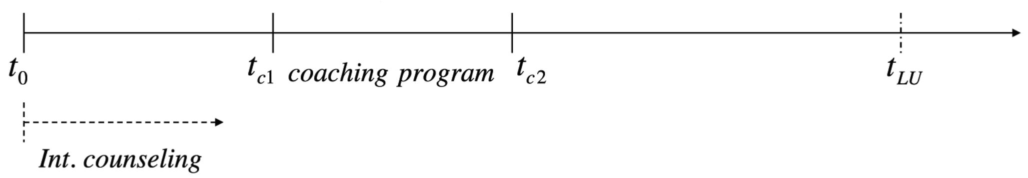
In this section, I will first describe the interventions that constitute the treatment plan. Then, I will shortly outline the institutional background: the Swiss unemployment insurance system and some facts about the (long-term) unemployment situation in the region of the project. Next, the specific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

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 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 through the whole spell until unemployment exit, if this happens within a year. Otherwise, the frequency goes back to normal after one year of unemployment when the concerned individuals reach the long-term unemployment threshold. At this point, the project is finished.

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.* 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 and increased demands towards search effort of the job seeker.

The *coaching program*, the second main measure, starts in median after 46 days (48 days for those who really participate, 45 days until potential coaching entry for the others[2]). Thus, the

[2] Note that, due to the fact that the timing of the measures was fixed ex-ante, I can identify the *potential*

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). 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, see section 2.4).

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 active labor market programs normally started clearly later since the status quo doesn't feature an early intervention principle. 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 (see section 3.1).

This experimental project was performed in two 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 (the two assignment principles are: by occupation; by village/region). Caseworker and PES fixed effects will be taken into account in the estimations.

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 potential duration of unemployment benefits in the Swiss UI system is 2 years 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

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. 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. This exogenous timing is important for identification of the treatment effects, see section 4.

'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 age of 56 years on, job seekers profit of a benefit duration which is prolonged by about half a year (120 working 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. 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, see non-compliance analysis in section 3.2.2.

[Figure 1 about here]

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 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 1 and this project) it typically rises sharply to levels comparable to the first peak.

[Figure 2 about here]

Long-term unemployment (LU) incidence is highly age-dependent. For the region under consideration, Figure 2 shows this strong pattern in terms of proportion of LU in the unemployed population of a certain age category. Figure 2 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 56+ receive a benefit duration extension. The precentage numbers to the right of Figure 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 clearly shows that individuals of age 45+ face a markedly increased risk of long-term unemployment.

2.3 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 treatment plan section 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 two 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 randomly assigned to the treatment group (60%) and the control group (40%), by use of a randomised list. Like that, the final sample amounts to 327 individuals with 186/141 in the treatment/control group.

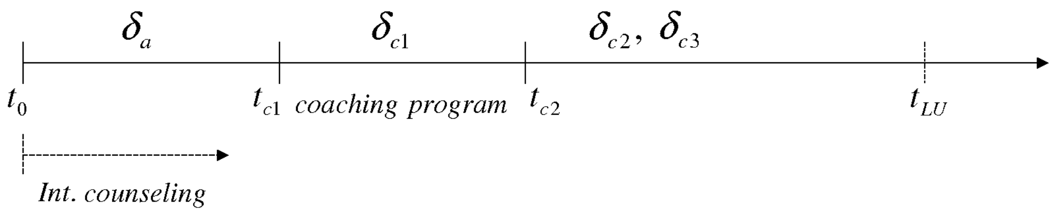
It is important to know which *information* was available for the treatment and control group at time t_0. In their first interview with the caseworker, the job seekers of both groups were informed in written form that they participate in a project for "quality control". This was necessary since both groups had to fill out repeated surveys over the duration of their unemployment spell (see section 3.1). 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 that the caseworker-job seeker assignment mechanism is exogenous to the treatment (the two assignment principles are: by occupation; by village/region). Note as well 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 3.2.2 shows, intentional non-compliance could only be observed in a negligibly small number of cases.

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 (unemployment exit 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 2.1, 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 46 days until (potential) coaching entry, see section 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.

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 or entry into long-term unemployment, captures the *post-coaching* effects. These are the cumulative outcome of coaching and the parallely ongoing high-frequency counseling. I split this effect up into a short-run effect δ_{c2}, which operates in the first 80 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. In an extension of this treatment effects setup I will δ_{c2} and δ_{c3} further distinguish with respect to what was the conclusion of the coaching. If it ends up in a job search strategy change, 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 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 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 the exit rate. 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 negative effect on the unemployment exit rate. 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. The survey data generated within this experimental project (see section 3.1) can shed light on these behavioral hypotheses. This is up for future research.

To complete the potential effects discussion, one has to note that in addition to treatment-related effects also a *project effect* could arise. This means that the mere existence of the project may result in a positive or negative effect on the unemployment exit rate for *both*, treatment and control group. Two possible sources of such an effect are: that (i) the caseworkers who participate in the project work in a more committed and motivated (and maybe more self-reflected) way; and that (ii) the job seekers may feel more monitored by the fact of being repeatedly surveyed[3]. Note that such a project effect may be detected when comparing the treatment and control group exit rates to those of a validly comparable external control group which was not affected by the project. This will be done in the gross effects analysis in section 5.1.

3 Data and Descriptive Analysis

3.1 Combined Register and Survey Data

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.

[3] Potentially, one could as well imagine the existence of interaction effects between control and treatment group. But since the individuals of the two groups have no (institutionalised) opportunity to systematically meet (different labor market programs, counseling is individual etc.), such interaction effects seem rather improbable.

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 they exit or latest until the end of the first year of unemployment. This is the threshold for long-term unemployment – at this point the experimental project and the special treatment plan stop. The register data include the common observable characteristics (see table in section 3.2.1). They track as well past unemployment histories up to three years before entry in the spell under consideration. The broad availability of UI register data allows to construct further external control groups: for other public employment service (PES) regions in the same canton, and for the younger job seekers (aged 30-44). These external control groups will be used to evaluate the project effect, see section 5.1.

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 strategy & intensity (applications and their chances, job search channels and the change of their use), reservation wage and health state. All the three perspectives of the project parties are represented: Caseworkers, job seekers and the coach are surveyed. This allows to contrast the job seekers' and the caseworkers' perception of the same above-mentioned behavioral measures. 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 at 1st interview, then subsequent surveys after 1/2/3/4/9/12 months of unemployment and at exit. So, the sample of the last surveys is smaller since a considerable amount of job seekers left unemployment already. 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/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).

3.2 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. Moreover, I report a series of analyses to assess whether intentional non-compliance in terms of not participating in the coaching program is an issue.

3.2.1 Observables: Did the Randomisation Work?

Tab. 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	

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 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 2.3) shapes the absolute values of the figures in Table 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 cancelation period[4]; this restricts their availability to interviews. A second reason is that some job seekers may be engaged in a shorter temporary 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

[4] 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.

on, and with a slightly higher percentage of random assignments to the treatment groups. Since this was all fixed ex-ante, it doesn't affect randomisation.

[Figure 3 about here]

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 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[5]. 85% of the unemployment exits were explicitly indicated as exits to jobs. Among the rest ('exit to unknown destination') there is a considerable part of individuals who either did not indicate to the caseworker that they found a job (this is not compulsory) or found a job just one or two months after exit, as the exit survey shows. The 'exit to unknown destination' ratio for the individuals aged 60+ is only slightly higher (23%)[6]. Therefore, there is no early retirement issue that biases the results, it is thus sufficient to use controls for age groups.

3.2.2 Understanding (non-)compliance behavior

A final question that is covered by descriptive analysis is the issue of potential non-compliance. Even though participation in the coaching program is compulsory and pre-assigned for the treatment group, non-compliance is still – under the risk of being sanctioned – potentially possible. Therefore, I analyse in the following a subgroup of potential non-compliers as well as the determinants of coaching entry.

In order to identify the group of potential non-compliers I use a filtering algorithm that features several steps. 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[7]: 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. These 'early exits' are visualised in the survivor curve with the same name in Figure 4. After this filter step, 40 individuals remained with 'unexplained non-participation' (survivor curve 'non-participants'

[5] The oldest participant is of age 62.5; even the full extended benefits duration (520 days) is not enough to cover the time until official retirement at 65. See also institutional background section 2.2.

[6] In the current version of the paper, the exit indicator does include the cases of 'unknown destination'. As a sensitivity analysis, I will perform the estimations with an indicator which considers 'unknown destination' exits as censored.

[7] 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.

in Figure 4). Finally, 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%*.

Moreover, in order to get to know more about which characterstics may possibly codetermine coaching entry, I performed a respective probit regression. The probit analysis on coaching entry propensity, see Table 3 in the Appendix, does not reveal any concern about intentional non-compliance. The first regression includes all members of the treatment group, the second one excludes the explained cases of early exit. When moving from the first to the second results, systematic patterns of explanation of coaching non-participation vanish. I.e., these determinants – which are age- and skill-dependence (measured by foreign language skills) – become insignificant as soon as the early exit cases are removed. Thus, early exits to jobs before coaching start are more often observed for younger individuals (mainly below age 50) and people who speak two or more foreign languages. The remaining significant variables in the second regression (civil status, non-German-speaking individuals, rest category of occupation, caseworker 4) do not provide indication for intentional non-compliance. In addition, the inflow dummies for March/April 2008 and Nov/Dec 2008 get significant. This points to a small overbooking of the coaching programs starting after these periods. Since the booking was made in order of inflow, potential non-compliance behavior cannot influence the booking process.

