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

Patrick Arni, IZA*

(& DEEP/University of Lausanne, CAFE/Aarhus University)

October 2013

Abstract: Empirically, not much is known about *how* policies like job search assistance and training operate in order to support job finding. This is mostly due to the lack of data that document behavior and subjective beliefs of job seekers. This study is based on a unique dataset that combines a randomized field experiment on a coaching intervention with detailed register data and a panel of repeated surveys conducted over the unemployment spell. This provides the opportunity to causally link the dynamics of search behavior to the treatment interventions. The coaching policy, focused on older job seekers, turned out to be successful: the treatment group incorporates 9 percentage points more job finders than the control group (72% vs. 63%). The treatment effect is driven by a reduction of reservation wages and, as different indicators of search behavior suggest, an increase in search efficiency. Moreover, I find short-run treatment effects on motivation, self-confidence, subjective well-being and beliefs. The job seekers overestimate a bit less their chances with respect to job interviews and salaries. Overall, the focus on realistic self-perception and on search strategy seems to be important for the success of such a policy.

JEL Classification: J64, J65, J68, J14

Keywords: Field experiment, labor market policy evaluation, older workers, reservation wages, job search behavior, subjective outcomes, dynamic treatment effects.

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

1 Introduction

The recent economic downturn is marked by a massive increase of the incidence of long-term unemployment in many countries. Particularly hit by this phenomenon are the U.S.[1] and Southern Europe. In this context, the call for supportive labor market policy came back into the focus of the debate: OECD urges to invest the additional resources in unemployment insurance (UI) rather into job training and re-employment services than into prolonged potential benefit durations (OECD 2012b). Given that harms of long-term unemployment for human capital are often severe and long-lasting (Machin/Manning 1999, von Wachter et al. 2009), supportive labor market policy could be helpful in dampening these negative consequences. But how should these programs be designed to fulfill this condition?

Evidence on this question is, however, incomplete and scarce. The results found in the existing literature on training and job search assistance is usually restricted to reporting the "total effect" of the program on immediate outcomes like job finding rates or unemployment duration (see Card et al. 2010 for an overview). In most cases it is empirically not visible ***why*** these interventions affect the outcomes. This would be, however, crucial knowledge for program design: Unlike interventions that exert their effects via threat and dislike[2], the program type considered here is supposed to act via its *content*. So, it is key to get to know how different elements of the intervention translate into the *behavior* of the job seeker. Evaluating this sets high conditions to the data and research design.

This study proposes, as its core contribution, a novel combination of data and design: On the one hand, the design of the study features a randomized controlled trial. This is complemented with a rigorous ex-ante setting of the timing of different treatment periods – in order to identify their specific impact on outcomes. On the other hand, extensive register data are combined with a panel of surveys (filled in by job seekers and caseworkers) which were repeated over the unemployment spell. This combination allows to track the *dynamics of search behavior* and to analyze it in parallel with the labor market outcomes[3].

Moreover, this paper contributes to the still small part of the UI and job search literature which is based on field experiments. Whereas in the case of the U.S. the published studies date back to the early nineties (see e.g. Ashenfelter et al. 2005, Meyer 1995), the European literature is more recent but focused on a small number of experiments in Scandinavia and the Netherlands. A series of field experiments in Denmark (Graversen/van Ours 2008, Rosholm 2008 and follow-ups) and one in Sweden (Hägglund 2006) find positive effects of monitoring and job search assistance interventions. The only randomized trial in the literature so far which reports

[1] In 2008, the incidence of unemployment of at least 6 months was at 19.7% – ranging from 15.9% for individuals aged 20 to 24 to 26.4% for the unemployed of age 55+. In 2011, the same figure drove up to 43.7% – ranging from 34.7% to 55.3% (source: bls.gov). An exception from the increases in long-term unemployment is Germany: the incidence in 2011 (48.0%) is lower than in 2000 (51.5%) – though, the level is still high (OECD 2012a).

[2] E.g. Black et al. (2003), Rosholm and Svarer (2008) and Graversen/van Ours (2011) report threat effects of training and job search assistance programs.

[3] Krueger/Mueller (2010) report job search dynamics in an observational data context: Using U.S. time use data they find that, e.g., job search is inversely related to the generosity of unemployment benefits. Brown et al. (2011) conducted real-time search experiments in the lab and revealed declining reservation wage profiles in search duration.

evidence on a particular indicator of job search is Van den Berg/van der Klaauw (2006). They observe a shift from informal to formal job search as a consequence of increased monitoring on formal search channels in a field experiment in two Dutch cities.

As a third contribution, this paper provides evidence on the effects of labor market policy (LMP) programs *targeted on older job seekers*. Interestingly, empirical evaluation literature on this issue is missing so far, to my knowledge[4]. A strand of literature explores discontinuities or reforms of potential benefit duration for older job seekers; a usual finding is that extensions led to declines in the transition rate to employment whereas reductions showed the opposite effect (e.g. Hunt 1995, Lalive/Zweimüller 2004; Kyyrä/Wilke 2007)[5]. Whereas this literature empirically assesses the importance of the moral hazard and liquidity constraint motives (Schmieder et al. 2012, Card et al. 2007) for the re-employment propensity of older job seekers, a third channel to potentially affect their job chances has been less explored: Updating their employability by use of specific, targeted LMP programs. The second contribution of this paper to the literature is to provide a first element of evidence whether this strategy may be a valid policy response in the case of older job seekers.

This study analyzes a randomized controlled trial that has been conducted with job seekers aged 45 to 61 in 2008 in Switzerland. The intervention featured, as its core, a highly intense coaching program of 20 working days. The content of the coaching was focused on different aspects of self-assessment and search behavior (see section 2). This was complemented by a doubling of the counseling intensity by the UI caseworkers during the first four months of unemployment. The control group followed the usual procedures (monthly counseling sessions at the UI agency). The systematic timing of the interventions allows the distinction of four treatment periods along the unemployment spell (which are also defined for the control group). The register data can track these in daily precision. Moreover, job seekers and caseworkers were extensively and repeatedly surveyed on issues of job search behavior – search effort, channels, strategy; reservation wages; motivation etc. These surveys along the unemployment spell are assigned to the respective treatment period. This design allows the analysis of treatment effects on search behavior, alongside with the effects on the re-employment propensity.

The analysis proceeds in three steps. First, a non-parametric analysis of the field experiment establishes the basic results. The intense coaching interventions resulted in a higher proportion of job finders in the treatment group (+9 percentage points). Unemployment duration was slightly reduced, however insignificantly. How and why did this positive effect on job finding come along? To explore this question, a dynamic analysis is set up – as the second step – in order to identify the treatment effects by treatment period. Since the initial randomization can potentially be confounded by dynamic selection in later stages of the treatment, some more structure needs to be assumed in the econometric analysis (Abbring/van den Berg 2005). We apply a semi-parametric timing-of-events approach for the duration-related data and diff-in-diff

[4] The only piece of literature focusing on the same topic that I found stems from social work practice research: Rife/Belcher (1994) found in a small experiment (52 individuals aged 50+) that a job club intervention in a town in North Carolina increased the reemployment propensity of the participants. There is, however, no economic analysis of the outcome.

[5] The size of this effect is, however, dependent on the business cycle (Schmieder et al. 2012).

procedures for the analysis of job search outcomes and subjective outcomes. The first approach allows additional tests for selectivity issues by means of adding unobserved heterogeneity.

These analyses reveal an interesting pattern of effects: The treatment group's transition to employment is lower in anticipation and during the coaching program – the first can be named as "attraction" effect (being the opposite of the known "threat" effect), the second is the well-known lock-in effect (e.g. Card et al. 2010). However, due to the fact that the interventions were timed very early in the unemployment spell, these early-stage negative impacts on re-employment were outweighed by the positive treatment effects later on. So, there is an advantage in timing supportive LMP programs very early in order to avoid strong lock-in phenomena.

Why did the coaching intervention finally boost the job finding? The period-wise analysis of search behavior reveals that the success of this type of coaching cannot be explained by the classical argument of "put more search effort and you will find a job": Quantitative search effort – measured as the number of applications or the number and frequency of used search channels – did not go up during or after the coaching. Many participants, however, seem to have invested in optimizing their search strategy by help of the coach: the proportion of individuals willing to extend their scope of search (w.r.t. types of jobs or occupations, geographical location etc.) was massively higher in the treatment group. The focus of the coaching on realistic self-assessment of the chances on the labor market (w.r.t. wages or job types) seems to be reflected in the result: Reservation wages of treated job seekers are significantly lower after coaching than in the treatment group. The downward-sloping reservation wage path of the treated fits well to the Burdett/Vishwanath (1988) model about sequential learning on the wage distribution of the job offers.

Moreover, the dynamic analysis of the treatment effects on subjective outcomes and beliefs yields the insight that some of these factors reacted on the coaching intervention in the short-run: We find some positive impacts on job search motivation, self-confidence, reliability and the distortion of beliefs during and up to 3 months after coaching. The overestimation of chances to get job interviews and on wage expectations has been temporarily and slightly reduced. In the longer run, only the positive treatment effects on subjective well-being remain visible (up to 5 months after unemployment exit). So overall, improving success of search of older job seekers seems less an issue of reducing moral hazard behavior (shirking), but more one of realistically adapting wage expectations and motivating the job seekers to search in a more directed and effective way.

In a final step, we extend the observation window to the post-unemployment period. The mentioned timing-of-events model can straightforwardly be extended to including the estimation of employment stability, i.e. the risk of transitions back into unemployment. The result that we find is that the recurrence rate is lower in the treatment group, in a period of 1.5 years after unemployment exit. This lower recurrence rate saves 23 days of future unemployment over this period, as a simulation of the model shows. This saving of future UI benefits is about 1.7 times higher than the cost of the intense coaching and counseling intervention.

The remainder of the paper is structured as follows: Section 2 presents the institutional background of the Swiss UI, the experimental setup, and the data. Section 3 reports the results of the non-parametric analysis of the experiment. Section 4 covers the results by treatment period, after introducing the applied estimation approaches. Section 5 discusses employment-stability outcomes, cost-benefit and some tests on how to further optimize the policy. Section 6 concludes.

2 The Experiment & the Data

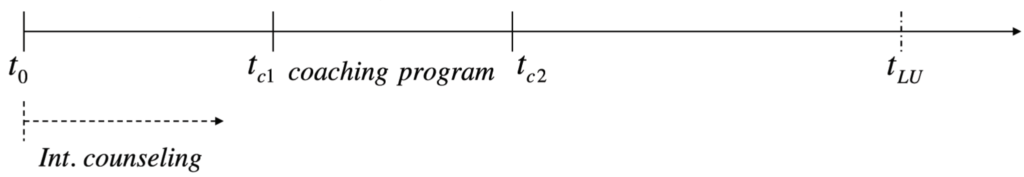
In this section, I will first describe the interventions that constitute the treatment plan. Then, I will shortly outline the institutional background: the Swiss unemployment insurance system and some facts about the (long-term) unemployment situation in the region of the project. Next, the specific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

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 4.1.

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



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

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

The *coaching program*, the second main measure, starts in median after 50 days (48.5 days for those who really participate, 52 days until potential coaching entry for the others[6]). Thus, the principle of early intervention is taken literally. The coaching was performed in small groups of 10-15 persons. An external, private-sector coaching firm was mandated to perform the coaching program. One coach ran all the coaching programs which took place during the year of inflow (December 2007 to December 2008; last program started in January 2009). The content and strategy of the coaching focused on three points: (i) increasing the self-marketing skills for the labor market; (ii) improving self-assessment which should result in a better and more realistic self-profiling, which helps again for successful self-marketing and efficiency of job search; (iii) optimisation of search strategy with a particular focus on assessing the potential of reorientations (towards other industries, regions, working times, search channels etc.). Thus, the coaching program features a strong element of human capital development (in terms of core competences and employability). The coaching program lasts 54 or 70 days (due to Christmas/New Year break). Job seekers were 3 to 4 full days per week in the program; in addition, homework had to be done as well. So the coaching program is highly intense and features a high work load (which results in a restriction of job search time, see section B.1 on potential effects).

The *content of the coaching* is crucial for understanding the treatment effects of this type of supportive labor market policy. In the following, I describe the five core elements that have been covered by the coaching program[7] over a net duration of 20 working days:

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

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

3. *Improvement of job application skills:* Interview training & feedback; role plays; use and

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

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

promotion of electronic applications, spontaneous applications by telephone (incl practical training).

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

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

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

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

2.2 Institutional Background

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

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

It is important to note that all the assignments to active labor market policy programs and the interview appointments – i.e. the described treatment plan of this experiment – are compulsory for job seekers[8]. 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. (2013). This strict sanctioning regime results in high compliance with the rules. This is the case as well here, see section 3.1 for details.

[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 (LTU) 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 (AMOSA 2007) reveals that this proportion amounts to 18.4% for individuals aged 30-34 – and increases up to 39.0% for individuals aged 55-59. Note that the last figure may be affected by the above-mentioned fact that job seekers of age 55+ and 61+ receive a benefit duration extension. The precentage numbers to the right of Figure 2 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

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

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

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

As a third step, the cases were *randomly assigned* to the treatment group (60%) and the control group (40%)[9], 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 2.4). On the other hand, the caseworkers were not allowed to use the terms 'long-term unemployment (risk group)' and 'randomisation'. The former was to avoid stigmatisation biases, the latter to prevent discussions which could potentially increase the risk of non-compliance.

Note, finally, that all the assignments to the treatment measures were compulsory (and could be sanctioned in the case of non-compliance, see last section). Still, non-compliance by the treated job seeker in terms of intentionally avoiding the coaching program can not be excluded with 100% certainty. But, as the non-compliance analysis in section 3.1 shows, intentional non-compliance could only be observed in a negligibly small number of cases.

2.4 The Data: Register and Survey

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

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

[9] In the first quarter of 2007, the random assignment ratio was 50%–50%. As a consequence of good economic conditions, inflow was lower than expected. We therefore decided to switch to a 60%–40% assignment rule. This explains why the treatment-control ration reported in the descriptive analysis in section 3.1 is in-between the two rules. Note that this switch has no impact on the quality of randomisation.

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

The register data include a rich set of observable characteristics (see table in section). Beyond socio-demographics, education and occupation, they track as well past unemployment histories up to three years before entry in the spell under consideration. The tables in the descriptive section 3.1 and, in particular, the first table in the section 4.1.1 on the results of the duration model (Table 3) report the collection of used observables.

The additional *survey data* used here stem from the repeated surveys of the LZAR data base. This data base, which features repeated surveys of job seekers and caseworkers over the unemployment spells in this project (see Arni 2011 for details), is fully linked to the register data. After the counseling meetings, the caseworkers had to fill in an online tool which complemented the information of the register data base. Job seekers filled in a repeated survey as well. Note that reporting of this information is not compulsory for the job seekers. I will analyse response rates and balancing in the next section.

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

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

The observed sample in the surveys is naturally subject to dynamic selection – individuals gradually leave unemployment for a job (or non-employment). Table B1 in the Appendix shows the dynamic development of the numbers of job seekers still present in unemployment at the mentioned points in time. These numbers provide the benchmark for a response rate of 100%. Of course, this response rate was not reached. The response rates are high in the earlier parts of unemployment, then they go down gradually, as the table shows. In the final survey, response rate is considerably higher again.

