The Effects of Targeting Effort: Evidence from Binding and Non-Binding Job Search Requirements

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Abstract

This paper provides novel evidence on how individual effort targets affect real-world labor market outcomes. Using unique large-scale register data, we estimate the returns to effort changes imposed by job search requirements in UI. Quasi-randomness in the job seeker-caseworker match allows identifying the effects of binding and non-binding requirements. A requirement is binding to job seekers who provided less effort before requirement imposition. We find that binding requirements raise job finding. The estimated effect is concave with respect to the required effort increase, suggesting decreasing marginal returns to induced search. Effects on job finding come at the cost of increased non-compliance and decreased job stability. Surprisingly, non-binding requirements also affect search outcomes – by reducing the quantity of effort for the benefit of higher job match stability. This suggests that non-binding targets operate as defaults or reference points for effort provision.

Keywords: Effort Targets, Job Search Behavior, Unemployment Insurance, Incentive Effects

JEL Codes: J64, J65

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1 Introduction

Targets on effort provision are used to improve productivity and counteract moral hazard in many contexts of the labor market. Commonly known examples include performance targets in firms,[1] as well as the enforcement of minimum job search effort in unemployment insurance (UI) and welfare systems. For the successful design of these targets, it is key to understand how they translate into effort provision and into the final economic outcome. Most of the existing empirical evidence comes however from small-scale laboratory or field experiments with limited external validity (e.g. Abeler et al. 2011, Fehr and Goette 2007, Hennig-Schmidt et al. 2010, Armantier and Boly 2015). Evidence based on large-scale representative data is very scarce, as standard data sources rarely report the individual target, the provided effort and the economic outcome simultaneously. We address this gap by means of novel register data which allow analyzing the effects of effort requirements on real-world labor market outcomes.

We consider the context of job search, where unemployment insurance (UI) regimes systematically use search requirements to regulate the provision of effort. Requirements set a minimum number of monthly job applications and are enforced through the threat of benefit sanctions. Although they are increasingly used both in Europe and in the U.S., individual-level evidence about their effects on search behavior and outcomes is very scarce: how do search requirements change the job seeker's choice of effort? Do job seekers choose not to comply when the required change in effort becomes large? Can required effort changes translate into increased job finding and does this compromise job match stability?

To answer these questions, we present theoretical predictions from job search theory and confront them with empirical evidence. The analysis is based on on unique register data from the Swiss UI, reporting for the first time required and provided effort (job applications) on an individual level. It is further observed how many applications the individual job seeker sent out before knowing her requirement. We provide evidence that this 'pre-requirement' effort choice is a reasonable proxy for the job seeker's voluntary effort at the beginning of the unemployment spell. From the individual perspective, the relevant treatment exposure is thus the *difference between the imposed requirement and the pre-requirement effort*. If this difference is positive, the job seeker is required to increase the number of provided applications and thus faces a *binding* requirement. We assess in this study the effect of binding and non-binding requirements, which provide different incentives for changing the amount of provided effort.

In a first step, we show descriptively that the job seekers' effort choices are strongly responsive to the treatment exposure: job seekers with binding requirements increase their number of applications. Unexpectedly, also job seekers with non-binding requirements react, by decreasing

[1] See for example Prendergast (1999) for an overview on financial incentives in firms.

the number of provided applications.

Based on these observations, we investigate whether the effort reactions come along with impacts on individual labor market outcomes. To this purpose, we develop an empirical analysis to estimate the causal effect of required effort changes on compliance with the requirement, job finding and job stability. For identification, we exploit that the caseworker's requirement choice and the job seeker's effort choice are quasi-randomly matched. Job seekers cannot select their caseworker due to exogenously fixed assignment mechanisms. As each caseworker has own criteria of mapping a job seeker's characteristics into search requirements, the random caseworker-job seeker match generates variation in the required effort change. We isolate this match-driven variation by conditioning on those components in the job seeker's effort choice and the caseworker's requirement setting behavior which are potentially endogenous to search outcomes.

Specifically, we control for three elements: first, job seekers choose their pre-requirement number of applications based on their situation and motivation. We hold this choice constant through dummies and thus exploit variation within a given choice of effort. Second, caseworkers assign requirements based on their assessment of the job seeker's characteristics. We therefore control for the job seeker's position in the caseworker's distribution of requirements.[2] Third, the caseworker's average leniency may confound with other policy choices (e.g. the assignment of training programs). We absorb these through caseworker fixed effects. The remaining variation is due to the random job seeker-caseworker match: a job seeker would face a different level of treatment exposure if she had been matched with another caseworker who had differently mapped her characteristics into a requirement. We show empirically that – conditional on the job seeker's pre-requirement effort choice, her position in the caseworker's distribution of requirement levels and her caseworker's average policy – the treatment exposure is neither related to the job seeker's characteristics nor to other labor market policy assignments (Placebo outcomes).[3]

The results show that job search outcomes react substantially to the required effort increase. First, the probability not to comply with the requirement rises with the treatment exposure. This reveals that the elasticity of search effort to binding requirements is strong but imperfect: when the required increase in search effort is high, not complying and taking the risk of a benefit sanction becomes more attractive.

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 is required to increase

[2] This in done through cubic polynomials for the share of job seekers receiving a lower/higher requirement by the same caseworker and through dummies for the job seeker's rank in the caseworker's distribution of requirements.

[3] A related literature uses average choices of judges or examiners as instruments for individual treatments (e.g. Kling 2006, Maestas et al. 2013, French and Song 2013, Dahl et al. 2014, Autor et al. 2015). Unlike judges, UI caseworkers take policy decisions in multiple dimensions – e.g. monitoring or the assignment of training programs. Therefore, average caseworker choices are in our context not a valid instrument.

effort by one application, the probability of finding a job within six months raises on average by .5 percentage points. The size of this effect is concave to the required amount of applications, which suggests that policy makers cannot maximize job finding rates simply by increasing requirement levels. We also find that effects are strongest in local labor markets with high vacancy rates and in low-skilled service occupations. Furthermore, the increase in job finding is driven entirely by transitions to unstable jobs. This reveals a clear policy trade-off which needs to be accounted for when designing requirement policies: induced search effort can successfully translate into job finding, but only at the cost of reduced job quality.

Finally, the effort reduction which is induced by non-binding requirements has effects on the job search outcome: non-binding requirements have slight negative effects on early job finding and substantial positive effects on job stability. They thus appear to provoke a shift from application quantity to quality and to narrow the job search process. This suggests that the search requirement operates not only as a binding constraint that is enforced by the sanction threat, but also as a non-binding default or reference point. Through the simple suggestion of an "optimal" search quantity, requirements appear to change both effort provision and the final job search outcome.

These findings provide first estimates of the returns to individual search effort induced by targets. This has relevant implications for the understanding of job search behavior and the design of modern UI regimes. The extensive use of job search requirements across OECD countries is usually motivated by the assertion that UI benefits can induce an under-provision of search effort. This claim is based on a large literature showing that increasing the generosity of UI benefits prolongs nonemployment durations (e.g. Katz and Meyer 1990, Card and Levine 2000, Chetty 2008, Lalive 2008, Schmieder et al. 2012). Further, the theoretical literature on optimal UI suggests that the enforcement of minimum effort provision can be welfare improving, as compared to a situation without monitoring (Pavoni and Violante 2007). There is, however, no previous evidence on whether required search effort changes translate into relevant outcomes, as standard UI registers do not report individual-level effort data.[4] This study fills the gap by documenting elasticities of job search outcomes to required effort changes.

More generally, this paper makes a novel contribution to the literature on behavioral labor economics.[5] For the case of job search, this literature has found that the job seeker's effort decision does not entirely follow the rule of rational behavior, as it may be influenced by hyperbolic discounting (DellaVigna and Paserman 2005), biased beliefs (Spinnewijn 2013; Falk et al. 2006,

[4] A few contributions show how the introduction or strengthening of a job search monitoring regime changes job finding outcomes (Van den Berg and Van der Klaauw 2006, McVicar 2008, Petrongolo 2008, Manning 2009, Bloemen et al. 2013). Another set of studies exploits variation in job search monitoring resulting from field experiments in different U.S. states (Johnson and Klepinger 1994, Meyer 1995, Klepinger et al. 2002, Ashenfelter et al. 2005). However, none of these studies can analyze required effort changes that vary at the individual level.

[5] Examples include work on pay equity (Kahnemann et al. 1986; Card et al. 2012) or reference-dependent labor supply (see above).

Arni 2015) and reference points (DellaVigna et al. 2015). We contribute to the literature by showing that job seekers are highly reactive to policy interventions which provide signals on the "right" quantity of search effort.

The paper is structured as follows: we begin by discussing theoretical predictions on the effects of effort requirements on job search behavior and outcomes (section 2). Section 3 presents the institutional background and data sources. In section 4, we provide descriptive evidence on required and provided search effort. Section 5 discusses the identification strategy and section 6 presents the results. Section 7 concludes.

2 Theoretical Discussion

To fix ideas, we first derive predictions on the effects of individual search requirements arising from a standard job search model (2.1). We then discuss alternative hypotheses which become relevant when the assumption of fully rational behavior is relaxed (2.2).

Throughout the analysis, the definition of search effort as the number of job applications is limited to its quantitative dimension. This is in line with the design of search requirements across OECD countries, which regulate the number of applications to be submitted.[6] Further note that the discussion refers to a situation in which the job seeker's benefit exhaustion is not yet relevant. Our analysis focuses on required effort changes at an *early stage* of the unemployment spell.