Thus, it can be clearly concluded from these non-compliance analyses that intentional non-compliance is a negligible issue in this experiment.

4 Econometric Modeling

In the following I develop the econometric strategy in analysing the social experiment: It ranges from a non-parametric approach based on Kaplan-Meier survivors which offers, due to randomisation, a clean picture on the gross effects of the treatment. Then, I go on developing a more parametrised strategy based on duration models with dynamic treatment effects, in order to distinguish the specific effects of different stages and parts (programs) of the treatment plan.

4.1 The advantages of randomisation

The design of this program evaluation as a randomised experiment brings a series of advantages in terms of cleanness of the design and in terms of clarity of the interpretation of the effects.

First, randomisation at t_0 allows for a "clean" identification of treatment effect that start 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* of a later treatment that starts at a $t > t_0$.

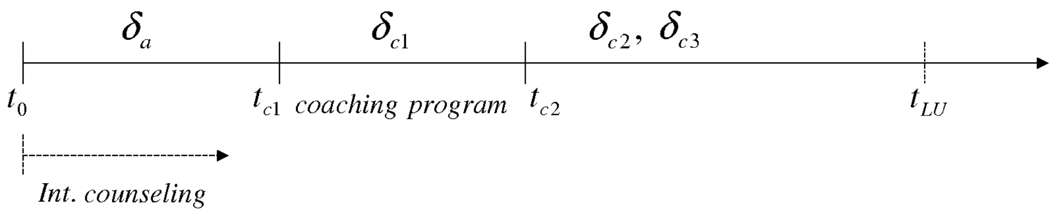
Second, all the data generated at t_0 profit from randomisation as well: they provide a well-balanced, representative picture since the relative distribution of these characteristics is not distorted by endogenous selection. This is specially relevant in the case of this study since a broad range of survey data generated at t_0 are at disposal. Thus, measures like motivation and job chances are exogenous with respect to upcoming outcomes and treatments.

Third, 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 econometric design of later treatment effects. After the start of treatment at t_0 the relative proportions of unobserved characteristics begin to change endogenously, i.e. in a potentially different way in treatment and control group (dynamic selection). Inflow in later treatment stages are not necessarily random any more. But due to randomisation and exogenous, "mechanistic" timing, the ongoing selection is uncorrelated to the propensity to enter later treatments. Therefore, it is not necessary to model inflow processes as additional equations as proposed in Abbring et al. (2003). A control of unobserved heterogeneity is enough to cope with the ongoing dynamic selection.

Fourth, due to the same argument issues of potential non-compliance can be handeled in a simplified way. Thanks to randomisation and information about later treatments at time t_0, potential endogenous non-compliances will be captured by the estimated anticipation effect(s) – when parallely controlling for unobserved heterogeneity.

4.2 Gross Effects and Intention-to-treat (ITT)

In the following, I model the basic setup of treatments of our experimental project using a duration model framework. As described earlier, two crucial treatments were implemented: the *intensified counseling* (interviews with caseworker every second week) and the *targeted coaching program* which starts approximatively 60 days after unemployment entry and lasts approximatively 60 days. Thus, this may be represented in the following way:



The *Model I* that I first estimate, as a basic benchmark, features just one *baseline treatment effect* (δ_b, not shown in the figure above) which allows a shift of the hazard rate from t_0 until unemployment exit for all treated individuals. This model is comparable in design to the non-parametric gross effects analysis using comparison of survivors – now just in a context of a (more parametric) duration model. Note that due to randomisation no issue of endogenous selection is involved here.

Model II distinguishes *different stages of the treatment plan*, following the figure above. In the early stage of unemployment, from t_0 on, the *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. δ_{c1} measures the effect of being (potentially) 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 80 days after end of coaching ($t_{c2} + 66$) and ends at unemployment exit (or censoring).

It is important to point out that in Model II, too, that by construction *no* endogenous selection processes bias the (causal) estimation of effects of later-stage treatments. This is the case since through all the described stages I use *all* the remaining observations in the treatment-control comparison. Due to the exogenous timing frame (known to the individuals at t_0) the randomisation carries over to later-stage potential treatments. The crucial point is that by using all individuals *assigned* to the treatment – independently if they really were in the later treatment (coaching)[8] – identification of later treatment effects does not rely on potentially endogenous selection into later treatment. Thus, I perform a what is called in the literature *intention-to-treat (ITT) analysis*. This implies that later stage treatment effects (from δ_{c1} on) measure in fact a combination of the effect of being coached and of being assigned to coaching but not having participated (for mostly exogenous reasons as the non-compliance analysis in section 3.2.2 shows). Thus, this design allows to answer policy questions about the *gross* program effect in different stages. These gross effects results per treatment stage are highly relevant for policy makers – since they reflect the *total impact of the policy* assigned at t_0, taking into account that

[8] 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.

in practice a mixture of treatment participation and a certain non-participation in later stages is realised.

Following the timing-of-events model 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:

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

where θ_u is the exit rate from unemployment and t_u is the unemployment duration. x is a vector of individual characteristics, 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 dummies and v_u represents the unobserved heterogeneity component which will only be of use for treatment specific analysis from Model III on. δ_i with $i \in \{a; c_1; c_2; c_3\}$ are the mentioned treatment effects. Specifically, 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.

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

$$\lambda_u(t_u) \quad = \quad exp(\sum_k(\lambda_{u,k} \cdot I_k(t_u)) \qquad (2)$$

where $k = 0, \ldots, 5$ 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 1) I define the six time intervals as follows: 0-50/51-100/101-150/151-250/251-350/351+ days.

4.3 Treatment-specific Effects

Model III aims at identifying treatment effects which are caused by participating in specific parts of the treatment plan. Therefore, the econometric design of Model II described in the last subsection has to be adapted in two ways. First, now the *effective participation* in later stage treatment (coaching) determines the described treatment dummies. This means that, except from the anticipation effect which is completely identical to Model II, the treatment effects δ_{c1}, δ_{c2} and δ_{c3} are identified by comparing the subgroup of individuals of the treatment group who really participated in the coaching with the control group. This implies that endogenous selection (non-participation) may potentially bias the effect estimation. Therefore, unobserved heterogeneity (but not a second equation that designs treatment entry, see section 4.1) has to be introduced.

I follow the standard non-parametric way of introducing unobserved heterogeneity which consists in modeling a discrete mixture distribution for v_u. I choose the simplest possible design in that I allow v_u to have two points of support. This implies the estimation of following probabilities of mass point combinations:

$$p_j = P(v_u = v_u^n) \qquad \text{with} \qquad n = 1, 2 \tag{3}$$

The above probabilities are designed in a logistic form, i.e. $p_n = \frac{exp(a_n)}{1+exp(a_1)}$. Thus, this implies the additional estimation of the two probability parameters a_n and of two baseline hazard intercepts λ_0^n instead of one.

Controlling for unobserved heterogeneity which may potentially be generated at coaching in- and outflow handles the endogenous selection issue for later stage treatments, such that causal statements about during- and post-effects of participation in the coaching program can be made.

4.4 Extensions: Age 55+ & Search Strategy Change

It is of major interest to further disentangle the treatment effects which arise from the treatment plan with respect to two dimensions: First, considering the age-relatedness of the policy issue analysed in this paper, it is of high interest to assess to which extent the treatment effects are *age-dependent*. I therefore introduce distinct treatment effects for the younger and older individuals in the 45+ sample. Second, it is of major interest to know more about *why* the observed effects materialised. In order to gain *behavioral explanations*, I introduce *intermediary oucomes* using the survey data. In particular, I estimate a model that allows to distinguish the treatment effect of the coaching on post-coaching outcome dependent on what the conclusion of the coaching was: a recommendation/joint decision to change the job search strategy or not.

The first extension, *Model IV*, constitutes in introducing a dummy variable which is one for individuals of age 55+ and zero otherwise. This dummy is interacted with the treatment effects of all stages as in Model III. Thus, adding these interactions results in estimating additional incremental treatment effects for individuals aged 55+. The cumulation of the respective baseline treatment effect and its increment (which is reported in the column 'transformations' of the respective estimation tables, see also results section) yields the treatment effects specific for the older participants. This model does not imply any further issues of endogenous selection since age is exogenous.

The second extension leads to *Model V*, which distinguishes – also based on Model III – separate post-coaching treatment effects dependent on whether a search strategy change was decided to be implemented or not. I want to generalise this approach to discuss the idea of introducing *intermediary outcomes* into the duration model framework as described so far. The repeated survey data that were collected with this project – they cover behavioral measures about job search behavior, reservation wages and motivation etc. (see data section 3.1) – allow for the analysis of behavioral explanations why certain outcomes materialised. The core idea is to construct *causal chains*: treatment generates a behavioral reaction like search strategy or motivation change and this results in an effect on the final outcome (unemployment exit). From a causal point of view, it is sensible to relate the behavioral change during a certain part of the treatment to the effect of that part of the treatment on the final outcome that materialises later.