Note that the above-mentioned time structure of the surveys is then translated – using the exact date of each survey response – into a *timing structure relative to the treatment plan.*[10]. This structure allows the identification of treatment effects on the different outcomes by treatment period. The treatment periods are further described in the section 2.1 and visualised in the graphs on behavioral outcomes in the results sections.

2.4.1 Measures for Search, Reservation Wages and Subjective Outcomes

I will consider different empirical measures to capture the dimensions of job search behavior: search activity and reservation wage. I here briefly describe the survey items, on the base of which these6 measures have been constructed, and the resulting design and units of measurement. I provide as well some information on averages and spreads of the measures.

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

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

A third element of search behavior is *search channel choice.* Based on the same block of items as above, I analyse for each of the mentioned channels their *frequency of use.* The frequency is measured on a 6-point-scale: 3 = "daily", 2.5 = "several times per week", 2 = "weekly", 1.5 = "several times per month", 1 = "monthly or rarer", 0 = "never". I assign the aforementioned values to the respective points of the frequency scale. This offers the big advantage that the frequency distribution can be characterised with common means and standard deviations. This,

[10] Note that this relieves the problem of low response rates in the latest survey waves M9 and M13 (see Table B1 in the Appendix). The treatment stage 'later', as reported in the results, starts 90 days after (potential) coaching exit (t_3), thus it gathers survey information from M4, M9 and M13. If several surveys are available, the one nearest to 100 days after t_3 is chosen.

however, implies the assumption of the scale being approximatively metric. The facts that the frequency points are chosen in regular time steps and that the frequency distributions are not dominated by outliers suggest that this assumption can be justified[11]. These frequency measures allow two statements: First, how often is a certain channel used in the treatment and control group. Second, is there a shift to the more or less frequent use of some channels visible. The variety of frequency of use is, naturally, considerable between the different types of channels: Most frequent is the use of newspapers (mean 2.33, i.e. several times per week) and of internet (mean 2.24). Least frequent is the use of spontaneous written applications (mean 0.82, i.e. less than monthly) and of the contacts to former school mates and colleagues from education programs (mean 0.77).

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

The second fundamental dimension of search behavior is *reservation wages*. They are surveyed by the classical question about the minimum (gross) wage the job seekers still would accept. They are finally reported by the caseworkers survey[12] and contain the minimal monthly gross salary (not wage) the job seekers would accept. Over all treatment periods, the median reservation salary amounts to 5200 CHF (mean 5417 CHF, p25 4200 CHF, p75 6500 CHF).

As a further dimension I consider a series of *intermediate subjective outcomes*. As a first measure, we can analyze *motivation for job search*. This direct question to the job seeker is measured on a 5-point-scale. In order to reduce it to a tractable measure, I use the proportion of "very highly" motivated individuals (scale value 1) as an outcome variable. The distribution is skewed to the scale-values of high motivation. Second, I consider two measures of *self-efficacy* which were part of the caseworker survey. They assessed the self-confidence and the reliability of the job seekers on 4-point-scales. Again I use the probability of "high" self-confidence or reliability, respectively. There is another block of variables that cover different dimensions of

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

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

the willingness to compromise of the job seekers (longer commuting time, willingness to move, to change the occupational sector etc.). The analysis of those measures revealed no significant treatment effect. As a consequence, they are not further discussed in the paper.

Moreover, the repeated surveys allow to construct some measures of *beliefs about job chances*. I analyze, on one hand, the beliefs about the chances to get job interviews. This is a composite measure that considers the difference between the interviews expected and the interviews realized, each based on the applications of the last month (and standardized per sent application). Second, I consider a further question of the job seeker survey which asks about the expectations to earn more, the same or less (5-point-scale) than before unemployment. The goal of this measure is to reflect wage expectations. Note that the descriptive analysis of these measures of beliefs confirm findings of the behavioral economic literature that individuals have tendency to be *overconfident* with respect to their skills (see e.g. Burks et al., 2013).

Finally, I include the classical survey measure of *subjective well-being* in my analysis. The job seekers have been repeatedly asked about their life satisfaction, using a 9-point-scale. The average scale level of the job seeker's life satisfaction is at about 6.1 at the beginning of unemployment, then steadily decreases to about 5.3 and jumps up to 6.7 three months after unemployment exit.

3 Nonparametric Analysis of Main Treatment Effects

3.1 Descriptive Analysis

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

[Table 1 about here]

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 cancellation period[13]; this restricts their availability to participate in interviews and labor market policy. A second reason is that some job seekers may be engaged in a shorter temporary subsidized job such that they get available some weeks later. A majority of 57% is available for ALMP within 20 days. Note that the PES 2 joined the experiment inflow later, from June 2008 on. This, combined with the slightly changed random assignment ratio over time (see footnote 9), mechanically explains the slightly higher percentage of random assignments to the treatment group. Since this was all fixed ex-ante, it doesn't affect randomisation.

The median *age* of the participants in the social experiments is 52 years. The total age range of the participants lies between 45 and 63 years. Figure 12 in the Appendix shows the age distribution of the sample. 40% of the individuals in the sample are of age 45-49, 27.5% of age 50-54, 21.7% of age 55-59 and 10.7% of age 60-63. Note that none of this latter group had the possibility to pass to early retirement by means of unemployment insurance.

As compared to Table 1, to which degree are the used survey items balanced? The response rates are not perfect but high in the first and the final caseworker survey: 92.4 and 81.3%, respectively. The fact that not all the job seekers found a job and that reporting of job/salary information is not compulsory results in 163 remaining observations. This means that 68.5% of the individuals responded to the salary questions, measured as a proportion of the total of the job finders. This response rate is highly balanced between treatment and control group (68.1 vs. 69.2%)[14]. Slightly more women and part-time workers are among these job finders (salary info sample). Otherwise, observable characteristics are highly comparable to the full sample. The three survey samples are well balanced in their observable characterstics, as Table B2 in the Appendix reports. No significant differences in observables between treatment and control group are found, except from the proportion of married people. In total, there is *no indication of a significant response bias.*

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

3.2 Non-Parametric Results on Main Outcomes

What can be learned on the impacts of the social experiment without imposing any econometric structure? Given the successful randomisation at t_0 (see section 3.1), *causal statements on the*

[13] 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.

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

total/net effect of the treatment plan as a whole can be inferred in a nonparametric manner – by use of means comparisons and Kaplan-Meier survivor analysis. This is done in the following. Four main results materialise. They are documented in Table 2 and a series of survivor graphs.

[Table 2 here]

The first result arises from the nonparametric analysis of the question: How did the new labor market policy affect the (total) unemployment durations of individuals? The first row in Table 2 reports the comparison of the mean and median unemployment durations by treatment group (TG) vs. control group (CG). This yields a clear result: There is *no significant effect of the treatment plan on the unemployment duration.* The respective t-values report that the TG-CG differences are clearly not significant. Median unemployment durations do differ only marginally (139.5 vs 138 days). The mean unemployment duration of TG members (235 days) is 7 days shorter than the corresponding mean duration for the CG (242 days). Note that in order to provide a realistic picture of mean durations and to restrict the impact of extreme outlier values, durations have been (exogenously) censored at 570 days (19 months)[15].

In the light of the existing ALMP evaluation evidence (see references in introduction) the result of *no prolongation* of unemployment duration due to the new ALMP can be interpreted as being positive. The predominant result in the literature on training-oriented ALMPs is that they increase unemployment duration due to the lock-in effect (less search during the program) and/or uneffectiveness of the program with respect to labor market chances. Even though the new program evaluated here implies high workload and time consumption in the first four months of unemployment, this did not translate into a prolongation of unemployment duration. Possible explanations are a reduced lock-in effect and/or a substantial improvement of effectiveness in job finding after coaching. This can and will be tested in the upcoming sections 4.1 and 4.4 by use of a duration model.

[Figure 3 here]

Some important evidence concerning this question can already be gained when looking at the nonparametric survivor analysis of unemployment duration and of duration to job finding, see Figures 3. The first figure reports the proportion of individuals in the TG and CG who are still in unemployment. The dotted vertical lines indicate the median starting and ending of the (potential) coaching program[16]. The two curves of the survivor overlap over the course of the

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

[16] In the upcoming analysis by treatment period in section 4.4.1 I will use, of course, the exact timing by individual

first 270 days of unemployment; thereafter, they slightly begin to diverge, in favor of more exits from unemployment in the treatment group. This picture is consistent with the above-found slight but insignificant reduction of the mean unemployment duration due to the treatment. The survivor shows that a positive impact of the treatment on the rate of unemployment exit begins to kick in in later stages of unemployment.

This conclusion gets reinforced when analysing the durations until job finding (second figure in Figure 3). Unlike the first survivor comparison, the analysis here defines only those cases as a positive transition out of the initial status which end up in job finding; other cases of exits are censored. Beyond 250 days, the survivors of treatment and control groups more remarkably diverge, leading to a higher job finding proportion in the treatment group in the later stages of unemployment. As discussed further below, this effect of more frequent job finding is significant in total. Thus, this analysis shows that the new ALMP takes some time until it develops beneficial effects on job finding. So, *unemployment duration does not get shorter, but more individuals end up in a job in the treatment group.*

This result of a longer-run positive effect has not yet fully materialised at the threshold of long-term unemployment. The proportion of individuals remaining in unemployment for longer than 360 days is visibly smaller in the treatment group, but the difference does not get statistically significant as Table 2 shows. Thus, if the success of the new ALMP is narrowly judged by a reduction of the LTU ratio, this evaluation cannot provide a significantly positive result. However, this is not the case, the policy makers who ordered this pilot project defined more general policy goals: they mainly focus on the question whether the new policy was able to increase labor market chances of older job seekers. If labor market chances are measured by job finding, the program can be considered as being successful.

Which part of the population in the treatment group did especially profit from the new policy, which not? To explore this question two dimensions are further analysed: age and the timing of intervention[17]. Do individuals in the upper and the lower part of the considered age distribution behave differently as a result of the treatment? They do, but not much gets significant in terms of total/net unemployment durations. Table 2 reports that individuals below age 55 show some insignificant reduction of the mean unemployment duration, medians do not differ. This group dominates thus the above-discussed total effect on mean and median unemployment duration. Individuals aged 55+ do, however, clearly not profit from the treatment intervention in terms of unemployment duration: this gets prolonged by 16 days in mean and 92 days in median, the latter result being highly significant. So, the mentioned positive interpretation of the new program not prolonging unemployment duration does not hold for oldest subgroup of job seekers beyond age 55.

Can the impacts of the program be improved if interventions take place earlier? As discussed in the descriptive analysis of durations to coaching program start (see section 3.1), the core mechanism assigning anticipation durations to individuals is exogenous (timing of coaching fixed ex-ante). Thus, variation in time to coaching program entry can be used to assess a potential saving (or extension) of unemployment duration if the intervention takes place earlier (or later). I distinguish three subgroups: median anticipation durations of 35 to 70 days – yielding a median

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

of exactly 50 days, thus the default group – versus short anticipation durations (1 to 34 days, median 19 days, thus intervention 1 month earlier) or long anticipation durations (70+ days, median 102 days). Analysis of mean and median unemployment durations and of differences in treatment effects, see Table 2, reveals that the pattern indeed goes in the expected direction, but differences do not get significant. Note that the sizes of the used subsamples are quite small such that standard errors naturally get quite large and the threshold for significance quite high.

[Figure 4 about here]

Taking into account the nature of the treatment plan and its potential effects (see section B.1), early intervention could have distinct impacts in different periods: In the anticipation period, the attraction effect – which I find in the analysis by treatment period in section 4.4.1 – could be reduced by early intervention (higher early exit hazard); this would, though, help to reduce unemployment duration. In the stages thereafter, early intervention could be beneficial as well since individuals leave coaching, and therefore the related lock-in period, earlier. The respective survivor analysis is presented in Figure 4a. The solid line, representing early intervention (coaching start one month earlier), reveals why the total duration effect of early intervention is not stronger: In the anticipation period and during coaching (thus up to 80 days), the exit to job rate was indeed higher – this effect is clearly significant as the duration model in section **??** will show. But thereafter, from day 80 to 120, individuals remained in some lock-in. Finally, from day 120 on, the survivor curve is not distinguishable any more from the default group's. Thus, early intervention works to reduce the duration-prolonging attraction effect, but earlier exit from coaching could not be translated into earlier job finding. The latter fact can be explained by learning: individuals need some time until they efficiently apply the inputs of coaching (see also introduction). This learning time seems to be longer in the case of early intervention. A possible explanation for this is that the early-intervention-individuals had less opportunity to profit from the support of intensified counseling (only through 80 days, instead of the default of 120 days). Finally, the 70+ days-survivor in Figure 4a shows that late intervention resulted in some procrastination of job finding in all stages of unemployment.

The second main result documents the impact of the new policy on job finding. Table 2 shows that the *proportion of individuals who found a job is significantly higher in the treatment group – by 9 percentage points.* Whereas 63% of the CG individuals left unemployment to a job, the proportion of TG individuals leaving for a job amounts to 72%. Combining this insight with the survivor analysis above about duration to unemployment exit and to exit to job (see Figure 3) yields the following conclusion: *The treatment caused significantly more individuals to find a job. But since it took some time until treatment resulted in increased job finding, the total unemployment durations did not significantly reduce.*

A more detailed look on the exit destinations[18] reveals interesting supplementary insights to the result of more job finding in the treatment group. The TG individuals left less often unemployment for non-employment (8.6% vs 13.5% in CG) and were less often censored (i.e. less long unemployment durations, 14.0% vs 19.9% in CG). "Unknown status after unemployment exit" is a bit more frequent in the TG (5.4% vs 3.5%). More than two thirds of these cases deregistered from unemployment insurance in order to avoid controls or to renounce to services of the UI; the rest left the country to search for a job elsewhere. Since it is most probable that a clear majority of these individuals found in the near future a job too, I report these percentages (77.4% vs 66.7%) as well in Table 2. For this measure, importance and significance of the TG-CG-difference is even higher.

A final interesting observation with respect to job finding is that the additional job finding in the treatment group predominantly originates from "referrals by PES". It has to be noted that this subcategory is also used as part of the performance reporting of the PES. So, caseworkers have an incentive to report a found job as "referred by PES" even if the job does not directly stem from the PES-run job database, but the job finding procedure was substantially supported by the caseworker. Thus, it is most probable that this result reflects the stronger guidance by the caseworker due to intensified counseling in the treatment group. This would mean that intensified counseling was an important complement to the coaching program in generating the positive treatment effect on job finding[19].

Was the higher proportion of job finders in the treatment group probably reached through the acceptance of lower quality jobs? The answer is clearly no, as the third main result of nonparametric analysis of this experiment shows. The *monthly gross salaries realised after unemployment exit are not lower in the treatment group*, as Table 2 reports. It has to be noted that this result is based on a subsample of those individuals who found a job and reported their salary. So, there are two potential sources of bias: selectivity with respect to job finding and unbalanced non-response behavior. The analysis in section 3.1 shows that the latter is not the case. The selection issue with respect to job finding will be further discussed in the next section.

In older working age, reestablishment on the labor market after unemployment often implies a wage loss (due to weaker negotiation power, among other reasons). This is found for the here analysed population as well. On average, a pre-to-post-unemployment gross salary loss of 341 CHF is incurred, which is significantly different from zero. However, when comparing treatment and control group I do not find a significant difference in the size of the salary loss (see Table 2). This confirms the result discussed above that the treated did not choose jobs of lower quality than the controls. Moreover, a glimpse on the weekly average pensum (official working hours

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

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

per week) reveals that there is no significant difference in this job quality dimension too.