2.1 Predictions from Job Search Theory

The following theoretical predictions are derived from a simple job search framework with requirements and benefit sanctions. The model is based on Abbring et al. (2005), who analyze the effects of benefit sanctions in the standard Mortensen (1987) framework.[7] We present the formal model in Appendix 1.2.

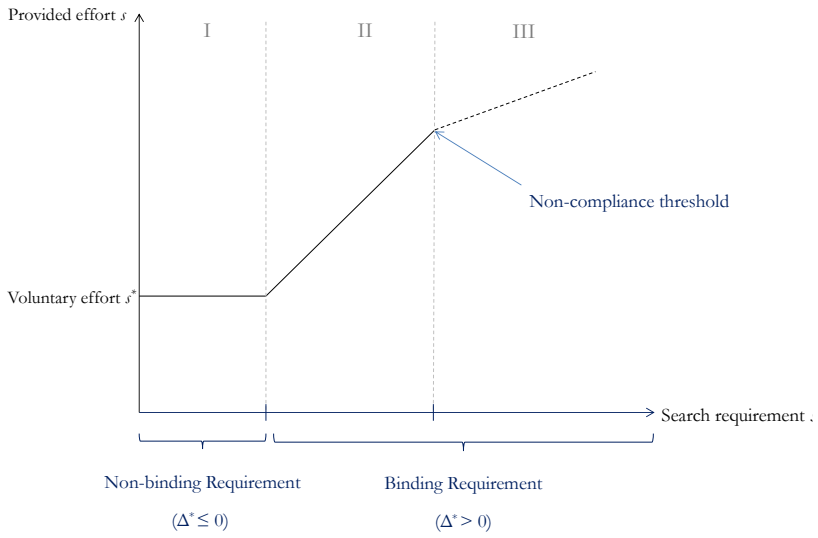
The variables of the model which are relevant for the empirical analysis are: (i) the job seeker's voluntary effort choice s^* – the effort that the job seeker would provide in absence of the requirement; (ii) the requirement s^r; (iii) the provided effort s. From the individual perspective, the distance between s^r and s^* determines the extent to which the requirement imposes a change in search effort. We denote this difference as $\Delta^* = s^r - s^*$, which is the additional required effort beyond the job seeker's voluntary effort choice.

[6] In most countries, monitoring of compliance with the requirement also includes guaranteeing some minimum quality standard, as caseworkers can e.g. check the content of application letters. This is also the case in Switzerland (c.f. section 3).

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

Figure 1 presents the predicted reaction of effort provision to the treatment exposure Δ^*. The reaction is clearly non-linear, due to the existence of two central kink points, which separate three different situations: in area (I), the requirement is lower than the voluntary effort and thus non-binding ($\Delta^* \leqslant 0$). Beyond the first kinkpoint, the requirement is binding and the treatment exposure is positive ($\Delta^* > 0$). In area (II), the job seeker increases search effort in response to the requirement. The second kinkpoint is the job seeker's individual non-compliance threshold: it is the point at which the provision of additional search effort becomes more costly than the risk of receiving a benefit sanction (area III). The position of this threshold depends on the probability and severity of imposed sanctions as well as on the job seeker's cost of effort. The provided effort still exceeds s^* because providing a level of search effort that is close to the requirement reduces the sanction probability.[8]

Figure 1: Illustration of Theoretical Predictions



These effort reactions imply two main predictions which will be tested in the empirical analysis:

1. Non-binding requirements do neither affect the provision of search effort, nor any final job search outcome.

2. Binding requirements raise the quantity of provided effort. Increasing marginal costs of compliance however make this effect non-linear: the probability not to comply by providing $s < s^r$ rises with the treatment exposure Δ^*. Both under compliance (area II) and under non-compliance (area III), job seekers with binding requirements are forced to deviate from

[8] Also note that the dashed line in Figure 1 is an approximation and could be non-linear, depending in particular on the functional form of the job seeker's effort cost.

their optimal behavior s^* and therefore experience a loss in their reservation value. Through the increase in effort and the decrease in the reservation value, the imposition of a binding requirement is expected to increase job finding, where this effect increases in Δ^*. Effects on job quality (measured as job stability)[9] are ambiguous: through the decrease in the value of unemployment, the requirement pushes job seekers into the acceptance of lower quality jobs. At the same time, the induced increase in effort can raise the quality of obtained job offers.

2.2 Predictions from Alternative Models

Job search requirements as effort targets may generate additional effects if individuals do not behave in a fully rational way. Recent contributions to the literature emphasize the relevance of defaults and reference points in the context of effort provision.

First, requirement thresholds can steer the job seeker's effort choice through *default effects*. The job seeker can perceive the imposed requirement as the default effort level and follow it due to inertia and status quo bias (Madrian and Shea 2011, Kahnemann et al. 1991). Moreover, a current study by Altmann et al. (2015) suggests that the alignment of interests between default setters and decision makers, as well as their relative level of information are key drivers of default effects. Following this reasoning, job seekers follow the requirement mostly if they believe that the caseworker supports their interests and disposes of more information on efficient job search than they do. Empirically, this implies the convergence of provided effort towards the requirement threshold, both for binding and non-binding requirements.

A similar hypothesis can be derived from the current literature on the role of reference points for effort provision. For example, Abeler et al. (2011) present evidence that individual expectations can serve as reference points. Again, this suggests that if job seekers believe that the caseworker's requirement choice aims at supporting their search efficiency, they may expect a positive payoff of searching at that level.[10]

[9] Other dimension of job quality, such as earnings, are not observed in our data.

[10] Note that in psychology there is a long-standing branch of literature on goal setting as a motivating mechanism. However, Heath et al. (1999) show that the main predictions of this literature can be directly explained by a standard model of reference-dependent preferences: i.e., goals on effort provision serve as reference points in the individual's value function.

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 have contributed for at least six months during the two previous years.[11] The potential duration of unemployment benefits is usually 1.5 years for eligible prime age individuals. It varies by realized contribution period, age and family situation. The replacement ratio is between 70% and 80% of previous earnings, depending on the individual family situation and the level of past earnings.

The organization of counseling and monitoring is ensured by Public Employment Service (PES) offices, which are the organizational units of caseworkers. When individuals register at the PES to claim benefits, they are assigned to a caseworker based upon caseworker availability (caseload formula) or the job seeker's previous industry, previous occupation or place of residence. The assignment mechanism is fixed by the cantonal UI authority and cannot be influenced by the job seeker.

Job Search Monitoring Swiss UI law requires individuals to start actively searching for work as soon as they learn about their future unemployment.[12] Before the first meeting with the caseworker, this obligation does not include a fixed requirement threshold. It thus appeals to the job seeker's own definition of active job search. However, job seekers are asked to report their previous search effort at their first caseworker meeting. Therefore, the database of job search monitoring creates by default an entry for the number of applications that the job seeker submitted in the month previous to the first meeting. Caseworkers ask for proofs of the reported effort and enforce benefit cuts if they conclude that it was insufficient. In practice, benefit cuts happen mostly in cases in which the job seeker reports no previous application activity at all.

The first caseworker meeting usually takes place around three weeks after registration (c.f. Table 11 in Appendix A.3 for details). At this meeting, the caseworker defines the requirement, i.e. the minimum number of job applications which the job seeker must submit on a monthly basis to avoid benefit cuts. From then onward, job seekers document their application activity in a monthly "protocol of search effort". Compliance is monitored by the caseworkers, who again ask for proofs of submitted applications at their regular (usually monthly) meetings with the job seeker.[13] Once non-compliance with the search requirement is detected, benefit cuts may be

[11] To be eligible for the full benefit period, the contribution period extends to 12 to 22 months, depending on the individual situation.

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

[13] Moreover, it is part of the caseworker's job tasks to occasionally check the truthfulness of reported applications by calling the prospective employer.

imposed. In the estimation sample, a job seeker who does not comply with the requirement at least once during the unemployment spell[14] faces a probability of 60% to receive at least one notification that a non-compliance has been detected and a probability of 45% to actually receive a benefit sanction. The median size of a sanction is the monetary equivalent of 7 days of UI benefits.

3.2 Data and Measurement

Data Source and Sampling Criteria We base the empirical analysis on individual data from the Swiss UI register covering all job seekers who entered UI between 2010 and 2012 in the cantons of Bern, Fribourg, Solothurn, Graubuenden and Tessin. In these cantons, job search monitoring is systematically reported in the central database, to which we have access.[15] The five cantons cover around 25% of the unemployed population and three different geographic and language regions in Switzerland. The database includes extensive information on entry into and exit from formal unemployment, socio-demographics, potential benefit duration as well as employment and unemployment history including past earnings.[16] It is further reported which PES and caseworker the job seeker was assigned to.

We limit the estimation sample to job seekers who are full-time unemployed, eligible for UI payments and not eligible for disability insurance benefits. We also exclude job seekers who are younger than 20 or older than 55 years and job seekers whose previous unemployment spell ended less than a month previous to their current registration.

Our analysis is at the intensive margin, as we are interested in the effects of more or less binding requirements *on the population of eligible job seekers*. Therefore, all job seekers who do not become eligible for the requirement policy are irrelevant for the analysis. As compliance with the requirement is monitored on a monthly basis, job seekers who exit within less than one month of unemployment do by definition not become eligible and are excluded from the sample. Everyone else faces the job search obligation by law, but there are possible exemptions due to the individual's situation. In the data appendix (A.2), we describe how we define individuals who were systematically affected by the job search obligation and report the shares of excluded spells. It is clear that job seekers who are excluded from the intensive margin approach are a selective group. However, this sample is the most suitable one to identify the policy-relevant average treatment effects on the treated – i.e. on the job seekers eligible for the requirement policy.