So, define a dummy variable Δr_{s-1} which is one if a behavioral reaction, like search strategy change in our case, is found in the period before a certain treatment part ends (at time s, e.g. at coaching end). One may now interact Δr_{s-1} with the upcoming treatment effect(s) $\delta(s)$, in our case with the post-coaching effects. These interactions may be added to the hazard equation (1) that belongs to Model III. The interactions of treatment effects with intermediary outcomes are useful to gain answers on a broad range of behavioral questions which can be analysed by using the available repeated survey information. One further application, besides the described search strategy change explanation, is the behavioral analysis of the anticipation effect: The survey data may reveal if the anticipation effect is crucially driven by the motivation of job seekers. Using interactions with low and high motivation levels at t_0, one may distinguish subgroups for which the prospect of coaching rather acted as a "threat" or, on the contrary, was considered as being attractive since the concerned individuals expected the coaching to be helpful.

Note that distinguishing later treatment effects (beyond the anticipation effect) by the result of an intermediary outcome introduces an additional potential source of endogenous selection. Accordingly, the control for unobserved heterogeneity is necessary.

5 Estimation Results

In the following, I discuss the estimation results on the effects of the treatment plan, starting with the evaluation of the gross effects. The total effect of the treatment plan can be assessed non-parametrically by the use of survival analysis. Then, I report the results of the gross effects of specific treatment stages based on an ITT analysis using the duration model described in the last section. Next, the results of the treatment-specific analysis, based as well on the same kind of MPH model, are discussed. Then, a detailed look at the anticipation effect, using non-parametric hazard rates analysis, is provided. Finally, I report the MPH results on age- and behavior-dependent treatment effects.

5.1 Survival Analysis

Due to initial randomisation, a simple non-parametric analysis of the *total effect* of the treatment plan is feasible. It consists in comparing the Kaplan-Meier survivor curves for the treatment and for the control group. This analysis of the total effect is clean in the sense that no endogenous selection processes bias the initially randomised treatment-control comparison. Thus, it causally answers the policy question about a potential total effect of the treatment plan. Note that the comparison at later stages of the survivor curves gives a measure of a gross effect that entails a combination of specific treatment effects and dynamic selection. These two are not distinguishable. But this is not necessary to assess the total effect.

[Figure 5 about here]

Figure 5 reveals that the total effect of the treatment plan is zero. The two survivor curves for treatment and control group do not differ significantly. Also, the median durations of unemployment are very similar: 139.5 days in the treatment group, 138 days in the control group[9]. The dotted lines do approximatively indicate the starting and ending of the coaching program. (In the later analysis, of course, the exact timing is used.) About 20% of the unemployed leave unemployment before the potential coaching entry. Up to about 130 days, the survivor of the treated is slightly higher, but clearly not in a significant amount. The negative anticipation effect is slightly visible.

It is well conceivable that the zero gross effect is the result of a series of overlapping effects (see also potential effects section 2.4). For example, a negative lock-in effect during the coaching program may be overlapped by a higher exit rate of individuals in the treatment group who do not participate in the coaching. This is indeed the case, as the further results will show.

[Figure 6 about here]

Availability of data about neighboring PES allow the construction of external control groups, in order to analyse *project effects* that affect treatment *and* control group. Specifically, I choose one further PES which is highly comparable in terms of rate of urbanisation, belonging to the same labor market, culture and population structure. In addition, I consider as well the individuals of age 30-44 in the two PES of the project as well as in the mentioned further PES as additional control groups. All external control groups are drawn on the base of the same inflow criteria as for the project (see section 2.3). The first graph of Figure 6 shows that the chosen external-control-group-PES is indeed highly comparable to the PES of the project. For the group of individuals aged 30-44 who are completely unaffected by the project the two project PES and the external PES perform equally well in terms of unemployment survival/exit rates.

But when contrasting the 45+ individuals of the external control group with those of the two groups in the project, one detects a deviation of the survivor curves after about 100 days. *Both, treatment and control group, do better in terms of unemployment exit* than the individuals of the comparable external PES. Obviously, the mere fact that a pilot project featuring a new treatment plan was introduced led to a positive movement of treatment and control group. A combination of two explanations is highly probable: First, the existence of this new project

[9] Note that due to a delay in the availability of the most recent register data some longer durations are still censored. This is not problematic for the analysis in this paper since the evaluated interventions happen very early in the spell and the duration models properly take into account censoring. The first 250 days are almost completely observed (10% of censoring). The total censoring proportion amounts to 29.7%.

may have increased the commitment of the participating caseworkers. And second, the job seekers may be affected in a general way by the project through the fact of being constantly surveyed (comparable to the Hawthorne effect which was found in medical and industrial field experiments). This repeated surveying may have an additional monitoring effect.

5.2 Intention-to-treat (ITT) Analysis

Tab. 2: The gross effect the complete treatment plan. (MPH duration model)

	Model I: Gross TE		
	Coeff.	z-value	Transf.
Total effect (δ_b/in %)	-0.070	-0.451	-0.067
Control Variables		Yes	
Unobserved heterogeneity		No	
-Log-Likelihood		1467.65	
BIC		1568.97	
N		327	

Notes: Asymptotic z-values. Transformation: effect of treatment as change of hazard rate in %.
Source: Own estimations, merged UIR-LZAR database.

The *ITT* analysis allows, as described in the respective econometric section (4.2), for a clean comparison of gross treatment effects in different stages of the treatment plan. The direct comparison of the individuals *assigned* to the treatment group vs. those assigned to the control group in different (exogenously timed) stages of the treatment yields an analysis of the gross effect of treatment in the respective stages: This means that in later stages of the treatment plan (after the anticipation period where no treatment-related selection happens) the ITT analysis captures a combined effect of treatment – for the coaching participants – and treatment assignment – for the coaching non-participants of the treatment group. Thus, it answers the relevant policy question about the in total observed *gross* effect of certain stages of treatment (independent of real participation or not).

When only allowing for one constant, permanent treatment effect (δ_b) for the control group, I find an insignificant, slightly negative effect of the treatment plan, as Table 2 shows. Not surprisingly, this result of *Model I* exactly reflects the non-parametric result on the total effect found in the survival analysis. The slight negative treatment effect amounts to a reduction of the unemployment exit rate of 6.7% ($= exp(\delta_b) - 1$) on average over the whole unemployment duration when comparing treatment and control group.

Model II, based on the describe ITT analysis separated by exogenous treatment stage, provides basically two insights, as the first columns of Table 4 in the Appendix reveal. First, I find a clear and highly significant negative anticipation effect. Treated individuals have an on average 37.7% lower unemployment exit rate in the period between unemployment inflow and (potential) coaching entry. Thus, obviously results the prospect of being coached in a 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. If this waiting behavior is rather driven by a smaller job search effort or by being more picky in accepting jobs, will be an issue for future research using the survey data. In section 5.3.1 of the treatment-related analysis, I will further analyse the dynamic nature of the anticipation effect.

The second insight from Model II is that the later stage treatment effects – during and after coaching – are in gross zero. As we will see in the next section, the combination of coaching participants and non-participants in the treatment group "masks" here specific treatment effects like lock-in and positive and negative post-coaching effects.

Table 4 in the Appendix reports as well all the results for the estimation of the duration dependence (piecewise-constant hazard) and of the coefficients of the control variables. Calculating the piecewise-constant exit hazard levels for an "average" individual (see Notes of the respective table), one may conclude that the estimation very appropriately fits the shape of the empirical hazard (see Figure 1). Over the different duration pieces, the monthly unemployment exit rate goes from 11.5% to above 18% and then down to 10% from 151 days on. Considering the observables, I find a significant reduction of the exit rate for individuals of age 55+. Women show a slightly significantly higher exit rate. The other individual characteristics, including variables of (lower) qualification and past unemployment history, reveal to be insignificant. The time-of-inflow controls do not get significant either (except a slight negative significance of the inflow in March/April 2008). Contrarily, several of the caseworker fixed effects get clearly significant: which caseworker is in charge (whereby the job seeker doesn't have an influence on the caseworker assignment) is significant.

5.3 Treatment-specific Analysis

In a next step, I turn to the analysis of treatment-specific effects. *Model III*, reported in Table 4 in the Appendix, features an estimation of treatment effects following the same timing structure as Model II, but now their identification relies on the subgroups who really participated in the later stage treatment (coaching).

The result for the anticipation effect is the same as in Model II since it is designed in the same way: In this first period, everybody of the treatment group is by default in the treatment plan. The dynamic nature of the anticipation effect will be further discussed in the next section.