Finally, let's adopt the long-run view on how the labor market outcomes evolved *beyond* unemployment exit. Was maybe the long-run job quality diminished due to the treatment? This is measured by means of *recurrence behavior* – i.e. by analysing the probability that the job finders fell back into unemployment within 1.5 years. Such a measure reports, thus, *employment stability* within the given post-unemployment period. The question above can be answered with no: Table 2 reveals that 23% of the treated reentered unemployment within 1.5 years, whereas the recurrence propensity in the control group amounts to 28%. This difference is, though, statistically not significant.

[Figure 5 about here]

How does employment stability compare between TG and CG in a time-dynamic perspective? Figure 5 shows that the post-unemployment survivor curve of the treatment group is located clearly above the one of the control group – *treated individuals remain, thus, on average longer outside unemployment*. 300 days after unemployment exit, about 83% of the job finders in the TG remain in employment, whereas the same rate in the CG amounts to about 74%. In other words, the reentry rate back into unemployment is on average smaller in the TG over the course of 1.5 years of post-unemployment.

However, it is important to note that this long-run measure of recurrence is prone to a selectivity issue: Selection into jobs is, as we found above, (positively) different between treatment and control groups; this potential imbalance in observables and unobservables between the two groups could affect recurrence behavior. Taking this into account will indeed show in section 5.1.2 that the treatment effect on employment stability gets more distinct: The difference in the recurrence (hazard) rates in the post-unemployment period becomes bigger and significant – the new policy caused a significant reduction of unemployment reentry.

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

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

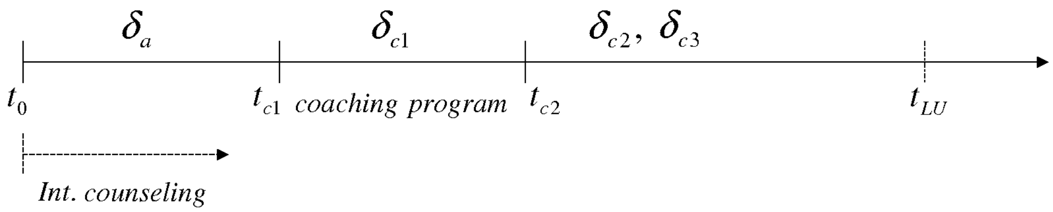
4 Analysis by Treatment Period

4.1 Econometric Framework: Duration Outcomes

In this section, I will apply the *timing-of-events approach* to the treatment plan setup of the new policy (see section 2.1). This provides three *key advantages* for gaining more detailed insights into the (short- and long-run) *dynamics* of the treatment effects of the new policy: First, the identification of sub-treatment-effects by use of the exact timing of the different treatment periods allows to further explain what really happened during the program. Which *part of the treatment plan* did contribute in which way to the observed net/total effect? Those results by treatment period help as well to search for *policy improvements* (section **??** is dedicated to that issue). Second, this duration model approach allows to take *dynamic selection* into account. This is mainly of importance when analysing *post-unemployment* recurrence outcomes as they base on a sub-sample of job finders, which implies additional potential selectivity. Finally, this modeling approach allows to quantify the *employment stability effect* (in days of avoided future unemployment), which is done in section 5.1.2.

4.1.1 Duration Model with Subsequent Treatment Periods

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



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

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

where θ_u is the exit rate from unemployment to a job and t_u is the unemployment duration. x is a vector of individual characteristics[20], 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,

[20] See the descriptive analysis in section 3.1 and the first results table (Table 3) in the section 4.4.1 for a list of controlled observable characteristics.

for the specific time and business cycle conditions at inflow into the sample. C_k are caseworker fixed effects and v_u represents the unobserved heterogeneity component which will be further discussed in section 4.2. The component $\sum_i \delta_i D_i(t_u)$ will be differently specified according to the gradual steps of the upcoming analysis. These specifications will be further discussed below.

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

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

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. Unemployment durations are exogenously censored at March 31, 2010 (end of observation window), if necessary. Note that the analysis in this paper focuses on *exits to job* rather than on general unemployment exits. This is done in the light of the results found in section 3.2 that the new policy significantly increased job findings. Therefore, we are explicitly interested in the effects of different parts of the treatment on job finding hazards[21]. Moreover, this concept is consistent with the goal of this paper to study as well the long-run impacts of the new policy on employment persistence and quality. Accordingly, the non-censoring indicator in this model is 1 for individuals who found a job (see section 3.2 for details on exit destinations).

Based on this model setup, I perform a sequence of analyses whereby the specification of $\sum_i \delta_i D_i(t_u)$ changes gradually. The first model I estimate is a (simplified) replication of the nonparametric survivor analysis of the total effect (see section 3.2) by means of a (M)PH model of the form of (1). This means that the treatment component only consists of one element: $\delta_b D_b$, whereby D_b is a dummy variable indicating that an individual is member of the treatment group. Thus, the estimated *baseline treatment effect* δ_b (not shown in the figure above) allows a shift of the hazard rate from t_0 on until unemployment exit for all treated individuals. Note that this model is clearly more restrictive than the nonparametric one since it requires the hazard rate shift to be constant over time (which is not the case in the nonparametric analysis). Still it is useful to run this model just as a baseline benchmark. Note, moreover, that due to randomisation no issue of endogenous selection is involved here.

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

Let us describe the content of the different treatment effects a bit more in detail: In the early stage of unemployment, from t_0 on, the *(gross) anticipation effect* δ_a is identified, due to

[21] In the Appendix I provide, as a supplement, all the estimation results for the case of exit from unemployment in general. They would be especially useful for quantifying the impact of the program on duration in unemployment insurance. But this treatment effect is, net, zero as section 3.2 reports.

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

It is important to point out that the definitions of the treatment effects in the models described above imply that the respective effects are identified by the population who *effectively participated* in the later stage treatment periods (from t_{c1} on). This makes sense here since we are interested in the *effective impact of intensified counseling and coaching on those who really followed it.* However, this makes the period-specific treatment effects subject to potential dynamic selection and endogenous non-compliance biases. Note, though, that the latter issue is very marginal here since only 3.2% of intentional non-compliance was found (see section 3.1). These two issues can be handled by introducing unobserved heterogeneity to the model (whereas a second equation to design later treatment entry is not necessary here, see section 4.1.2). This will be further discussed and then analysed in the next section and in section 4.2.

However, it can be, in addition, of policy interest how the *gross program effects* in different stages look like. Such an intention-to-treat (ITT) analysis uses in every stage all individuals remaining in unemployment who are *assigned* to the treatment – independently if they really were participating in the later treatment stages[22]. This reflects the total impact of the policy assigned at t_0, given that there is some non-participation. The vast majority of the non-participation is not due to intentional non-compliance, as section 3.1 demonstrates, but due to the announcement to have found a job (unemployment exit in some weeks or months) or a temporary subsidized job (remaining in unemployment but not subject to labor market policy during that time), thus due to normal reasons of dynamic selection which apply as well to the control group. This fact, combined with randomisation and ex-ante timing of the treatment plan at t_0, alleviates the potential issue of bias due to endogenous selection. The ITT analysis is reported (following the same sequence of analyses as described above) in the Appendix in Table B4.

[22] Note that *all* individuals in the treatment group were informed at t_0 about the date for the upcoming coaching program. Thus, I dispose of the exact date of potential coaching entry for all treated individuals. This date is used to determine t_{c1}, t_{c2} and t_{c3} for treated individuals who finally didn't participate in the coaching. For further details, see footnote 6.

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

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

First, randomisation at t_0 allows for a "clean" identification of the treatment effect that starts right at t_0. This is not possible for non-randomised studies since they cannot distinguish between endogenous selection and the real treatment effect in the first period from t_0 on (Abbring et al. 2005). In contrast, randomised treatment assignment leads to a balanced distribution of unobserved characteristics at t_0. This solves the selection issue at t_0 and allows therefore to identify, in particular, the *anticipation effect*[23] of a later treatment that starts at a $t > t_0$.

Second, randomisation combined with an exogenous timing of treatments and information (timing and characteristics of the treatment plan is revealed to the individuals at t_0) brings as well advantages – simplifications – for the identification of later treatment effects. In the standard case of the timing-of-events approach without randomisation Abbring and Van den Berg (2003) show that the identification of the effect of a treatment starting at $t_1 > t_0$, i.e. a hazard shift at t_1, requires the *no anticipation assumption* which basically implies that the counterfactual hazards (for TG and CG) must be equal up to t_1[24]. In the case here, however, of randomisation and full information at t_0 we encounter a situation of *perfect anticipation*. Since the sample is fully balanced at t_0 (between TG and CG)[25] and, in particular, the TG members have full information about the upcoming treatment periods, they can immediately and transparently act on this information – which is captured, without bias, by the anticipation effect δ_a, estimated over the period from t_0 to t_1 (or to t_{c1} in the specific case of this experiment). Thus, the no anticipation assumption is replaced by measurable *perfect anticipation*[26]. Finally, this full-information-argument carries over to the later treatment periods: Conditional on observables, unobservables, the previous treatment history and full (ex-ante) information about the treatment plan, the anticipation about the treatment in the next period is captured by the treatment effect in the ongoing period.

Third, a further advantage of randomisation and full information at t_0 is that these properties *make the separate modeling (by means of a further equation) of the inflow process into later stage*

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

[24] This could be expressed (in simplified notation) as $\theta^T(\tau_0|x, v_u) = \theta^C(\tau_0|x, v_u)$ where θ^T and θ^C are the counterfactual hazard rates a time $\tau_0 \in]t_0, t_1[$. Note, moreover, that the no anticipation assumption refers in fact to no probabilistic anticipation. Deterministic anticipation, i.e. acting on information which is available to everybody at t_0 (like general monitoring behavior of the PES or generally distributed information on a program etc.), does not break the assumption since this information is equally available for treatment and control group. See Arni et al. (2013) for a further discussion and example.

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

[26] So, more formally, the equality $\theta^T(\tau_0|x, v_u, D_a)\exp(\delta_a) = \theta^C(\tau_0|x, v_u, D_a)$ holds here and describes perfect anticipation – as compared to the no anticipation assumption in footnote 24 (using the same notation as there).

treatment[27] *unnecessary*. Thus, a control of unobserved heterogeneity is enough to cope with the ongoing dynamic selection. I.e., to cope with the fact that inflow into later treatment stages is not necessarily random any more, since – after the start of treatment at t_0 – the relative proportions of unobserved characteristics may change in a potentially different way in treatment and control group. The explanation for the redundancy of a separate modeling of later stage treatment inflow is the following: Due to randomisation and exogenous, ex-ante timing, the ongoing selection is *uncorrelated* to the propensity to enter the later treatment (coaching), *conditional* on the anticipation effect. In other words, the anticipation effect captures changes (related to early treatment) in the propensity to enter later treatment[28]. Again, this argument carries over to all the later stage treatment parts (D_{c1}, D_{c2}, D_{c3}). Moreover, by the same line of argumentation one can conclude that as well issues of potential non-compliance can be handled in the same, simplified way.

4.2 Dynamic Selection and Unobserved Heterogeneity

Dynamic selection is a potential issue in the context of this study, even though it is designed as a field experiment. Initially, at t_0, randomisation indeed yields a balanced proportions of unobservable characteristics between treatment and control group at t_0. But as soon as treatment starts, here right after t_0, the balancing potentially gets compromised. This is the case if treatment causes dynamic selection to be *different* in the two groups (if balancing is equal, no problem arises for the identification of later treatment effects). This potential imbalance is taken into account in the timing-of-events models by allowing for *unobserved heterogeneity*. Moreover, section 4.1.2 shows that in our context of randomisation and full information at t_0, controlling for unobserved heterogeneity is sufficient to take into account potentially endogenous selections coming from take-up behavior of later treatment stages and intentional non-compliance.

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

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

$$p_n = P(v_u = v_u^n) \qquad \text{with} \quad n = 1, 2 \qquad \text{if only process } u \tag{3}$$

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

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

[28] This means that for our main model (1) here the following orthogonality applies: $v_u \perp\!\!\!\perp D_{c1} | x, v_u, D_a$. If this independence is given, no further equation is necessary to model the relation between later treatment inflow and unobserved heterogeneity.

The above probabilities are designed in a logistic form, i.e. $p_n = \frac{exp(a_n)}{1+exp(a_1)}$ for the case (3) and $p_j = \frac{exp(a_j)}{1+exp(a_1)+exp(a_2)+exp(a_3)}$ for the case (4) (normalising one parameter to being 0). Thus, this implies the additional estimation of maximum two/four probability parameters a_n/a_j and of maximum two/four baseline hazard intercepts λ_0^n/λ_0^j in the 1/2 process/es model, respectively. By allowing for all possible mass points combinations in the latter case of two processes, I model the (potential) correlation of unobservables between the two processes, which is generated by the selective inflow into the post-unemployment employment status.

Combining the unobserved heterogeneity structure (3) from above with the main model (1) for the first process, I use an iterative procedure to find the optimal locations, proportions and numbers of mass points. This iterative estimation procedure largely follows the implementation of the NPMLE as proposed by Baker and Melino (2000). In the Appendix A I provide a more detailed description of how I implemented the algorithm of grid search and step-wise estimation. The decision criterion to find the optimal model is the highest log likelihood, following the suggestions by Gaure et al. (2007).

This NPMLE procedure applied to (1) resulted in suggesting a 2-mass-points model as being the best choice[29]. Grid search for a third mass point (following the procedure by Gaure et al. 2007, see Appendix A) did not provide any specification yielding a higher log likelihood. Estimation of the best 2-mass-points model delivers a log likelihood of -1536.16 – whereas the model *without* unobserved heterogeneity yields a log likelihood of -1455.45 (see Table 4). Therefore, the conclusion is that for our 1-process model there is *no gain in explanatory value by adding unobserved heterogeneity*. As a consequence, I report in section 4.4 the models without unobserved heterogeneity.

The same procedure was applied to the *2-processes model*, which combines equations (1) and (6) with the unobserved heterogeneity specification (4). The resulting best-choice-specification is reported as estimation 2 in Table 11. Two of the four possible mass point combinations turn out to be non-zero. But again, the log likelihood of -1987.05 is lower than the one resorting from estimation of the 2-processes model without unobserved heterogeneity (log lik of -1455.45+(-459.05)=-1914.5, see Tables 4 and 11, estimation 1). Thus, the conclusion for the 2-processes model is as well that *no gain in explanatory value by adding unobserved heterogeneity* can be achieved. (Estimation 2 is still reported for comparative reasons.)

Thus, the analysis of unobserved heterogeneity models reveals that the *size of imbalance in unobservables due to dynamic and endogenous selection is statistically not relevant here. Therefore, the models without unobserved heterogeneity can be interpreted causally.* There are different possible reasons for the non-importance of unobserved heterogeneity in the context of this study. First, the tight sampling criteria applied in the preselection into the sample may have avoided the generation of too big imbalances over the course of treatment: Individuals are in the same age group, in the same labor market, comparable in terms of employability and in terms of skills. Second, the selection caused by the found treatment effects by period could be of a balanced nature: i.e., the individuals who found a job due to the program are not fundamentally different from the job finders in the control group. Finally, it is not completely excludable that

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

the non-identification of further mass points may be due to the small sample size. However, this is not very probable since Monte Carlo simulations in Baker and Melino (2000) have shown that it is well possible to identify several mass points with 500 observations.