[14] We define a non-compliance as the submission of less than 75% of required applications, as a "lighter" non-compliance rarely leads to a registration by the caseworker.

[15] Federal Swiss law prescribes the enforcement of job search requirements. Therefore, it is ensured that cantons excluded from the estimation 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.

[16] Table 12 in Appendix A.3 shows summary statistics on these variables.

Variables on Search Effort We extract information on provided and required applications from the database used by caseworkers for job search monitoring. The database reports applications on a monthly basis and contains three main variables of interest for our analysis: (i) s_i^0, the number of applications sent out in month t_0 by individual i before knowing the requirement; (ii) the search requirement $s^r_{c(i)}$[17] assigned to i by caseworker c; and (iii) $s_i^1|s^r_{c(i)}$, the number of applications provided in month t_1 in response to the requirement.

We define t_0 and t_1 as follows:

- t_0 is the first month of registered job search, preceding the first meeting with the caseworker and thus requirement imposition.[18] s_i^0 is the number of applications provided in this month.
- t_1 is the first month of job search with imposed requirement, i.e. the month subsequent to the caseworker meeting.[19] $s^r_{c(i)}$ is the required and s_i^1 the provided number of applications during that month.[20]

Figure 2 illustrates the definitions of t_0 and t_1. It also shows that variation in the timing of the first meeting creates small heterogeneity in the exact position of t_0 and t_1 within the job seeker's spell. In practice, this is due to the fact that caseworkers and PES have different habits of timing the first meeting. As a further source of heterogeneity, an entry in the database always refers to a full calendar month, but job seekers enter unemployment and meet their caseworkers at different dates during a given calendar month. This heterogeneity occurs however within a small time window at the beginning of the unemployment spell. For instance, t_0 is in 89% of the spells either the month of registration or the month previous to registration.

[17] The subscript c(i) expresses that caseworkers c generate variation in the requirement. The level of variation in requirements is the individual i.

[18] For the majority of the sample, this ensures that t_0 is a month previous to the month of the job seeker's first meeting with the caseworker. In 14.44% of the sample, the month of the first meeting is the first entry in the database and therefore used as t_0. Excluding these entries does not affect the results (c.f. section 6.3)

[19] For the majority of the sample, this ensures that t_1 is a month after the month of job seeker's first meeting with the caseworker. In 13.06% of the sample, the month of the first meeting is the last entry in the database and therefore used as t_1. Excluding these entries does not affect the results (c.f. section 6.3)

[20] If the requirement entry is missing at t_1, we define $s^r_{c(i)}$ as the maximum required effort reported for any search period over the spell.

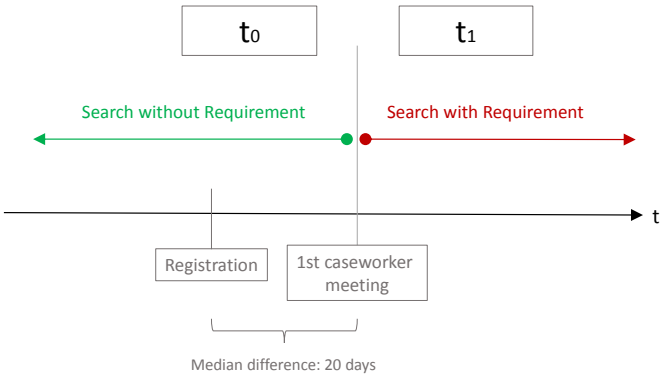


Figure 2: Beginning of Unemployment Spell: Timeline

This figure illustrates the job search activities of individuals in the sample. t_0 and t_1 each refer to one full calendar month.

To ensure that differences in timing do not influence the estimates, we run all regressions with fixed effects for the time at which s_i^0 and $s_{c(i)}^r$ are measured (c.f. section 5 and summary statistics in Appendix A.3, Table 11). Further, caseworker fixed effects will account for caseworker-specific differences in timing of the first meeting and the reporting of search effort. Finally, we run robustness checks showing that heterogeneity in the timing of reported search effort does not drive the results (c.f. section 6.3).

8.24% of requirements are missing and need to be imputed by a method described in appendix A.2.2. The imputation is straightforward, given that we know the requirement setting behavior of each job seeker's caseworker. Results are unaffected when job seekers with imputed requirements are excluded (c.f. section 6.3).

4 Descriptive Evidence on Requirements and Effort Choices

We use s_i^0, the number of applications chosen by job seeker i before requirement imposition to measure i's voluntary effort. $\Delta_i = s_{c(i)}^r - s_i^0$ defines how many additional applications the job seeker has to provide after requirement imposition. In 4.1, we show distributions of these variables. We then provide evidence that s_i^0 provides relevant information on the job seeker's voluntary effort choice (4.2). 4.3 documents how effort provision reacts to the treatment exposure.

4.1 Sample Distributions

Pre-requirement Effort s_i^0 Figure 4a plots the sample distribution of the pre-requirement effort s_i^0 chosen by job seeker i. The median job seeker provides $s_i^0 = 6$ job applications. 22% of

job seekers do not submit any application before registering at the PES.[21] A vast majority reports s_i^0 within the range of 1 to 20 and around 10% beyond that range.

Search Requirement $s^r_{c(i)}$ Figure 4b illustrates the distribution of requirement levels $s^r_{c(i)}$ assigned by caseworker c to individual i. The median requirement is $s^r_{c(i)} = 8$ applications per month. As revealed by Figure 4c, caseworkers have different average requirement levels. Further, Figure 4d shows that caseworkers do not apply the entire distribution of requirements to their cases. Most often, they use two or three different requirement thresholds among the majority of their clients. We will exploit this feature as part of the identification strategy.

[Insert Figure 4]

Treatment Exposure Δ_i The treatment exposure $\Delta_i = s^r_{c(i)} - s_i^0$ results from the match between the effort choice s_i^0 and the requirement $s^r_{c(i)}$. Individuals with the same pre-requirement effort choice can face a binding or a non-binding Δ_i, depending on the requirement they get assigned to. Similarly, job seekers who get assigned one same requirement can face a binding or non-binding requirement, depending on their chosen s_i^0.

Figure 5 documents the distribution of Δ_i, grouped into categories. Job seekers with $\Delta_i \epsilon [-2, 2]$ are the baseline category, as very small Δ_i do not impose any relevant changes in effort.[22] 46% of job seekers face binding requirements ($\Delta_i > 2$) and thus need to increase their effort relative to s_i^0 to achieve compliance. 29% are in the baseline category of $\Delta_i \epsilon [-2, 2]$ and 25% receive a requirement which is lower than their previous effort ($\Delta_i < -2$).

[Insert Figure 5]

4.2 s_i^0 as a Proxy of Voluntary Effort

s_i^0 is the effort a job seeker provides before knowing the requirement $s^r_{c(i)}$, i.e. before the first caseworker meeting. We assume that $s^r_{c(i)}$ is more binding if its difference to s_i^0 is large. The assumption if s_i^0 reveals meaningful information on the job seeker's voluntary choice of effort at the beginning of the unemployment spell (c.f. s^* in the job search model from section 2). This first implies that job seekers need to truthfully report s_i^0. This is guaranteed by the institutional setting: given that low levels of s_i^0 can result in benefit sanctions, there is no incentive for understating

[21] For 50% of these job seekers, the caseworker registers insufficient pre-requirement search effort and opens an enforcement process. Note that these job seekers do not drive the empirical results, as shown in the robustness analysis (section 6.3).

[22] The results are robust to alternative pooling choices, such as choosing the smaller baseline category $\Delta_i \epsilon [-1, 1]$. Results are available upon request.

s_i^0. Overstatement is not feasible, as job seekers are asked to prove their application activity (c.f. section 3).

Less trivially, s_i^0 should not systematically be driven by expectations on the upcoming requirement. As a first test of this assumption, we use the case of repeated spells. For job seekers who registered at the PES during a previous unemployment spell, we know the requirement of that previous spell. Figure 6a plots the distribution of the current s_i^0 against the requirement of the previous spell. Although the past requirement allows job seekers to form an informed expectation on their future requirement, we see no systematic correlation. This supports the idea that in the pre-requirement period, individuals provide the amount of effort which is optimal from their individual perspective, without systematically taking into account expectations about the upcoming requirement.[23] In addition, the robustness analysis shows that estimates do not change when we exclude job seekers with previous unemployment spells, who have prior experiences with the requirement policy (c.f. section 6.3).

[Insert Figure 6a]

Second, we test whether s_i^0 reveals information on the job seeker's effort cost. The idea is that an increase in the cost of effort is associated both with a decrease in the voluntary effort choice (s^*) and with an increase in the non-compliance probability (c.f. section 2). We cannot directly measure cost of effort. However, we know that job seekers with high effort costs have a higher probability of not complying with the requirement, which we observe in the data. Figure 6b plots the share of job seekers who are non-compliant in the first month under the requirement against s_i^0.[24] It shows that s_i^0 is indeed highly correlated with the probability not to comply with the requirement. s_i^0 thus reveals information on the job seeker's effort cost which is in line with the concept of the voluntary effort choice s^*.

[Insert Figure 6b]

4.3 Search Effort under Binding and Non-binding Requirements

How does the presence of the requirement $s^r_{c(i)}$ affect the amount of provided search effort? Figure 7 plots the average pre-to-post change in provided effort ($s_i^1 - s_i^0$) against the treatment exposure Δ_i. On the right half of the graph, requirements are binding and clearly induce positive effort changes.

