The Effects of Binding and Non-Binding Job Search Requirements

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Abstract

Job search requirements constrain the effort choice of unemployment insurance recipients by enforcing a minimum number of monthly applications. Based on novel register data, this paper is the first to assess how individual search effort, job finding and job stability react to this constraint. Standard job search theory predicts that requirements affect each job seeker relative to her unconstrained effort choice. We therefore define the *incremental effort which is necessary to comply with the constraint* as our behavioral treatment intensity of interest. A proxy of this intensity at the beginning of each spell is directly observed in our data: the difference between the individual requirement threshold and the unconstrained search effort before requirement imposition. Our econometric approach exploits that – conditional on a broad set of choice fixed effects – the match between the job seeker's effort choice and the caseworker's requirement setting behavior is arbitrary. It therefore provides exogenous variation in the treatment assignment. We find that binding search requirements, which exceed the job seeker's unconstrained effort choice, increase job finding in a substantial way. These effects are highly heterogeneous with respect to the job seeker's characteristics. They come at the cost of increased non-compliance and sanction imposition rates. Moreover, binding requirements have striking negative effects on job stability. Finally, we find that also non-binding requirements can affect search outcomes. This suggests that requirements can operate as signals and thereby generate behavioral effects which are not predicted by standard job search theory.

Keywords: Job Search Behavior, Unemployment Insurance, Incentive Effects

JEL Codes: J64, J65

1 Introduction

Little is known about the job seeker's reaction to a core element of modern unemployment insurance (UI): the enforcement of minimum search effort levels. How do individuals at the beginning of their unemployment spell change their effort choice when it is constrained by a job search requirement? Do constraints on search effort reduce intrinsic effort? Can quantitative search requirements have any effect on job finding at all and do they compromise job stability? Systematic evidence on these questions is broadly missing, as the job seeker's constrained and unconstrained

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effort choice are unobserved in standard data sources. This paper addresses this gap by presenting detailed empirical results from novel, individual-level register data.

Job search requirements – the setting of a minimum number of applications to be submitted per time span – have become a widely used instrument among OECD countries (Venn 2012). In recent years, their strength has risen among both U.S. states and European countries. Requirements condition benefit receipt on sufficient search effort from the beginning of the spell on, while leaving the overall level of insurance unaffected. The job seeker's compliance with the requirement is generally monitored by the Public Employment Service (PES) and enforced by a credible sanction threat: if the submitted number of applications is lower than the required number, job seekers face a high chance of seeing their benefits temporarily cut.

The enforcement of requirements is usually motivated by the assertion that generous UI benefits can lead to the under-provision of search effort. This phenomenon is discussed in a broad strand of empirical literature on the impacts of benefit generosity in UI schemes. The evidence suggests that high benefit payments and a long benefit duration reduce the unemployment exit rate (e.g. Meyer 1990, Katz and Meyer 1990, Hunt 1995, Card and Levine 2000, Chetty 2008, Lalive 2008, Schmieder et al. 2012, Caliendo et al. 2013). Direct empirical evidence on how individual effort choices contribute to these aggregate effects is however absent, as standard UI registers do not provide data on effort provision. Novel Swiss register data allow us to measure both the requirement constraint and a proxy for unconstrained effort at the beginning of each individual unemployment spell. We define the difference between these two variables as our treatment intensity of interest and propose a method to evaluate its effects. We can thereby establish a direct link between a required change in search effort and the job seeker's outcomes. While this link is confirmed by standard job search theory, it has to our knowledge not yet been tested empirically. A few contributions investigate how the introduction of a job search monitoring *regime* changes job finding rates and job quality (Van den Berg and Van der Klaauw 2006, McVicar 2008, Petrongolo 2008, Manning 2009). This regime however features a whole "package" of treatments, including requirements, the knowledge of being monitored and the incidence of benefit sanctions. Another strand exploits variation in the strength and enforcement of search requirements resulting from field experiments run in different U.S. states (Johnson and Klepinger 1994, Meyer 1995, Klepinger et al. 2002, Ashenfelter et al. 2005). While some of these studies allow to separately identify the effect of changes in the requirement strength and the enforcement mechanisms, they are not able to study required effort changes at the individual level.

Our empirical analysis focuses on the individual effort constraint and assesses whether it induces reactions that are in line with standard search theory. A small and growing literature on behavioral labor economics identifies departures from standard rational theories and proposes alternative behavioral explanations for common labor topics.[1] In the context of job search, this literature suggests that the job seeker's effort decision does not follow the rule of rational behavior, as it is influenced by hyperbolic discounting (DellaVigna and Paserman 2005), biased beliefs (Spinnewijn 2013; Falk et al. 2006) and reference-dependent preferences (Della Vigna et al. 2014). We contribute to this literature by analyzing how the individual behaves when confronted with a requirement constraint that is different from her preferred effort choice.

As a starting point, we derive predictions from a basic job search model with enforced requirements, as introduced in Abbring et al. (2005) on the grounds of Mortensen (1987).[2] A key feature of search requirements is that they affect each job seeker *relative to her unconstrained effort choice*: a requirement is binding when it exceeds the effort that the job seeker would provide in its absence; it is non-binding otherwise. While non-binding requirements do in the model not affect the job seeker's behavior, a binding requirement is predicted to increase search effort. However, the possibility of non-compliance makes this effect non-linear: the job seeker's cost of compliance increases with the difference between her unconstrained effort and the requirement. As a consequence, non-compliance rates and the incidence of benefit sanctions are expected to rise, since taking the risk of benefit cuts becomes marginally more attractive.

We bring these predictions to a reduced-form framework and define our treatment intensity of interest as the distance from the individual's requirement threshold to her unconstrained effort level at the beginning of the spell. We thus model the degree to which the requirement is binding or non-binding to the individual effort choice, at a stage of the unemployment spell at which benefit exhaustion is still far. Our data base reports individual-level requirement thresholds as well as provided search effort measured as the number of monthly job applications. We start our empirical analysis by showing that the effort level provided by the job seeker before she learned about her individual requirement reveals substantial information on her cost of effort. We argue that this level can therefore be used as a proxy for the job seeker's unconstrained search effort.

We then proceed to identifying the causal effect of this individual treatment intensity on job finding, job stability and compliance behavior. For identification we exploit the fact that there is arbitrariness in the matching of caseworkers to job seekers. The Public Employment Service (PES) bases its assignment of job seekers to caseworkers on observable determinants, like caseload or municipality. Conditional on these determinants, the job seeker may randomly be confronted with different requirement setting behaviors by caseworkers. To isolate this randomness, we apply several sets of fixed effects that control for potential endogeneities with respect to the job seeker's

[1] Examples include, e.g., work on pay equity (Kahnemann et al. 1986; Card et al. 2012) or reference-dependent labor supply (Fehr and Goette 2007).

[2] A similar version is introduced in Lalive et al. (2005)

and the caseworker's behavior. First, we control for the job seeker's unconstrained effort level; these effects take into account the individual search productivity or motivation. Second, we address that requirement thresholds are allocated on a non-random basis by caseworkers at their first meeting with the job seeker. Our key argument is here that selection occurs with respect to the *level* of the requirement, not with respect to its difference to the pre-requirement effort choice. Holding this level constant therefore amounts to excluding the caseworker's assessment of a job seeker's characteristics from the variation in our treatment intensity of interest. To this purpose, we introduce controls for the level of the requirement assigned to the individual (as deviations from the caseworker's median requirement choice). Finally, we add caseworker effects, which hold other potentially correlated policy choices and local labor market conditions constant. We provide evidence on the quasi-randomness of the assignment process conditional on the choice fixed effects.

Our analysis results in the following main findings: we first confirm the theoretical predictions that the elasticity of search effort with respect to binding requirements is strong but imperfect, as compliance becomes costly when the requirement increases. Our results show that the probability of non-compliances rises substantially in response to a required increase in effort. When the required increase in search effort relative to the unconstrained choice is high, job seekers find it more attractive to incur the risk of a benefit sanction. This translates into increased sanction imposition rates. Policy makers should keep these non-compliance effects in mind when designing requirement thresholds.

We then identify a substantial positive effect of binding search requirements on job finding, in particular at early stages of the unemployment spell. If a job seeker has to increase her search effort due to the requirement by one application, her probability of finding a job within six months will increase on average by about .5 percentage points. This effect is non-linear (concave), which suggests that policy makers cannot maximize job finding rates simply by maximizing requirement levels. Further, these effects differ with respect to the labor demand situation: they are strongest in local labor markets in which vacancy rates are high. It is also striking that job finding rates of low service occupations and low education groups react most. It appears that job finding in these groups is most responsive to the quantity of job applications.

The requirement-driven increase in job finding goes along larger job instability. Estimates of the unemployment recurrence rate – i.e. the risk of returning to unemployment within 6 months after job finding – show remarkable effects: the risk that individuals take up temporary or instable jobs, which end up in a new unemployment spell, turns out to be substantially higher when individuals face strongly binding requirements. We even find that the entire positive effect of binding requirements on early job finding are driven entirely by exits to unstable jobs.

Finally, our findings reveal interesting insights into the role of non-binding requirements: they also affect job search outcomes, which is not in line with the predictions of standard job search theory. After receiving a search requirement, job seekers move their realized search effort towards the requirement threshold, also when their unconstrained search level was significantly higher. This reduction in search effort negatively affects the probability of job finding during the first three months of unemployment. At the same time, non-binding requirements positively affect job stability. This finding suggests that the search requirement operates not only when it represents a binding constraint to the individual that is enforced by a benefit sanction. It also works by signaling a reference point on the optimal search quantity and can, through this channel, move the effort of all job seekers towards the requirement threshold. More broadly, our findings suggest that providing reference points has the potential of changing job search behavior, no matter whether they represent binding or non-binding constraints. The importance of reference points in job search has already been highlighted by DellaVigna et al. (2014), who suggest that search intensity increases when individuals experience income losses compared to their situation in the previous period. In our analysis, the job seeker's reference point is not his individual past situation, but the signal given by the search requirement. It appears that job seekers are very reactive to interventions that signal the "optimal" search quantity as defined by the policy maker.

Our paper is structured as follows: we begin by discussing the theoretical prediction on the intensive margin effects of requirements on job search behavior and job finding (Section 2). Section 3 presents the institutional background and the structure of our data sources. In Section 4, we provide descriptive evidence on the behavior of constrained and unconstrained search effort. Section 5 discusses our econometric model and Section 6 presents our results. Section 7 concludes.

2 Theoretical Discussion

Model Description Our discussion on the effects of search requirements as constraints is based on a framework developed by Abbring, van den Berg and van Ours (2005).[3] The authors introduce requirements and benefit sanctions in a search framework as proposed by Mortensen (1987). It is important to note that our definition of search effort is limited to its quantitative dimension. This is mainly due to the design of search requirements in OECD countries, which target the amount of applications to be submitted.[4] Also note that our discussion refers to a situation in which the job seeker's benefit exhaustion is still irrelevant. Our entire analysis will focus on required effort changes at the beginning of the unemployment spell.

The search requirement s_r and its enforcement, i.e. the probability of sanction in case of non-

[3]Lalive et al. (2005) present a very similar framework in their analysis of UI benefit sanctions.

[4]Note that in most countries, monitoring of compliance with the requirement also includes some minimum quality standard, as caseworkers can e.g. ask for the application letters sent out. This is also the case in Switzerland (c.f. Section 3).

compliance p_0, affect the job seeker's behavior *before* the occurrence of a sanction. According to a slightly adopted version of Abbring, van den Berg and van Ours (2005),[5] the job seeker's value function before any enforcement writes:

$$\rho R = \max_s \left[b - c(s) + \lambda(s) \int_{\phi}^{\infty} (\frac{w}{\rho} - R) dF(w) + I(s < s_r)(1 - \frac{s}{s_r}) p_0 (R_{sanc} - R) \right]$$

where $R_{sanc} < R$ is the value of unemployment after benefits have been cut by the sanction amount.[6] b is the unemployment benefit, s the realized number of applications and w the wage of the final job match. ϕ denotes the reservation wage, which equals ρR after optimization. When no requirement policy is in place, the job seeker chooses her optimal effort level s^*. s^* results from a trade-off between the marginal cost of effort $c'(s)$ and its marginal benefit, which consists of an increase in the job arrival rate $\lambda'(s)$ and the associated differential in value between employment and unemployment $\int_{\phi}^{\infty} (\frac{w}{\rho} - R) dF(w)$.

Given s^*, the job seeker chooses her provided level of effort s in a system *with requirements*. The requirement threshold enters through the term $I(s < s_r)(1 - \frac{s}{s_r}) p_0 (R_{sanc} - R))$: in case the job seeker provides a search effort that is lower than the requirement $(I(s < s_r))$, there is an exogenous probability p_0 that the job seeker moves to the sanctioned state. This probability becomes larger when the distance between the provided and the required effort increases $(1 - \frac{s}{s_r})$.

It is the distance between the requirement and the job seeker's unconstrained effort, $\Delta^*_{s_r} = s_r - s^*$, that determines how individual search behavior is affected. The following three cases, which are illustrated in Figure 1, can be distinguished:

(I) $\Delta^*_{s_r} \leqslant 0$: the job seeker faces a requirement that is lower than her unconstrained effort level. In phase I, all search outcomes are therefore *unaffected by the search requirement.*

(II) $\Delta^*_{s_r} > 0$ and compliance: the job seeker has to increase search effort by $\Delta^*_{s_r}$ in order to to comply with the requirement. In phase II, the individual cost of complying is lower than her cost of facing the risk of a benefit sanction. *The job seeker therefore chooses to submit $s = s_r$ applications.* As this effort level is sub-optimal from the individual perspective, this behavioral change goes along with a decrease in the present value of unemployment.

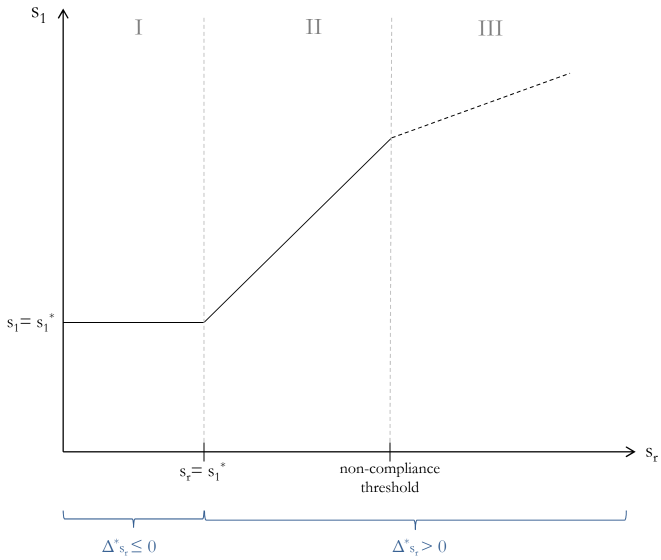
(III) $\Delta^*_{s_r} > 0$ and non-compliance: from an individual-specific threshold onward, the job seeker's value of submitting less applications than required and incurring a given risk of sanction is

[5] We introduce the term $1 - \frac{s}{s_r}$ in order to account for the empirical fact that the probability of sanction becomes more likely when the ratio of provided to required effort becomes high.