4.3 Econometric Framework: Search Behavior Outcomes

The following econometric model is set up to analyse the impact of the experimental policy intervention on the evolution of the 6 considered measures of job search behavior. This is a dynamic problem due to the dynamic nature of the treatment. In fact, the policy intervention features a full treatment plan with different stages (see section 2.1), and every of those stages potentially influences the behavioral variables in a different way. I therefore estimate the impact of the treatment on the 6 behavioral variables for every stage of the intervention plan separately: *anticipation (t_0 to t_1), during coaching (t_1 to t_2), up to 90 days after coaching (t_2 to t_3), beyond 90 days after coaching* (after t_3). Note that the fact that this timing was fixed ex ante and communicated at t_0 provides the means to identify separate treatment effect by treatment period (Abbring et al. 2005). I refine this sequential strategy by the use of a *difference-in-differences (DiD)* approach following the implementation of Meyer (1995). Thus, I estimate regression models of the following type (omitting the individual subscript i):

$$y \;=\; \alpha + \gamma^{TG} D^{TG} + \gamma_t T_t + \delta_t D^{TG} T_t + x'\beta + \varepsilon \qquad \text{for } t = 1, \ldots, 4 \tag{5}$$

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

The coefficient of key interest is the DiD parameter δ_t which measures the treatment effect in period t of the intervention on a certain behavioral outcome variable y. y represents the six mentioned behavioral measures. t indicates the four distinct periods of the treatment plan (see above). The sequential equations (5) above are estimated by OLS[30] or, in the case of reservation wages, by median (quantile) regression. Due to the skewed (typically approx. loglinear) distribution and broad range of wages, analysis of medians yields a more appropriate picture than of means, as they are not sensitive to outliers.

The benefits of using DiD in this context are twofold. First, DiD corrects for ex-ante differences in the behavioral outcomes. Even though groups are randomised at t_0 (and randomisation worked well in this experiment, see section 3.1) it can happen by chance that the initial levels of some behavioral variables are not fully balanced. DiD is a straightforward means to take this ex-ante difference into account; it will be captured by γ^{TG} in the model (5). Second, DiD does the same job with unbalanced unobservables which are constant over time. This is an important tool to reduce the impact of dynamic selection.

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

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

However, unbalanced dynamic selection can be an issue of bias in the later stages of the treatment plan. The results of the estimation of the anticipation effect, presented later on, indicate the direction of the potential selectivity: Individuals in the treatment group tend to exit less from unemployment in the anticipation period (attraction effect). As a consequence, more "high types" – e.g. in terms of ability and/or chances to find a job – remain in the treated group. If this selectivity issue can be solved by the use of DiD depends on nature of the impact of being high type on the intermediate outcome: If being high type influences the outcome in a constant extent over time, DiD will handle the issue, γ^{TG} will capture the unobservable. If the influence changes over time, estimation of δ_t after coaching can potentially be biased. If there was a bias in the treatment effect in late stages, in which direction would it go? The treatment effect on reservation wages would be underestimated, if coaching acts in the theoretically predicted way. Thus, treatment would decrease reservation wages, whereas selection (more high types) would increase them – i.e., we observe an underestimation of the decrease. For the other dimensions, the direction of potential bias depends on which of the possible treatment impacts prevail.

It is important to note, however, that there are several empirical indications which suggest that the issue of unbalanced dynamic selection is of small size. First, the estimation of duration models featuring unobserved heterogeneity as presented above show that such heterogeneity is statistically not relevant. Second, the descriptive analyses discussed in section 3.1 yield as a result that also in the later treatment periods almost no observables are imbalanced (see Table B2 in the Appendix). This suggests that the initial randomisation (plus the homogeneity of the initial sample) translated in a considerable degree to the later stages of treatment and unemployment.

4.4 Results by Treatment Period

4.4.1 Reentry into Employment: Dynamic Treatment Effects

This section aims at providing insights about the *specific impact patterns over time* caused by the new policy. In the following, I report and discuss the results of the series of duration models which identify the dynamic effects of different treatment steps on the reentry into employment.

At the start of this analysis of treatment effects by treatment period a glance shall be thrown on the *baseline model* which estimates the total effect of the program on duration to job finding (see section 4.1.1 for the model setups). This is, thus, the semi-parametric version of the non-parametric analysis of unemployment duration, and serves as a baseline benchmark. Table 3 reports the results. When only allowing for one constant, permanent treatment effect (δ_b), a zero effect of the treatment plan on the duration outcome is found. This zero effect clearly reflects the non-parametric result from the means and median comparisons between treatment group (TG) and control group (CG). Note, however, that results are not exactly comparable since this semi-parametric model presents a treatment effect averaged over time and puts therefore relatively more weight on early results (as the proportion of exits in the first 5 months is high, see Figure 1). The non-parametric survivor analysis, on the other hand, is more flexible in the sense that it exactly reports the survivor differences at every point in time. Therefore, the positive effect on job finding – which kicks in after some time – only gets visible in the survivor analysis (see Figure 3), but not in this baseline duration model. We need, thus, a split-up in treatment periods in order to get more specific insights.

[Table 3 about here]

Before doing so, let's complete the baseline picture by a short look at the role of the control variables and the fit of the baseline hazard estimation. The most prominent role among sociodemographic impact factors for job finding plays age. Not very surprisingly, the difference in the exit to job rate between individuals aged 45-49 and those aged above 55 is important. Moreover, female job seekers are relatively more successful (or quicker) in finding a job[31]. Individuals of low employability[32] have, interestingly, a higher exit to job hazard rate. Moreover, caseworker fixed effects turn out to be of sizable importance: Since caseworkers are assigned by occupation (see section 2.3)[33], these effects reflect occupation-specific job chances – besides caseworker-specific differences in success in giving job finding support. The fact that not so many control variables are statistically significant may be partially explained by the relatively high homogeneity of the experimental population (similar age and employability, same labor market etc). Finally, when looking at the piecewise-constant baseline hazard rates for an "average" individual (see Notes of the Table 3 for the specific calculation) 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 6.4% to about 15% and then down to 8% and less from 151 days on.

[Table 4 about here]

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

[32] The employability rating is assessed by the PES employees at the time of registration. Here, the initial population only consists of individuals of employability medium and low; see section 2.3 for the sampling before randomisation.

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

How do the *specific treatment effects by treatment period* look like? Table 4 reports these results which are based on model (1) with the same control variables. The dynamics of the treatment effects reveals indeed a pattern which was not yet visible in the nonparametric analysis (due to overlaps of treatment periods): The found zero effect on unemployment duration was, in fact, generated by the interplay of a period of lower exit rates, followed by one of higher exit rates. The *anticipation effect* (δ_a) is highly significantly negative. Treated individuals have an on average 37.6% ($= exp(\delta_a - 1)$ lower unemployment exit rate in the period between unemployment inflow and (potential) coaching entry. Thus, the prospect of being coached obviously results in a smaller propensity to exit early to a job. The treated people seem to expect a positive outcome or at least some helpful support of the coaching program. Therefore, one may call this negative anticipation effect an *"attraction effect"* – as an opposite to the commonly found "threat effect" in the analysis of other kinds of programs (see e.g. Rosholm and Svarer 2008, and introduction of this paper). The analysis of search behavior, as introduced in the next section, can provide some empirical insights if this "attraction effect" is rather driven by a smaller job search effort or by being more picky in accepting jobs.

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

Six months after the end of coaching, the treatment effect (for the coached individuals, δ_{c3}) reveals to be clearly positive but insignificant. The higher exit rate to a job of the coached reflects the insight of the nonparametric analysis that in later stages of the unemployment the positive impact of the new program kicks in. However, since the exits to job are quite dispersed over time (given the small sample) beyond 181+ days after coaching, the estimated δ_{c3} gets "averaged out" and therefore not that big – compared to the cross-sectionally measured significant effect on job finding proportions (see Table2). Note, in addition, that the standard error of δ_{c3} is comparably high due to the small sample size remaining at this late stage of unemployment. Moreover, it is interesting to consider as well the ITT analysis of the post-coaching effects. The ITT post-coaching effect beyond 180 days (δ_{c3}), reported in Table B4 in the Appendix, turns out to be higher than the specific one and to become significant. The ITT effects encompass the whole treatment group, thus as well the non-coached TG participants. These are individuals (except from the 3.2% non-compliers, see section 3.1) who announced in the period before coaching to have found a job or a temporary subsidised job[34]. So, they show by default (dynamic selection) a higher exit rate, but note that this kind of dynamic selection (and the availability of temporary subsidised jobs) is present as well in the control group. Thus, the interpretation of the higher post-coaching effect is that the intensified counseling led to additional job findings, beyond the

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

coaching.

Finally, a glance at the results for the corresponding models for unemployment exit – see Tables B5 and B6 in the Appendix – shows that the treatment effects are very comparable to the exit-to-job analysis from before. The only salient difference is that the post-coaching effects are weaker and always insignificant (treatment-specific and ITT). This reflects the result found in the nonparametric analysis (see section 3.2) that the treatment caused more individuals to exit to a job instead of exiting to non-employment (which is in these models here considered as an exit).

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

4.4.2 Search Behavior: Dynamic Treatment Effects

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

Was the content of coaching & counseling, as described in section 2.1, indeed implemented in practice? Did it find its way to the job seekers? The measure of *search strategy extensions* offers a direct opportunity to assess this question with respect to some elements of the coaching content: An important part of the latter is dedicated to discussing search strategy and search efficiency optimizations. The indicator analysed here becomes one if the respective individual agreed with the coach (and/or caseworker) to extend the scope of search in at least one of the following seven dimensions: change of industry, of occupation, of geographical place of work, kind of employer, workload searched for, permanent vs temporary job, work hours & shifts (see section 2.4.1 for some descriptives on the indicator).

[Figure 6 and Table 5 about here]

Figure 6 and Table 5 show a very distinctly shaped picture: Whereas the propensity to extend the scope of search is around 0.2 for the treatment group (TG) and the control group (CG) in the anticipation period as well as after coaching, the amount of strategy extension massively increases for the treated during coaching: 48% of them extend search strategy as a consequence of the treatment, whereas only 18% of the CG members extend strategy during the same period. This is reflected in the regression estimates of the treatment effect by treatment period. Table 5 reports a *massive and highly significant increase of the propensity to extend the scope of search* by 42.4 percentage points[35]. So, the initial question about the implementation of respective contents can clearly answered by yes. It is interesting, however, to remark that

[35] Note that, due to the fact that at t_0 no strategy changes are possible yet, this regression is not DiD as modeled in section 4.3. But given the zero level of the outcome at t_0, the direct regression per period is equivalent to DiD.

this strategy extension behavior is solely shown during coaching This strongly suggests that this kind of behavior is causally linked to the presence in the coaching program[36] – high-frequency counseling plays here a minor role as there is no tendency to strategy extensions visible in the pre- and post-coaching times.

[Figure 7 and Table 6 about here]

A second aspect of the fundamental dimension of search is the pure *search effort*. The most striking result is that the coached & counseled individuals in the treatment group *never searched more than the control group*, in all the periods potentially affected by the treatment (from anticipation until exit). Table 6 shows that *during coaching and in the first 3 months after coaching treated individuals sent out significantly less applications* (-1.64/-1.95). The difference in the anticipation period is of the same size, but it doesn't become significant. Beyond three months after (potential) coaching, the negative impact of the treatment on the quantitative level of search effort tends to vanish, as the Table 6 and the Figure 7 show.

A look at the control variables in Table 6 reveals that mainly individuals beyond age 60 exert less search effort (about 1 application less) than younger job seekers[37]. In particular in the first stages of unemployment (until end of coaching period) people with very low employability show significantly lower search effort. The constantly significant dummy for PES 2 shows that this PES permanently requires about 3 applications more per month. Due to the high federalism of the organisation of unemployment insurance, such differences in policy implementation by PES are common in Switzerland. Finally, the significance of some of the caseworker dummies indicates that the requirements on job search effort posed by the caseworkers may differ by industry (since caseworker assignment is by industry). Of course, the caseworker fixed effects cover as well other differences in caseworker behavior.

I shortly want to discuss here, at the beginning, the interpretation of the other two coefficients which come together with the DiD coefficient (δ_t, see equation (5)). The coefficient called 'time' (γ_t in (5)) captures the effect of the ongoing duration of unemployment, as compared to t_0. It's size of about 4 in Table 6 reflects the fact that individuals sent out a smaller amount of applications before the initial meeting since they mostly haven't been on job search for already 4 weeks. γ^{TG}, the coefficient on the treatment dummy D^{TG} captures the initial difference in levels of search. By coincidence (generated by the randomisation[38]), the initial levels are not that well balanced for this measure. Note that the coefficient γ^{TG} also partially captures the unobserved influence of dynamic selection on balancing, if it acts in a constant way over time

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

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

[38] Note that the descriptive analysis, see section 3.1, shows that the randomisation worked really well. I found as well in further descriptive analyses no indication of a systematic bias in reporting of search effort. The initial difference can, therefore, be interpreted as an (exogenous) random event generated by the randomisation (combined with the fact of the comparably small sample size).

(see discussion in section 4.3). If this unobserved influence changes over time, the treatment effect on search effort could be slightly biased in the post-coaching periods. But the amount of potential bias is low, given the result above that introduction of unobserved heterogeneity turned out not to be statistically relevant and only slightly changed the sizes of late treatment effects.

[Figure 8 and Table 7 about here]

Let's have a look, next, at the effect of the policy intervention on the *variety of used channels of search*. In parallel to the result on search effort, I find here as well that *the treated never increased channel variety, but some time reduced it*, as compared to the control group. Figure 8 and mainly Table 7 reveal that the treated individuals used a *significantly lower variety of channels (-1.2 channels) in the first three months after coaching*. Smaller (and insignificant) reductions in the channel variety are found as well thereafter, and in the anticipation period. An interesting side observation is that women used a significantly lower channel variation (about -1 channel) as well as individuals aged 55+.

[Figure 9 and Table 8 about here]

How did the treatment affect *channel choice* and the *frequency of channel use*? The available data allow the analysis of these questions by looking at the results for each channel of search separately. This is done in Table 8, where I report the six most important search channels. A first observation is that the negative signs on the DiD coefficients clearly prevail. Thus, as observed for the effort and channel variety dimensions of search, *frequencies of use are in tendency reduced and not increased*, too. I distinguish three formal channels – newspapers, internet and private recruiters – and three informal channels – network (weak ties) and spontaneous applications by telephone or by mail. The most prominent result is that *the treatment caused significant reductions of frequencies of use of formal channels after coaching*. The negative treatment effect for newspapers gets significant beyond 3 months after coaching, the one for the internet in both post-coaching periods and the one for the reduced use of private recruiters in the first 3 months after coaching. Note that there is as well a tendency for reduced frequencies of formal channels in the anticipation period (which becomes significant for the case of newspapers). Figure 9 graphically illustrates the example of the frequencies of use of the internet. This figure and the analysis of the general time trend T_t (which is identified by the control group behavior) reveal that the found negative treatment effect in later stages is due to the fact that the CG individuals increased the use of internet (over time) more than the TG people.