[23] A further reason why job seekers do not form expectations based on past experience is the exogenous assignment mechanism to caseworkers: job seekers may realize that they will most likely be confronted with a different caseworker who assigns requirements differently.

[24] We define a non-compliance as the submission of less than 75% of required applications, as a "lighter" non-compliance rarely leads to a registration by the caseworker.

Strikingly, also job seekers with non-binding requirements (left half of the graph) react strongly, by adjusting their effort towards the requirement and decreasing the amount of applications.

Figure 8 adds to this evidence by describing how the difference between provided and required effort ($s_i^1 - s_{c(i)}^r$) reacts to Δ_i. First, the Figure confirms that job seekers tend to provide on average the required amount of effort. However, it also suggests that the elasticity of effort to the requirement is imperfect: job seekers with a high treatment exposure submit on average less applications than required. This observation is in line with the job search model, which predicted that a high required effort increase provokes non-compliance. In the econometric analysis, we will assess the causal effect of Δ_i on the job seeker's probability of non-compliance.

[Insert Figures 7 and 8]

5 Empirical Strategy

The aim is to estimate the causal effect of the treatment exposure $\Delta_i = s_{c(i)}^r - s_i^0$ on a set of job search outcomes. Δ_i measures the individual's required effort change as the distance between the imposed requirement and the job seeker's pre-requirement effort choice. Before setting up the empirical model, we discuss the identifying variation.

5.1 Identifying Variation

Our identification strategy exploits the existence of quasi-randomness in the match between the job seeker (i) and the caseworker (c). This match determines Δ_i. At their first meeting with the job seeker, caseworkers assign a requirement level based on the information they observe, e.g. socio-demographics, occupation, employment history.[25] Each caseworker has own criteria of mapping job seeker characteristics into requirements. Due to exogenous assignment mechanisms (conditioning on fixed, observed criteria; c.f. section 3.1), job seekers can neither choose nor anticipate the requirement setting criteria applied by their caseworker, and caseworkers cannot influence the pre-requirement effort choice of their job seekers. This conditional random assignment of job seeker choices to caseworker choices generates exogenous variation in Δ_i.

The approach to exploit quasi-randomness in caseworker assignments is related to a recent literature which uses average choices of judges or examiners as instruments for individual treatments (e.g. Kling 2006, Maestas et al. 2013, French and Song 2013, Dahl et al. 2014, Autor et al. 2015). Unlike judges, UI caseworkers take policy decisions in multiple dimensions – such as the leniency

[25] Source: Qualitative survey among 43 caseworkers in the canton of Bern. More information available upon request.

of monitoring, the assignment of training programs or job referrals, the choice of counseling intensity etc.. These decisions are possibly correlated within case. Therefore, average caseworker choices are in our context not a valid instrument. However, the exogenous job seeker-caseworker match also generates idiosyncratic variation in Δ_i conditional on average caseworker choices. We illustrate this feature in the following.

As a thought experiment, consider in Figure 3 two identical job seekers, where one happens to be assigned to caseworker $cw1$ and the other to $cw2$. Assuming for simplification that the caseworkers both assign their requirements with equal shares, they have the same average requirement choice ($\overline{s_c^r} = 8$). Nevertheless, they differ in their requirement choices and in how they assign those to the job seekers.

Figure 3: Examples of resulting matches between job seeker and caseworker

job seeker		*caseworker*	*...requirement categories*		*...requirement decision*		*match (treatment)*
$js1$	$s_i^0 = 5$	$cw1$	$s^{r\,L} = 4$ $s^{r\,M} = 8$ $s^{r\,H} = 12$	$\rightarrow$	$s_{c(i)}^{r\,M}$	$\Rightarrow$	$\Delta_i = 3$
$js2$	$s_i^0 = 5$	$cw2$	$s^{r\,L} = 6$ $s^{r\,H} = 10$	$\rightarrow$	$s_{c(i)}^{r\,L}$	$\Rightarrow$	$\Delta_i = 1$

Both caseworkers apply discrete, restricted sets of requirement categories, and they potentially differ in how they weight characteristics of job seekers when assigning those categories. As documented in section 4.1, caseworkers predominantly assign 2 to 4 distinct requirements. There is no rule or obvious criteria that guides the caseworkers on how many and which requirements to use. The same applies to their leeway in how to weight different characteristics of job seekers when mapping them into requirement categories. This discontinuous nature and varying form of the caseworkers' assignment functions generates idiosyncratic variation. In the example, $cw1$ thinks that the job seeker ought to receive the "medium" requirement and assigns $s_{c(i)}^{r\,M} = 8$. $cw2$ only applies two different levels and assigns the job seeker to the lower one ($s_{c(i)}^{r\,L} = 6$).

The challenge is to isolate the described quasi-randomness from other sources of variation which are potentially endogenous to job search outcomes. For illustration, we decompose the variation in the treatment exposure Δ_i into the following components of job seeker and caseworker behavior:

$$\Delta_i = g(s_i^0) + \phi_c + \eta_{c(i)} + x_i'\xi + \mu_{c(i)} \tag{1}$$

Δ_i is mechanically a function g of the job seeker's choice of s_i^0. It thus correlates with the job

seeker's intrinsic effort choice, which is likely to be endogenous to the job search outcome.

Conditional on s_i^0, Δ_i is also affected by the caseworker's average leniency or preference for higher vs. lower requirements, ϕ_c. As this average caseworker effect may correlate with other policy choices by the caseworker, it could also create a non-causal relation between Δ_i and the job search outcome.

In addition, Δ_i is driven by the caseworker's assessment of the job seeker within the match, $\eta_{c(i)}$. Essentially, the caseworker positions job seekers into different requirement levels. This position may not only correlate with observed job seeker characteristics (x_i), but also with characteristics that are unobserved to the econometrician. For example, the caseworker's assessment of the job seeker's labor market prospects may influence whether the job seeker is assigned a higher or lower requirement by the caseworker.

The remaining variation $\mu_{c(i)}$ arises from the exogenous matching between the caseworker's requirement setting and the job seeker: on the one hand, the same caseworker is mechanically confronted with different types of job seekers (and thus different s_i^0). On the other hand, the same job seeker can face different levels of Δ_i depending on how the caseworker maps individuals into requirements, as detailed above.

In the following, we describe how we control for ϕ_c and $\eta_{c(i)}$ in the econometric model to ensure that the exogenous $\mu_{c(i)}$ drives the results. We then assess the identifying assumption.

5.2 Estimation Model

We estimate the following model:

$$y_i = \alpha + \delta_i^{\Delta} + \gamma_i^{s_0} + {r_{c(i)}^{s_r}}'\kappa + \pi_c + \omega_\tau + \vartheta_t + x_i'\beta + \varepsilon_i \tag{2}$$

The main parameters of interest are contained in δ_i^{Δ}, which measures the non-linear effects of the required effort change $\Delta_i = s_{c(i)}^r - s_i^0$ on outcome y of job seeker i. δ_i^{Δ} features dummy variables for bins in the distribution of Δ_i. The baseline category pools job seekers with $\Delta_i \epsilon [-2, 2]$, i.e. whose pre-requirement effort is very close to the requirement level.[26]

The following three elements are crucial to identifying δ_i^{Δ}: first, to exclude potential unobserved confounders of the decision on pre-requirement effort s_i^0, we introduce group effects (dummy variables) for each possible choice of s_i^0, denoted by $\gamma_i^{s_0}$. Thus, we only exploit variation in Δ_i within groups of job seekers having chosen one same level of pre-requirement s_i^0 (the same "search types").

[26] Results are robust to choosing different cutoff values for Δ_i. Documentation is available upon request.

Second, we control for the job seeker's position in the distribution of requirements applied by the caseworker ($\eta_{c(i)}$ in equation 1). To this purpose, we introduce the vector $r^{s_r}_{c(i)}$, which contains: (i) a cubic polynomial of both $P(s^r_{-i} < s^r_{c(i)})$ and $P(s^r_{-i} > s^r_{c(i)})$, i.e. the share of other individuals within caseworker, $-i$, with a requirement that is below/above the one of job seeker i. Note that these two probabilities are not collinear, since caseworkers assign one requirement level to more than one job seeker. (ii) a step function of dummy variables for the rank position of i's requirement within her caseworker's distribution of requirements[27].

Third, caseworker fixed effects π_c take into account the average strictness in requirement setting (ϕ_c in equation 1), which could be correlated to other caseworker-specific policies.

In addition, x_i contains observable job seeker characteristics, which however turn out not to affect the estimates. ω_τ contains fixed effects for season and calendar year of the individual's inflow into unemployment. ϑ_t holds dummies for the number of months between t_0 and the start of formal unemployment and for the number of full weeks between the start of formal unemployment and the first caseworker meeting, to account for heterogeneity in timing with respect to the arrival of the requirement threshold (c.f. section 3.2).

Note that the control variables and fixed effects contained in equation 2 explain 33% of the variation in Δ_i.[28] The variation in Δ_i used for identification (i.e. $\mu_{c(i)}$ from equation 1) is thus substantial. Its distribution is plotted in Figure 16, Appendix A.3.

Summary statistics on the variables included in the econometric model can be found in Table 12 as well as in Tables 10 and 11 in Appendix A.3.