[6] Abbring et al. (2005) assume for simplicity that a sanction reduces the present value of unemployment for the remaining unemployment spell. We follow this assumption.

larger than the value of complying. *The job seeker now chooses an effort level* $s \, \epsilon[s^*, s_r)$ *and does therefore not comply.* As the probability of a benefit sanction is now positive, the job seeker's present value of unemployment again decreases.

Figure 1: Illustration of Theoretical Predictions



Predictions for the Empirical Analysis The above reasoning shows that the difference between s^* and s_r, which we denoted as $\Delta^*_{s_r}$. is at the center of the requirement's effects on job search behavior. We take the following main predictions on the effects of $\Delta^*_{s_r}$ to our empirical approach:

1. If $\Delta^*_{s_r} < 0$ the requirement is non-binding from the individual's perspective and does from a search theoretical perspective not provoke any changes in search behavior

2. If $\Delta^*_{s_r} > 0$, the requirement is binding from the individual's perspective. An increase in $\Delta^*_{s_r}$ is now predicted to have the following effects:

   i) The cost of compliance increases with $\Delta^*_{s_r}$ if $\Delta^*_{s_r} > 0$. Given a fixed amount and probability of sanction, a high cost of compliance makes non-compliance, i.e. the provision of $s_1 < s_r$, relatively more attractive. Therefore, we expect the incidence of non-compliance and the sanction imposition rate to increase with $\Delta^*_{s_r}$.

   ii) If $\Delta^*_{s_r} > 0$, an increase in $\Delta^*_{s_r}$ increases search effort and reduces reservation wages. We therefore expect job finding rates to increase.

iii) Due to the reduction in the job seeker's reservation value both under compliance (phase II) and under non-compliance (phase III), we expect the increase in job finding rates to go along with a decrease in job quality.[7]

3 Institutions and Data

3.1 Institutional Background

The Swiss Unemployment Insurance (UI) System In Switzerland, job seekers are entitled to UI benefits if they meet two main prerequisites: first, they must have contributed for at least six months in the two years prior to registering at the Public Employment Service (PES). The contribution period is extended to 12 months for those individuals who have been registered at least once in the three previous years. Second, job seekers must be able to be "employable" in a regular job. Otherwise, there is the possibility to collect social assistance. The potential duration of unemployment benefits is two years for eligible job seekers. The replacement ratio is between 70% and 80% of previous earnings, depending on the individual situation.

The organization of counseling and monitoring is ensured by Public Employment Service (PES) offices, which are the organizational unit of caseworkers. When individuals register at the PES office they are assigned to a caseworker on the basis of either previous industry, previous occupation, place of residence or the caseworker's availability (caseload formula).

Job Search Monitoring in Switzerland Swiss UI law requires individuals to start actively searching for work at the moment they they learn about their future unemployment.[8] This is usually three months before a job loss becomes effective, as employers have to announce a layoff three months in advance. Before the first meeting with the caseworker, the job search obligation does not include a fixed requirement threshold. It thus appeals to the job seeker's own definition of active job search. After having entered formal unemployment, job seekers are informed about their individual search requirement threshold when they first meet their caseworker. They are then required to submit this exact number of applications on a monthly basis in order to avoid UI benefit cuts. This number varies between 1 and 15 monthly applications in our sample (c.f. Section 4.2).

The PES is prescribed by law to monitor and enforce the compliance with the job search obligation before and after the job seeker enters formal unemployment. As a consequence, the database of job search monitoring creates by default an entry to monitor pre-requirement effort.

[7] In our empirical framework, we measure job quality as job stability.

[8] c.f. State Secretary for Economic Affairs (SECO), 2014: AVIG-Praxis ALE (UI practice guidelines), paragraph B314

Caseworkers are supposed to fill this entry by asking job seekers to report their search activity previous to the first meeting. They can ask for proofs of this activity and enforce benefit cuts if they deem that the pre-requirement effort was insufficient. After the first meeting took place, the application activity is documented in a "protocol of search effort", which job seekers submit until the 5th day of the following month. The compliance with the search requirement threshold is monitored by the caseworker. Caseworkers can again ask for proofs of submitted applications during one of their meetings with the job seeker. In addition, the submission of applications can be checked by contacting the human resources department of the potential employer. Once a non-compliance with search requirement is detected, benefit cuts enter into force. In our sample, a job seeker who submits less that 75% of the required job applications faces a probability of 38% that this non-compliance is detected and registered. 72% of registered non-compliances result in a benefit sanction. The median amount of a sanction is the equivalent of 7 days of UI benefits.

3.2 Data Sources and Sampling

Data Sources Our empirical analysis is based on Swiss administrative data. The sample covers all benefit recipients entering UI between January 1 2010 and December 31 2012. It includes extensive information on entry into and exit from formal unemployment, socio-demographics as well as employment and unemployment histories. It further reports which PES and caseworker the job seeker was assigned to. We measure the duration of unemployment as the number of days elapsed between the date of registration at the PES and the date of de-registration at which the job seeker's file was closed.

We match these records to the database used by caseworkers to monitor job search effort. It reports the required and realized number of applications for each job seeker on a monthly basis. In addition, we observe when a non-compliance with the requirement is detected by the caseworker and when it results in a cut of benefit payments. A particular feature of the database is that we can also identify the number of applications sent out before the job seeker learned about his requirement, as these are also monitored (c.f. Subsection 3.1). We argue that this pre-requirement job search effort, denoted s_0, is not influenced by a fixed requirement level. As there is a legal obligation to search for work before the first caseworker meeting, we do not consider s_0 to be completely unrestricted by the monitoring regime. Nevertheless, it is not influenced by the exact requirement level s_r, which we argue to be unknown by the job seeker at t_0.[9] s_0 is thus a suitable indicator for what the job seeker herself considers to be the amount of job search that is sufficient for the beginning of the unemployment spell. We will further discuss the content of s_0 as a measure

[9]This might hold less for job seekers who have during previous unemployment spells learned about the requirement system. We will in a robustness check exclude these job seekers and show that they do not drive our results.

of the theoretical s_1^* in Section 4.3.

The database, whose structure is illustrated in Figure 2, provides three main parameters of interest for our analysis: the pre-requirement effort level s_0, the search requirement s_r imposed by the caseworker for the unemployment spell and the effort level $s_t|s_r$ provided in month t in response to the requirement. As we focus in this paper on the effort levels provided at the beginning of the job seeker's unemployment spell, we are exclusively interested in $s_1|s_r$.

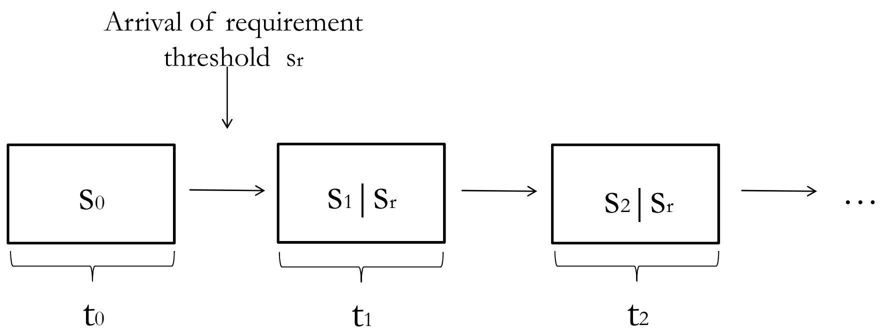


Figure 2: Basic structure of data on job search monitoring

Sampling In principle, our data set contains the entire population of Swiss UI job seekers who enter UI during our sample period. We are however obliged to limit our sample to job seekers registered in those cantons where job search monitoring is systematically reported in the central database (to which we have access to). Federal Swiss law prescribes the enforcement of job search requirements. Therefore, it is ensured that cantons excluded from our sample participated at the requirement policy. Anecdotal evidence suggests that these cantons have their own system of requirement registration rather than employing the central data base. Our sample contains the cantons Bern, Fribourg, Solothurn, Graubuenden and Tessin, which cover three different geographic and language regions in Switzerland.

The obligation to engage in active job search needs to be fulfilled in exchange to the payment of UI benefits. Our framework aims at identifying how the difference between the requirement threshold and the job seeker's unconstrained effort choice affects search behavior Therefore, we want to include in our sample only those job seekers who faced a requirement which was monitored during their unemployment spell. These are job seekers who are full-time unemployed, eligible for UI payments and not eligible for other benefits (in particular disability insurance). We also exclude job seekers who are younger than 20 or older than 55 years, as these might face particular incentives and labor market conditions. In addition, we exclude job seekers whose previous unemployment spell ended less than a month previous to their current registration. These are most likely particular cases to which the institutional setting underlying our analysis does not apply. Further restrictions are imposed by the design of the requirement policy. As our analysis of binding vs. non-binding requirements is done at the intensive margin, it only concerns individuals who were subject to the search obligation from the beginning of their spell onwards. Everyone faces this obgligation by law, but there are possible exemptions due to the individual's situation. In the data appendix A.1, we describe how we defined whether the individual was systematically affected by the search obligation, as well as the percentages of excluded spells. We there also provide a detailed description of how we extract the variables s_0, s_r and $s_1 \mid s_r$ from the database on job search monitoring and of how we impute requirement thresholds for the 8.2% of job seekers whose search effort was monitored, but whose requirement level is missing in the data. In essence, imputation is straightforward as we know the requirement setting behavior of the caseworker each job seeker got assigned to. Excluding job seekers with missing requirement thresholds does not affect our results (c.f. robustness analysis).

4 Descriptive Evidence on Requirement Thresholds and Effort Choices

In the following, we show descriptive evidence on the distribution and the observable determinants of effort choices and requirement levels. We also provide a discussion and descriptive evidence on whether s_0 is suitable to measure the job seeker's unconstrained effort choice.

4.1 Sample Distributions

In the following, we show features of the distribution of s_0, s_r and s_1. Detailed summary statistics can be found in Table 12 in Appendix A.2.1.

Unconstrained Effort s_0 Figure 3a displays the distribution of s_0 for job seekers in our sample. Around 20% of job seekers do not submit any applications before registering at the PES, a vast majority reports s_0 within the range of 1 to 20 and around 10% beyond that range. 25% of job seekers in our sample receive a notification that their s_0 was deemed insufficient and might result in a benefit sanction. Figure 3b shows the distribution of s_0 for these job seekers.

[Insert Figure 3]

Search Requirement s_r Figure 4a shows how requirement levels are distributed within our sample of job seekers. Differences in requirement levels result from two main sources of variation: first, PES have different baseline policies that consist of setting higher or lower average requirement levels, as displayed in Figure 4b. Caseworker can also have preferences for average policies (Figure 4c). Second, we know from a qualitative caseworker survey that caseworkers set requirement levels at a personal contact with the job seeker. They can therefore differentiate the requirement level according to the job seeker's characteristics observed at this personal contact. We observe parts of these characteristics, such as age, education, occupation. Other determinants, such as motivation, health and appearance remain unobserved to us. As shown in Figure 4d, most caseworkers distribute two or three different requirement thresholds among their population of job seekers.[10] We will come back to this feature when describing our econometric analysis.

[Insert Figure 4]

Constrained effort $s_1 \mid s_r$ We measure the constrained search effort $s_1|s_r$ as the effort provided in the first month in which the search requirement was known. Figure 5 displays its distribution. It peaks at the most commonly imposed requirement thresholds 6, 8, 10, 12, suggesting that most job seekers submit exactly the required amount of applications. The share of job seekers submitting zero effort diminished substantially; around 5% of job seekers still provide zero effort at t_1. We observe in the data that these are indeed perceived as non-compliant by the monitoring regime, as around 50% of them receive a benefit sanction for insufficient effort during the first three month of unemployment (vs. 12% of job seekers who submit a positive s_1).

[Insert Figure 5]

4.2 Observable Determinants of s_r and s_0

We now present some descriptive evidence on the observable determinants of s_0 and s_r. Table 1 displays regressions of different labor market conditions on the two variables, which for the sake of comparability all include caseworker fixed effects. Column (1) shows that female job seekers have a higher unconstrained effort level s_0. Further, s_0 increases with age. The service sector has a larger level of s_0 that the blue collar sector. Interestingly, the determinants of the requirement threshold point at least partly in the other direction, as shown in Column (2): female and older job seekers are assigned slightly lower requirements. Education, sector and function in the last job are important determinants of s_r, which is in line with the answers given by caseworkers from Bern in a survey that we run with them.[11]

[10] A requirement level is counted as part of the PES' "portfolio" if at least 10% of the PES' job seekers receive that requirement threshold.

[11] More detailed results from the survey will be made available in the Appendix.

In Column (3), it is shown that the importance of the different covariates does not change significantly after introducing fixed effects for s_0. We will come back to this feature when we discuss our identification problem and strategy.

[Insert Table 1]

4.3 The content of s_0 as a measure of s_1^*

s_0 is the effort provided by each job seeker in the month before the requirement threshold was announced to him. In order to use it as a measure for s^*, it needs to be assumed that the job seeker reveals her true preference of search effort when reporting s_0.

A first part of the assumption implies that the job seeker does not lie about his provided effort level s_0. This is guaranteed by our institutional setting: given that low levels of s_0 can result in benefit sanctions, there is no incentive for understating s_0. Overstatement is not feasible, as job seekers are asked to prove their application activity.

The second part of the assumption is less trivial: we need to assume that s_0 indeed reveals the effort level which she would provide if no requirement threshold was to arrive at the beginning of the spell. s_0 reflects the pre-requirement effort decision, which is not (yet) constrained by an imposed requirement. Job seekers decide by themselves about which level of search is optimal for them to implement. However, they may be aware about the legal obligation to search for a job and about the fact that the imposition of a search requirement threshold is upcoming. They may thus build expectations about the future required search level. But since they don't know yet about their caseworker and haven't received yet the full information about how the UI system will work, their expectations are marked by uncertainty.