The during-coaching effect now becomes negative, but only barely significant (20% level). Individuals participating in the coaching program show a lock-in behavior: presumably due to the high work load of the program, they do not exert the same job search effort than without such a program. The post-coaching effect until 80 days after the end of the coaching program reveals to be slightly positive but insignificant. The later post-coaching effect isn't significant as well but negative. Obviously, coaching shows a better effect in the short run as in the middle run. This could be due to an additional boost of motivation and support coming from the coaching; after some time, these short-run effects seem to run out.

Note that the identification of the later treatment effects (during and after coaching) relies on the appropriate control of unobserved heterogeneity. Estimation with unobserved heterogeneity components resulted in the conclusion that there is no unobserved heterogeneity with respect to the outcome process. Even after a comprehensive grid search for possible mass points, not more than one mass point could be identified. The reason for the nonexistence of unobserved heterogeneity presumably lies in the tight sampling criteria which were applied in the pre-selection into the sample: Individuals are in the same age group, in the same labor market, comparable in terms of employability and in terms of skills. It is not completely excludable that the non-identification of further mass points may be due to the small sample size. But 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. Further investigations (simulations) of this issue are planned for future research.

5.3.1 The anticipation effect

The interesting result that a threat effect does not exist for this coaching program but instead an attraction effect materialises shall be further explored here. How does the dynamics of the negative anticipation look like? Such an analysis, based on (non-parametric) comparisons of hazards, may give further insights into the behavioral patterns of job seekers and caseworkers in the anticipation period. Note that, due to initial randomisation, hazard comparison results in unbiased results.

[Figure 7 about here]

First, looking at panel a of Figure 7, direct treatment-control comparison of the empirical hazards leads to the same result of a negative anticipation effect as found in the duration models. In a second step, hazards are plotted with respect to the remaining duration until potential coaching entry, see panel b. This reveals that the negative hazard difference is especially high in the period early before coaching entry (about 30 days before) and in another period rather far away of coaching entry (more than 90 days). These two peaks could be produced by the fact that there is remarkable variation in the (exogenously given) duration until coaching entry. It is conceivable that individuals with a long duration until potential coaching start behave differently than those who are shortly before coaching start. As a consequence, I split the treatment group hazard in two subgroups, those with explicitly short durations until coaching (less than 50 days) and those with long ones (more than 90 days, ignoring those in between). This leads to panel c in Figure 7.

This panel reveals a distinct pattern. Obviously, the negative anticipation effect is of a dynamic nature: It is clearly lowest in the first weeks and becomes then steadily less negative.

This pattern applies independently of the length of the duration until coaching entry. A sensible interpretation of such a dynamic of the coaching effect is that in an early stage of the anticipation effect the pure "attraction effect" dominates: Individuals expect positive results and support from the coaching and therefore diminish their efforts and/or willingness to take up immediately a job. After some time in the anticipation period, the caseworker realises that the treated individual exerts a lower effort. Therefore, the caseworker increases the (monitoring) pressure on the job seeker, which results in a reduction in the negative difference of the unemployment exit rate compared to the control group. By the end of the anticipation period, the difference has practically disappeared.

5.3.2 Are older job seekers (55+) differently affected by the treatment?

In this section, I tackle the question whether the effects of specific elements of the treatment plan are age-dependent. For that, I interacted all the modeled treatment effects with a dummy for older job seekers of age 55+. This results in *Model IV*.

The first columns of Table 6 in the Appendix show that the treatment effects in all four stages are clearly age-dependent. They reveal a consistent pattern that can be summarised by the statement that the treatment plan is less attractive and successful for individuals of age 55+. This seems already to be anticipated: correspondingly, the anticipation effect is less negative. Since individuals of age 55+ seem to believe less in the success of coaching, they seemingly invest less in the coaching. As a consequence, the lock-in effect of the program vanishes. Finally, the post-coaching effect for older aged individuals is significantly more negative. In fact, those individuals face a reduction of the unemployment exit rate of about 65% in the whole post-coaching period. On the contrary, individuals below age 55 show a clearly positive post-coaching effect in the first 80 days, then it starts getting slightly negative. Thus, the coaching program seems to work best, relatively, for individuals in the age category of 45 to 55 years.

5.3.3 The effect of a search strategy change

As a final step of extension of the duration model, I introduce search strategy change as an intermediate outcome into the analysis of post-coaching treatment effects. This results in *Model V*, see Table 6.

In which way may a search strategy change, recommended by the coach (whereby this recommendation is the result of the self-assessment and the coaching process during that part of the treatment), affect the later unemployment exit outcome? The Figure 8, where the survivor for the coached individuals of the treatment group is subdivided by the fact whether a search strategy change was recommended or not, shows a distinct picture: Individuals with search strategy change recommendation seem to have implemented that recommendation; reorientation of their search strategy obviously results in a clear reduction of the unemployment exit propensity – these individuals seem to be in an additional "reorientation lock-in" period. The reorientation of the search strategy consumes time that otherwise would go into already well-known search procedures. After some time of reorientation, the survivor starts falling again. The people seem to have adapted to the new search strategy. But there is so far no evidence that they would overtake the individuals without search strategy change in terms of higher exit rates in later stages[10].

[Figure 8 about here]

In a next step, I perform a probit regression analysis to explain the search strategy change behavior, see Table 5. It reveals the interesting result that it is not predominantly the job seekers with lower labor market chances who get a job search change recommendation. In fact, being of older age, less skilled and having insufficient key qualifications in terms of systematic-analytic thinking all reduces the propensity of reorientation. Women and divorced individuals are more probable to change search strategy. Thus, it can be concluded that search strategy change recommendations are – in terms of observables – not just an indicator of decreased labor market chances. This may be a further explanation why no unobserved heterogeneity, i.e. remarkable endogenous selection, was found. Arguably, these recommendation decisions rely on a more complex pattern of opportunities. I.e., they may be based on the assessment whether there are opportunities as well as willingness for reorientation. Such an assessment is obviously not significantly correlated to unobservable characteristics that drive as well the outcome.

The results of Model V, see Table 6 in the Appendix, show the above-described differentiated exit pattern visible in Figure 8. Individuals with search strategy change show a clearly negative post-coaching effect in short- and mid-run. The difference to individuals who do not change their search strategy is highly significant. In the first 80 days after coaching, the unemployment exit rate for job seekers with search strategy change is 27.6% lower than the one of the control group. Beyond 80 days, the corresponding exit rate even falls to a level which is 56.6% below the one of the control group. On the other hand, individuals who did not execute a search strategy change experience a clear and highly significant improve of the unemployment exit rate as a result of the coaching. The rate more than a doubles in the first 80 days after the coaching program. The positive post-coaching effect reduces in size over time.

Thus, it can be concluded of this analysis that a search strategy change due to coaching results in an additional lock-in period in the short- and mid run after coaching. This is presumably due to the fact that reorientation is time consuming and reduces search effectiveness in the first period after change. Statements about a possible positive effect of reorientation in the longer run can not yet be made based on the analyses performed so far. Individuals who do not engage in a search strategy change after coaching experience a considerably positive post-coaching effect on their unemployment exit rate. This positive effect clearly reduces in the mid run.

[10] Note that due to the censoring of some longer durations results beyond 300 days of unemployment are still imprecise so far. See footnote 9

5.4 Post-unemployment job stability

Of the full sample of 327 individuals, 273 job seekers exit unemployment until the end of March 2010 (exogenous censoring date). This yields the basic population for the post-unemployment analysis. The classical selection problem about non-random selection into the post-unemployment population is in the context of this study barely an issue. Several reasons speak for a very low selectivity: First, the fact of disposing of a randomised treatment-control sample at t_0 carries to a certain degree over the post-unemployment period. Second, having found a zero gross effect in terms of unemployment duration points as well to a situation where the dynamic selection processes in the two groups are very similar. Third, the estimations of the duration models in the last sections showed that no unobserved heterogeneity can be found in the population.

Figure 9 shows the effect of the new program on the stability of the first post-unemployment job. The propensity of the treatment group to reenter unemployment is clearly lower as the survivor analysis shows. This positive post-unemployment effect of the program remains over a period of about 600 days after unemployment exit.

6 Conclusion

This paper evaluates a new social experiment which implements a novel active labour market policy (ALMP) intervention in Switzerland that explicitly focusses 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 treatments that are individually adapted: high-frequency counseling (every second week, allowing for more time per individual job seeker) and a highly intense coaching program of 54 days in small groups that focusses on job search strategy and employability development as well as on self-marketing. A further focus lies on early intervention: High-frequency counseling starts right from the beginning on, coaching on average after about 50 days (the start date is exogenously given at entry into unemployment).

The evaluation of this treatment plan is enriched by the availability of a unique combination of register data and repeated surveys which allow to dynamically track behavioral changes of job seekers. Therefore, the analysis may further distinguish the treatment effects with respect to what were the behavioral reactions of job seekers on specific parts of the treatment plan. In particular, I evaluate the effect of search strategy changes (see below).

Initial randomisation of the assignment to the treatment plan allows for a simple evaluation of the total effect of the new policy, as well as for a clean ITT (intention-to-treat) analysis. Randomisation worked very well in terms of balancing all the observable characteristics; therefore, there is no reason to believe that the balancing did not work in terms of unobserved characteristics. Surveying of individuals and regression analysis shows that systematic non-compliance (through non-participation in the treatment) is extremely rare.