5.3 Assessment of the Identifying Assumption

When using equation 2 to identify the causal effect of Δ_i, we assume the exogeneity of Δ_i conditional on the econometric framework. Formally, this requires that $\mu_{c(i)}$ - the remaining variation after conditioning on on $\gamma^{s_0}_i$, π_c,$r^{s_r}_{c(i)}$ and x_i - is independent of the error term of equation 2:

$$E[\mu_{c(i)}\varepsilon_i|\gamma^{s_0}_i, \pi_c, r^{s_r}_{c(i)}, x_i] = 0 \tag{3}$$

To assess the validity of this assumption, we test in the following whether Δ_i is independent of job seeker characteristics and other treatments assigned by the caseworker, once we condition on $\gamma^{s_0}_i$, π_c and $r^{s_r}_{c(i)}$.

[27] For example, if a caseworker distributes requirements of the levels $s^r_{c(i)} \in \{6, 10, 12\}$ among her job seekers, those getting $s^r_{c(i)} = 6$ are in rank 1. For each rank there is one dummy variable.

[28] This is measured by the R-squared of a regression of Δ_i on the control variables and fixed effects contained in equation 2.

Independence of $\mu_{c(i)}$ from Job Seeker Characteristics Table 1 displays results from the first identification test. Column (1) shows that the unconditional probability of facing a binding requirement (i.e. a positive treatment exposure, $\Delta_i > 0$) is highly correlated with observable job seeker characteristics, such as gender, nationality, past earnings and occupation. We investigate whether these correlations disappear once we control for $\gamma_i^{s_0}$, π_c and $r_{c(i)}^{s_r}$.

Column (2) of Table 1 shows that conditioning on s_i^0 reduces the association of some job seeker characteristics with the probability of facing a binding requirement. However, significant correlation remains, suggesting selectivity in the assignment of requirement levels. But once we add π_c and $r_{c(i)}^{s_r}$ as controls for the caseworker's behavior, observable job seeker characteristics are no longer significantly correlated with the probability of facing a binding requirement (column 3).[29] This suggests that the econometric model is able to isolate the variation in Δ_i that is independent of job seeker characteristics.

The regression results in Section 6 will further support this finding. They show that controlling for job seeker socio-demographics and individual labor market histories does not affect the main estimates.

[Insert Table 1]

Independence of $\mu_{c(i)}$ from Other Treatment Assignments (Placebo Outcomes) As a second empirical test, we estimate Placebo regressions in which we use other caseworker policies as dependent variables. In addition to setting requirements, caseworkers assign labor market policies, such as training, counseling and sanctions. These assignments are clearly driven by the caseworker's evaluation of a job seeker's characteristics. If the identifying assumption is valid, we expect the assignment of such treatments not to correlate with Δ_i conditional on the econometric framework.

To test for this, we regress a job seeker's probability of being assigned to different treatments on equation 2. Placebo outcomes are: (1) the probability of being assigned to a training program in the first two months of unemployment, (2) the probability of having scheduled a second caseworker meeting within less than a month since the first meeting and (3) the probability of receiving a benefit sanction for a non-compliance unrelated to the requirement in the first two months of unemployment.[30]

We report estimated coefficients of δ_i^{Δ} in Table 2. All reported coefficients are statistically

[29] No coefficient is significant at the 5% level. the coefficient on "female" is significant at the 10% level. However, given the large number of covariates, one marginal significance is likely to occur by chance.

[30] We only use assignments made at the beginning of the unemployment spell to avoid that dynamic selection drives the results.

insignificant at the 5% level.[31]

[Insert Table 2]

5.4 Further Estimation Details

We estimate equation (2) by OLS for some outcomes. When we estimate effects on the duration to job finding, we specify the job finding rate θ^e as a Proportional Hazard (PH):

$$ln\ \theta^e = ln\ \lambda(t_e) + \delta_i^{\Delta} + \gamma_i^{s_0} + r_{c(i)}^{s_r}{}'\kappa + \pi_c + \omega_\tau + \vartheta_t + x_i'\beta \tag{4}$$

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

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

where k(= 1, . . . , 3) is a subscript for the 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, the main specification censors durations after six months. For this specification, we distinguish the following time intervals: 2-3 months, 3-4 months and 4-6 months.[32] In specifications where durations are censored after two years, we add the intervals 6-12 months and 12-24 months.[33] As we estimate a constant term, we normalize $\lambda(t_{e,1})$ to be 0.

6 Estimation Results

6.1 Compliance with the Requirement

The standard job search model predicts that the cost of compliance increases with the treatment exposure Δ_i (c.f. Section 2.1). In Table 3, column (1) shows that binding requirements indeed increase the probability of non-compliance.[34] For instance, job seekers in the two highest treatment bins, who have to increase effort by at least 5 applications relative to the pre-requirement period, have a non-compliance rate which is 5 percentage points higher than job seekers in the baseline category with $\Delta_i \epsilon [-2, 2] \approx 0$. Given the mean non-compliance probability of 12%, this effect is

[31] Two of the 21 coefficients are significant at the 10% level. Again, given the large number of estimated coefficients, this can be attributed to chance.

[32] Recall that job seekers with an unemployment duration of less than one month are excluded from the analysis because they are unaffected by the requirement regime (c.f. Appendix A.2.)

[33] The descriptive job finding hazard is plotted in Figure 15 in Appendix A.3

[34] We define a non-compliance as the submission of less than 75% of required applications, as a "lighter" non-compliance rarely leads to a registration by the caseworker.

substantial. It reveals that increased compliance costs actually translate into the incidence of non-compliance.

Column (2) shows that also the rate of non-compliance detection reacts strongly: for example, job seekers with $\Delta_i \epsilon [7, 8]$ face an increase in their probability of being registered as non-compliant within the first 2 months of unemployment[35] of 3 percentage points, compared to job seekers in the baseline category. Effects on the probability of receiving a benefit sanction are quantitatively similar, as shown in column (3). The results of column (2) and (3) are illustrated in Figure 9, showing that the effect of binding requirements on the detection and sanction rate is nearly linear.

All three columns also revels that job seekers with non-binding requirements ($\Delta_i \leqslant 0$) have a slightly decreased non-compliance compared to job seekers in the baseline category. Job seekers with non-binding requirements have the lowest compliance costs, as they can decrease effort while remaining compliant.

[Insert Table 3 and Figure 9]

6.2 Job Finding and Job Stability

6.2.1 Effect on Job Finding

We model job finding using a flexible proportional hazard (PH) model.[36] In the main regressions, we censor the duration of unemployment after six months, as Δ_i is expected to affect job search behavior mostly at the beginning of the spell. 45% of job seekers in the sample accept a job within six months.[37]

Table 4 provides estimates of the treatment effects for the main regression. Column (1) shows results for equation (2), excluding the vector of individual covariates x_i. In column (2), we add x_i, which does not lead to statistically significant changes in the coefficients. Both columns 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 increase effort by 3 or 4 applications increase their job finding hazard by 14% (=exp(.128)-1), compared to the baseline group with $\Delta_i \epsilon [-2, 2] \approx 0$. Column (3) reports the corresponding marginal effects,[38] which measure the impact of the treatment on the probability of job finding within six months.

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

[36] We also estimated OLS regressions on the linear probability of job finding, as a robustness check and for the assessment of job stability effects. In Table 8, we report the effects on the probability of job finding within 6 months and decompose it into stable and unstable jobs. Other OLS results on job finding are available upon request.

[37] The descriptive job finding hazard is plotted in Figure 15 in Appendix A.3

[38] 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.

The effect of $\Delta_i \epsilon [3,4]$ on this probability is 2 p.p. and increases up to 4 p.p. for job seekers with $\Delta_i \epsilon [7,8]$ or higher. On average, one additional required monthly application increases the probability of job finding within six months by about .5 p.p. The estimates do not report any significant effects on job seekers with non-binding requirements.

In columns (4) to (7), we present results from regressions with alternative outcome periods. Column (4), which reports coefficients on the job finding hazard censored after 3 months, and column (5), which shows the corresponding marginal effects, confirm that the effects of a required increase in effort are strongest at the very beginning of the unemployment spell. Columns (6) and (7) show that the effect of binding requirements averaged over the entire observation period (24 months) remains positive, but is significantly lower than for early job finding.

Interestingly, columns (4) and (5) also report a slight negative effect of non-binding requirements on job finding within the first three months of unemployment, which is at the margin of significance. Non-binding requirements thus appear to provoke a change in effort which may be relevant for early job finding.

Figure 11 (a) and (b) graphically illustrate the effects of Δ_i on job finding (coefficients and marginal effects) when choosing different censoring dates. Both show that the effect of binding requirements has a concave shape, indicating that there are decreasing marginal returns to required effort increases. From a theoretical point of view, this is consistent with a convex function of effort costs.

[Insert Table 4 and Figure 11]

6.2.2 Heterogeneous Effects

We next discuss how the effects of Δ_i on job finding differ according to both the job seeker's characteristics and the labor demand situation. All durations are censored after 6 months of unemployment to ease comparison with the baseline estimates in Table 4.

Table 5 decomposes results with respect to gender and the skill level of the previous job. Columns (1) and (2) and Figure 12 (a) show that male job seekers increase their job finding rate significantly less in response to binding requirements than female job seekers. For example, a binding search requirement of $\Delta_i \epsilon [5,6]$ increases the job finding hazard of female job seekers by around 30%, compared to an increase of 11% for male job seekers.

Columns (3) and (4) and Figure 12 (b) reveal that results are driven by job seekers with low educational attainment. Individuals who had a support function in their previous employment – a proxy for job seekers with low skills – react more than those who had a professional or management

function.[39] One possible explanation is that job seekers with a higher degree of education and specialization are bounded in their quantitative search effort by the availability of suitable offers. Further, the quality of applications might be of higher importance for highly educated job seekers. Therefore, search requirements that target the quantity of applications can be expected to have less effects on them.