By definition, we cannot formally test that s_0 is not systematically driven by expectations on the future requirement level. However, we can use the case of repeated spells to show that it is a reasonable assumption. For those who have already entered in contact with the PES during a past unemployment spell, we know the requirement threshold of their previous unemployment spell. Figure 6a plots for these individuals the distribution of the current s_0 against the requirement s_r of the previous spell. Although this past s_r could allow job seekers to form some prediction on their future s_r, we see no systematic correlation. This supports the idea that individuals use the pre-requirement period to provide the effort level that is optimal from their individual perspective.

In addition, we can provide descriptve evidence that s_0 has properties which are in line with the theoretical s^*. The choice of s^* results from a trade-off between the job seeker's cost of effort $c'(s_1)$ and his marginal benefit, which consists of an increase in the job arrival rate $\lambda'(s_1)$ and the associated differential in value between employment and unemployment (c.f. Section 2). Holding the marginal benefit of effort constant, individuals with a high effort cost will lead to a low level of s^*. At the same time, the cost of effort is also reflected in the job seeker's compliance choice under the requirement threshold. It was shown in Section 2 that the job seeker's choice of compliance depends on a trade-off between the cost of the additional effort necessary to achieve compliance and the risk of benefit reductions imposed in the case of non-compliance. As a consequence, non-compliant job seekers who prefer to face a given probability of non-compliance will on average have a higher cost of effort than compliant job seekers. We can now test whether the job seeker's s_0 is correlated to her cost of effort as revealed by the compliance with a given requirement. Figure 6b plots the share of non-compliant job seekers against s_0. It shows that s_0 is indeed highly correlated with the probability of being non-compliant at t_1 (measured here as providing less than .75 of $< s_r$). There thus exists descriptive evidence on the correlation between the chosen s_0 and the job seeker's cost of effort.

[Insert Figure 6b]

4.4 Search Effort under Binding and Non-binding Requirements

We now provide descriptive evidence on the distribution of required and realized effort changes in our sample population. Figure 7 displays the distribution of Δ_{s_r} in the categories that will be used in our empirical approach. The baseline category are job seekers with $\Delta_{s_r} \epsilon [-2, 2]$, which are pooled into the status $\Delta_{s_r} = 0$ under the assumption that very small Δ_{s_r} do not impose any strong changes in effort.[12] We see that around one third of job seekers is within the range of $\Delta_{s_r} \epsilon [-2, 2]$ and therefore not significantly affected by the presence of s_r. Around a half of job seekers face $\Delta_{s_r} > 2$, which implies that they have to significantly increase their effort level relative to s_0 in order to achieve compliance. The requirement constraint is thus binding for them. Around 20% of job seekers can reduce their effort level relative to s_0 without becoming non-compliant ($\Delta_{s_r} < -2$).

[Insert Figure 7]

How does the presence of s_r affect the amount of provided search effort? Figure 8a plots the average change in search effort, $s_1 - s_0$, that occurred in each of the treatment bins. It shows that binding requirements are clearly associated with positive effort changes, as the average realized effort change increases with the treatment intensity Δ_{s_r}. Strikingly, non-binding requirements ($\Delta_{s_r} < 0$ are associated with negative effort changes. This is not in line with prediction from

[12] Our results are robust to alternative pooling choices. Results are available upon request.

standard search theory, which implied that non-binding requirements do not affect search behavior at all (c.f. Figure 1).

Figure 8b confirms this picture: it shows that for job seekers with non-binding requirements, the average difference between the realized effort s_1 and the requirement s_r is only around one or two applications and thus far below the treatment intensity Δ_{s_r}. This again suggest that while these job seeker's effort levels remain on average above s_r, they adjust their effort towards s_r. We will test with our econometric framework whether these behavioral changes go along with changes in search outcomes.

Figure 8b also shows that job seekers with strongly binding requirements submit on average less applications than required, which goes along with the theoretical predictions that compliance becomes less likely when the distance between s_r and s_0 increases. In our econometric analysis, we will test for the causal effect of Δ_{s_r} on the job seeker's probability of non-compliance.

[Insert Figures 8a and 8b]

5 Econometric Model and Identification

Following our theoretical discussion, we want to identify how the difference between a job seeker's requirement threshold and her unconstrained search effort, $\Delta^*_{s_r} = s_r - s^*$, affects different job search outcomes. Based on the discussion in section 4.3, we use s_0 as a proxy for s^*. Our empirical approach therefore evaluates the effects of the treatment intensity $\Delta_{s_r} = s_r - s_0$. It is defined as the *additional effort required at the beginning of the unemployment spell, beyond the provided pre-requirement effort level*. This treatment intensity is positive in the case of a binding requirement threshold and negative in the case of a non-binding one, i.e. where the threshold is below the pre-requirement effort level.

The treatment intensity Δ_{s_r} results from a match between two endogenous variables: the job seeker's pre-requirement effort choice s_0 and his individual requirement level s_r as assigned by the caseworker. In order to isolate the exogenous component of this match, we apply a set of fixed effects that controls for the direct effect of the job seeker's effort choice and the caseworker's requirement setting behavior on our outcomes of interest. We will argue that the remaining variation in the match between a job seeker's effort type and a caseworker's requirement setting behavior is random and can therefore be exploited to identify the causal effect of Δ_{s_r}.

The empirical model applied for the estimations can be represented in the following baseline equation:

$$y_i = \alpha + x_i'\beta + \delta_i^{\Delta_{s_r}} + \gamma_i^{s_0} + \sigma_i^{s_r,c} + \pi_{c(i)} + \eta_t + u_i \tag{1}$$

The main parameters of interest are $\delta^{\Delta_{s_r}}$, which measure how Δ_{s_r}, the difference between the requirement s_r and the pre-requirement search effort s_0, affects the outcome variable. In order to allow for non-linear effects of Δ_{s_r}, a series of treatment intensity indicators is used, i.e. $\delta^{\Delta_{s_r}}$ represents dummy variables for bins in the distribution of Δ_{s_r}. The baseline category pools job seekers with $\Delta_{s_r}\epsilon[-2,2]$, i.e. whose pre-requirement effort is very close to the requirement level. The distribution of the Δ_{s_r} bins is discussed in section 4.[13]

5.1 Identification Strategy

We argue that we can isolate the causal effect of Δ_{s_r} by conditioning on the following set of fixed effects:

First, we control for the job seeker's pre-requirement effort choice s_0 through a series of fixed effects γ^{s_0}. γ^{s_0} features a dummy variable for each number of applications sent out in the month previous to the requirement. Thereby, it holds constant the direct impact of the individual's search "type". This can be seen as a measure of the individual search performance, driven by factors such as the intrinsic motivation, the assessment of labor market conditions and the experience with job search. In addition, η_t controls for the time at which s_0 is measured.[14]

Second, we address the issue that requirement thresholds are allocated on a non-random basis by caseworkers at their first meeting with the job seeker. Our key argument is here that selection occurs with respect to the *level* of s_r, not with respect to its difference to the pre-requirement effort choice s_0. Requirement policies aim at ensuring *a minimum effort level given the job seeker's labor market conditions.* Caseworkers are asked to have this target in mind during the assignment process. According to a survey we performed,[15] caseworkers indeed see the requirement policy as a means to ensure a certain level of search. They name the job seeker's labor market conditions as the most relevant determinants of this level. In contrast, pre-requirement search effort is not mentioned once as a criterion for the assignment of requirements. As a consequence, the influence that the caseworker's assessment of the job seeker's characteristics has on the requirement setting process should be fully reflected in the assigned requirement *level.* Holding this level constant therefore amounts to excluding the caseworker's assessment of a job seeker's characteristics from the variation in the treatment intensity Δ_{s_r}. To this purpose, we introduce the variables $\sigma^{s_r,c}$ into our baseline equation. They represent fixed effects for the difference between the individual's

[13] Our results are robust to choosing different cutoff values for Δ_{s_r}. Documentation is available upon request.

[14] η_t contains controls for the difference between t_0 and the start of formal unemployment as well as the difference between t_0 and the job seeker's availability for a new job. It also controls for the difference between the start of formal unemployment and the first caseworker meeting, in order to account for heterogeneity with respect to the arrival of the requirement threshold. Summary statistics on these variables are included in Appendix A.2.1, Table 13.

[15] Survey among 40 caseworkers in the canton of Bern

requirement level and the requirement of his caseworker's median case, $s_r - med_c(s_r)$. We thereby control for all systematic correlations between the individual's requirement threshold and the caseworker's assessment of the job seeker's labor market characteristics, relative to those of his median job seeker.[16] Table 12 in Appendix A.2.1 contains summary statistics on $\sigma^{s_r,c}$.

Finally, we account for the institutional environment of the requirement setting process. In particular, we control for the fact that some caseworkers tend to assign higher average requirement thresholds than others. This might for instance reflect local labor market conditions or caseworker "strictness". We account for such aspects by introducing caseworker fixed effects π_c into our baseline equation. The introduction of caseworker fixed effects further excludes all other policy choices[17] which might be correlated with the requirement policy – in particular monitoring and enforcement strictness or the emphasis placed on application quality – from our estimates.

Note that our empirical approach explicitly omits variation stemming from the average caseworker policy. It thereby differs from approaches in previous literature which exploit that type of variation to generate random treatment assignment (e.g. Kling 2006; French and Song, 2014; Dahl et al., forthcoming). In the named studies, the treatment consists in the caseworker's or the judge's final decision on one specific issue (e.g., a criminal case). Therefore, random assignment of cases will generate variation only with respect to that specific treatment. In our setting, the caseworker's discretionary power is not limited to our treatment of interest, but extends to other policy instruments in the area of search assistance and search monitoring. We do not want these policy choices to affect the final outcome in a way which is correlated with the effects of Δ_{s_r}. Therefore, we exploit variation conditional on caseworker fixed effects. This remainder variation reflects that different requirement setting preferences of one caseworker are (arbitrarily) matched to different pre-requirement effort types.

Thus, conditional on all the fixed effects contained in equation (1) and described above, the variation in Δ_{s_r} is driven by the *arbitrariness of the match between the job seeker's unconstrained search behavior and the caseworker's requirement setting behavior.* This arbitrariness is due to fixed assignment rules of "cases" to caseworkers in the PES. The most common assignment rules in the areas of the data sample are: by municipality, by occupation in the last job, by capacity (using a defined caseload formula)[18]. Therefore, the job seeker cannot select into a caseworker with a certain requirement setting behavior, nor can the caseworker choose job seekers with a certain pre-requirement behavior. It is important to recall here that the direct impacts of differences

[16] Note that this specification is more flexible than introducing fixed effects for individual requirement levels, as it accounts for the fact that caseworkers can have different assessments of a "high" or "low" requirement. (For instance, a requirement of 8 applications might be high for one caseworker and low for another caseworker.) Our results are, however, not substantially affected if we run a specification with fixed effects for s_r levels.

[17] Note that caseworker fixed effects also control for policy choices or impacts of the local economy at higher aggregate levels like the PES or the region.

[18] Source: inquiries at the national and some cantonal ministries of labor

between job seeker "types" (e.g., with different pre-requirement search behavior) on the outcomes are held constant through the fixed effects contained in γ^{s_0}. The direct influence of a caseworker's behavior on the outcome is controlled by the caseworker fixed effect contained in π^c. And the caseworker's assessment of the job seeker's characteristics, which may influence the requirement setting process as well, is accounted for by $\sigma^{s_r,c}$.

In our main specification, we also introduce a vector of job seeker characteristics x_i, which contains job seeker characteristics such as socio-demographics and labor market histories, seasonal fixed effects and year fixed effects.[19] Given that the specification without x_i excludes all non-random components in Δ_{s_r}, the introduction of x_i should not change our estimates. This will prove to be the case.

5.2 Further Estimation Details

Equation (1) will be estimated by OLS for some of the outcomes of interest. We are also interested in the effects of the treatment Δ_{s_r} on the duration to job finding. To this purpose, we will estimate the job finding rate θ^e, which is specified as a Proportional Hazard (PH):

$$ln\ \theta^e = ln\ \lambda(t_e) + x_i'\beta + \delta_i^{\Delta_{s_r}} + \gamma_i^{s_0} + \sigma_i^{s_r,c} + \pi_{c(i)} + \eta_t \tag{2}$$

When estimating θ^e, we model flexible duration dependence by using a step function

$$\lambda(t_e) = exp(\sum_k(\lambda(t_{e,k})I_k(t))$$

where k(= 1, . . . , 3) is a subscript for time intervals and $I_k(t)$ are time-varying dummy variables for subsequent intervals. As our focus is on the effect of required effort changes at the beginning of the spell, our main specification censors durations after six months. For this specification, we distinguish the following time intervals: 2-3 months,[20] 3-4 months and 4-6 months. In specifications where durations are censored after two years, we additionally distinguish the intervals 6-12 months and 12-24 months[[21]]. As we estimate a constant term, we normalize $\lambda(t_{e,1})$ to be 0.

[19]Summary statistics on the variables contained in x_i can be found in Table 14.

[20]Note that job seekers with an unemployment duration of less than one month are excluded from our analysis because they are unaffected by the requirement regime (c.f. Appendix A.1.

[21]The descriptive job finding hazard is plotted in Figure 19 in Appendix A.2

5.3 Discussion of Identifying Assumptions

We argued that conditional on the mentioned sets of identifying fixed effects, we can identify the causal effect of Δ_{s_r} on the outcomes of interest. This relies on three central assumptions, which we discuss and test in the following.

Selection on Requirement Levels The baseline equation controls for the job seeker's assignment of a requirement level through the variables $\sigma^{s_r,c}$. This omits the caseworker's assessment from the remaining variation in Δ_{s_r} if *the caseworker expresses this assessment in the level of the requirement*, not in its difference to the pre-requirement level s_0. Note that this assumption is not threatened by the fact that caseworkers observe s_0 at their first meeting with the job seeker. Caseworkers may take s_0 as a signal for the job seeker's characteristics and adjust their choice of s_r accordingly. As every job seeker provides such a signal, this mechanism is accounted for by the applied set of fixed effects. The assumption would, however, be violated if caseworkers made their requirement assignment with the explicit aim of generating a certain Δ_{s_r}. In this case, the caseworker's assessment of the job seeker would not be captured by $\sigma^{s_r,c}$, but be systematically correlated to Δ_{s_r}. As mentioned in the previous subsection, anecdotal and survey evidence strongly suggest that caseworkers aim at imposing a certain minimum search level on the job seeker. This claim is supported by Table 1, which shows that the impact of the job seeker's main observable characteristics (Gender, Education, Sector and Age) on the requirement threshold is not affected when s_0 is accounted for through fixed effects. s_0 does thus not provide the average caseworker with any information that affects the determinants of his requirement assignment. In addition, Figure 9 shows that the residuals predicted from a regression of s_r on caseworker fixed effects and the vector x is not systematically related to s_0.[22]

[Insert Figure 9]

Absence of Confounding Policy Choices Second, we assume that in our identifying framework, *the effect of Δ_{s_r} is not driven by other correlated policy choices.* While we control for the caseworker's average policy strategy through the caseworker effects, a caseworker might implement other job seeker-specific policies that correlate with Δ_{s_r} and a given outcome. In Table 2, we provide evidence that Δ_{s_r} is unrelated to the probability that another policy instrument is assigned by the caseworker: given our econometric framework, the treatment indicators D_Δ are neither correlated to the probability that an "early" second meeting[23] is scheduled, nor to the

[22]The residual is estimated as $\hat{\varepsilon} = \hat{s}_r - x'\hat{\beta} - \hat{\pi}^c$

[23]This is defined as a meeting that is scheduled less than 3 weeks after the first meeting.

probability that a benefit sanction which is unrelated to the compliance with the requirement is imposed during the first two months of the spell.