Survival analysis reveals that the total effect on the exit rate from unemployment is zero. Using a highly comparable external control group, I find that the fact of implementing this new policy, including related surveys, resulted in a slightly increased exit rate after 100 days for the treatment and the control group of the project. This may be driven by higher commitment of the participating caseworkers as well as by the potential monitoring effect that arises from being permanently surveyed.

The ITT analysis, based on a duration model with dynamic treatment effects, reveals that the prospect of being coached soon resulted in a substantial negative anticipation effect ("attraction effect"). The diminished propensity to exit early from unemployment may be driven by the job seeker's expectation of a positive effect of the upcoming coaching and by an attitude of passive waiting ("to be helped"). Detailed hazard analysis shows that negative anticipation effect decreases over anticipation duration. A possible explanation is that the attraction effect of the coaching resulting in lower search effort is counteracted by an increased pressure from the side of the caseworkers (once the low effort is detected). The pure comparison of treatment and control group in the later stages of the treatment plan (independently whether individuals indeed participated in treatment) reveals that neither a significant positive nor a negative (gross) effect materialises. The reason for this finding is that the *overlap* of heterogenous effects of the coaching and the fact that some individuals did not participate is responsible for the insignificant gross effect in later stages of the treatment.

The treatment-specific analysis shows a more clear-cut, but still not statistically significant, picture on the later treatment effects. During coaching a (almost significant) lock-in effect materialises. After the end of coaching, the exit rates of the participants are first slightly higher than in the control group, but already after 80 days the post-coaching treatment effect turns into being negative. Looking separately at the treatment effects of the oldest group of job seekers (55+), I find that the post-coaching effects are remarkably more negative and become significant. In turn, this group of individuals does not show to be locked in into the coaching program. On the other hand, results reveal that the subgroup aged 45-55 shows a (almost significant) positive post-coaching effect in the short run, but not in the long run. Moreover, it is clearly relevant for the post-coaching effect whether the coach recommends a job search strategy or not. Whereas in the latter case the post-coaching effect becomes clearly positive, the reorientation of search strategy in the first case results in a considerable reduction of the exit rate. This could be interpreted as a reorientation lock-in.

Two main insights can be gained from the experimental evaluation of this new active labor market policy: First, a human capital training program can be made less costly – in the sense that it doesn't prolong unemployment duration – by starting the intervention early, proceeding it in a highly intense manner and targeting it to a specific risk group. Second, such a kind of employability coaching doesn't threaten people from participating but, on the contrary, motivate them. This is a good base for the creation of a positive longterm human capital effect. This attraction effect provides a second reason to intervene as early as possible: the earlier, the shorter is the duration of the negative anticipation effect.

Going beyond these insights, it will be of high interest to conduct an evaluation of the post-unemployment effects of the treatment plan. Taking into account that the coaching program design included an important element of human capital development, the hypothesis of a positive effect on salaries in the first job after unemployment is tenable. A next step of the analysis of this social experiment, based on the available salary data, will tackle this question.

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Appendices

A. Figures

Fig. 1: Unemployment exit hazard

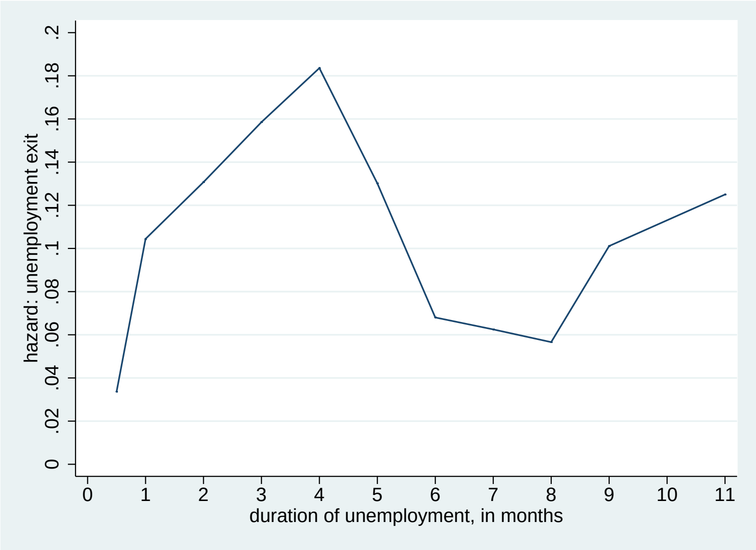
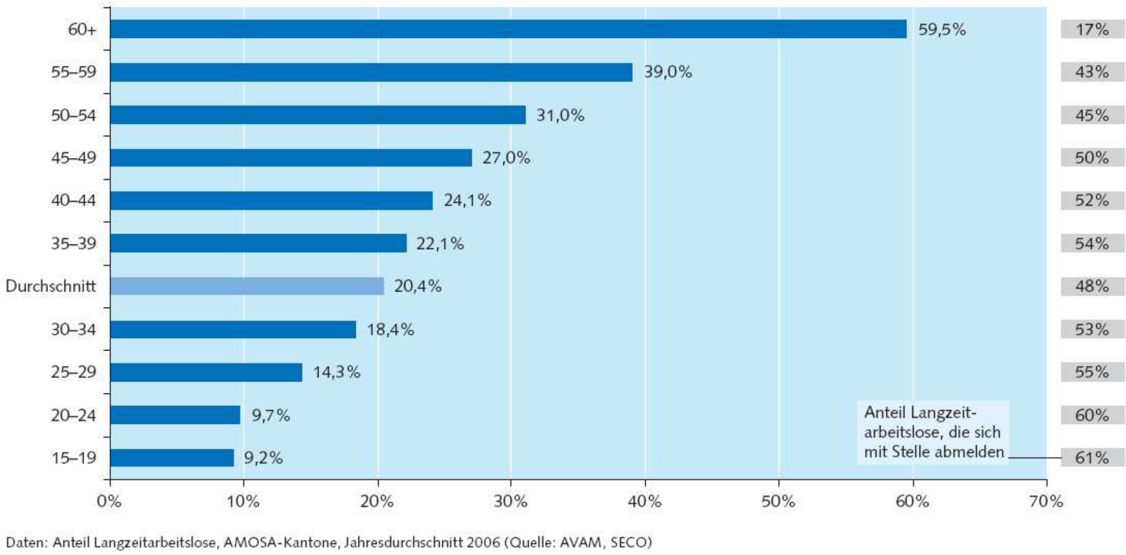


Fig. 2: Incidence of long-term unemployment by age groups



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 precentage figure to the right represents the age-related proportions of the long-term unemployed who deregister from unemployment insurance due to having found a job. *Source:* AMOSA 2007.