[Insert Table 5 and Figure 12]

Columns (1) to (3) in Table 6 and Figure 13 (a) decompose the effects by occupational groups. Effects are largely driven by job seekers in the low service sector, which comprises in particular the cleaning and restaurant sector. Blue collar workers, in turn, show no reaction.[40]

As a final heterogeneity analysis, columns (4) to (6) and Figure 13 (b) decompose effects by the local labor demand. We proxy labor demand by vacancy rates which are calculated as the ratio of posted vacancies over job seekers per month and region.[41] They are assigned to the job seeker on the basis of month and place of registration at the PES. "Low", "medium" and "high" corresponds to terciles in the distribution of vacancy rates. We observe that there is virtually no effect of requirements when the vacancy rate is in the lowest tercile. This result is intuitive, as additional applications can only result in job matches if the demand for labor is sufficiently large. Note that Switzerland is generally a country where unemployment is relatively low. In addition, there was no true economic downturn during the sample period. This suggests that search requirements might have limited effects in settings in which labor demand is truly stagnating.

[Insert Table 6 and Figure 13]

6.2.3 Job Stability

We observe in the UI data when a job seeker re-enters unemployment. This allows us to analyze how Δ_i affects job stability.[42] According to job search theory, an increase in Δ_i goes along with a reduction in the value of unemployment. This may induce job seekers to accept lower quality jobs. At the same time, a shortened duration of unemployment may increase the quality of job offers. Therefore, the net effect on job stability is ex-ante ambiguous.

[39] Further analyses show that 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. Results are available on request.

[40] These patterns still hold when gender-specific occupation choices are accounted for (results available upon request).

[41] We distinguish by local labor market regions ("MS-regions") of which 102 exist in Switzerland.

[42] Additional dimensions of job quality, such as earnings, are not observed in our data.

Table 7 presents estimated effects of Δ_i on the probability of recurring to unemployment within 6 or 12 months after job finding.[43] Column (1) and the illustration in Figure 14 reveal a substantial negative effect of binding requirements on job stability. An increase in Δ_i raises the probability that a job seeker takes up a job and then re-enters unemployment within six months. The average 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 more frequently to either temporary jobs or jobs that prove to be a bad match. Column (2) presents results of the same regressions for the sub-sample of job seekers with a low-skilled (i.e. support) function, whose job finding rate is particularly affected by Δ_i. Indeed, also the effect on job stability is particularly strong for these job seekers.

The results further report striking negative effects of non-binding requirements on the recurrence to unemployment within six months: receiving a requirement that allows reducing search effort with respect to s_i^0 significantly improves job match stability. This suggests that non-binding requirements decrease the quantity, but improve the targeting and/or the quality of job applications.

Columns (3) and (4) show that the effects are only relevant for short-term recurrence and disappear when the outcome is the probability of recurrence within 12 months.

[Insert Table 7 and Figure 14]

In Table 8, we assess to which degree the effects of Δ_i on job finding are driven by exits to unstable and stable jobs. Column (1) shows estimated treatment effects on the linear probability of job finding within six months. We then decompose them into an effect on finding unstable (column 2) versus stable jobs (column 3). In column (2), the outcome is coded as one if a job seeker finds a job within six months and recurs to unemployment within the following six months. The effect of Δ_i is substantial and suggests that requirement-induced job matches are non-permanent ones. Column (3) confirms the picture: binding requirements have no effect on the probability of exiting to a job that lasts more than six months. By contrast, non-binding requirements have a positive effect on this probability.

[Insert Table 8]

The presented findings point to a central policy trade-off concerning the job seeker's short term outcomes: requirement-induced search effort can shorten the duration of unemployment,

[43] As we observe entries into unemployment until August 2014 and our sample covers entries until December 2012, some of our observations are right censored. This does not affect the results, as this censoring is independent of the treatment.

but only at the cost of job match stability. Given our data setting, we cannot answer the long-term welfare question whether increased exits to temporary jobs improve the individual's long-run employment outcomes (e.g. through a stepping-stone mechanism) or whether it ultimately increases the aggregate time spent in unemployment. We leave this question for future research.

6.3 Robustness Checks

In a final step, we test the robustness of the main estimates with respect to the exclusion of sensitive subgroups and the introduction of additional control variables.

In Table 9, column (1) recalls our baseline estimates (job finding hazard with censoring after six months of unemployment). Column (2) excludes job seekers with s_i^0 from the estimation sample. This addresses the concern that $\gamma_i^{s_0}$ does not provide sufficient information to fully capture the job seeker's effort type. This is particular relevant for the 22% of job seekers who chose $s_i^0 = 0$, as their effort behavior is controlled for by means of one same dummy variable. It is therefore relevant to check whether this group drives the results. In column (3), we introduce an interaction between $\gamma_i^{s_0}$ and a vector of occupation group effects (service sector low/ service sector high/ blue collar) into the baseline regression. This should improve the precision of the control for s_i^0, as a job seeker's occupation is a central determinant of the effort choice.[44] Column (4) checks whether results are driven by heterogeneity in the time at which s_i^0 is measured; it reports estimates for the homogeneous subsample of job seekers for whom s_i^0 was provided exactly in the month of registration at the PES (45%). The estimates in column (5) only include job seekers without any unemployment spell in the two years previous to their current spell (79%). These job seekers have not recently got in touch with the requirement regime and are therefore not likely to strategically choose s_i^0 based on expectations concerning their future requirement level. A final concern related to measurement is addressed in column (6), where the 8% of job seekers with imputed requirements are excluded. None of the checks leads to statistically significant changes in the estimated coefficients.

[Insert Table 9]

7 Discussion and Conclusion

This paper provides first empirical estimates of the individual returns to job search effort induced by targets. We assess the effects of the incremental effort that the an individual requirement imposes beyond the voluntarily provided effort.

[44] We also interacted $\gamma_i^{s_0}$ with other job seeker characteristics. Results are unaffected (available upon request).

Our findings show that binding requirements *substantially increase job finding*. This implies that search requirements induce additional applications which are actually relevant for the success of job search instead of generating pure "spam" applications. The positive effect is concave in the amount of induced search, reflecting non-linear effort cost functions and an imperfectly elastic labor demand. The effects of induced job search are clearly heterogeneous. They mostly affect job finding of individuals in low-skilled jobs and in the service sector. In addition, requirements induce weaker impacts on job finding when labor demand, as measured by vacancy rates, is low.

Moreover, we identify two central economic trade-offs. First, the elasticity of search effort to the requirement is imperfect: besides the positive intensive-margin effects on effort provision, requirements cause negative reactions at the extensive margin due to *increased non-compliance*. Job seekers tend to take the risk of benefit sanctions when the required change in search behavior induces large costs. In line with this result, the probability of receiving a benefit sanction reacts strongly to a required increase in effort.

Second, there is a clear *trade-off between the speed of job finding and the quality of accepted jobs*. The positive effects on job finding come at the cost of job stability, as they are largely driven by the take-up of more temporary jobs. Binding requirements can thus help individuals in returning faster to the labor market, but they bear the risk of pushing weaker job seekers into unstable employment conditions. In the worst case, requirements may promote "revolving doors" careers through the repeated recurrence to unemployment, while in the better case the temporary jobs could serve as stepping stones. At this stage, we cannot yet assess the long-term welfare implications for employment careers and leave this to future research.

A final core result is that non-binding requirements also affect job search outcomes, contrary to 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 previous search level was significantly higher. Results suggest that this goes along with a slightly decreased job finding probability within the first three months of the unemployment spell. In turn, job match stability increases substantially. Thus, search requirements operate not only as enforced constraints, but also as defaults or reference points that signal the "right" search intensity. Through this mechanism, they appear to provoke a shift from application quantity to quality and an improved targeting of effort.

This paper contributes to the scarce empirical evidence on how individual outcomes react to targets and incentives provided by social insurance policies. In traditional theoretical analyses of the optimal UI problem, benefit levels are the social planner's only instrument to trade off moral hazard and insurance concerns, subject to budget constraints (Hopenhayn and Nicolini 1997). Pavoni and Violante (2007) show that introducing job search monitoring as an additional instrument into UI can be welfare improving. The intuition behind this central result is that

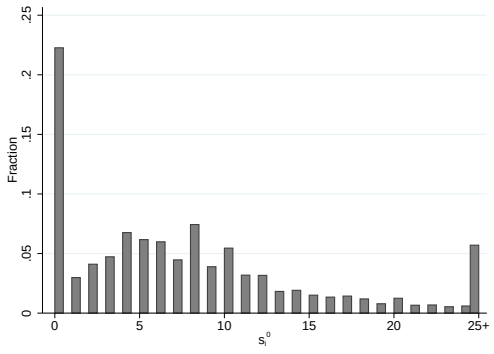
policy makers can afford to set higher benefit levels when monitoring counteracts moral hazard. Our findings show how monitored search requirements affect the outcomes of job seekers in a real-world context. This provides a base for future research that empirically assesses the welfare consequences of policy mixes that enforce effort targets.

More generally, our findings have two main implications for the design of effort targets in firms and institutions: first, principals need to consider that the target does not only operate through the threat of enforcement. It can be taken as a reference point or default and therefore guide effort provision either upward or downward. Through this channel, it may affect agents who are not in the focus of the policy maker. Second, the target can affect the relative weight that individuals assign to the quantity versus the quality of effort provision. Taking this dimension into account can help to design effort targets which effectively improve the final outcome.