[Insert Table 2]

Exogenous Match between Job Seeker Types and Caseworker Requirement Setting A third implication of our exogeneity assumption concerns the mapping according to which the caseworker assigns requirement thresholds to the job seeker based on their characteristics. We argue that *the match between the caseworker's requirement setting process and the job seeker's search type is random conditional on the set of identifying fixed effects.* In other words, we assume that a job seeker's type does not systematically affect how the caseworker distributes the requirements among her job seekers. One central feature of a caseworker's requirement setting criteria is the influence that a job seeker's characteristics have on her assigned requirement. In order to measure this influence, we run for each caseworker a regression that links a job seeker's requirement to her observable characteristics. We predict the resulting requirement as $\hat{s}_{r,c}$ and compute its standard deviation as a measure of its spread. We show in Figure 10 that this standard deviation is unrelated to the average s_0 of job seekers that are assigned to a given caseworker. There is thus no systematic relation between the average job seeker type assigned to a caseworker and the degree to which job seeker characteristics map into the caseworker's requirement decision.

[Insert Figure 10]

In addition to the discussed tests, the regression results presented in Section 6 support the argument that the variation in Δ_{s_r} is exogenous conditional on the identifying fixed effects γ^{s_0}, $\sigma^{s_r,c}$ and π^c. First, the introduction of the vector x_i does not change the results on the treatment effects. As x_i contains those factors which caseworkers name as essential determinants of their requirement setting decision (e.g. occupation, education, age), we would expect this if our baseline specification suffered from omitted variables. As an additional robustness test, we will introduce an interaction between s_0 and different sets of observable labor market characteristics. This should generate additional information on the job seeker's unobserved characteristics, as submitting a given level of s_0 might reveal a higher motivation in some occupations than in others. Again, our results are not affected by the introduction of these supplemental variables.

6 Results

6.1 Compliance with the Requirement

Our theoretical discussion showed that job seekers with high Δ_{s_r} have higher costs of compliance, which makes it relatively more attractive for them to submit less applications than required. In Table 3, we test this hypothesis. Column (1) estimates the impact of Δ_{s_r} on the probability of non-compliances, measured as $s_1/s_r < 3/4$. Job seekers who have to submit 7 or more applications than their pre-requirement effort choice are about 4 percentage points more likely not to comply with the requirement. Given that the mean non-compliance probability is around 12%, this effect is substantial. It confirms the theoretical prediction that non-compliance will systematically occur among job seekers who find compliance difficult to achieve. Column (2) shows that it translates into an increased non-compliance detection rate: for instance, job seekers who face $\Delta_{s_r}\epsilon[7,8]$ ($\Delta_{s_r} > 8$) increase their probability of being registered for a non-compliance within the first two months of unemployment[24] by 3 (5) percentage points, compared to job seekers in the baseline category $\Delta_{s_r} \approx 0$. This is again substantial compared to the average warning probability of 9%. The increased warning rate leads to an increased incidence of benefit sanction, as illustrated in Column (3). The results of Column (2) and (3) are illustrated in Figure 11. It shows that the effect of binding requirements on non-compliance detection and sanction enforcement are nearly linear. Job seekers with non-binding requirements have the same probability of non-compliance as job seekers in the baseline category. This is in line with the idea that all individuals with $\Delta_{s_r} \geqslant 0$ face the same cost of compliance.

Column (4) presents a Placebo regression, in which the outcome is the detection of a non-compliance related to the attendance at caseworker meetings within the first two months of unemployment. This type of non-compliance should not react to Δ_{s_r}; indeed, all estimates are statistically insignificant. This supports the interpretation that the non-compliance with search requirements is driven by an exogenously determined Δ_{s_r}.

The results from Table 3 evoke two conclusions: first, individuals translate increased compliance cost into their actual compliance behavior. Policy makers should have increased sanction rate as a side effect and potential cost of high search requirements in mind. Second, increased sanction rates are an additional channel through which the treatment Δ_{s_r} can affect the exit from unemployment. We do not estimate a multiple-step framework that is able to systematically assess this channel. We will however present a robustness check in which we introduce the incidence of a benefit sanction into our final outcome equation. Although an intermediate outcome is not a valid control variable, this gives a first idea that our final results are not driven exclusively by an

[24]We choose such a short time interval to avoid that dynamic selection drives our results.

increased sanction threat.

[Insert Table 3]

6.2 Job Finding and Job Stability

6.2.1 Effect on Job Finding

We estimate the impacts of Δ_{s_r} on job finding in a duration framework, which models the exit from unemployment as a proportional hazard (PH). In our main regressions, we censor durations after six months of unemployment. The reason is that Δ_{s_r} is expected to generate behavioral changes in search effort at the beginning of the spell. We therefore focus on the effect of Δ_{s_r} on the duration of unemployment and the probability of job finding within the first six months after registration at the PES. Also note that 45% of our sample exits unemployment within these six months.

Columns (1) to (3) of Table 4 provide coefficients for the main regression. It evaluates the impact of Δ_{s_r} with respect to the baseline category $\Delta_{s_r}\epsilon[-2, 2] \approx 0$. In Appendix A.2.2, we provide coefficients also for the vectors γ^{s_0} and $\sigma^{s_r,c}$. These are as expected: an increase in s_0 is associated with an increased job finding rate. An increase in $s_r - med_c(s_r)$, which means that the caseworker assigned the individual a higher requirement relative to his median case, is associated with a decreased job finding rate.

Columns (1) and (2) display coefficients on the job finding hazard, censored at 6 months. Column (1) shows results for equation (1), excluding the vector of individual covariates X_i. Column (2) adds X_i. The coefficients from the two columns are not statistically different from each other. They both show that job seekers who have to increase their search effort due to the presence of the search requirement substantially increase job finding. For instance, job seekers who have to write 3 or 4 applications more than in absence of the requirement raise their job finding hazard by 14.5% (=exp(.136)-1) compared to the baseline group with $\Delta_{s_r}\epsilon[-2, 2] \approx 0$. Column (3) reports the corresponding marginal effects,[25] which measure the effect of the treatment on the probability of job finding within six months. The effect of $\Delta_{s_r}\epsilon[3, 4]$ on this probability is 2 p.p and increases up to 4 (5) p.p for job seekers with $\Delta_{s_r}\epsilon[7, 8]$ ($\Delta_{s_r} > 8$). If we impose the effect of binding requirements to be linear, the effect of one additional required monthly application on the probability of job finding within six months by .5 p.p. The regression does not report any effects on job seekers with a negative treatment intensity, i.e. with $\Delta_{s_r} < 0$.

[25] Marginal effects of duration models are obtained by taking the difference of a predicted survivor rate under the treatment and a counterfactual survivor rate which imposes the treatment to equal zero. The change in survivors is averaged with respect to the individual characteristics in the sample population and gives the average marginal effect on the respective job finding probability

[Insert Table 4 and Figure 13]

In columns (4) to (7), we present results from hazard regressions with alternative censoring dates. Columns (4) and (5) confirm that the effects of a required search effort are strongest at the very beginning of the unemployment spell. Here, we strikingly even observe a negative effect of non-binding requirements on early job finding. Job seekers who receive a signal that their search effort was higher than the effort level considered as sufficient by the caseworker reduce their early exit rate. This finding contradicts standard job search theory, which predicts that only job seekers with $\hat{s}_1 < s_r$ change their behavior in response to the threshold. Requirement thresholds therefore seem to operate not only as a binding or non-binding constraint but also as signals which also affect behavior when the constraint is non-binding. We will shortly set these effects in relation to the effects of non-binding requirements on job stability.

Naturally, a treatment that occurs at the beginning of the unemployment spell can have impacts on the entire course of the unemployment spell. Columns (6) and (7) show that the effect of binding requirements averaged over the entire observation period is still positive and significant. Figures 13a 13b illustrate the effects of $Delta_{s_r}$ on the duration of unemployment when taking different censoring decisions, and on the probability of job finding within the censoring period.

6.2.2 Heterogeneous Effects

We now show that the effects of Δ_{s_r} on the duration to job finding differ according to both the job seeker's characteristics and the labor demand situation. Here, all durations are censored after 6 months of unemployment, as differences in behavioral reactions will be most visible at the beginning of the spell, where the treatment is most relevant.

Table 5 presents heterogeneity with respect to gender and education of the job seeker. Figure 18 graphically illustrates these results. Columns (1) and (2) and Figure 18 (a) show that the evoked " signalling phenomenon" is stronger for male job seekers, whose job finding duration within the first six months decreases significantly in response to a non-binding requirement. We further observe that male job seekers increase their job finding rate significantly less in response to a required effort increase than female job seekers. Female job seekers react stronger to the incentive for effort increases: their job finding rate increases significantly more in response to binding search requirements.

Columns (3) to (5) and Figure 18 (b) further illustrate that results are driven by job seekers with low educational attainment. Job finding hazards of unlearned job seekers respond the strongest to required effort increases; in turn, we find few significant effects on job finding hazards of job seekers with high school diploma and above. One possible explanation is that job seekers with a higher degree of education and specialization are bounded in their search effort by the availability of suitable offers. Further, the quality of applications might be of higher importance for this subgroup of job seekers, which is why search requirements that target the quantity of applications are less effective.

[Insert Table 5 and Figure 18]

Table 6 and Figure 15 decompose the effects by occupational degrees. Columns (1) to (3) and Figure 15 (a) show that average effects are largely driven by job seekers in the low service sector, i.e. the cleaning and restaurant sector. Blue collar workers show no reaction. The occupational patterns still hold when their interaction with the job seeker's gender is accounted for (results available upon request). In Columns (4) to (5) and Figure 15 (b), it can be seen that workers who had a support function in their previous employment react more than job seekers who had a professional or higher function. Again, this supports the central result that required changes in the quantity of effort lead to changes in outcomes mainly for job seekers with lower qualification.

[Insert Table 6 and Figure 15]

As a final heterogeneity analysis, Table 7 and Figure 16 present results for subgroups that face a relatively large vs. small labor demand. We use vacancy rates as proxies for the labor demand that the job seeker faces. Vacancy rates are calculated as the ration of posted vacancies over job seekers on a month-region basis. They are assigned to the job seeker depending on his month and place of registration at the PES. "Low", "medium" and "high" vacancy rates are in relative terms, i.e. we divide the distribution of vacancy rates by 3 to assign job seekers to one of these categories.

We observe that effects are largest for job seekers who face a relatively high vacancy rate and nearly absent if job seekers who face a relatively low vacancy rate. Note that Switzerland is generally a country where unemployment is relatively low. In addition, there was no true economic downturn during our sample period. This implies that search requirements might have a significantly lower effect in settings in which labor demand is truly stagnating.

[Insert Table 7]

6.2.3 Job Stability

One fundamental question is whether the substantial effects on job finding come at the cost of worsened job quality. We have no access to characteristics of the job match, but we observe in our UI data when a job seeker re-enters unemployment. As a consequence, we are able to estimate the effect of Δ_{s_r} on the probability of entering a job that leads to the recurrence to unemployment within (6/12) months.[26] Table 8 presents coefficients of Δ_{s_r} on these outcomes. Column (1) displays a substantial effect of Δ_{s_r} on the probability that any job seeker enters a job and re-enters unemployment within six months. If we impose the effect to be linear, the effect size is at the order of .7 percentage points per additional required application (on an outcome mean of 13.8%). This suggests that binding requirements induce job seekers to apply either to very temporary jobs or to jobs which prove to be a bad match. Interestingly, these effects disappear when we look at recurrence within 12 months (Column (3)). This suggests that affected job seekers might in any case have entered a temporary or bad-match jobs, but that in order to comply with the requirement, they enter jobs which are even more so. It is also interesting to see that the effects of non-binding requirements are nearly symmetric: job seekers who receive a requirement that allows to decrease search effort with respect to s_0 appear to decrease the quantity and increase the quality of their job applications.

Columns (2) and (4) run the same regressions on a sample of unlearned job seekers, which were a group whose job finding rate was particularly affected by Δ_{s_r}. Indeed, also the probability of recurrence within 6 month reacts particularly for unlearned job seekers.

[Insert Table 8]

These results raise the question whether the effects on job finding rate identifed in Section 6.2.1 are fully driven by exits to highly unstable jobs. We assess this issue in Table 9, which decomposes the effects of Δ_{s_r} on the probability of exiting to a job within 6 months into exits to permanent and non-permanent jobs. Column (1) shows estimates on the linear probability of job finding within six months. In Column (2), the outcome is coded as one when a job seeker finds a job within six months and recurs to unemployment within the following six months. The effect of Δ_{s_r} is substantial and suggests that requirement-induced job matches are non-permanent ones. Column (3) confirms this picture: binding requirements have no effect at all on the probability of exit to a job that lasts more that six months. On the contrary, non-binding requirements have a positive effect on this probability. It appears that job seekers who decrease their search intensity after receiving the requirement do so by applying less to temporary or bad-quality jobs. Through this channel, they have a higher chance of exiting to a permanent job within the first six months of their unemployment spell.

Given the presented results, we identify a central policy-trade-off concerning the job seeker's short term outcomes: requirement-induced search effort can shorten the duration of unemployment, but only at the cost of job match stability. We can in our setting not answer the long-term

[26] Note that as we observe entries to unemployment until August 2014 and our sample covers entries until December 2012, some of our observations are right censored. This should not affect our results, as this censoring is unaffected by the treatment.

welfare question which asks whether increased exits to temporary jobs improve the individual's long-run employment outcomes (e.g. through stepping-stone mechanism). We leave this important question for future research.

[Insert Table 9]

6.3 Robustness Checks

We now present a set of robustness checks that support the causal interpretation of our findings. We use the main regression on the job finding hazard with durations censored after six months of unemployment to run these checks.