Fig. 3: The age structure of the sample

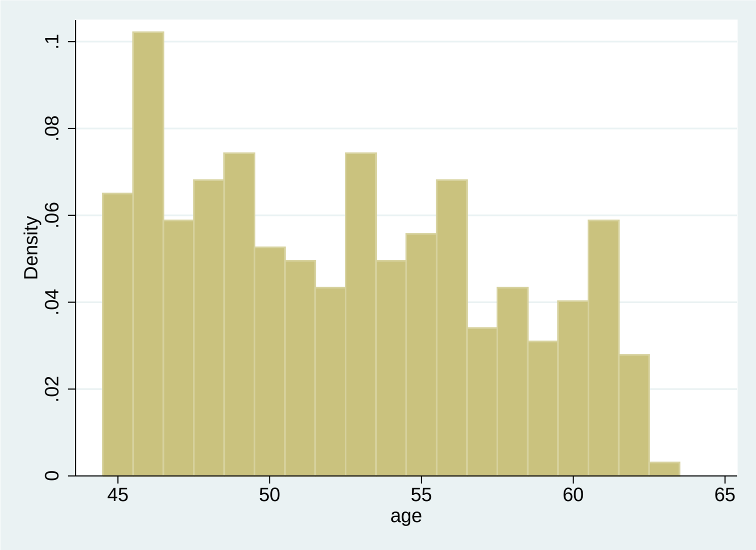


Fig. 4: Non-compliance analysis: remaining coaching non-participants

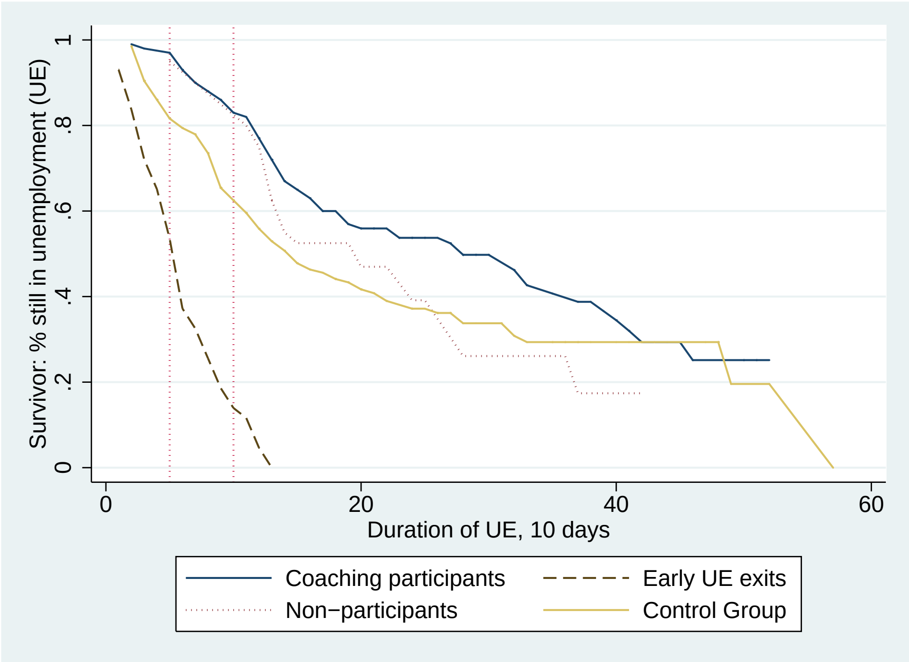


Fig. 5: Total treatment effect: survivor treatment vs control group

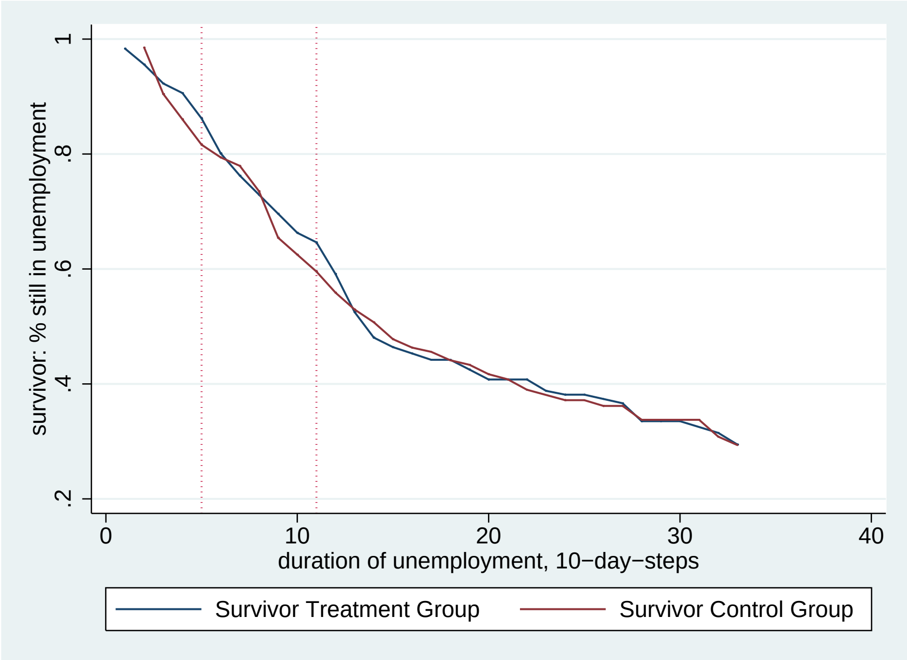


Fig. 6: The project effect: comparison to external control group

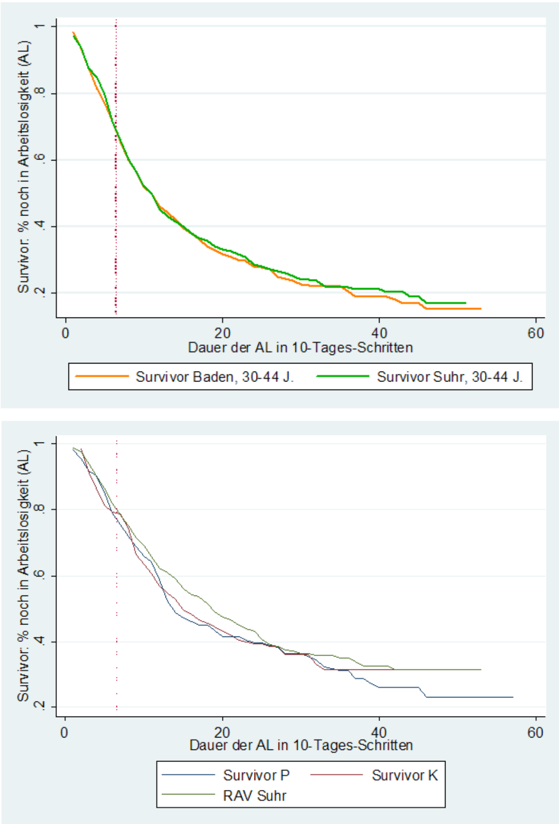
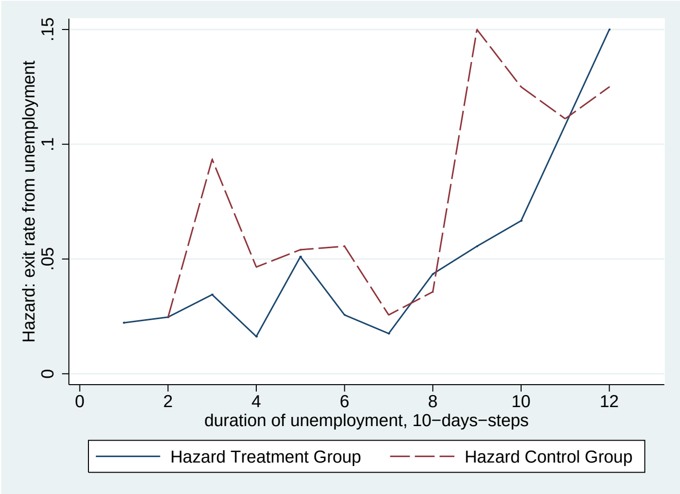
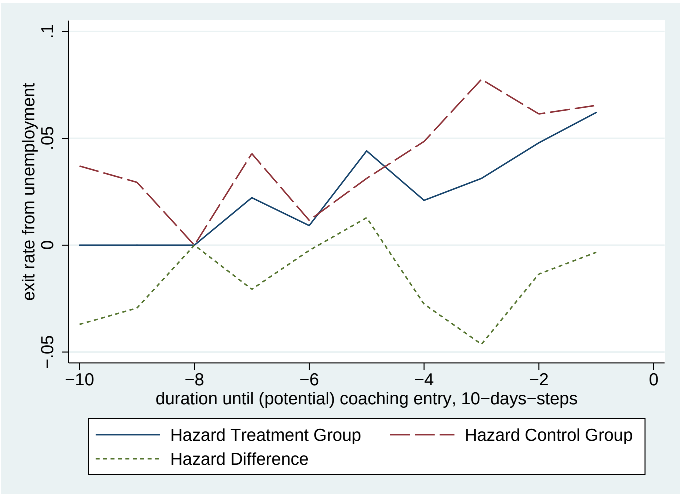


Fig. 7: The anticipation effect: treatment vs control group

a. Comparison of unemployment exit hazards



b. Exit rates by duration until (real or potential) coaching entry



c. Comparison of anticipation effect (hazard difference to control group) for individuals with long duration until (potential) coaching entry (more than 90 days) vs. those with short duration (less than 50 days)