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

[Figure 10 and Table 9 about here]

As a second fundamental dimension of job search behavior, I analyse the evolution of *reservation wages*. Note that, in fact, the empirical measure reports reservation salaries (i.e. minimal monthly gross earnings that still would be accepted by the job seeker). Figure 10, supported by the estimations in Table 9, reveal a remarkable pattern: *Reservation wages of the treated are reduced over time (after the anticipation period), whereas the control group keeps reservation wages at the intial level.* Median reservation wages of the treated are kept significantly higher in the anticipation period, as compared to the control group[39]. Then, the opposite trend kicks in: Reservation wages of the TG are significantly lower in the TG from the during coaching period on (whereas the latest period is not significant any more).

It is important to note here the corresponding labor market outcomes, i.e. that in the treatment group more people finally find a job, which pays on average the same salary than in the control group. This interesting *combination of lower reservation wages with higher job finding proportions at the same salary level* will be further discussed and put in a theoretical context in the next subsection. Specifically, the pre- and post-unemployment (gross) salaries are the following: The pre-unemployment median salary is 5500 CHF (1 CHF=0.78 EUR=1.11 USD) for the treatment and for the control group. The realised median salaries after unemployment are 5470/5350 CHF in the treatment/control group. Thus, the (gross) reservation wage of job seekers at t_0 is of equal level as the pre-unemployment salary. This zero difference suggests that individuals do not (yet) take into account that unemployment is often linked with human capital and wage loss, in particular for individuals of age 45+. Moreover, intentional overreporting could be another explanation for the high initial level of reservation wages. Note that, due to randomisation, overreporting behavior at t_0 should be balanced. The comparison of the last reported reservation wages with the realised salaries after unemployment reveals that *the control group's last reported reservation wages are above the median realised salaries whereas the treated' reservation wages are below.*

[39] Note that the control group shows a temporary reduction in reservation wages in the early stage of unemployment (see effect of T_t in anticipation and Figure 10), then they go back to the initial level. This pattern is consistent with the typical unemployment exit rate profile over time: the exit rate peaks in the first months (i.e. during the time of lower reservation wages) and then goes down (when reservation wages go up again). Thus, there seems to be a certain initial motivation to accept more jobs in order to early exit from unemployment, which then fades away.

4.5 Subjective Outcomes & Beliefs: Dynamic Treatment Effects

As a final step of the dynamic treatment effects analysis, I consider a series of *subjective intermediate outcomes*, as introduced at the end of section 2.4.1. The conceptual idea is that such types intermediate outcomes may materialize as an immediate effect of the treatment intervention. So, in a first step, a supportive treatment like coaching could affect the subjective self-assessments of the job seeker. These, in turn, could be a determinant of (non-)success of job finding.

We analyze the dynamic treatment effects on three groups of subjective outcomes: motivation, self-efficacy (self-confidence and reliability) and (biased) beliefs on job chances. The analysis follows the dynamic diff-in-diff procedure that has been outlined in the econometric section. In order to directly visualize the results of these repeated regressions, a series of graphs will be discussed in the following that reports the diff-in-diff results per subjective outcome and per treatment period.

[Figure 11 about here]

In the case of *motivation for job search* we find a significantly positive short-run treatment effect in the first three months after coaching participation. The proportion of very highly motivated individuals is there higher by about 0.2 in the group of the treated. The positive effect tapers off and becomes insignificant in the subsequent period. The same pattern is visible for *self-confidence*. As well the *reliability* of the job seekers seems to be boosted by the coaching in the short-run. The positive treatment effect becomes significant during the program and is still of the same dimension but imprecise in the period thereafter.

The positive *bias in beliefs* – i.e. the overestimation of job chances – gets reduced as a short-run effect of the treatment, as the data suggest. The first measure, the difference between expected and realized interviews, becomes significantly reduced during coaching. The surveyed wage expectations only get significantly reduced in the first three months after coaching.

It turns out that the impact of the treatment on *subjective well-being* (life satisfaction) is most persistent: The estimates reveal positive effects of being treated on the life satisfaction scale during coaching and the first three months thereafter. The positive impact is, however, as well visible three or five months after unemployment exit.

In total, one can state that there is a pattern of positive short-run treatment effects of the coaching intervention on the different groups of subjective intermediate outcomes. Given that we subsequently found significantly increased job finding, this pattern suggests that the short-run boost in becoming more motivated, more disciplined and less biased in beliefs may have been beneficial for the success of search activities later-on. The duration of (measurable) impact of this type of coaching intervention on subjective outcomes of treated individuals is restricted, however. The gain in subjective well-being from being supported by such a labor market policy is, on the other hand, more persistent.

5 Mid-run Outcomes, Cost-Benefit and Policy Optimizations

5.1 Post-Unemployment Job Quality: Stability?

5.1.1 The Model

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

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

whereby t_p is defined as the duration from the time of transition from unemployment to a job to the time of reentry into unemployment. The transition (or non-censoring) indicator is therefore 1 if a reentry to unemployment is observed up to 1.5 years (540 days) after unemployment exit (exogenous censoring). As in model (1), the baseline hazard rate $\lambda_p(t_p)$ adopts the form of a piecewise-constant function[40]. D_p is a dummy variable indicating membership to the treatment group. This means that one constant treatment effect[41] is estimated for the post-unemployment period.

It is important to note that equation (6) above is estimated on the non-random subsample of individuals who found a job after unemployment. As a consequence, this further endogenous selection process can potentially bias the estimation results of (6). Therefore, I apply as well a model that simultaneously estimates (1) and (6), taking the potential correlation of v_u and v_p into account. This will be discussed in the next section.

5.1.2 What about the Quality of Found Jobs?

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

First, I check whether the inclusion of the available observables into a (OLS) regression changes the result of no salary difference. This is not the case, the comparison of conditional means results as well in *no significant difference of monthly salaries realised after unemployment exit*[42]. Checking for unobservables is possible in the context of a duration model, thus for the

[40] Following the shape of the descriptive hazard, I estimate four intervals with splits at 210/390/480 days. Note, moreover, that I define a recurrence event as being at least 20 days out of initial unemployment before reentry. Therefore, the first interval starts at 20 days.

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

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

recurrence dimension of job quality. This inclusion of unobserved heterogeneity has been done in the form of the 2-process model described in section 4.1, which simultaneously estimates the unemployment exit-to-job process and the recurrence to unemployment process. The results, discussed in section 4.2 and Table 11, showed that including unobserved heterogeneity does not increase the explanatory value of the model. Due to this insignificant importance of heterogeneity, the best choice is to use the specification without unobserved heterogeneity for the final analysis. For the sake of completeness, however, both versions of the model are reported in Table 11.

[Table 11 about here]

The post-unemployment survivor analysis in Figure 5 and the means comparison of recurrence rates (see section 3.2) suggested a result of better employment stability in the treatment group over 1.5 years beyond unemployment. The means comparison did, however, not get significant. How does this picture change when controlling for observables and explicitly modeling post-unemployment duration? Table 11 (estimation 1) reveals that this results in a *significantly positive treatment effect on employment stability* over 1.5 years after unemployment[43]. This result will be further quantified (in terms of avoided unemployment) below. A glance on estimation 2 of Table 11, which features unobserved heterogeneity but has less explanatory value, shows that the result on employment stability does qualitatively not change; the treatment effect gets slightly stronger.

5.2 Cost-Benefit: Does the Program Pay Off?

Using the estimation results in Table 11 it is possible to quantify the positive impact of the new policy on employment stability in terms of avoided future unemployment duration. This amounts to calculating the expected values of the post-unemployment duration t_p for the two counterfactuals. The difference between the two yields the average treatment effect on the treated (ATET) in terms of t_p, i.e. the not realised future unemployment (in days) due to the treatment (within 1.5 years after the original unemployment spell). Using the estimation model developed in section 5.1 and estimated in Table 11, estimation 1, I simulate the following equation which describes the density of post-unemployment employment durations:

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

whereby $D \in \{T, C\}$ indicates the treatment status, i.e. the two counterfactuals. θ_p represents the hazard derived in equation (6) in section 5.1 (whereby x comprises as well the inflow month- and caseworker dummies), S_p is the corresponding survivor function. Based on this density, the expected value of the employment duration can be calculated as

$$E(t_p|x, v, D_p) = \int_{20}^{\eta} t_p\, f_p^D(t_p|x, v_p) dt_p + \left[1 - \int_{20}^{\eta} f_p^D(t_p|x, v_p) dt_p\right] \cdot \eta \tag{7}$$

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

This equation takes into account that the employment durations are exogenously censored on March 31th 2010 (last data availability) or after 540 days (1.5 years)[44], this is described by the parameter η.

This simulation is run twice, for the two counterfactuals of being treated or not. It yields the $ATET = E(t_p|x, v, T) - E(t_p|x, v, C)$. The result of this calculation is that, on average, *treated individuals avoid future unemployment of 23.16 days*. These not incurred unemployment days represent direct savings for the unemployment insurance (UI) accounts. Based on this quantification of the direct benefit of UI, I perform a cost-benefit accounting calculation in order to assess whether the investment in the new program pays off for the UI or not.

[Table 12 about here]

Table 12 provides the details on this cost-benefit analysis for the UI accounts. Based on the data available and additional cost information by the PES administration, I can perform a detailed calculation of the additional cost of the new policy (as compared to the status quo[45]). The cost-benefit analysis yields a clearly positive result: *The avoided future unemployment pays the additional cost of the new program more than fully, specifically it covers 1.73 times the additional cost.*

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

5.3 Potential Policy Optimizations

As a final step, the analysis aims at identifying possibilities of *potential policy improvements* by further targeting the new treatment plan to the *subpopulations where the interventions showed the best results*. This amounts to extending the treatment component $\sum_i \delta_i D_i(t_u)$ to allow for treatment effects for different subpopulations. The nonparametric analysis in section 3 showed that there are mainly two dimensions which happen to have a remarkable impact on the size of treatment effects – and are therefore of special interest for targeted policy design. The first dimension is the *timing of the coaching intervention*. As discussed in section 3.2, the impact on (early) outcomes changes considerably depending on when the individuals are supposed to enter the coaching program. In order to specifically identify and quantify the change of the anticipatory impact of the coaching announcement on the exit-to-job hazard, I allow the respective treatment effect to differ by time to entry into the program: The anticipation effect

[44] For more details on the empirical issues with respect to θ_p (censoring, baseline hazard splits, 20 days threshold), see section 5.1 and footnote 40.

[45] The status quo for the control group during the first four months of unemployment (policy implementation span) is monthly counseling and a short, standard job search assistance workshop. For details see section 2.3.

component $\delta_a D_a$ is therefore complemented by two incremental effects (interactions with D_a) which measure *early coaching intervention*, defined as time to coaching being smaller than 35 days (median: 19 days), and *late intervention*, which collects cases with time to coaching of 70+ days (median: 102 days).

[Table 10 about here]

The answer to the latter question has two aspects. With respect to avoiding the duration-prolonging attraction effect[46], early intervention is clearly successful. Table 10 reports that individuals who entered in median 30 days earlier into coaching show a hazard rate which is significantly higher than the negative anticipation effect of the average treatment group (in median 50 days to coaching). Thus, the *negative anticipation effect is significantly undone* by intervening earlier with coaching. Intervening later (subgroup 70+ days, median 102 days to coaching), in the opposite, does barely change the size of the attraction effect. This result is shown graphically as well in the hazard rate plots by anticipation groups in Figure 4a and 4b. Note that these hazard calculations are censored at the, real or potential, coaching entry – they thus only represent anticipation behavior. The figures reveal that beyond 20 days the exit rates increase in the control group, whereas they do not in the median and long anticipation duration subgroups of the TG. This generates the negative hazard differences as shown in Figure 4b. Finally, I perform a sensitivity analysis on potential endogeneity of prolonged anticipation durations[47]. It shows no impact of potential postponement behavior, thus the above-used anticipation variation can indeed be considered as being exogenous.

The second aspect of the early intervention question is whether it reduces total unemployment duration. This has been discussed in section 3.2. The nonparametric results there show that a move from a median (long) anticipation duration policy to a short anticipation policy yield a reduction of unemployment by 9.2 (48.3) days, which is not significant. The detailed survivor analysis in Figure 4a reveals that earlier exit from coaching could obviously not be translated into earlier job finding (see section 3.2 for more details). Taking the two aspects of the early intervention question together the policy conclusion could thus be the following: *The earlier intervention strategy works in the sense that it eliminates the duration-prolonging aspects of the attraction effect. But in order to significantly reduce unemployment duration, additional policy measures would be necessary which are able to translate the earlier coaching exit into earlier job finding.* An option would be to even more intensify guidance around the end of coaching, e.g. through (more) intensified counseling and probably monitoring.

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

[47] As discussed in section 3.1, the exogeneity of the coaching timing mechanism could be compromised by: duration to availability (i.e. being in cancellation period), a temporary subsidized job, calling in sick. By comparing real and potential coaching entry time (see footnote 6 for more on the latter), I identify 20 cases where they differ more than just a couple of days (natural break at ≤ 11 days; considered cases have delays of ≥ 45 days). Excluding them from the hazard calculation does barely change the mentioned hazard figures. Most probably, the delays are mainly due to administrative reasons (overbooking of the program, holidays from UI obligations).

The second policy experiment focuses on further *age-targeting* of the new policy. Do individuals below and above age 55 react in the same way to the interventions? They do not, as the nonparametric analysis in section 3.2 already showed. Whereas the new policy causes a zero effect on the unemployment duration of individuals aged 45-55, the median unemployment duration of people aged 55+ significantly increases. It is therefore worth to use the timing-of-events model for further analysis in this respect: We interact each of the subsequent treatment effects in $\sum_i \delta_i D_i(t_u)$ by an age dummy variable which indicates individuals aged 55+. This allows to estimate an increment to each period-specific treatment effect that captures differences in exit-to-job behavior of individuals aged 55+. The cumulation of the respective treatment effect and its 55+-increment (which is reported in the column 'transformations' of the respective estimation tables) yields the treatment effects specific for the older participants.

The results of the age-specific treatment effects model are reported in Table 10. It reveals that for the individuals of age 55+ (i.e., adding the increments) the attraction effect is reduced to being insignificant, the during coaching lock-in effect vanishes, and the post-coaching effects never get positive. This behavioral pattern is consistent with the people of age 55+ believing less in the success of (this type of) coaching. This belief seemingly reflects in the anticipation and the missing lock-in behavior (less time investment in coaching); after coaching, the non-success belief seems to be realised. The age-specific analysis shows, on the other hand, that for the individuals aged 45-55 the positive effect of the policy beyond 180 days post coaching is higher and gets significant (at 15% treatment-specific, at 10% ITT, see Table B4 in Appendix). Therefore, the new policy is more suitable for the age group 45-55 than beyond.

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

6 Conclusion

This paper aims at shedding some light on the question on *how* labor market policy which supports job search and human capital building actually works and affects the job seeker. To address this behavioral "blackbox", this study exploits a field experiment jointly with a novel combination of register data and repeated surveys which allows to track labor market outcomes *and* behavioral changes. This allows thus not only the analysis of causal treatment effects on labor market outcomes but also on search behavior and subjective attitudes and beliefs – to get an idea on the behavioral mechanisms that precede the labor market outcomes.