We first want to gain additional support for our assumption that Δ_{s_r} is exogenous, conditional on γ^{s_0}, $\sigma^{s_r,c}$ and π_c. The idea is that one level of s_0 can reveal different unobserved characteristics, depending on the job seeker's labor market situation: one level of s_0 can reveal a high or low intrinsic motivation, depending on the effort which is "standard" for the job seeker's gender or education group. If our identification strategy is able to account for this endogeneity, results should be unchanged when we interact our vector of s_0 with labor market characteristics. In Table 10 , Column (1) recalls our baseline estimates. In Columns (2) and (3), we introduce an interaction between γ_{s_0} and a gender dummy/a vector of educational categories into the baseline regression. Results are unaffected by this.

Column (4) presents a tentative check on the role of benefit sanctions. Previous research, e.g. by Lalive et al. (2005), Abbring et al. (2005), Arni et al. (2013) and Van Ours and Van der Klaauw (2014), shows that benefit sanctions substantially increase job finding. As the treatment intensity Δ_{s_r} increases non-compliance and the incidence of benefit sanctions, it is possible that job search behavior reacts above all to the intermediate outcome of benefit sanctions. Like in these studies, we allow in Column (4) the job finding hazard to shift when job seekers receive a warning on non-compliance detection or a sanction. These are quite obviously endogenous to the treatment and do therefore not represent good control variables. The fact that point estimates are only slightly and not statistically significantly decreased nevertheless points towards the conclusion that requirement constraints do not affect search outcomes only by generating additional benefit sanctions. Note that we are not able to check for the role of the "ex-ante" threat effect of an increased sanction probability.

Table 11 provides additional robustness checks by presenting results for sensitive subgroups. Column (1) recalls our baseline estimates, Columns (2) to (4) exclude observations for which measurement errors might be an issue. Column (2) shows results estimated only with job seekers for whom requirement levels were not imputed (91.8%). Column (3) reports estimates only for job seekers for whom s_0 concerns a month previous to registration at the PES (42.9%). Finally, estimates in Column (4) only contain job seekers who did not have another unemployment spell in the two years previous to entering their current spell (78.5%). For none of these subgroups, our estimates are significantly different from the baseline estimates.

7 Discussion and Conclusion

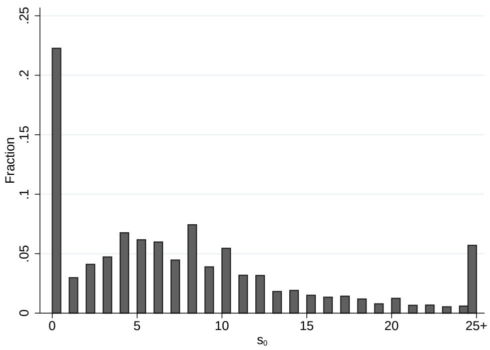
Our analysis has shown that job seekers substantially react to the presence of job search requirements. Job seekers with binding requirements increase their effort to comply with the constraint. In line with theoretical predictions, the elasticity of search effort to the requirement is imperfect: non-compliance rates react strongly to our treatment of interest. This suggests that job seekers may find it beneficial to take the risk of benefit sanctions when the required change in search behavior is large. Indeed, we show that the probability of receiving a benefit sanction reacts strongly to the required increase in effort. Policy makers should have this side effect in mind when designing requirement thresholds. In addition, we have shown that the applications generated by binding requirements lead to an increase in job finding rates, in particular at early stages at the unemployment spell. However, these effects largely come at the cost of job stability. At this stage, we can not conclude on long-term welfare implications of these phenomena.

Finally, our findings indicate that non-binding requirements also affect job search outcomes, which is not in line with the predictions of standard job search theory. After receiving a search requirement, job seekers move their realized search effort towards the requirement threshold, also when their unconstrained search level was significantly higher. This reduction in search effort negatively affects job finding of certain subgroups. Search requirement do not only appear to operate through the threat of sanction enforcement, but also as a reference point, which signals the "right" search intensity.

8 Figures and Tables

Figure 3

(a) Distribution of s_0, entire sample



(b) Distribution of s_0 deemed as insufficient by caseworker (25% of sample)

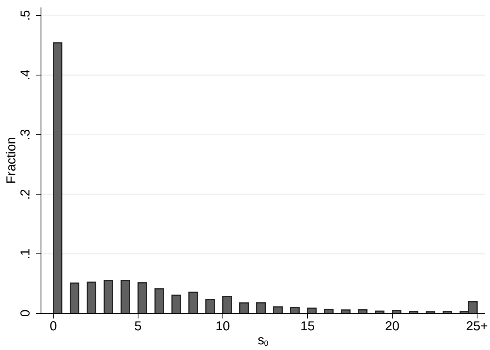
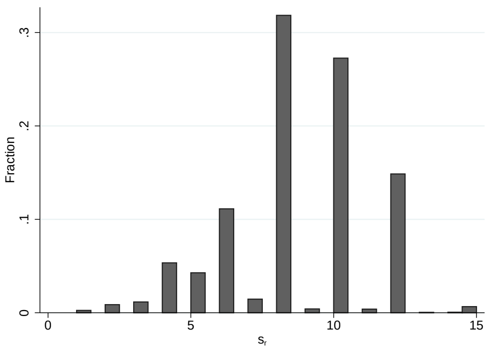
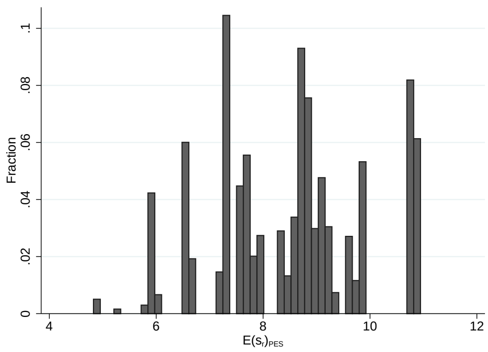


Figure 4

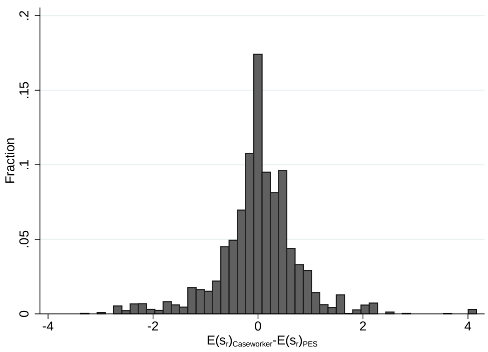
(a) Sample Distribution of s_r



(b) Distribution of $E(s_r)_{PES}$



(c) Deviation of $E(s_r)_{Caseworker}$ from $E(s_r)_{PES}$



(d) Caseworker "Requirement Portfolio" (weighted by no of job seekers)

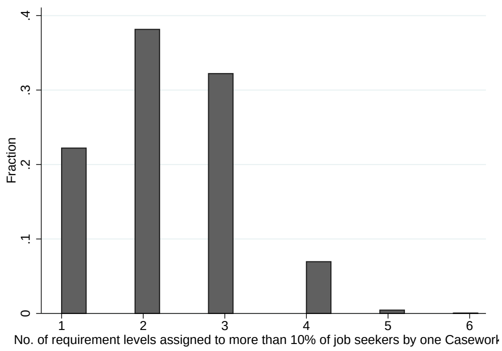


Figure 5: Distribution of $s_1 \mid s_r$

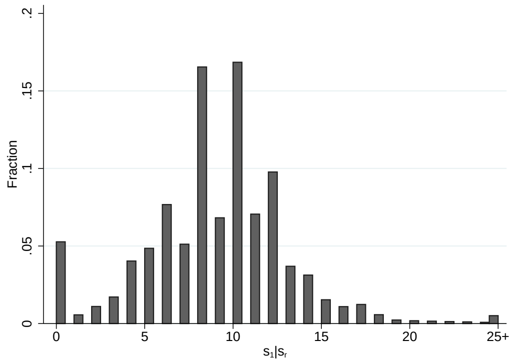


Table 1: Influence of Job Seeker Characteristics on s_r and s_0

	(1) s0	(2) sr	(3) sr | s0
Female	0.349***	-0.068***	-0.066***
	(0.069)	(0.020)	(0.020)
Low Education	-0.560***	0.085***	0.095***
	(0.123)	(0.021)	(0.020)
High Education	0.071	-0.589***	-0.577***
	(0.122)	(0.039)	(0.039)
Age 35-45	0.676***	-0.298***	-0.299***
	(0.063)	(0.022)	(0.023)
Age >45	0.943***	-0.560***	-0.557***
	(0.080)	(0.033)	(0.033)
Service Sector Low	0.698***	0.043	0.035
	(0.110)	(0.035)	(0.034)
Service Sector High	0.927***	0.066**	0.054*
	(0.089)	(0.028)	(0.028)
Professional+ Function	-0.037	-0.175***	-0.171***
	(0.088)	(0.024)	(0.024)
FE	Caseworker	Caseworker	Caseworker, s0
Outcome Mean	7.51	8.50	8.50
N	76404	76404	76404

$*p < 0.10, **p < 0.05, ***p < 0.01$ Standard errors are clustered at the PES level. Reference Categories are: Male, No Education/Unlearned, Age <35, Blue Collar Sector, Support Function.

Figure 6: The Content of s_0

(a) s_0 plotted over s_r of previous spell

(b) Share of non-compliers ($s_1 < s_r$) by s_0

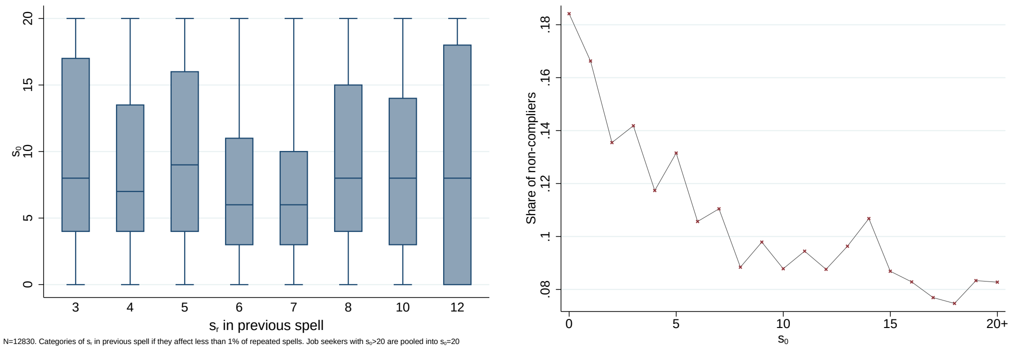


Figure 7: Distribution of Δ_{s_r}

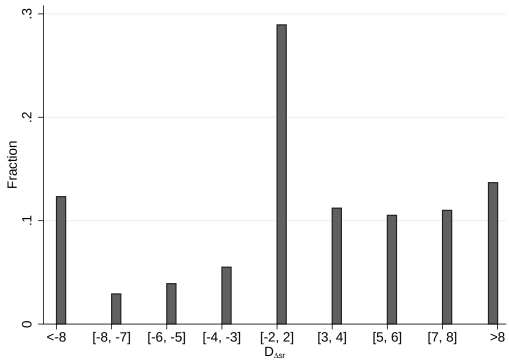
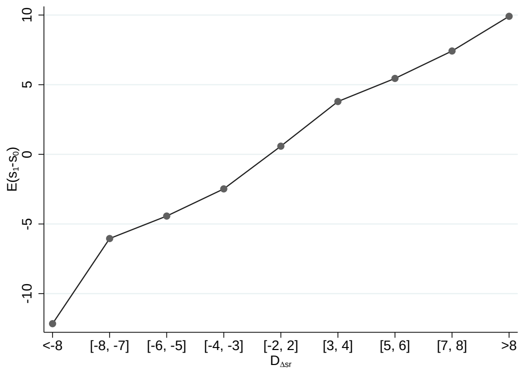


Figure 8

(a) $E(s_1 - s_0)|\Delta s_r$



(b) $E(s_1 - s_r)|\Delta s_r$

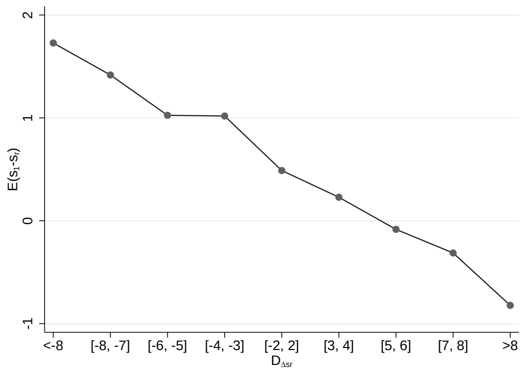


Figure 9: $\hat{\varepsilon} = \hat{s}_r - x'\hat{\beta} - \hat{\pi}_c$ plotted over s_0

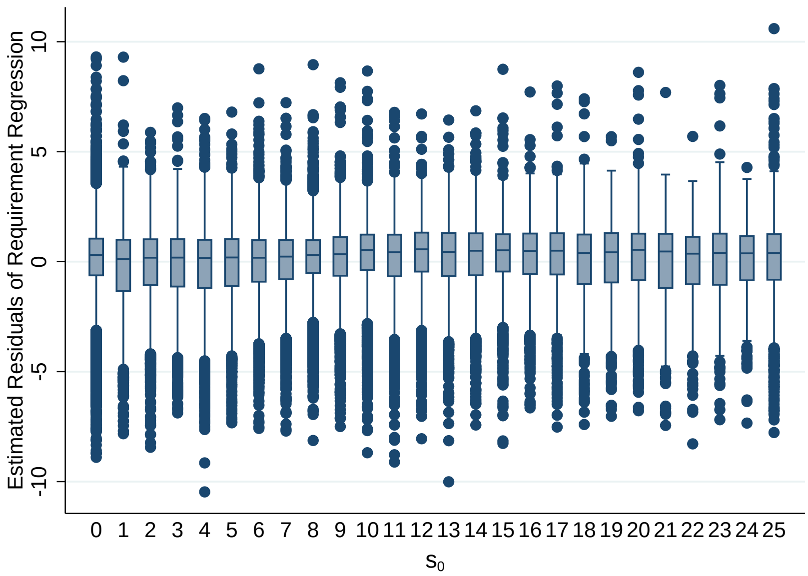


Table 2: "Placebo Policy" Regressions

	(1) Early Second Meeting	(2) Unrelated Sanction (2 Months)
$\Delta_{s_r} < -8$	0.011	-0.002
	(0.016)	(0.007)
$\Delta_{s_r} \epsilon [-8, -7]$	-0.008	0.002
	(0.013)	(0.006)
$\Delta_{s_r} \epsilon [-6, -5]$	0.005	-0.001
	(0.010)	(0.004)
$\Delta_{s_r} \epsilon [-4, -3]$	0.005	0.002
	(0.008)	(0.003)
$\Delta_{s_r} \epsilon [3, 4]$	0.002	0.001
	(0.007)	(0.004)
$\Delta_{s_r} \epsilon [5, 6]$	0.000	0.002
	(0.009)	(0.005)
$\Delta_{s_r} \epsilon [7, 8]$	-0.007	0.004
	(0.012)	(0.006)
$\Delta_{s_r} > 8$	-0.009	0.013
	(0.016)	(0.008)
Outcome Mean	0.164	0.045
N	76404	76404

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r} \epsilon [-2, 2]$. All regressions estimate equation 1 using OLS. They include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, household size, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2. "Early Second Meeting" is coded as one if the difference between the first caseworker meeting and the second scheduled meeting is less than three weeks. "Unrelated Sanction (2 Months)" is coded as one if the job seeker receives a sanction for delayed appearance or absence at a caseworker meeting during the first two months of UE.