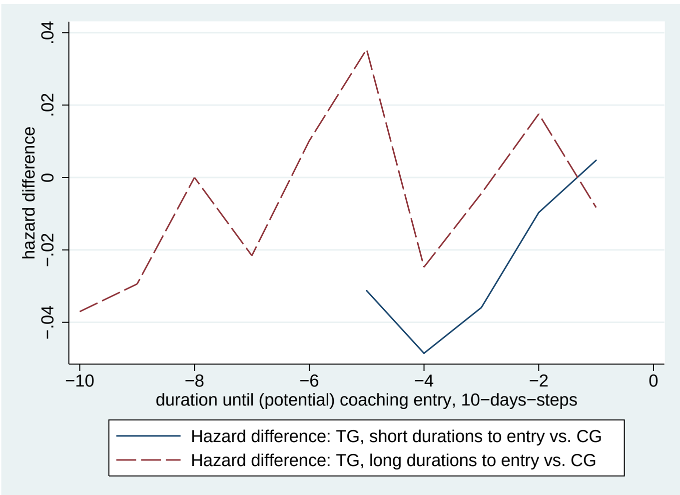


Fig. 8: The effect of reorientation: survivors

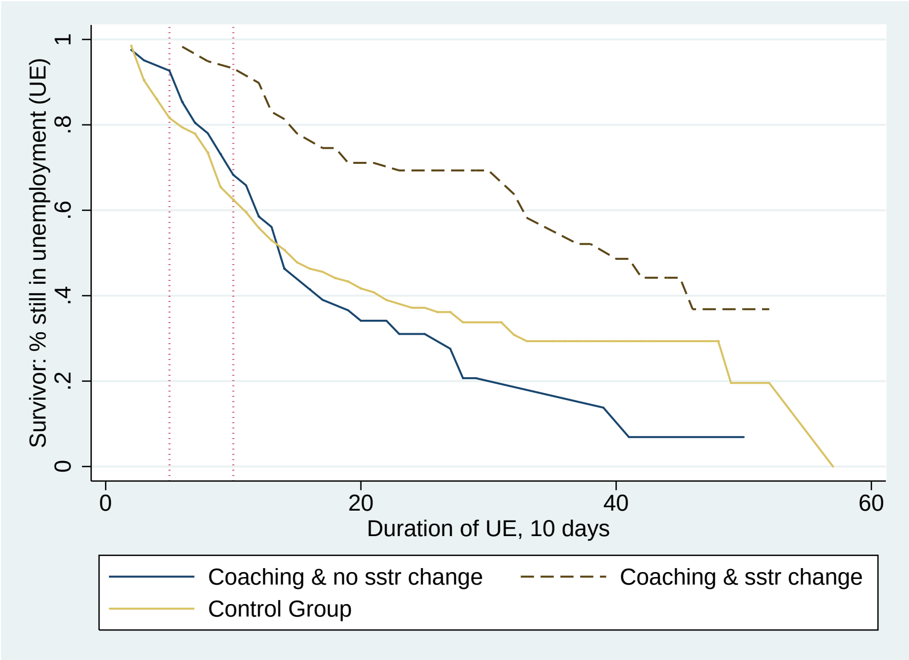
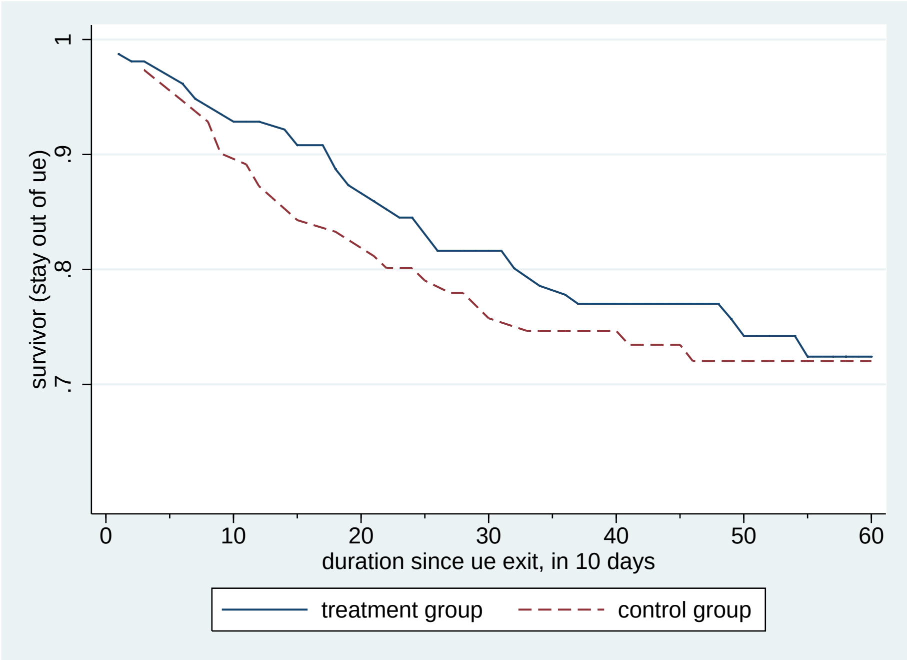


Fig. 9: Post-unemployment job stability: survivor of the reentry rate into unemployment



B. Tables

Tab. 3: Determinants of coaching entry. Probit regression

	Coaching entry (treatment group)		*Coaching entry (...without early exits)*	
	Coeff.	z-value	Coeff.	z-value
UE duration in past 3 years	-0.001	-1.04	-0.003	-1.50
duration until availability	-0.006	-1.73	-0.003	-0.52
age: 50-54 (base: 45-49)	0.580	2.10	0.373	0.85
age: 55-59	0.700	2.14	0.732	1.30
age: 60+	0.975	2.14	1.293	1.50
married (base: unmarried)	-0.478	-1.50	-2.091	-2.72
divorced	-0.237	-0.63	-1.462	-1.77
female	-0.495	-1.56	-0.276	-0.60
non-Swiss	-0.103	-0.26	0.650	0.90
low employability (base: medium)	0.224	0.47	1.471	1.67
semi-skilled (base: skilled)	-0.067	-0.15	-0.307	-0.43
unskilled	-0.115	-0.16	0.060	0.04
non-German-speaking	-0.895	-1.76	-2.341	-2.70
1 foreign language (base: 0)	1.096	2.33	1.247	1.52
2+ foreign languages	-0.830	-1.72	-1.000	-1.27
PES 2 (base: PES 1)	-0.352	-0.43	-1.180	-0.74
management (base: professionals)	0.005	0.01	-0.309	-0.51
support function	-0.613	-0.90	0.469	0.36
part-time (but above 50%)	0.174	0.48	1.120	1.60
Occupations (base: office, accounting):				
Blue-collar manufacturing, construction	0.249	0.57	0.049	0.07
Engineers, technicians, Informatics	-0.066	-0.15	0.818	0.93
Enterpreneurs, marketing, banking, insurance	0.454	1.05	0.514	0.80
Sales	-0.204	-0.48	-0.610	-0.97
Gastronomy, housekeeping, personal service	1.054	1.63	1.125	1.02
Science & arts, education, eealth occupations	-0.022	-0.05	0.231	0.26
Rest (mainly unskilled workers, helpers)	1.609	2.74	2.597	2.39
Month of entry in UE (base: Jan/Feb 2008):				
March/April 2008	-0.403	-0.98	-1.232	-1.80
May/June 2008	0.299	0.71	0.598	0.82
July/August 2008	-0.408	-1.04	-0.795	-1.21
Sept/Oct 2008	-0.173	-0.45	-0.740	-1.15
Nov/Dec 2008	-2.061	-3.50	-3.756	-3.65
Caseworker fixed effects (base: CW 1):				
CW 2	0.090	0.16	0.246	0.26
CW 3	0.512	0.93	0.603	0.75
CW 4	0.270	0.45	2.325	2.10
CW 5	-0.517	-0.81	-0.370	-0.39
CW 6	-0.996	-1.72	-1.121	-1.28
CW 7	0.471	0.84	0.564	0.73
CW 8	-1.179	-1.84	-1.305	-1.40
CW 9	0.430	0.46	1.578	0.89
CW 10	1.549	1.52	2.476	1.38
CW: rest (smaller charges)	0.315	0.49	1.638	1.30
Constant	0.558	0.91	2.380	2.14
N	186		140	
Pseudo R^2	23.85		42.53	

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

Tab. 4: The effect of the treatment plan: ITT and treatment-specific. (MPH duration models)