The evaluated policy, which has been implemented in a pilot project in Northern Switzerland, focuses on the coaching of older job seekers. The randomized experiment features two intense supportive treatments: high-frequency counseling (every second week, double intensity than normal) and an intense coaching program of 54 days in small groups. The latter focuses on job search efficiency, self-assessment (realistic perspectives), self-marketing and revision of job search strategy and scope. The new policy intervenes early in the unemployment spell: High-frequency counseling starts right from the beginning on (and lasts four months), coaching on average after 50 days. The timing schedule of the treatment plan was fixed ex ante, which allows identification of detailed treatment effects.

This new supportive labor market policy causes significantly positive treatment effects in the longer run and avoids too strong lock-in effects in the shorter run. The results of the field experiment can be summarized in five main points. *First, the treatment intervention does not prolong unemployment duration.* Unlike the standard result found in evaluations of supportive labor market policy (training etc.), the lock-in effect (job seekers search less during the program due to high workload) is not so dominant here. The decomposition of the treatment effect shows countervailing tendencies: I find an *"attraction effect"* before coaching and the lock-in effect during coaching which reduce unemployment exits in early stages. The attraction effect is a phenomenon which has been rarely reported in the literature so far: It is the opposite of the more typical threat effect. After the coaching, the positive effects of the treatment more and more prevail and affect the job finding propensity. Such that, in net, the unemployment duration is not prolonged by the policy.

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

Based on the presented results, in particular the following policy elements may be put forward as recommendations for targeted policy design: First, for supportive policy programs, the principle 'early and short but intense' seems beneficial. Given the result that it takes some time until such a coaching & counseling measure generates job finding success, early intervention makes sense. If the program is, in addition, attractive, early intervention helps reducing negative pre-program effects. The intense design of coaching (or training) helps restricting the lock-in effect. Second, it can pay off to invest in supportive policy measures for job seekers if these measures are strictly targeted (in age, content) and implemented. The age-specific analysis of the policy effects suggests that targeting the content to age-specific issues is of key importance.

The positive impact of this coaching & counseling strategy on job finding raises the question about which reactions of *search behavior* have been driving the outcome. The unique data of this paper offer the opportunity to analyze this question in detail – using the evidence on search behavior variables collected in repeated surveys. The results of this field experiment can be summarized in four core insights on the behavioral reactions of older unemployed job seekers on coaching: First, the job seekers did *not search more, but more effectively*, due to the intense support by coaching. The number of applications sent out remained constant or even slightly decreased – but the job finding rate increased after coaching, compared to the control group. This implies an increase in search efficiency, which is supported by further observations: The treated individuals adapted their search strategy (extension of scope of search). Moreover, they focused the use of search channels – by decreasing channel variety and frequency of use, and by putting relatively some more weight on informal channels.

Second, the treated older job seekers *reduced their reservation wages*. Whereas the control group members kept their reservation wages about at the level of their pre-unemployment salary, the coached individuals lowered their reservation wage level over time (of the spell). This is consistent with a model like the one of Burdett/Vishwanath (1988) which suggests that job seekers may learn over time to get a more specific picture of the wage and job offer distribution relevant (reachable) for them. Coaching presumably has supported this learning process about "realistic" wage and job offers (given that older unemployed job seekers often incur the risk of wage loss). Thus, this type of learning could be labeled by "*disillusion*".

Third, the analysis of *subjective outcomes like motivation, reliability and self-confidence and biased beliefs* about chances for job interviews and expected wages suggests that the coaching intervention had a positive short-run impact on these factors. From a policy point of view, reactions in these intermediate outcomes could be seen as early indicators for upcoming improvements (or decreases) in job search success. Such indicators could, thus, be a useful policy tool for early detection of long-term unemployment risk. The experiment demonstrates as well that targeted supportive LMP is in principle able to affect the bias in the beliefs of job seekers about their chances of success, although in a moderate amount. Finally, a more persistent positive treatment effect of this coaching intervention has been found on *subjective well-being*.

A concluding important insight of this behavioral analysis is that *targeted supportive LMP interventions*, like this coaching program, *are able to affect the (search) behavior of job seekers*. They may indeed adapt their strategies, as a consequence of coaching. This may call for the design of *specific* labor market policies that clearly target some goals or issues of behavior to be affected, in order to optimize success of job search. Thus, the results of this field experiment call for more research in behavioral labor market policy design, in order to uncover behavioral mechanisms which can be targeted by precise and efficient policy interventions.

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Figures

Fig. 1: Unemployment exit hazard

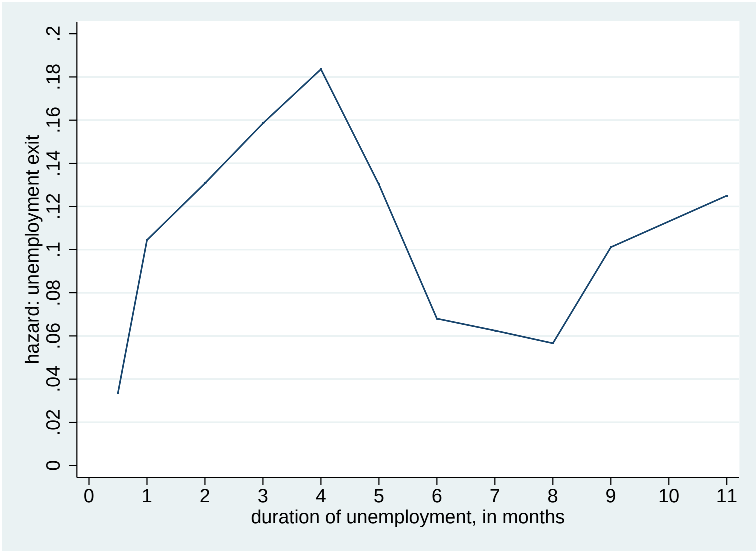
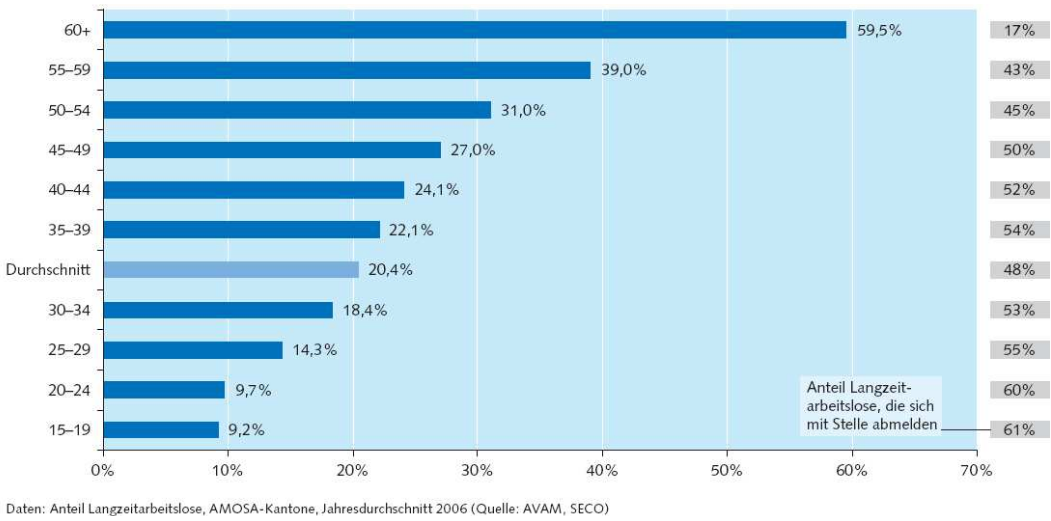


Fig. 2: Incidence of long-term unemployment by age groups



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 figure to the right reports the age-related proportions of the long-term unemployed who deregister from unemployment insurance due to having found a job.

Source: AMOSA 2007.

Fig. 3: Total treatment effect on duration of unemployment and on duration to job finding, survivors treatment vs control group

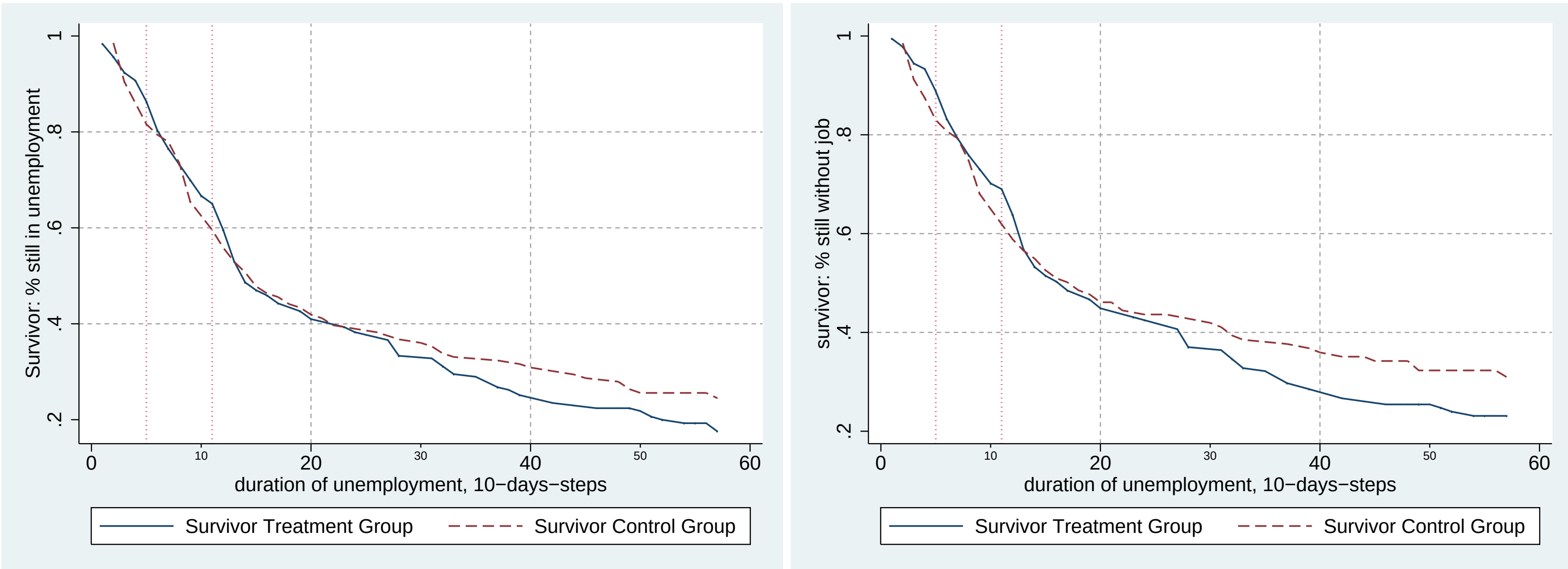
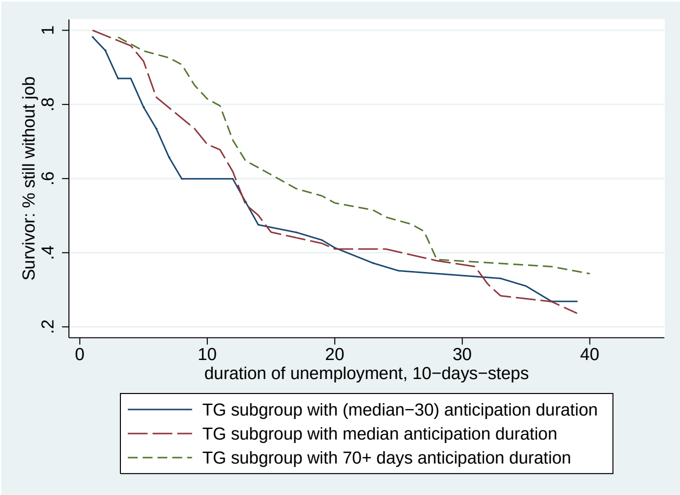
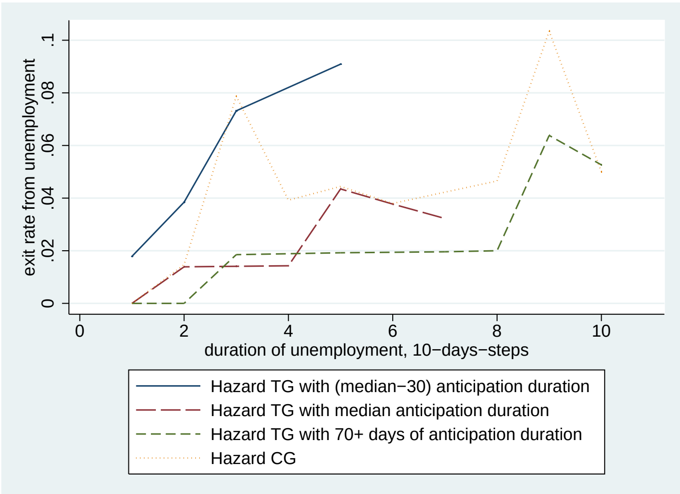


Fig. 4: Anticipation effect: the impact of anticipation (time to program) duration

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



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



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

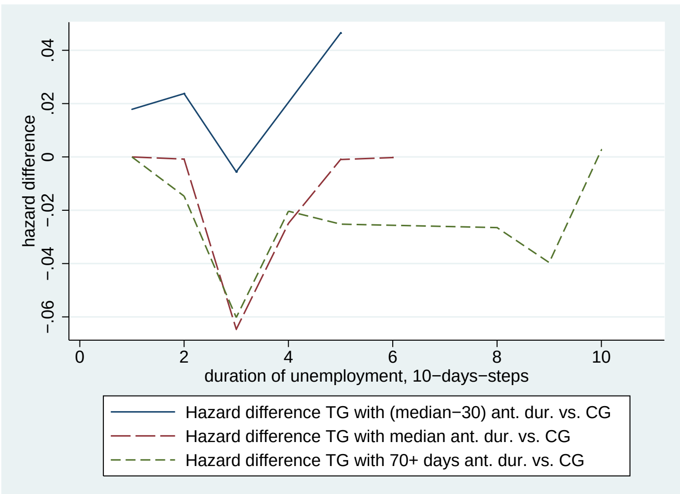


Fig. 5: Post-unemployment job stability: Survivor of the reentry rate into unemployment

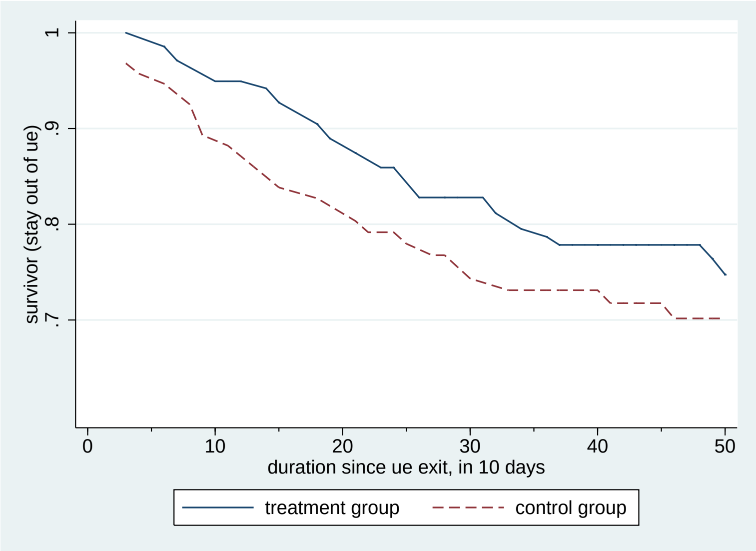


Fig. 6: Probability of search strategy change: extension

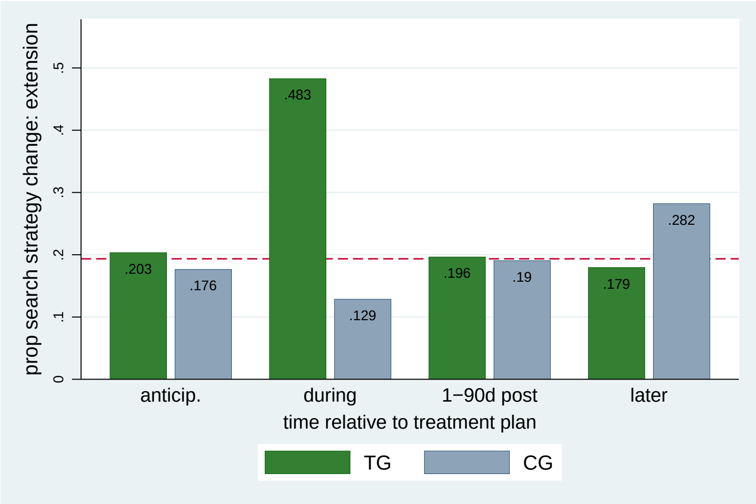


Fig. 7: Search effort: number of applications sent out (in 4 weeks)