Figure 10: $\hat{s}_{r,c}$ plotted over $\bar{s}_{0,c}$

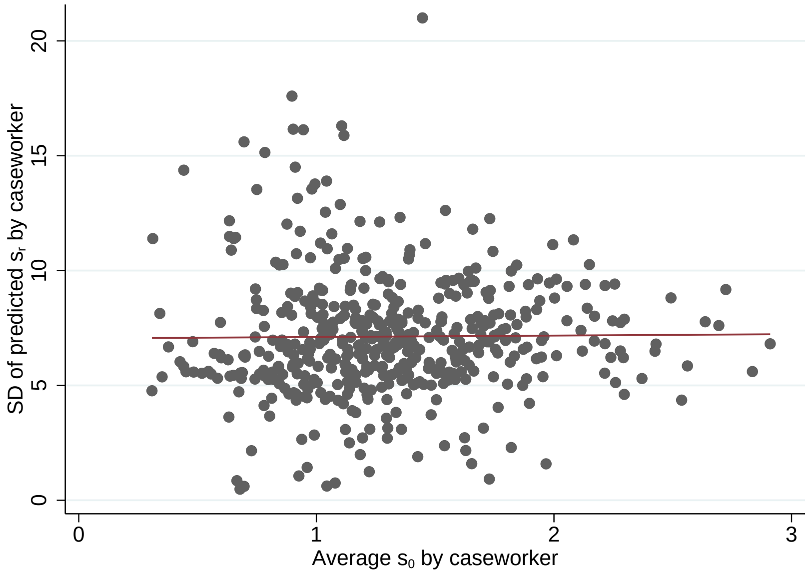


Table 3: Probability of Non-Compliance, Non-Compliance Detection and Occurrence of Benefit Sanction within the first 2 months of UE

	(1) Non-Compliance	(2) Detection	(3) Sanction	(4) Other Sanction
$\Delta_{s_r} < -8$	-0.003	-0.014	-0.006	-0.002
	(0.014)	(0.011)	(0.008)	(0.007)
$\Delta_{s_r} \epsilon [-8, -7]$	-0.001	-0.010	-0.005	0.002
	(0.011)	(0.009)	(0.006)	(0.006)
$\Delta_{s_r} \epsilon [-6, -5]$	0.018**	-0.009	-0.001	-0.001
	(0.008)	(0.007)	(0.005)	(0.004)
$\Delta_{s_r} \epsilon [-4, -3]$	-0.000	-0.008	-0.002	0.002
	(0.006)	(0.005)	(0.004)	(0.003)
$\Delta_{s_r} \epsilon [3, 4]$	0.014**	0.010*	0.008*	0.001
	(0.006)	(0.005)	(0.004)	(0.004)
$\Delta_{s_r} \epsilon [5, 6]$	0.033***	0.019***	0.016***	0.002
	(0.008)	(0.007)	(0.005)	(0.005)
$\Delta_{s_r} \epsilon [7, 8]$	0.042***	0.031***	0.027***	0.004
	(0.010)	(0.010)	(0.008)	(0.006)
$\Delta_{s_r} > 8$	0.040***	0.053***	0.050***	0.012
	(0.013)	(0.013)	(0.010)	(0.008)
Outcome Mean	0.123	0.093	0.058	0.045
N	76404	76404	76404	76404

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r} \epsilon [-2, 2]$. All regressions estimate equation 1 using OLS. They include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, household size, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2. "Non-Compliance" is coded as one if the job seeker submits less than 3/4 of required applications in the first month under the requirement constraint. "Detection" is coded as one if the job seeker receives a warning on non-compliance detection during the first two months of UE. "Sanction" is coded as one if the job seeker receives a benefit sanction for a non-compliance that was detected during the first two months of UE. "Other Sanction" is coded as one if the job seeker receives a sanction for delayed appearance or absence at a caseworker meeting during the first two months of UE.

Figure 11: Illustration of Results in Table 3, Columns (2) and (3) (with 90%CIs)

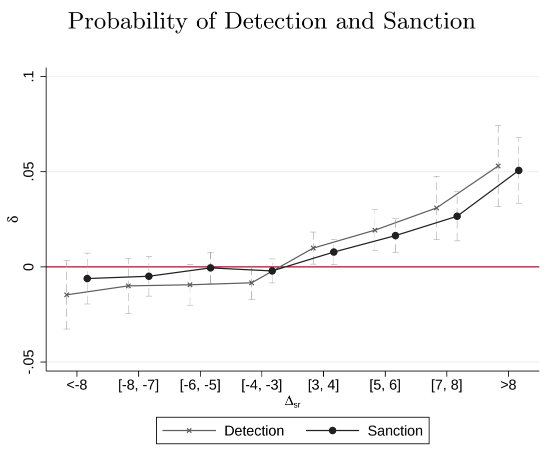


Table 4: Effects on Job Finding Hazard, Censored at Different Durations

	6 months			3 months		24 months	
	(1) Coeff	(2) Coeff	(3) Marg. Effect	(4) Coeff	(5) Marg. Effect	(6) Coeff	(7) Marg. Effect
$\Delta_{s_r} < -8$	-0.017	-0.050	-0.007	-0.310***	-0.024	-0.000	0.000
	(0.074)	(0.072)		(0.093)		(0.063)	
$\Delta_{s_r}\epsilon[-8,-7]$	0.023	-0.013	-0.002	-0.159*	-0.013	-0.049	-0.009
	(0.064)	(0.063)		(0.084)		(0.058)	
$\Delta_{s_r}\epsilon[-6,-5]$	-0.032	-0.047	-0.007	-0.139*	-0.012	-0.019	-0.004
	(0.056)	(0.056)		(0.079)		(0.049)	
$\Delta_{s_r}\epsilon[-4,-3]$	-0.036	-0.046	-0.007	-0.089	-0.008	-0.026	-0.005
	(0.038)	(0.036)		(0.054)		(0.029)	
$\Delta_{s_r}\epsilon[3,4]$	0.141***	0.136***	0.021	0.191***	0.018	0.101***	0.019
	(0.038)	(0.035)		(0.050)		(0.030)	
$\Delta_{s_r}\epsilon[5,6]$	0.181***	0.173***	0.027	0.315***	0.031	0.129***	0.025
	(0.056)	(0.052)		(0.069)		(0.045)	
$\Delta_{s_r}\epsilon[7,8]$	0.259***	0.251***	0.041	0.505***	0.054	0.172***	0.033
	(0.069)	(0.063)		(0.080)		(0.056)	
$\Delta_{s_r} > 8$	0.306***	0.300***	0.050	0.542***	0.058	0.198**	0.039
	(0.095)	(0.087)		(0.117)		(0.078)	
X_i	No	Yes		Yes		Yes	
Observations	76404	76404		76404		76404	
Exits	34065	34065		14027		54112	

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r}\epsilon[-2,2]$. Columns (1), (2), (4) and (6) estimate Equation 2 using Maximum Likelihood with durations censored after 180/90/730 days. Columns (3), (5) and (7) report the difference between the survivor function with treatment and the counterfactual survivor function without treatment at the sample average. All columns are based in regressions that include all identifying fixed effects (discussed in section 5). In columns (2) to (7), regressions include all covariates, which control for gender, age, immigration status, civil status, household size, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2.

Figure 13: Illustration of Results in Table 4, (with 90%CIs)

(a) Effects on Job Finding Hazard (Columns (2), (4) and (6))

(b) Marginal Effects on Job Finding Probability (Columns (3), (5) and (7))

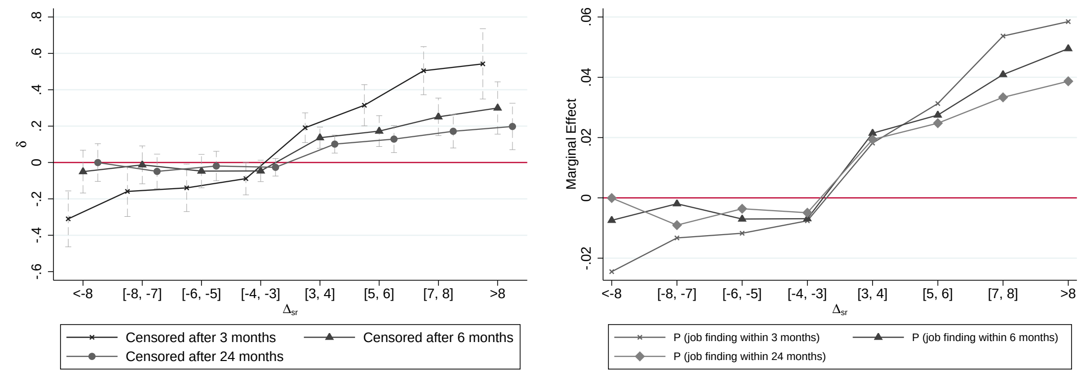


Table 5: Duration to Job Finding, Subgroup Analysis: Gender and Education

	(1) Female	(2) Male	(3) Unlearned	(4) Apprenticeship	(5) High School+
$\Delta_{s_r} < -8$	-0.072	-0.041	-0.055	0.007	-0.016
	(0.112)	(0.087)	(0.121)	(0.103)	(0.165)
$\Delta_{s_r} \epsilon [-8, -7]$	0.063	-0.068	-0.048	0.073	-0.041
	(0.104)	(0.073)	(0.109)	(0.082)	(0.127)
$\Delta_{s_r} \epsilon [-6, -5]$	0.012	-0.105*	-0.034	-0.021	-0.079
	(0.079)	(0.061)	(0.092)	(0.067)	(0.108)
$\Delta_{s_r} \epsilon [-4, -3]$	0.038	-0.096**	-0.084	-0.030	0.069
	(0.053)	(0.041)	(0.059)	(0.047)	(0.071)
$\Delta_{s_r} \epsilon [3, 4]$	0.234***	0.071**	0.186***	0.113***	0.006
	(0.057)	(0.036)	(0.056)	(0.042)	(0.073)
$\Delta_{s_r} \epsilon [5, 6]$	0.275***	0.103**	0.218***	0.114**	0.108
	(0.081)	(0.050)	(0.083)	(0.056)	(0.085)
$\Delta_{s_r} \epsilon [7, 8]$	0.408***	0.146**	0.258***	0.175**	0.216**
	(0.094)	(0.061)	(0.095)	(0.070)	(0.108)
$\Delta_{s_r} > 8$	0.546***	0.141*	0.336**	0.215**	0.126
	(0.138)	(0.077)	(0.139)	(0.090)	(0.143)
Observations	30890	45514	30601	32806	12997
Exits	20520	13368	13004	15489	5395

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r} \epsilon [-2, 2]$. Regressions estimate equation 2 using Maximum Likelihood, with durations censored after 180 days of unemployment. They include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2. "Unlearned" job seekers have neither an educational nor a practical formal degree. Job seekers with an apprenticeship followed a practical education. Job seeker with "High School+" have at least the highest Swiss high school degree ("Abitur").

Figure 14: Illustration of Results in Table 5 (with 90%CIs)

(a) Gender

(b) Education

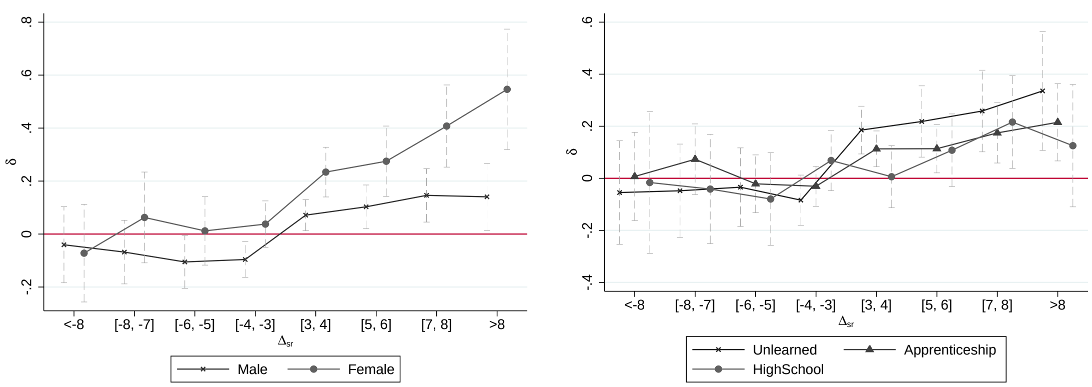


Table 6: Duration to Job Finding, Subgroup Analysis: Occupation and Function in Last Job

	(1) Blue Collar	(2) Service Low	(3) Service High	(4) Support Function	(5) Professional+ Function
$\Delta_{s_r} < -8$	0.018	-0.001	0.006	-0.070	0.023
	(0.122)	(0.130)	(0.099)	(0.113)	(0.079)
$\Delta_{s_r}\epsilon[-8,-7]$	0.074	-0.031	0.070	-0.060	0.066
	(0.101)	(0.128)	(0.079)	(0.112)	(0.064)
$\Delta_{s_r}\epsilon[-6,-5]$	-0.032	-0.051	0.024	-0.112	0.022
	(0.066)	(0.097)	(0.069)	(0.086)	(0.052)
$\Delta_{s_r}\epsilon[-4,-3]$	-0.021	-0.084	0.043	-0.100*	-0.002
	(0.053)	(0.075)	(0.047)	(0.055)	(0.034)
$\Delta_{s_r}\epsilon[3,4]$	-0.031	0.270***	0.115**	0.200***	0.069*
	(0.045)	(0.060)	(0.046)	(0.049)	(0.036)
$\Delta_{s_r}\epsilon[5,6]$	-0.019	0.288***	0.118**	0.250***	0.076
	(0.060)	(0.090)	(0.056)	(0.075)	(0.046)
$\Delta_{s_r}\epsilon[7,8]$	-0.057	0.459***	0.217***	0.368***	0.121**
	(0.078)	(0.097)	(0.069)	(0.086)	(0.054)
$\Delta_{s_r} > 8$	-0.048	0.601***	0.185**	0.493***	0.105
	(0.099)	(0.145)	(0.091)	(0.126)	(0.073)
Observations	26382	22477	27545	34169	42235
Exits	12135	9251	12502	19786	14102

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r}\epsilon[-2,2]$. Regressions estimate equation 2 using Maximum Likelihood, with durations censored after 180 days of unemployment. They include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2.