	Model II: ITT			*Model III: treatment-specific*		
	Coeff.	z-value	Transf.	Coeff.	z-value	Transf.
Treatment effects						
Anticipation effect (δ_a/in %)	-0.473	-2.12	-0.377	-0.503	-2.24	-0.395
During coaching (δ_{c1}/in %)	0.107	0.49	0.113	-0.355	-1.27	-0.299
Post-coaching, 14-80 days (δ_{c2}/in %)	-0.147	-0.50	-0.136	0.122	0.44	0.130
Post-Coaching, 80+ days (δ_{c3}/in %)	-0.008	-0.02	-0.008	-0.463	-1.17	-0.370
Exit rate from unemployment						
$\lambda_b/exp(u_b)$, 1-50 days	-5.800	-13.95	11.52	-5.765	-13.83	11.28
$\lambda_2/exp(u_2)$, 51-100 days $^{1)}$	0.446	1.98	17.98	0.457	2.05	17.81
$\lambda_3/exp(u_3)$, 101-150 days $^{1)}$	0.461	1.76	18.26	0.374	1.46	16.40
$\lambda_4/exp(u_4)$, 151-250 days $^{1)}$	-0.150	-0.52	9.92	-0.176	-0.63	9.46
$\lambda_5/exp(u_5)$, 251-350 days $^{1)}$	-0.153	-0.38	9.88	-0.038	-0.10	10.86
$\lambda_6/exp(u_6)$, 351+ days $^{1)}$	-0.095	-0.17	10.47	0.050	0.10	11.86
Control variables						
UE duration in past 3 years	0.000	-0.10	0.000	0.000	-0.28	0.000
duration until availability	-0.003	-1.09	-0.003	-0.003	-0.97	-0.003
age: 50-54 (base: 45-49)	-0.285	-1.42	-0.248	-0.243	-1.20	-0.216
age: 55-59	-0.507	-2.511	-0.40	-0.447	-2.15	-0.361
age: 60+	-1.169	-3.554	-0.69	-1.201	-3.61	-0.699
married (base: unmarried)	0.010	0.05	0.010	-0.078	-0.39	-0.075
divorced	0.055	0.23	0.056	0.022	0.09	0.022
female	0.345	1.53	0.412	0.340	1.51	0.405
non-Swiss	0.214	0.83	0.239	0.205	0.77	0.227
low employability (base: medium)	0.193	0.70	0.213	0.263	0.94	0.301
semi-skilled (base: skilled)	0.047	0.13	0.048	-0.055	-0.15	-0.053
unskilled	0.003	0.01	0.003	-0.014	-0.03	-0.014
non-German-speaking	0.100	0.30	0.105	0.109	0.32	0.115
1 foreign language (base: 0)	-0.252	-0.98	-0.222	-0.207	-0.77	-0.187
2+ foreign languages	0.264	0.94	0.302	0.182	0.62	0.199
PES 2 (base: PES 1)	0.279	0.53	0.322	0.239	0.45	0.270
management (base: professionals)	-0.311	-0.83	-0.268	-0.433	-1.18	-0.351
support function	0.266	0.47	0.304	0.151	0.26	0.163
part-time (but above 50%)	0.051	0.22	0.052	0.088	0.37	0.092
occupations (base: office, accounting):						
Blue-collar manufacturing, construction	-0.061	-0.23	-0.059	-0.123	-0.44	-0.116
Engineers, technicians, Informatics	-0.226	-0.72	-0.202	-0.188	-0.58	-0.172
Enterpreneurs, marketing, banking, insurance	-0.294	-0.91	-0.254	-0.282	-0.88	-0.245
Sales	0.180	0.58	0.197	0.119	0.39	0.127
Gastronomy, housekeeping, personal service	-0.118	-0.32	-0.111	-0.060	-0.16	-0.058
Science & arts, education, eealth occupations	0.048	0.16	0.049	0.072	0.24	0.075
Rest (mainly unskilled workers, helpers)	-0.111	-0.29	-0.105	0.017	0.04	0.017
Month of entry in UE (base: Jan/Feb 2008):						
March/April 2008	-0.406	-1.51	-0.334	-0.467	-1.66	-0.373
May/June 2008	0.076	0.30	0.079	0.072	0.28	0.074
July/August 2008	-0.115	-0.40	-0.109	-0.183	-0.65	-0.168
Sept/Oct 2008	-0.047	-0.18	-0.046	-0.082	-0.31	-0.079
Nov/Dec 2008	-0.174	-0.56	-0.159	-0.294	-0.92	-0.255
Caseworker fixed effects (base: CW 1):						
CW 2	0.811	1.86	1.251	0.888	2.07	1.430
CW 3	0.511	1.28	0.668	0.609	1.48	0.839
CW 4	0.489	1.37	0.631	0.646	1.76	0.908
CW 5	0.125	0.28	0.133	0.212	0.47	0.236
CW 6	0.670	1.82	0.953	0.648	1.74	0.912
CW 7	0.664	1.67	0.943	0.710	1.75	1.035
CW 8	0.693	1.85	1.000	0.651	1.71	0.918
CW 9	0.563	0.84	0.755	0.640	0.99	0.897
CW 10	0.402	0.56	0.496	0.548	0.76	0.729
CW: rest (smaller charges)	0.700	1.50	1.015	0.868	1.86	1.381
Unobserved heterogeneity		No			No	
-Log-Likelihood		1457.19			1449.84	
BIC		1604.83			1600.38	
N		327			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). 1) 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: $u_j = \lambda_b + \lambda_j + \bar{x}'\beta_j + \sum_i \tau_i \bar{M}_i + \sum_k \gamma_k C_k$ where $j = 2, \ldots, 6$ ($\lambda_j = 0$ for first segment) and the bars are means, except for the past unemployment and the duration until availability where medians are used. Asymptotic z-values. UE=unemployment.

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

Tab. 5: 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.
Source: Own estimations based on merged UIR-LZAR database.

Tab. 6: Treatment effects for higher ages (55+) & for search strategy change. (MPH models)

	Model IV: age 55+				*Model V: reorientation*		
	Coeff.	z-value	Transf.		Coeff.	z-value	Transf.
Treatment effects							
Anticipation effect (δ_a/in %)	-0.553	-2.01	-0.425		-0.496	-2.19	-0.391
... for age 55+ [1)]	0.249	0.55	-0.262				
During coaching (δ_{c1}/in %)	-0.469	-1.36	-0.374		-0.353	-1.26	-0.297
... for age 55+ [1)]	0.519	0.87	0.051				
Post-coaching, 14-80 days (δ_{c2}/in %)	0.387	1.27	0.472		0.786	2.14	1.195
... for age 55+ [1)]	-1.457	-1.67	-0.657	... reo.[1)]	-1.110	-2.42	-0.276
Post-Coaching, 80+ days (δ_{c3}/in %)	-0.231	-0.45	-0.206		0.414	0.73	0.512
... for age 55+ [1)]	-0.763	-1.03	-0.630	... reo.[1)]	-1.249	-1.84	-0.566
Exit rate from unemployment							
$\lambda_b/exp(u_b)$, 1-50 days	-5.757	-13.37	11.33		-5.865	-13.68	11.04
$\lambda_2/exp(u_2)$, 51-100 days [2)]	0.455	2.04	17.87		0.450	2.00	17.30
$\lambda_3/exp(u_3)$, 101-150 days [2)]	0.364	1.40	16.31		0.370	1.42	15.97
$\lambda_4/exp(u_4)$, 151-250 days [2)]	-0.183	-0.65	9.44		-0.149	-0.51	9.51
$\lambda_5/exp(u_5)$, 251-350 days [2)]	-0.055	-0.14	10.73		0.043	0.11	11.52
$\lambda_6/exp(u_6)$, 351+ days [2)]	0.075	0.14	12.22		0.159	0.31	12.93
Control variables							
UE duration in past 3 years	0.000	-0.30	0.000		0.000	-0.38	0.000
duration until availability	-0.003	-1.14	-0.003		-0.003	-1.09	-0.003
age: 50-54 (base: 45-49)	-0.265	-1.31	-0.233		-0.238	-1.17	-0.212
age: 55-59	-0.359	-1.32	-0.301		-0.438	-2.06	-0.355
age: 60+	-1.131	-2.98	-0.677		-1.362	-4.01	-0.744
married (base: unmarried)	-0.064	-0.32	-0.062		-0.015	-0.07	-0.015
divorced	0.028	0.12	0.028		0.074	0.31	0.077
female	0.360	1.57	0.433		0.407	1.83	0.503
non-Swiss	0.205	0.78	0.227		0.191	0.75	0.210
low employability (base: medium)	0.280	0.98	0.323		0.247	0.88	0.280
semi-skilled (base: skilled)	-0.089	-0.25	-0.085		-0.023	-0.06	-0.023
unskilled	-0.031	-0.06	-0.030		-0.057	-0.12	-0.056
non-German-speaking	0.140	0.40	0.151		0.023	0.07	0.023
1 foreign language (base: 0)	-0.173	-0.63	-0.159		-0.134	-0.48	-0.126
2+ foreign languages	0.158	0.53	0.171		0.145	0.48	0.156
PES 2 (base: PES 1)	0.290	0.52	0.337		0.136	0.25	0.146
management (base: professionals)	-0.383	-1.02	-0.318		-0.373	-1.05	-0.312
support function	0.168	0.31	0.182		0.112	0.20	0.119
part-time (but above 50%)	0.083	0.35	0.086		0.033	0.14	0.033
Month of entry in UE (base: Jan/Feb 2008):							
March/April 2008	-0.525	-1.80	-0.409		-0.405	-1.39	-0.333
May/June 2008	0.020	0.08	0.020		0.143	0.54	0.154
July/August 2008	-0.224	-0.78	-0.201		-0.111	-0.39	-0.105
Sept/Oct 2008	-0.129	-0.47	-0.121		-0.069	-0.25	-0.067
Nov/Dec 2008	-0.316	-0.97	-0.271		-0.244	-0.75	-0.216
Caseworker fixed effects (base: CW 1):							
CW 2	0.888	2.03	1.430		0.938	2.15	1.556
CW 3	0.602	1.46	0.827		0.600	1.46	0.822
CW 4	0.661	1.79	0.936		0.642	1.69	0.901
CW 5	0.272	0.61	0.313		0.247	0.54	0.280
CW 6	0.649	1.73	0.914		0.641	1.69	0.898
CW 7	0.684	1.71	0.982		0.755	1.80	1.128
CW 8	0.672	1.76	0.958		0.718	1.87	1.050
CW 9	0.597	0.88	0.817		0.605	0.94	0.831
CW 10	0.435	0.59	0.544		0.427	0.61	0.532
CW: rest (smaller charges)	0.778	1.57	1.178		0.962	2.03	1.618
Controls Occupation		Yes, all insign.				Yes, all insign.	
Unobserved heterogeneity		No				No	
-Log-Likelihood		1445.56				1444.82	
BIC		1610.58				1601.15	
N		327				327	

Notes: Coefficients and their transformations are reported: Transformed treatment effects are changes in %. 1) Note that 'age 55+'- and 'reorientation' coefficients are incremental to the main effect; the transformation expresses the total change. Transition rates are in % per month (for the respective piece of the hazard). 2) 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: $u_j = \lambda_b + \lambda_j + \bar{x}'\beta_j + \sum_i \tau_i \bar{M}_i + \sum_k \gamma_k C_k$ where $j = 2, \ldots, 6$ ($\lambda_j = 0$ for first segment) and the bars are means, except for the past unemployment and the duration until availability where medians are used. Asymptotic z-values.

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