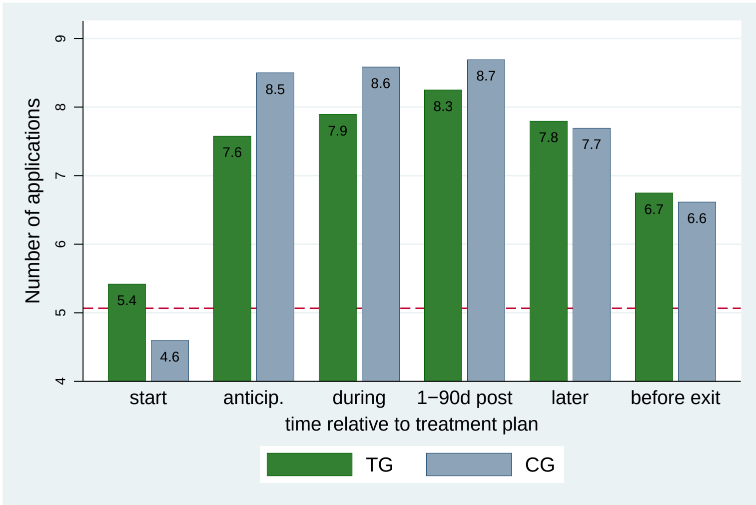


Fig. 8: Number of search channels used

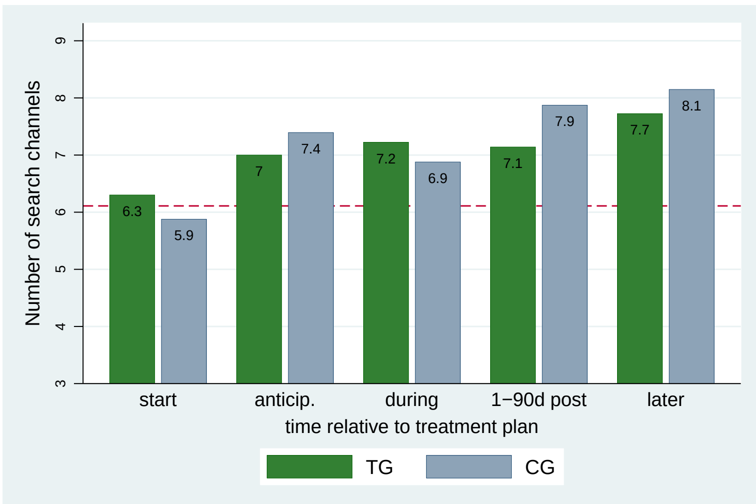


Fig. 9: Frequency of search channels use: internet

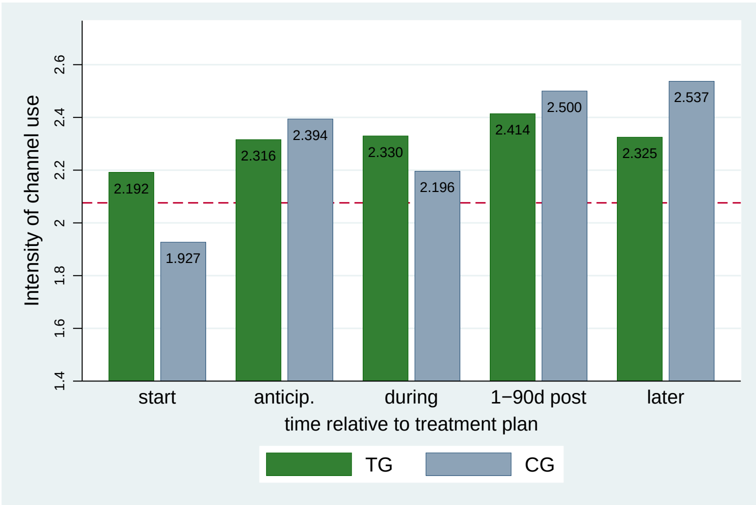


Fig. 10: Reservation wages by periods of the treatment plan

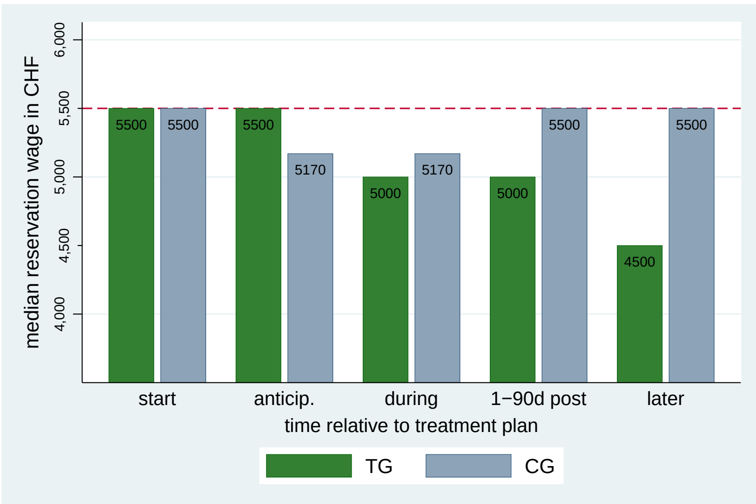
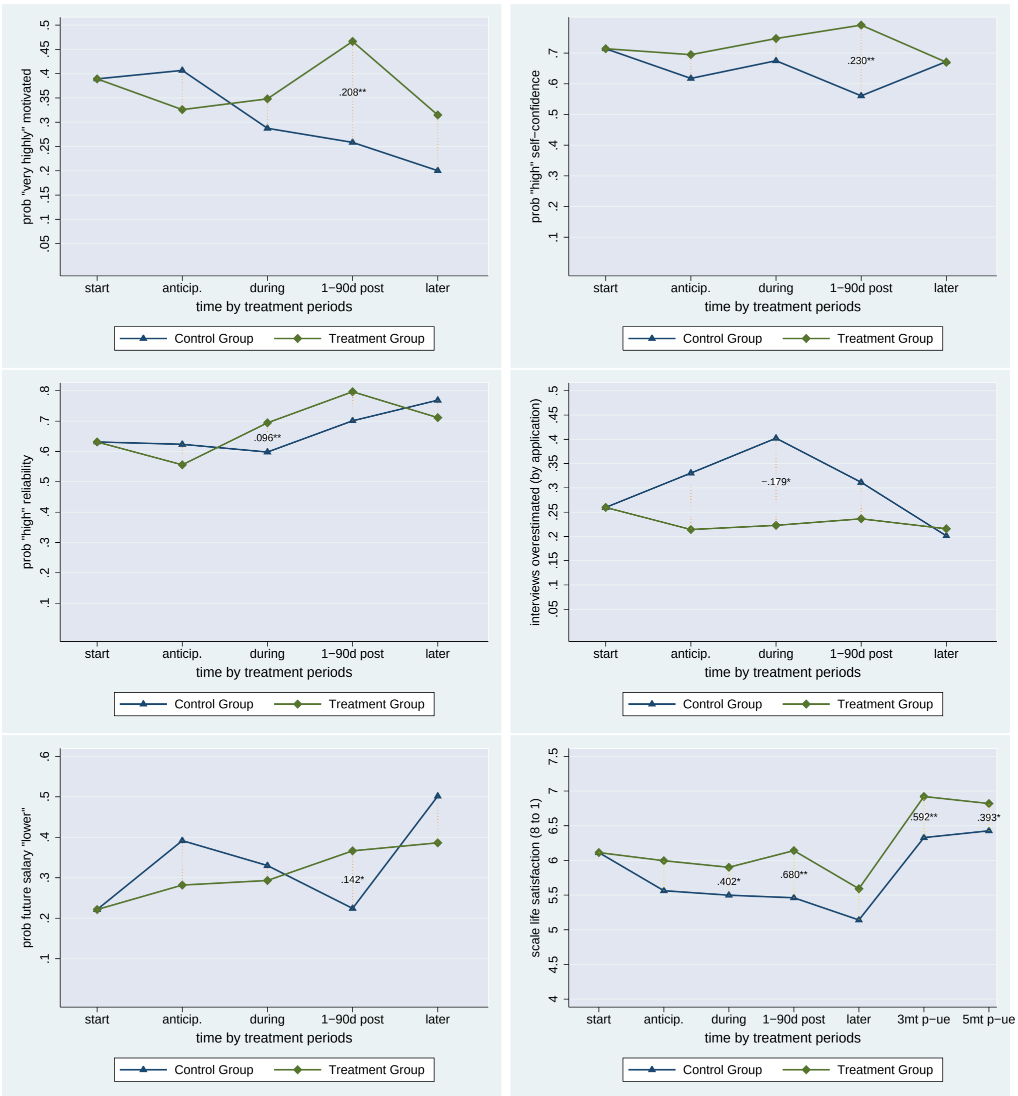


Fig. 11: Subjective Outcomes by periods of the treatment plan: Diff-in-Diff treatment effects (only significant effects reported)



Tables

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	
... in %	56.9%	43.1%	

Notes: Frequency percentages for different observable characteristics by treatment and control group are reported. t-values are based on unpaired t-tests with equal variances.

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

Tab. 2: Non-parametric comparison of main outcomes: unemployment duration (means, medians), proportion in longterm unemployment, job finders, gross salaries, recurrence to unemployment

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

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

Tab. 3: The total/net effect of the new policy on duration to job finding. (PH duration model)

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

Tab. 4: Effects of the treatment plan on the exit to job rate. (PH duration model)

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

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

Tab. 5: Treatment effect on search strategy extension: OLS regressions

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

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

Tab. 6: Treatment effect on search effort (number of applications): OLS regressions, difference-in-differences

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

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

Tab. 7: Treatment effect on number of used search channels: OLS regressions, difference-in-differences

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

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

Tab. 8: Treatment effect on the frequency of use of different search channels: OLS regressions, difference-in-differences

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

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

Tab. 9: Treatment effect on reservation wages: median regressions, difference-in-differences

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

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

Tab. 10: Change of the anticipation effect as a function of time to coaching intervention. And age-specific treatment effects: age 45-54 vs age 55+. (PH duration models)

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

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

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

Tab. 11: Employment stability: Effect of new policy on reentry rate into unemployment (20–540 days after UE); sensitivity analysis: model with unobserved heterogeneity

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

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

Tab. 12: Analysis of costs vs benefits of new policy for the UI accounts: avoided future unemployment vs. additional program cost; in CHF per job seeker in treatment group (TG)

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

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

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

Appendices

A Estimation of Unobserved Heterogeneity Mass Points by Grid Search

In this section of the Appendix I describe the systematic procedure I applied to search for unobserved heterogeneity in the context of the models developed in the sections 4.1.1, 5.1 and 4.2. Such a procedure amounts to searching for additional mass points in order to establish a discrete mixture distribution for v_u and v_p (described in section 4.2). Thus, the benchmark and starting point is the model with 1 mass point, i.e. with no unobserved heterogeneity in the baseline hazard profile. In the following, I demonstrate step-by-step the iterative procedure – an interplay between grid search and estimation – I use to establish a second mass point and then to search for further ones.

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

2. Start with an initial set of 2 mass points (per process), i.e. the aim is to estimate their probabilities and locations (=intercept of the transition rate/baseline hazard): p_1 and λ^a as well as p_2 and λ^b[48].

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

   (a) Loop over the sign (i.e. 2 runs) of the difference between the two locations. Note that this loop is used to set the starting values for the location estimation: I.e., set $\lambda^b = \lambda^a \pm 3$, whereby λ^a is the location (intercept) of the baseline hazard of the model without unobserved heterogeneity[49].

   (b) Loop over the i increments (here of 0.01) of the probabilities which are to be grid-searched: $p_1 = 1 - i \cdot 0.01$, whereby $p_2 = 1 - p_1$. Choice criterion: Take the set (p_1^*, p_2^*) with the corresponding estimated $(\lambda^{a*}, \lambda^{b*})$ which yields the highest likelihood[50].

4. *Estimation of the probabilities:* Fix the location of the mass points at λ^{a*} and λ^{b*}. Use p_1^* and p_2^* to calculate the starting values for the parameters a_1 and a_2 (the probabilities are designed in a logistic form, see section 4.2). Estimate these parameters (i.e. the probabilities) in the model.

5. *Fully free estimation:* Un-fix the location of the mass points, and use them and the estimated probabilities as starting values for the fully free estimation. If this estimation

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

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

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

yields a higher likelihood, continue with the next step; otherwise stop and choose the model without unobserved heterogeneity as the best one.

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

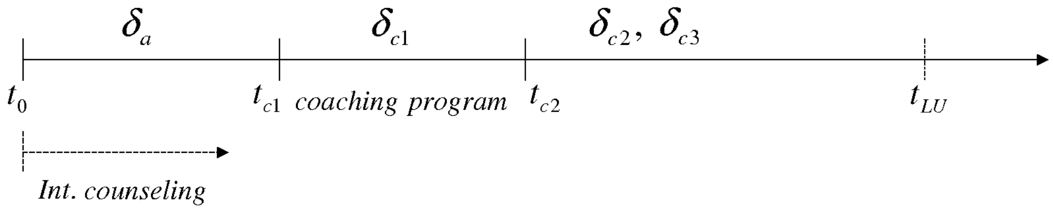
7. *Stopping rule:* After having performed step 6, check whether the chosen model with 3 mass points yields a higher likelihood. If no, stop and take the previous model as the best. If yes, continue by adding a fourth mass point... and so on.

B Further Discussion & Interpretation

B.1 Potential Effects by Treatment Period

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

Following the strict timing of the treatment plan as described in section 2.1, the treatment effects can be shaped as follows:



The first treatment period, from t_0 to t_{c1}, is the *anticipation* period. Two things may happen in this period. First, the anticipation of the upcoming coaching (whereby t_{c1} is known ex-ante) may result in an "attraction effect" or a "threat effect". If individuals expect support and positive impact of the coaching, the former effect will materialise – δ_a will be negative; if individuals do not have positive expectations and consider the coaching as a disturbing factor in their job search, the latter effect will prevail and δ_a becomes positive. Second, the intensified counseling could result in a quick job finding success, thus δ_a would increase. But note that the anticipation period is rather short (it takes in median 50 days until (potential) coaching entry, see section 2.1), such that the full effect of double-frequency counseling is normally not yet developed. Not as well that a quick job finding success in general, i.e. not driven by the doubling of counseling, will not result in a treatment effect. Due to randomisation such a treatment-unrelated event can happen with the same probability in the control group. In other words, such events of treatment-unrelated dynamic selection do not affect the balancing of the two groups.