Figure 15: Illustration of Results in Table 6 (with 90%CIs)

(a) Occupation

(b) Function in Last Job

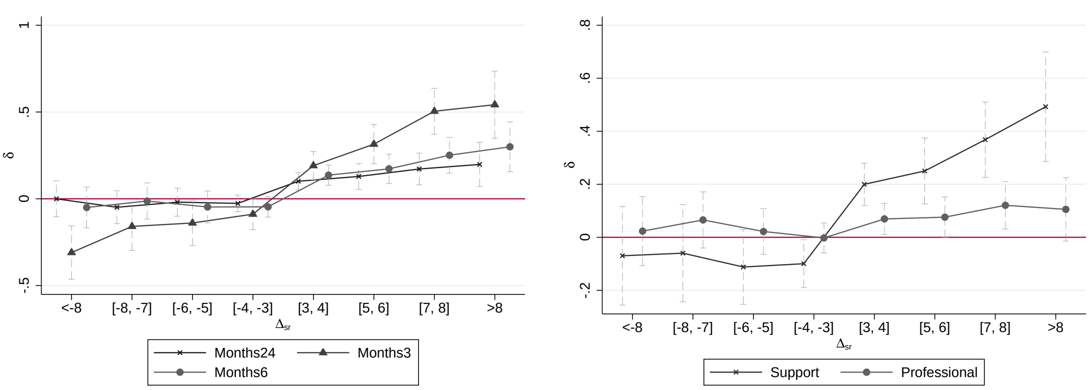


Table 7: Duration to Job Finding, Subgroup Analysis: Vacancy Rate

	(1) Low Vacancy Rate	(2) Medium Vacancy Rate	(3) High Vacancy Rate
$\Delta_{s_r} < -8$	-0.005	-0.216	-0.084
	(0.134)	(0.138)	(0.108)
$\Delta_{s_r}\epsilon[-8,-7]$	0.002	-0.069	-0.042
	(0.108)	(0.116)	(0.088)
$\Delta_{s_r}\epsilon[-6,-5]$	-0.057	-0.184*	-0.009
	(0.076)	(0.103)	(0.082)
$\Delta_{s_r}\epsilon[-4,-3]$	-0.065	-0.099	-0.034
	(0.058)	(0.067)	(0.054)
$\Delta_{s_r}\epsilon[3,4]$	0.109**	0.134**	0.143***
	(0.053)	(0.058)	(0.049)
$\Delta_{s_r}\epsilon[5,6]$	0.074	0.163**	0.242***
	(0.067)	(0.075)	(0.074)
$\Delta_{s_r}\epsilon[7,8]$	0.146*	0.250**	0.330***
	(0.080)	(0.099)	(0.088)
$\Delta_{s_r} > 8$	0.149	0.348***	0.380***
	(0.113)	(0.132)	(0.125)
Observations	25402	25428	25560
Exits	10480	10240	13164

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r}\epsilon[-2,2]$. Regressions estimate equation 2 using Maximum Likelihood, with durations censored after 180 days of unemployment. They include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2. Vacancy rates are calculated as the ration of posted vacancies over job seekers on a month-region basis. They are assigned to the job seeker depending on his month of registration at the PES. "Low", "medium" and "high" vacancy rates are understood in relative terms, i.e. we divide the distribution of vacancy rates by 3 to assign job seekers one of these categories. The mean vacancy rate in our sample is .086.

Figure 16: Illustration of Results in Table 7 (with 90%CIs)

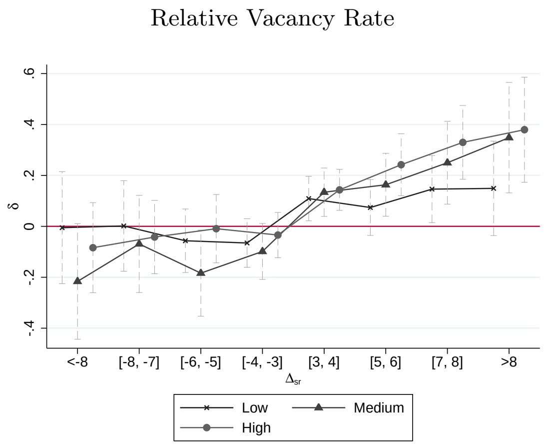


Table 8: Probability of Job Finding and Recurrence to Unemployment within (6/12) Months

	(1) 6 Months, All	(2) 6 Months, Unlearned	(3) 12 Months, All	(4) 12 Months, Unlearned
$\Delta_{s_r} < -8$	-0.050***	-0.081***	-0.000	0.002
	(0.019)	(0.029)	(0.021)	(0.029)
$\Delta_{s_r}\epsilon[-8,-7]$	-0.042***	-0.079***	-0.021	-0.034
	(0.016)	(0.025)	(0.016)	(0.023)
$\Delta_{s_r}\epsilon[-6,-5]$	-0.033***	-0.067***	-0.002	-0.033*
	(0.012)	(0.021)	(0.013)	(0.019)
$\Delta_{s_r}\epsilon[-4,-3]$	-0.021**	-0.031**	0.001	-0.000
	(0.009)	(0.014)	(0.009)	(0.015)
$\Delta_{s_r}\epsilon[3,4]$	0.021***	0.039***	0.006	0.005
	(0.008)	(0.012)	(0.008)	(0.013)
$\Delta_{s_r}\epsilon[5,6]$	0.023**	0.054***	-0.004	-0.000
	(0.011)	(0.017)	(0.010)	(0.016)
$\Delta_{s_r}\epsilon[7,8]$	0.047***	0.091***	0.004	0.012
	(0.015)	(0.021)	(0.014)	(0.021)
$\Delta_{s_r} > 8$	0.063***	0.112***	0.001	0.007
	(0.020)	(0.028)	(0.018)	(0.028)
Outcome Mean	0.138	0.167	0.267	0.341
N	76404	30601	76404	30601

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r}\epsilon[-2,2]$. All regressions estimate Equation 1 using OLS and include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2. In addition, the incidence of benefit sanctions and the duration of unemployment in the first spell are controlled for. The outcome is coded as 1 if the job seeker exits employment and recurs to unemployment after within the following (6/12) months. "Unlearned" job seekers have neither an educational nor a practical formal degree.

Figure 17: Illustration of Results in Table 8, Column (1) (with 90%CIs)

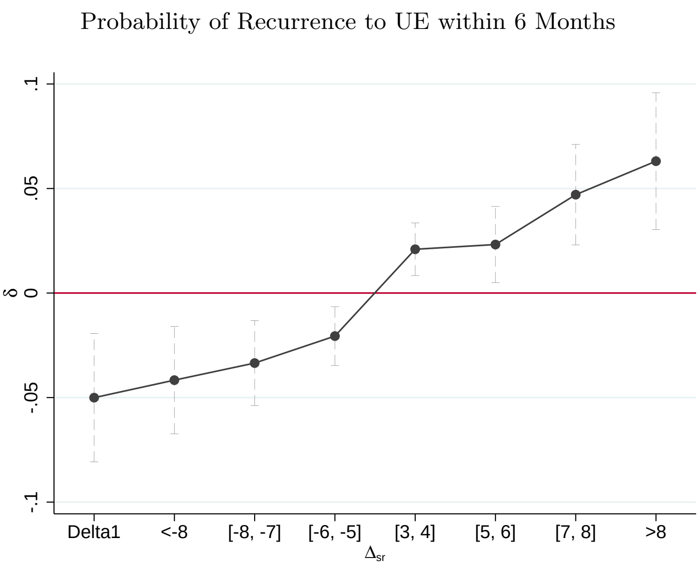


Table 9: Probability of Job Finding within 6 Month, with and without Recurrence within 6 Months

	(1) Job Finding	(2) Job Finding and Recurrence	(3) Job Finding and No Recurrence
$\Delta_{s_r} < -8$	0.025	-0.036**	0.062**
	(0.023)	(0.018)	(0.029)
$\Delta_{s_r} \epsilon[-8,-7]$	0.026	-0.033**	0.059***
	(0.017)	(0.016)	(0.022)
$\Delta_{s_r} \epsilon[-6,-5]$	0.013	-0.030**	0.043***
	(0.013)	(0.012)	(0.016)
$\Delta_{s_r} \epsilon[-4,-3]$	0.002	-0.019**	0.021*
	(0.009)	(0.009)	(0.012)
$\Delta_{s_r} \epsilon[3,4]$	0.023***	0.025***	-0.003
	(0.008)	(0.008)	(0.011)
$\Delta_{s_r} \epsilon[5,6]$	0.032***	0.042***	-0.010
	(0.011)	(0.012)	(0.015)
$\Delta_{s_r} \epsilon[7,8]$	0.033**	0.059***	-0.026
	(0.015)	(0.016)	(0.021)
$\Delta_{s_r} > 8$	0.045**	0.085***	-0.040
	(0.019)	(0.023)	(0.028)
Outcome Mean	0.379	0.073	0.305
N	76404	76404	76404

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r}\epsilon[-2,2]$. All regressions estimate Equation 1 using OLS and include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2. In addition, the incidence of benefit sanctions and the duration of unemployment in the first spell are controlled for.

Table 10: Robustness Check: additional controls

	(1) Baseline Results	(2) s0*Education	(3) s0*Gender	(4) Detection and Sanction
Duration				
$\Delta_{s_r} < -8$	-0.050	-0.046	-0.054	-0.044
	(0.072)	(0.072)	(0.072)	(0.071)
$\Delta_{s_r} \epsilon [-8, -7]$	-0.013	-0.017	-0.015	-0.006
	(0.063)	(0.063)	(0.064)	(0.063)
$\Delta_{s_r} \epsilon [-6, -5]$	-0.047	-0.045	-0.050	-0.043
	(0.056)	(0.057)	(0.056)	(0.055)
$\Delta_{s_r} \epsilon [-4, -3]$	-0.046	-0.041	-0.048	-0.044
	(0.036)	(0.036)	(0.036)	(0.036)
$\Delta_{s_r} \epsilon [3, 4]$	0.136***	0.141***	0.137***	0.131***
	(0.035)	(0.036)	(0.036)	(0.035)
$\Delta_{s_r} \epsilon [5, 6]$	0.173***	0.177***	0.173***	0.163***
	(0.052)	(0.052)	(0.052)	(0.051)
$\Delta_{s_r} \epsilon [7, 8]$	0.251***	0.257***	0.251***	0.238***
	(0.063)	(0.064)	(0.063)	(0.062)
$\Delta_{s_r} > 8$	0.300***	0.308***	0.301***	0.286***
	(0.087)	(0.089)	(0.088)	(0.087)
Observations	76404	76404	76404	76404
Exits	33888	33888	33888	33888

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r} \epsilon [-2, 2]$. All regressions estimate Equation 2 using Maximum Likelihood, with durations censored after 180 days of unemployment. They include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2. In Column (2), γ^{s0} is interacted with indicators for no education, apprenticeship and high education. In Column (3), γ^{s0} is interacted with a gender dummy. In Column (4), the detection and the enforcement of a beneft sanction are introduced as events that are allowed to shift the job finding hazard.

Table 11: Robustness Check: Subsample Analysis

	(1) Baseline Results	(2) Not Imputed	(3) Before Registration	(4) First UE
$\Delta_{s_r} < -8$	-0.050	-0.098	0.033	-0.068
	(0.072)	(0.076)	(0.108)	(0.095)
$\Delta_{s_r} \epsilon [-8, -7]$	-0.013	-0.061	-0.000	0.033
	(0.063)	(0.066)	(0.082)	(0.080)
$\Delta_{s_r} \epsilon [-6, -5]$	-0.047	-0.094	-0.093	-0.048
	(0.056)	(0.059)	(0.081)	(0.065)
$\Delta_{s_r} \epsilon [-4, -3]$	-0.046	-0.091**	-0.066	-0.022
	(0.036)	(0.038)	(0.047)	(0.045)
$\Delta_{s_r} \epsilon [3, 4]$	0.136***	0.162***	0.082*	0.137***
	(0.035)	(0.038)	(0.048)	(0.042)
$\Delta_{s_r} \epsilon [5, 6]$	0.173***	0.202***	0.157**	0.174***
	(0.052)	(0.055)	(0.065)	(0.057)
$\Delta_{s_r} \epsilon [7, 8]$	0.251***	0.284***	0.247***	0.261***
	(0.063)	(0.067)	(0.078)	(0.069)
$\Delta_{s_r} > 8$	0.300***	0.372***	0.259**	0.284***
	(0.087)	(0.090)	(0.112)	(0.092)
Observations	76404	70106	32637	59989
Exits	33888	29798	15790	24748

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. The reference category is $\Delta_{s_r} \epsilon [-2, 2]$. All regressions estimate Equation 2 using Maximum Likelihood, with durations censored after 180 days of unemployment. They include all identifying fixed effects (discussed in section 5) and all covariates, which control for gender, age, immigration status, civil status, education, employment and unemployment history, quarter and year of UE entry. Summary statistics on all explanatory variables can be found in Appendix A.2.

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A Appendix

A.1 Data Appendix

A.1.1 Sampling criteria for job search data

The database on search monitoring contains one entry for each calender month during which the job seeker had a legal obligation to search. There are several reasons for which job seekers may be unaffected or only partly affected by this obligation during their spell. We want to exclude these job seekers, as our analysis focuses on requirement effects for individuals who are fully subject to the search obligation. To this purpose, we make the following sampling restrictions and plausability assumptions:

First, job seekers are by definition unaffected by the search obligation if they exit unemployment within one month of unemployment or before their first caseworker meeting takes place (in sum 8.9%).[27] We also exclude job seekers for whom no first caseworker meeting is reported during the first 90 days of unemployment (1.8%), as these are most likely special cases. In addition, we exclude job seekers whose previous unemployment spell ended less than a month previous to their current registration (1.2%). These are again most likely particular cases to which the institutional setting underlying our analysis does not apply.