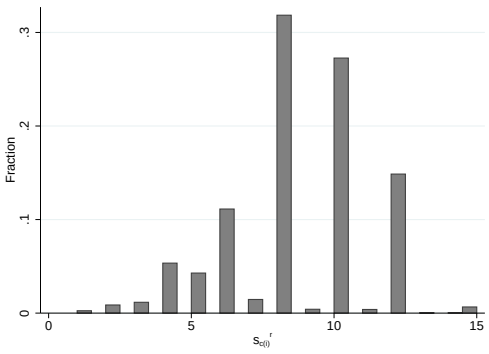
8 Figures and Tables

Figure 4: Sample Distributions

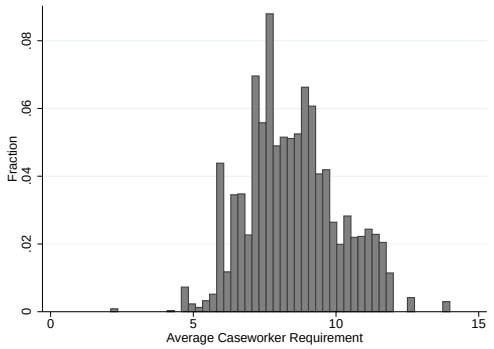
(a) Pre-Requirement Effort s_i^0



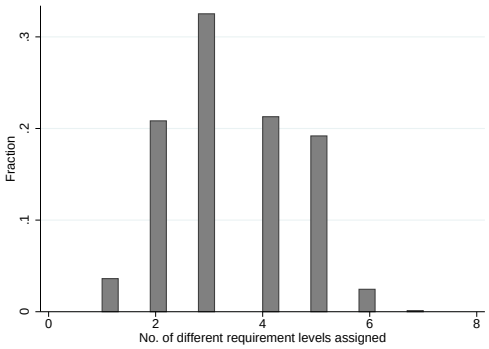
(b) Individual Requirement $s_{c(i)}^r$



(c) Average Caseworker Requirement

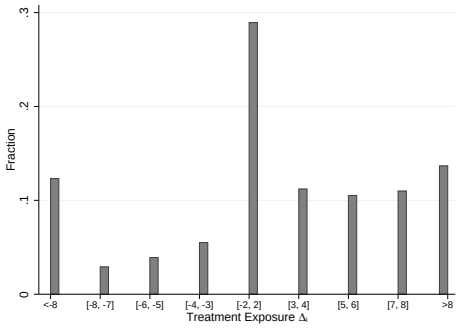


(d) Caseworker "Requirement Portfolio"



Pre-requirement effort and requirements are measured as the individual number of job applications per month. Panels (c) and (d) are weighted by the number of job seekers per caseworker. In panel (d), a requirement is counted as part of the caseworker's "portfolio" if at least 5% of the caseworker's job seekers are assigned to that requirement level.

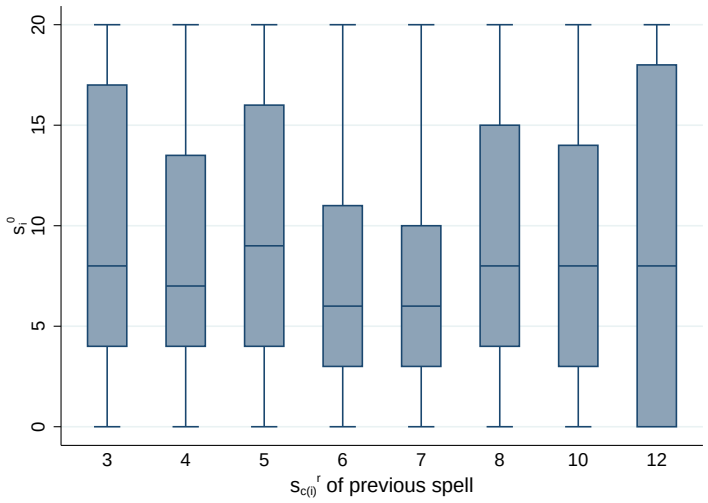
Figure 5: Distribution of the Treatment Exposure Δ_i



Effort is measured as the number of job applications per month. The treatment exposure Δ_i (x-axis) is the difference between an individual's requirement $s_{c(i)}^r$ and pre-requirement effort s_i^0.

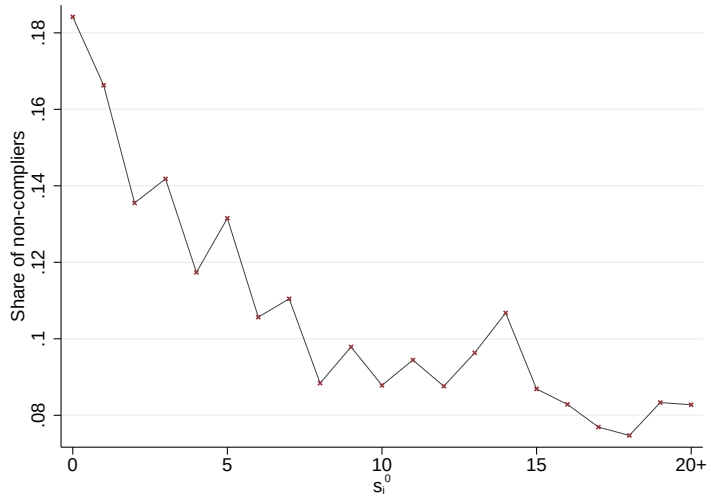
Figure 6: Properties of Pre-Requirement Effort s_i^0

(a) Pre-Requirement Effort s_i^0 Plotted over Requirement $s^r_{c(i)}$ of the Previous Unemployment Spell



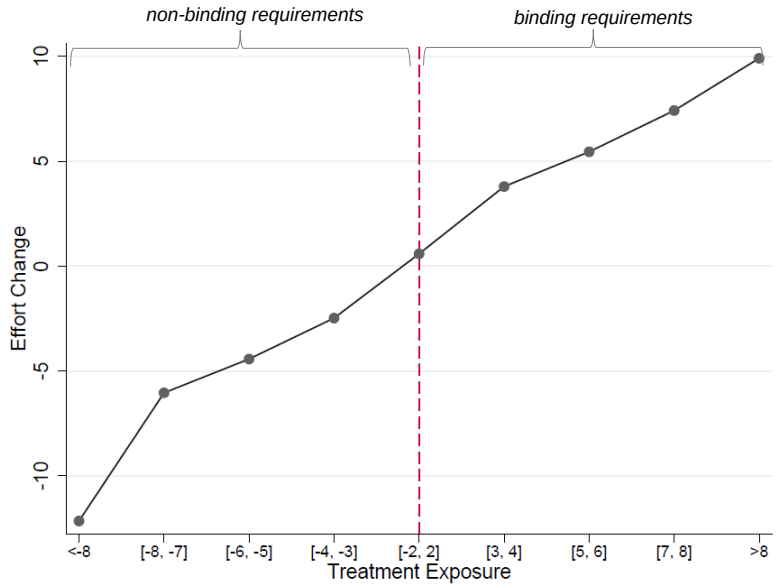
Information on previous requirements is available for 12830 job seekers. Categories of requirements (x-axis) which applied less than 1% of repeated spells are excluded from the figure. Job seekers with $s_i^0 > 20$ are pooled into $s_i^0 = 20$.

(b) Share of Non-Compliers by Pre-Requirement Effort s_i^0



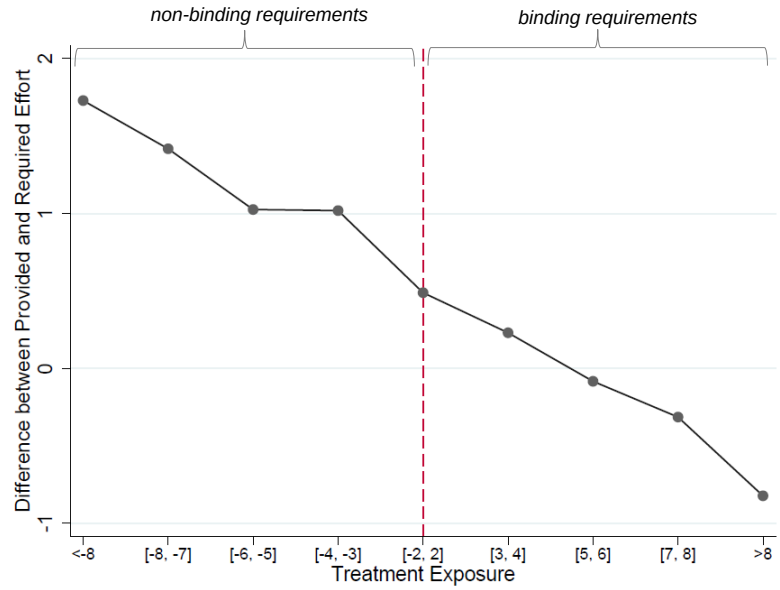
Non-compliance is coded as one if the job seeker submits less than 3/4 of required applications in the first month under the requirement.

Figure 7: Response of Search Effort to Treatment Exposure Δ_i



Effort is measured as the number of job applications per month. The x-axis shows the treatment exposure Δ_i (grouped into categories), which is the difference between an individual's requirement $s^r_{c(i)}$ and pre-requirement effort s^0_i. The y-axis shows for each category of Δ_i the average change from pre- to post-requirement effort $(s^1_i - s^0_i)$.