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

The third treatment period, from t_{c2} on until unemployment exit, captures the *post-coaching* effects. These are the cumulative outcome of coaching and the parallely ongoing high-frequency counseling (in the first four months of unemployment). I split this effect up into a short-run effect δ_{c2}, which operates in the first 180 days after coaching, and in the mid-run effect δ_{c3} thereafter. The aim of the policy is clearly that this effect should become positive. Note, though, that if coaching results in a substantial job search strategy change (which is one of the core assessment elements in the coaching, see section 2.1), the potential effects could be twofold: In the short run, reorientation of search strategy may lead to a further lock-in situation; the job seeker first needs to learn and to put the effort in the development of the new strategy instead of fully searching for the same kind of jobs. In the longer run, the change of job search strategy could result in a higher success rate in job finding.

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

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

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

B.2 Interpretation: Linking Dynamic Treatment Effects and Behavioral Results

The results found and reported in the main text shall be linked here to the discussion on possible behavioral explanations, as done in section B.1, and the main labor market outcomes, as reported in sections 3.2 and 4.4. This aims at providing further insights on the behavioral mechanisms operating within the setup of this supportive labor market treatment.

A first general observation is that coaching & counseling *indeed managed to manipulate the behavior of individuals, most often in the direction intended by the content.* One striking result is, as reported above, the massive increase in search strategy extensions as a reaction to coaching. A second result supporting this observation is the remarkable increase of the use of spontaneous applications by telephone during coaching; this channel was explicitly promoted in the coaching. However, these two changes were not very sustainable in the post coaching period. Two further elements of the content of the measures (see section 2.1) that, arguably, have been taken up are the promotion of more efficient search and the setting of more realistic demands towards potential future jobs. This two elements will be further discussed below.

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

The findings in section 4.4.2 that the *coached & counseled individuals did not search more. Whereas they still found in total more jobs.* This suggests two insights: First, the *relation between effort and job finding is not monotonically increasing*; the marginal benefits of additional effort may get too low. The learning process induced by coaching & counseling may have fostered this insight. A second conclusion may be that it can be more successful (in terms of job finding) to increase *search efficiency* (or productivity) than pure effort quantity. This may be especially the case for older job seekers whose job finding problems are, arguably, less caused by moral hazard behavior than by insufficient or outdated search skills. It seems that the focus of the coaching on search efficiency has had its impact on the outcomes.

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

Considering the *choice of search channel types* the results revealed that it was *predominantly the formal channels where frequency of use reduced after coaching.* This can be well understood in the context of the above-discussed interpretation: If individuals indeed search in a more efficient and directed way it is natural that mostly the frequency of the formal channels like newspapers and internet reduce – since they are used most often and in the broadest way; so efficiency gains are highest there. This argument of search efficiency – or search productivity – gains is supported by existing literature (e.g. Holzer 1988, Weber and Mahringer 2008). A final insight with respect to search channel choice that may be deduced from the found results is that *the use of informal channels only increases if the respective channels are explicitly promoted by the labor market policy.* In the case of the coaching here, the use of spontaneous applications by telephone significantly increased, whereas the use of personal networks did not. This is consistent with the fact that the former was explicitly trained and promoted by the coach[51], whereas the latter was not.

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

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

C Further Robustness Analyses

C.1 Patterns of Coaching Program Participation

Figure 13 shows that there is considerable variation in the duration until entry into the coaching program. Median duration from start of unemployment until coaching entry is 50 days. Duration to coaching entry varies from 0 (coaching start by coincidence at the day of unemployment entry) to 290 days. It is important to mention that this variation is predominantly exogenous – due to the fact that all the dates of the coaching program (see footnote 6 for details) were *fixed in advance* with the coaching supplier. The exogeneity of the mechanism could be compromised by the following factors: duration to availability, a temporary subsidized job, calling in sick. I perform some sensitivity analyses on whether these factors affect the labor market

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

outcome when discussing the anticipation effect in section **??**. I do not find such evidence. The variation in coaching entry timing offers therefore the opportunity to estimate the elasticity of the anticipation effect with respect to anticipation duration, see section **??**.

[Figure 13 about here]

Next, in order to get to know more about which characteristics codetermine *early dynamic selection* and therefore *coaching entry*, I perform a respective probit regression. The analysis on coaching entry propensity, see Table B3 in the Appendix, reveals the following pattern of dynamic selection in the pre-program stage of the unemployment spell: The probability to enter coaching (in the treatment group) is higher for individuals who are of older age, unmarried, male, relatively less skilled (”only” one foreign language and not two, low-skill- and unskilled occupations). Inversely, one can state that early exits are more prominent among younger (age 45-49), married, female people speaking 2+ languages. Individuals with a longer duration to availability show a lower probability to enter coaching – this can also be explained by dynamic selection: it seems that those people who registered at the UI already during cancellation period had a higher propensity to quickly find a job. Moreover, non-German-speaking individuals had a lower probability to enter coaching; the two possible explanations are early exit from unemployment or insufficient knowledge of the German language to follow the coaching[52]. The significance of the inflow dummy for Nov/Dec 2008 points to a small overbooking of the coaching programs starting at the end of 2008. Note that since the booking was made in order of inflow, potential non-compliance behavior cannot influence the booking process.

The described pattern of coaching entry propensities that arises above is typical for early exit behavior: The relatively younger and better skilled exit more quickly from unemployment such that more of them are not unemployed any more at the time of planned program entry (either they already exited from UI or they found a job starting in the near future such that coaching participation was not of use any more). Thus, this points to common dynamic selection behavior over the course of the unemployment spell. As far as this dynamic selection is independent from the anticipation behavior with respect to the upcoming coaching program and from the early impacts of intensified counseling, it does not harm the balancing between treatment and control group. But, however, the part of dynamic selection that gets reinforced by coaching anticipation can potentially harm the comparability of the two groups. This is a problem if the imbalance is correlated with the labor market outcomes. In such a case of un-balanced impact of dynamic selection controls of observables and unobservables need to be introduced by use of a respective econometric model. This is done in section 4.1 – the analysis in section 4.2 shows, though, that the importance of unobservables is insignificant over the course of the unemployment spell, given the control for the observables characteristics.

A final dimension of the selection process during unemployment is *intentional non-compliance*, i.e. individuals who intentionally ignored the (compulsory and exogenous) treatment

[52] In this case the insufficient language proficiency was, seemingly, not yet visible at t_0, otherwise they would have been filtered out at the beginning, see section 2.3.

assignment. Intentional non-compliance behavior can, potentially, be correlated with unobservables that influence as well the labor market outcomes; this would generate another reason for introducing unobservables into an econometric model. I use a filtering algorithm that features several steps to analyse this question. First, I restrict the focus to people who are in the treatment group but did not participate in the coaching program. This is the case for 86 of the 186 individuals. Second, I identified the cases of early exits in this subgroup[53]: The majority of this subgroup (53.5%) did not participate by default since they found a job early in unemployment, i.e. before potential coaching entry. This has obviously nothing to do with non-compliance and corresponds to the above-described "normal" dynamic selection process. After this filter step, 40 individuals remained to be further analysed. The caseworkers of these individuals were surveyed about the reason for the non-participation in coaching. The vast majority of these cases turned out to have valid (and legally accepted) reasons for non-participation: 35% found a temporary subsidised job shortly after unemployment start, so that they became unavailable for coaching; 22.5% had an offer for a job starting in the near future (within the next 2-3 months normally); 27.5% had other valid reasons which are unrelated to non-compliance (like caseworker error or the fact that the job seeker recently followed another coaching). The remaining cases – 4 to 6 individuals – can be considered as having shown intentional non-compliance. 2 cases reported health problems, 4 cases showed 'high unwillingness to participate' in the coaching. Thus, the *non-compliance rate amounts only to 3.2%* – which is negligible.

[53] 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.

D Additional Tables

Tab. B1: Repeated surveys: Filled questionnaires and response rate by time of survey

Job seeker surveys

	Entry	*M2*	*M3*	*M4*	*M9*	*M13*	*Exit*
Registered job seekers	327	258	210	182	112	87	273
Questionnaires	298	198	137	106	42	31	154
Response rate	91.1%	76.7%	65.2%	58.2%	37.5%	35.6%	56.4%

Caseworker surveys

	Entry	*M2*	*M3*	*M4*	*M9*	*M13*	*Exit*
Registered job seekers	327	258	210	182	112	87	273
Questionnaires	302	213	141	114	48	42	222
Response rate	92.4%	82.6%	67.1%	62.6%	42.9%	48.3%	81.3%

Notes: See section **??** for a description of the survey timing and an exact definition of the *Entry, M2, ... Exit* dates.

Source: LZAR database.

Tab. B2: Repeated surveys: Balancing of observables, by treatment (TG) and control group (CG) and periods of the treatment plan

	Start		Anticipation		During Coaching		1-90d post Coaching		Later		Both Surveys @ t_2	
Job seeker surveys	TG	CG	TG	CG	TG	CG	TG	CG	TG	CG	TG	CG
Gender: woman	44.05%	43.08%	45.61%	42.42%	46.81%	44.59%	43.75%	36.17%	47.50%	37.04%	44.05%	46.15%
Married (incl. Separated)	55.95%	47.69%	66.67%	51.52%	57.45%	48.65%	64.06%	51.06%	50.00%	37.04%	57.14%	50.77%
Age	52.45	52.38	53.23	52.85	52.51	52.64	52.98	53.19	54.65	53.48	52.71	52.77
Nationality: CH	85.12%	86.15%	87.72%	78.79%	80.85%**	93.24%**	84.38%	85.11%	87.50%	88.89%	80.95%**	93.85%**
Qualification: (semi-)skilled	97.02%	96.15%	98.25%	100.00%	96.81%	95.95%	96.88%	95.74%	100%*	92.59%*	97.62%	96.92%
Employability: 4	20.83%	21.54%	15.79%	15.15%	21.28%	17.57%	18.75%	21.28%	22.50%	14.81%	22.62%	20.00%
At least 1 foreign language	58.33%	55.38%	66.67%	63.64%	57.45%	59.46%	60.94%	59.57%	55.00%	66.67%	55.95%	55.38%
Job < 100%	17.26%	17.69%	21.05%	18.18%	15.96%	21.62%	15.63%	19.15%	22.50%	11.11%	15.48%	23.08%
PES 2	14.29%	10.77%	10.53%	6.06%	14.89%	12.16%	20.31%**	6.38%**	12.50%	7.41%	15.48%	10.77%
Observations	168	130	57	33	94	74	64	47	40	27	84	65
... in %	56.38%	43.62%	63.33%	36.67%	55.95%	44.05%	57.66%	42.34%	59.70%	40.30%	56.38%	43.62%
Caseworker surveys												
Gender: woman	43.60%	43.85%	45.00%	44.12%	45.45%	45.68%	41.38%	34.88%	43.90%	35.00%		
Married (incl. Separated)	56.40%*	46.15%*	63.33%	55.88%	55.56%	49.38%	63.79%	48.84%	48.78%	42.50%		
Age	52.53	52.28	52.97	52.71	52.58	52.53	53.12	53.58	54.63	53.98		
Nationality: CH	84.88%	85.38%	91.67%*	79.41%*	81.82%**	93.83%**	82.76%	86.05%	85.37%	85.00%		
Qualification: (semi-)skilled	97.09%	96.15%	96.67%	97.06%	97.98%	96.30%	96.55%	95.35%	100%	97.50%		
Employability: 4	21.51%	22.31%	18.33%	17.65%	23.23%	17.28%	17.24%	27.91%	21.95%	17.50%		
At least 1 foreign language	58.72%	55.38%	60.00%	61.76%	54.55%	60.49%	58.62%	53.49%	51.22%	57.50%		
Job < 100%	17.44%	18.46%	23.33%	14.71%	15.15%	20.99%	17.24%	18.60%	21.95%	15.00%		
PES 2	13.95%	10.77%	8.33%	5.88%	15.15%	11.11%	22.41%**	4.65%**	9.76%	2.50%		
Observations	172	130	60	34	99	81	58	43	41	40		
... in %	56.95%	43.05%	63.83%	36.17%	55.00%	45.00%	57.43%	42.57%	50.62%	49.38%		

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

Tab. B3: Determinants of coaching entry. Probit regression

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

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

E Additional Figures

Fig. 12: The age structure of the sample

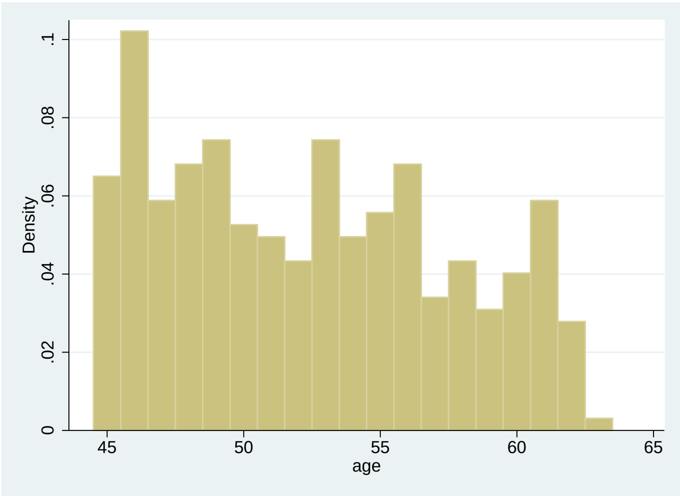
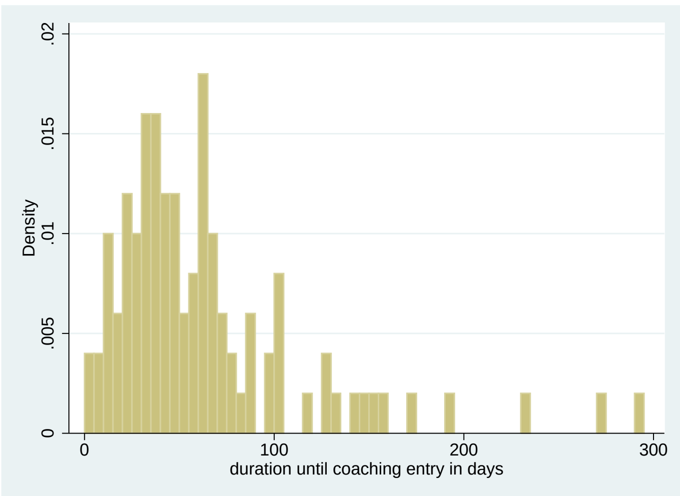


Fig. 13: Variation in coaching entry timing



Tab. B4: Effects of the treatment plan on the exit to job rate: by treatment periods; anticipation effect by time to coaching; age-specific treatment effects. ITT (intention-to-treat) models

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

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

Tab. B5: The total/net effect of the new policy on unemployment duration. (PH duration model)

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

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

Tab. B6: Effects of the treatment plan on unemployment exit rate: by treatment periods; anticipation effect by time to coaching; age-specific treatment effects. (PH duration models)

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

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