For the remaining sample, there is still the possibility that job seekers are systematically exempted from the monitoring regime because they qualified for special exemption reasons (maternity, preparation of self-employment, participation at a long-run training program etc.) or because they exited unemployment before the monitoring regime became effective. In order to assess whether a job seeker was monitored during a given search period, we use a variable that reports for each period whether the effort was monitored and whether the job seeker was exempted from the requirement of active job search in that month. We define an individual's unemployment spell as systematically affected by the monitoring regime if at least two search periods are monitored up to the third month of unemployment (one of these two periods may refer to the month previous to the month of registration). We exclude individuals who do not meet this criterion (7.46%).[28]

Further, individuals are only relevant for our intensive margin analysis if both their unconstrained and their constrained search was monitored. We exclude individuals who did not become subject to search monitoring under the requirement constraint, i.e. who do not report a monitored entry s_1. These are job seekers whose last monitored entry refers to the month previous to their first meeting with the caseworker (2.06%). Conversely, we also exclude individuals whose pre-requirement search effort s_0 was not monitored, i.e. whose first monitored entry refers to the month following their first meeting with the caseworker (2.2%).

As both the monitoring of pre-requirement effort s_0 and the monitoring of constrained effort $s_1 \mid s_r$ are prescribed by law, individuals for whom one of these two entries is not monitored are most likely exempted for special cases. We therefore consider them as being irrelevant for our analysis.

[27] We define the first caseworker meeting as the first completed meeting of at least 30 minutes. An exception is Tessin, for which the meeting duration is not reported. We assume here that the first realized meeting after the date of registration is the first caseworker meeting.

[28] All percentages refer to the sample that was relevant before the sampling criterion applied.

A.1.2 Definition of t_0 and t_1 and Extraction of Effort Variables

For the remaining sample, we define t_0 as the first monitored search period (we restrict it to be the earliest the month before entry into unemployment). We then define t_1 as the first monitored search period following the month of the first meeting. If the last monitored search period is the month of the first meeting, we define it as t_1. This definition implies that t_1 is not always the month following right after t_0. However, it reduces the number of search periods in which it is unclear whether the search effort was restricted or unrestricted. It thereby ensures with a high degree of certainty that the effort provided in t_0 was indeed unrestricted and the effort provided in t_1 was indeed restricted by a requirement. If t_0 or t_1 is in the month of the first caseworker meeting, some uncertainty remains on whether it refers to restricted or to unrestricted effort. We show that our results are unaffected to the exclusion of such cases, which represent a minority.

We then extract the effort variables as follows: s_0 is the effort reported for period t_0, s_1 is the effort reported for period t_1. s_r is the required effort reported reported at period t_1. If this entry is missing at t_1, we define s_r as the maximum required effort reported for any search period over the spell. As there are few changes in s_r over the spell, this seems a plausible assumption. If s_r is not reported over the entire spell, it is categorized as missing. We drop 304 (0.4%) observations because they belong to a caseworker who reports more than 90% missing requirement. For the other 8.24% of job seekers for whom s_r is missing, we impute the requirement threshold. This imputation can be done in a straight-forward way.

A.1.3 Imputation of Missing Requirement Levels

In the first step of our imputation of the few missing requirement entries, we predict an individual's requirement from a linear regression on the main socio-demographics gender, education, occupation, age and a caseworker effect. We round the linear prediction $\hat{s}_r$ to the next integer and adjust it in order to account for the caseworker's requirement setting habits: each caseworker has a very limited set of requirement thresholds that she distributes to more than 10% of her cases. It is highly plausible that job seekers with missing $\hat{s}_r$ were assigned to one of these levels. To account for this, we proceed as follows:

1. We compile for each caseworker a candidate list of plausible requirement thresholds $C_{CW} := \{s_i | \text{Caseworker assigns } s_i \text{ in at least 10\% of her cases}\}$
2. We choose $s_C \in C_{CW}$ with $|s_C - \hat{s}_r|$ minimal. If S_C is unique, we accept this value as the imputed prediction.
3. If there are two plausible requirements S_C with the same absolute distance to the predicted value, we choose the one that the caseworker assigns more frequently.

In our empirical analysis, we show that imputed values do not drive our results, as our results are robust to their exclusion from the estimation sample.

A.2 Additional Graphs and Tables

Figure 18: Effects of s_0 and $s_r - med_c(s_r)$ on Job Finding Hazard (durations censored after 6 months, c.f. table 4, column (2))

(a) Coefficients of γ^{s_0} (Baseline: s_0=0) (b) Coefficients of $\sigma^{s_r,c}$ (Baseline: $s_r - med_c(s_r)$=0)

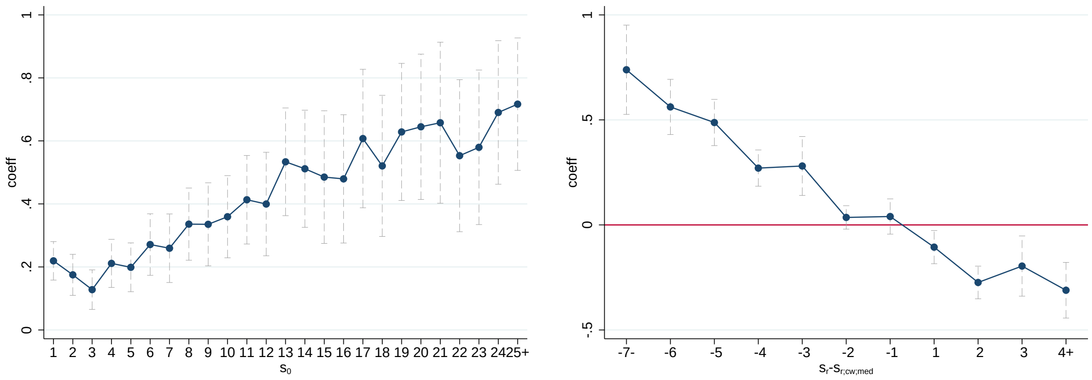

Figure 19: Empirical Job Finding Hazard (monthly intervals)

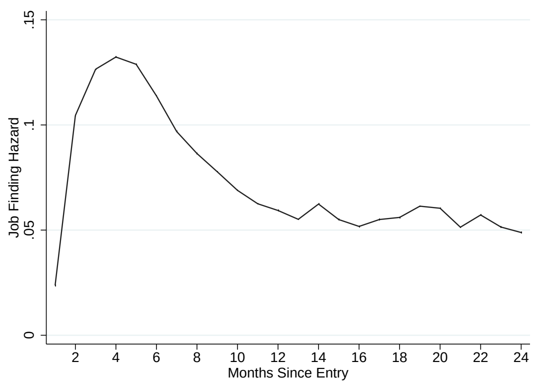

Table 12: Summary statistics: Effort Variables (Vectors D_{Δ}, γ_{s0} and σ_{sr})

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
$\Delta_{s_r} < -8$	0.123	0.329	0	1
$\Delta_{s_r} \epsilon [-8, -7]$	0.029	0.168	0	1
$\Delta_{s_r} \epsilon [-6, -5]$	0.039	0.194	0	1
$\Delta_{s_r} \epsilon [-4, -3]$	0.055	0.228	0	1
$\Delta_{s_r} \epsilon [-2, 2]$	0.289	0.453	0	1
$\Delta_{s_r} \epsilon [3, 4]$	0.112	0.316	0	1
$\Delta_{s_r} \epsilon [5, 6]$	0.105	0.307	0	1
$\Delta_{s_r} \epsilon [7, 8]$	0.11	0.313	0	1
$\Delta_{s_r} > 8$	0.137	0.344	0	1
s_0=0	0.223	0.416	0	1
s_0=1	0.03	0.17	0	1
s_0=2	0.041	0.198	0	1
s_0=3	0.047	0.212	0	1
s_0=4	0.068	0.251	0	1
s_0=5	0.062	0.241	0	1
s_0=6	0.06	0.237	0	1
s_0=7	0.045	0.207	0	1
s_0=8	0.074	0.262	0	1
s_0=9	0.039	0.193	0	1
s_0=10	0.055	0.227	0	1
s_0=11	0.032	0.176	0	1
s_0=12	0.032	0.175	0	1
s_0=13	0.018	0.134	0	1
s_0=14	0.019	0.137	0	1
s_0=15	0.015	0.122	0	1
s_0=16	0.013	0.115	0	1
s_0=17	0.014	0.119	0	1
s_0=18	0.012	0.108	0	1
s_0=19	0.008	0.088	0	1
s_0=20	0.012	0.111	0	1
s_0=21	0.007	0.081	0	1
s_0=22	0.007	0.082	0	1
s_0=23	0.005	0.073	0	1
s_0=24	0.006	0.077	0	1
$s_0 \geqslant 25$	0.057	0.232	0	1
$s_r - s_{r,cw,med} \leqslant -7$	0.007	0.082	0	1
$s_r - s_{r,cw,med} = -6$	0.013	0.114	0	1
$s_r - s_{r,cw,med} = -5$	0.021	0.142	0	1
$s_r - s_{r,cw,med} = -4$	0.067	0.25	0	1
$s_r - s_{r,cw,med} = -3$	0.028	0.166	0	1
$s_r - s_{r,cw,med} = -2$	0.105	0.307	0	1
$s_r - s_{r,cw,med} = -1$	0.027	0.163	0	1
$s_r - s_{r,cw,med} = 1$	0.025	0.155	0	1
$s_r - s_{r,cw,med} = 2$	0.099	0.299	0	1
$s_r - s_{r,cw,med} = 3$	0.006	0.078	0	1
$s_r - s_{r,cw,med} \geqslant 4$	0.025	0.155	0	1
$s_r - s_{r,cw,med} = 0$	0.577	0.494	0	1
N		76404		

Table 13: Summary statistics: Timing of Requirement Policy (Vector η_t)

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Weeks between registration and 1st meeting (rounded):1	0.147	0.354	0	1
Weeks between registration and 1st meeting (rounded):3	0.193	0.395	0	1
Weeks between registration and 1st meeting (rounded):4	0.126	0.332	0	1
Weeks between registration and 1st meeting (rounded):5	0.092	0.289	0	1
Weeks between registration and 1st meeting (rounded):6	0.06	0.237	0	1
Weeks between registration and 1st meeting (rounded):7	0.038	0.191	0	1
Weeks between registration and 1st meeting (rounded):8	0.025	0.155	0	1
Weeks between registration and 1st meeting (rounded):9	0.019	0.137	0	1
Weeks between registration and 1st meeting (rounded):10	0.014	0.116	0	1
Weeks between registration and 1st meeting (rounded):11	0.009	0.092	0	1
Weeks between registration and 1st meeting (rounded):12	0.006	0.078	0	1
Weeks between registration and 1st meeting (rounded):13	0.002	0.048	0	1
Weeks between registration and 1st meeting (rounded):0	0.016	0.127	0	1
t_0-month of registration=-1	0.427	0.495	0	1
t_0-month of registration=1	0.104	0.305	0	1
t_0-month of registration=2	0.014	0.119	0	1
t_0-month of registration $= 0$	0.454	0.498	0	1
t_0-month of availability < 0	0.763	0.425	0	1
t_0-month of availability > 0	0.02	0.142	0	1
t_0-month of availability $= 0$	0.217	0.412	0	1
N		76404		

Table 14: Summary statistics: Covariates (Vector X_i)

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Female	0.404	0.491	0	1
Age	34.736	10.143	20	55
Non-Swiss nationality	0.457	0.498	0	1
Non-permanent resident	0.258	0.437	0	1
Education: obligatory schooling	0.251	0.433	0	1
Education: short	0.05	0.218	0	1
Education: high school degree	0.073	0.26	0	1
Education: university of applied science	0.044	0.206	0	1
Education: university	0.053	0.223	0	1
Education: missing information	0.1	0.3	0	1
Education: apprencticeship	0.429	0.495	0	1
Function in last job: self-employed	0.002	0.048	0	1
Function in last job: management	0.026	0.16	0	1
Function in last job: support	0.447	0.497	0	1
Function in last job: professional	0.524	0.499	0	1
Last profession: food & agriculture	0.032	0.175	0	1
Last profession: raw material preparation	0.014	0.116	0	1
Last profession: production (blue collar)	0.123	0.328	0	1
Last profession: electro & watches	0.006	0.077	0	1
Last profession: chemistry	0.003	0.055	0	1
Last profession: engineers, technicians	0.018	0.132	0	1
Last profession: informatics	0.018	0.131	0	1
Last profession: construction	0.136	0.343	0	1
Last profession: sales	0.104	0.305	0	1
Last profession: tourism,communication	0.01	0.101	0	1
Last profession: transportation	0.034	0.181	0	1
Last profession: banking, trust & insurance	0.012	0.108	0	1
Last profession: gastronomy	0.204	0.403	0	1
Last profession: cleaning & pers service	0.035	0.183	0	1
Last profession: management & hr	0.033	0.178	0	1
Last profession: security & law	0.01	0.101	0	1
Last profession: jouralism & arts	0.013	0.111	0	1
Last profession: social occupations	0.012	0.109	0	1
Last profession: education	0.011	0.104	0	1
Last profession: science	0.008	0.091	0	1
Last profession: health	0.033	0.179	0	1
Last profession: others (skilled)	0.057	0.231	0	1
Last profession: missing information	0.001	0.027	0	1
Last profession: office & admin	0.074	0.261	0	1
Number of past UE spells	0.878	1.151	0	5
Insured earnings (CHF per month): $\leqslant$1500	0.048	0.213	0	1
Insured earnings (CHF per month): >1500,$\leqslant$2000	0.029	0.169	0	1
Insured earnings (CHF per month): >2000,$\leqslant$2500	0.039	0.193	0	1
Insured earnings (CHF per month): >2500,$\leqslant$3000	0.057	0.231	0	1
Insured earnings (CHF per month): >3000,$\leqslant$3500	0.104	0.305	0	1
Insured earnings (CHF per month): >4000,$\leqslant$4500	0.132	0.339	0	1
Insured earnings (CHF per month): >4500,$\leqslant$5000	0.13	0.337	0	1
Insured earnings (CHF per month): >5000,$\leqslant$5500	0.105	0.307	0	1
Insured earnings (CHF per month): >5500,$\leqslant$6000	0.069	0.254	0	1
Insured earnings (CHF per month): >6000	0.143	0.35	0	1
Insured earnings (CHF per month): >3500,$\leqslant$4000	0.144	0.352	0	1
Potential benefit duration: $\leqslant$90	0.048	0.215	0	1
Potential benefit duration: >90,$\leqslant$260	0.36	0.48	0	1
Potential benefit duration: >400,$\leqslant$520	0.022	0.146	0	1
Potential benefit duration: >260,$\leqslant$400	0.570	0.495	0	1
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