Figure 8: Difference between Provided and Required Effort by Treatment Exposure Δ_i



Effort is measured as the number of job applications per month. The x-axis shows the treatment exposure Δ_i (grouped into categories), which is the difference between an individual's requirement $s^r_{c(i)}$ and pre-requirement effort s^0_i. The y-axis shows for each category of Δ_i the average difference between the provided post-requirement effort s^1_i and the requirement $s^r_{c(i)}$.

Table 1: Test of Conditional Random Assignment: Probability of Receiving a Binding Requirement ($\Delta_i > 0$)

	(1) Unconditional	(2) Conditional on γ_i^{s0}	(3) Conditional on γ_i^{s0}, $r_{c(i)}^{s_r}$ and π_c
Female	-0.038***	-0.008***	0.004*
	(0.005)	(0.003)	(0.002)
Age	-0.002***	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)
Non-Swiss	-0.009	0.010***	-0.002
	(0.007)	(0.004)	(0.003)
Non-permanent resident	-0.044**	0.000	0.004
	(0.018)	(0.004)	(0.003)
Married	-0.031***	-0.005**	0.001
	(0.005)	(0.003)	(0.002)
Children	0.004	-0.005*	0.001
	(0.006)	(0.003)	(0.003)
Previous earnings > median	-0.051***	-0.015***	0.003
	(0.006)	(0.003)	(0.002)
First UE since 2 years	0.085***	-0.000	0.003
	(0.009)	(0.003)	(0.002)
Potential benefit duration > median	0.005	0.000	0.001
	(0.005)	(0.002)	(0.002)
Blue collar	0.075***	-0.001	-0.002
	(0.009)	(0.004)	(0.002)
Highest degree: minimum schooling	-0.042***	0.000	0.002
	(0.013)	(0.004)	(0.002)
Professional+ function	-0.022***	-0.014***	0.003
	(0.007)	(0.003)	(0.002)
Outcome Mean	0.57	0.57	0.57
R-Squared	0.07	0.69	0.74
N	76404	76404	76404

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. Age is a linear variable; all other variables are 0/1-Dummies.
Column (1) includes fixed effects for the timing of the requirement policy and the time of registration into unemployment. Column (2) adds dummy variables for each possible choice of s_i^0 (c.f. γ_i^{s0} in equation 2). Column (3) adds controls for the job seeker's position in her caseworker's distribution of requirements (c.f. $r_{c(i)}^{sr}$), as well as caseworker fixed effects (c.f. π_c).

Table 2: Effects on Probability of Receiving an Unrelated Other Treatment Assignment (Placebo Outcomes)

	(1) Training	(2) Early 2nd Meeting	(3) Unrelated Sanction
$\Delta_i < -8$	0.011	0.003	-0.002
	(0.015)	(0.022)	(0.005)
$\Delta_i \epsilon [-8, -7]$	0.015	-0.010	-0.000
	(0.014)	(0.016)	(0.004)
$\Delta_i \epsilon [-6, -5]$	0.008	-0.001	-0.001
	(0.011)	(0.014)	(0.003)
$\Delta_i \epsilon [-4, -3]$	0.001	-0.006	0.001
	(0.008)	(0.010)	(0.002)
$\Delta_i \epsilon [3, 4]$	0.003	0.016*	-0.000
	(0.007)	(0.008)	(0.002)
$\Delta_i \epsilon [5, 6]$	0.006	0.003	-0.001
	(0.009)	(0.011)	(0.003)
$\Delta_i \epsilon [7, 8]$	0.003	0.002	0.001
	(0.012)	(0.015)	(0.004)
$\Delta_i > 8$	-0.006	0.006	0.010*
	(0.015)	(0.020)	(0.006)
Covariates	YES	YES	YES
Outcome Mean	0.157	0.308	0.020
N	76404	76404	76404

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. Δ_i is the difference between the requirement and the pre-requirement effort choice. The reference category is $\Delta_i \epsilon [-2, 2]$. All regressions estimate equation 2 using OLS. They include all identifying dummy variables and 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. "Training" is coded as one if the job seeker is assigned a training program during the first 2 months of UE. "Early 2nd 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" is coded as one if the job seeker receives a sanction for a reason unrelated to the requirement during the first 2 months of UE.

Table 3: Probability of Non-Compliance/Non-Compliance Detection/Benefit Sanction within the First 2 Months of UE

	(1) Non-Compliance	(2) Detection	(3) Sanction
$\Delta_i < -8$	-0.028**	-0.022**	-0.011
	(0.013)	(0.011)	(0.007)
$\Delta_i \epsilon [-8, -7]$	-0.019*	-0.017*	-0.013**
	(0.011)	(0.009)	(0.006)
$\Delta_i \epsilon [-6, -5]$	0.006	-0.015**	-0.006
	(0.008)	(0.006)	(0.004)
$\Delta_i \epsilon [-4, -3]$	-0.007	-0.012**	-0.005
	(0.006)	(0.005)	(0.003)
$\Delta_i \epsilon [3, 4]$	0.019***	0.010**	0.010***
	(0.006)	(0.005)	(0.004)
$\Delta_i \epsilon [5, 6]$	0.042***	0.020***	0.018***
	(0.008)	(0.006)	(0.005)
$\Delta_i \epsilon [7, 8]$	0.054***	0.032***	0.029***
	(0.011)	(0.010)	(0.007)
$\Delta_i > 8$	0.061***	0.057***	0.057***
	(0.014)	(0.013)	(0.010)
Outcome Mean	0.123	0.089	0.052
N	76404	76404	76404

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. Δ_i is the difference between the requirement and the pre-requirement effort choice. The reference category is $\Delta_i \epsilon [-2, 2]$. All regressions estimate equation 2 using OLS. They include all identifying dummy variables and 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. "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. "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.

Figure 9: Illustration of Results in Table 3, Columns (2) and (3) (with 90%CIs)

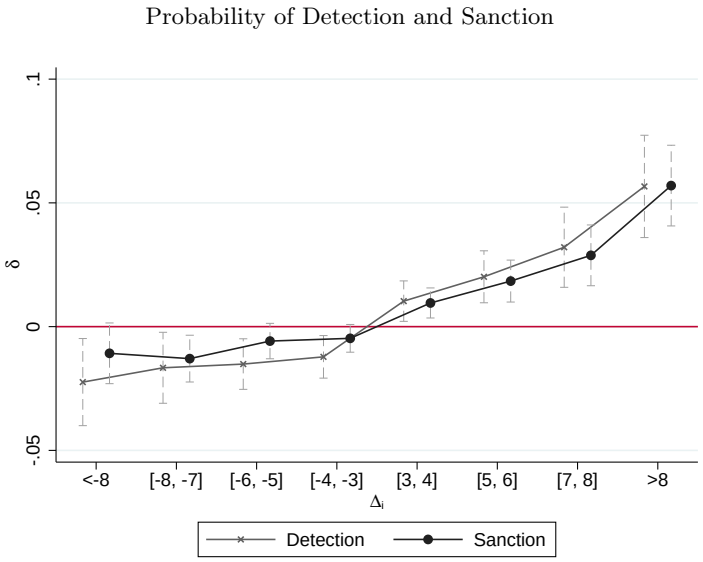


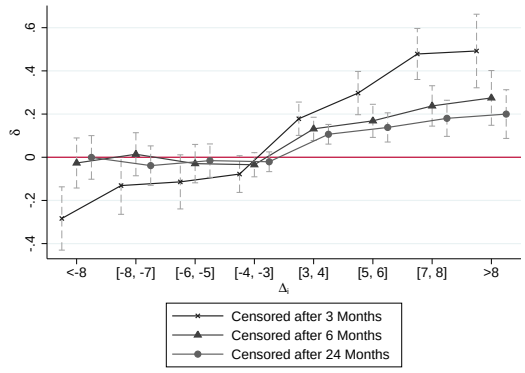
Table 4: Effects on Job Finding Hazard, Under Different Censoring Choices

	6 months			3 months		24 months	
	(1) Coeff	(2) Coeff	(3) Marg. Effect	(4) Coeff	(5) Marg. Effect	(6) Coeff	(7) Marg. Effect
$\Delta_{c,i} < -8$	0.000	-0.021	-0.003	-0.277***	-0.022	0.003	0.001
	(0.073)	(0.071)		(0.089)		(0.062)	
$\Delta_i \epsilon [-8, -7]$	0.040	0.018	0.003	-0.131	-0.011	-0.036	-0.007
	(0.060)	(0.061)		(0.080)		(0.056)	
$\Delta_i \epsilon [-6, -5]$	-0.017	-0.024	-0.004	-0.106	-0.009	-0.013	-0.002
	(0.052)	(0.054)		(0.077)		(0.047)	
$\Delta_i \epsilon [-4, -3]$	-0.028	-0.031	-0.005	-0.070	-0.006	-0.019	-0.004
	(0.035)	(0.034)		(0.050)		(0.028)	
$\Delta_i \epsilon [3, 4]$	0.134***	0.128***	0.020	0.168***	0.016	0.105***	0.020
	(0.035)	(0.032)		(0.047)		(0.027)	
$\Delta_i \epsilon [5, 6]$	0.174***	0.164***	0.026	0.286***	0.028	0.137***	0.026
	(0.049)	(0.046)		(0.059)		(0.041)	
$\Delta_i \epsilon [7, 8]$	0.241***	0.228***	0.037	0.455***	0.048	0.176***	0.034
	(0.062)	(0.056)		(0.069)		(0.050)	
$\Delta_i > 8$	0.275***	0.264***	0.043	0.466***	0.049	0.195***	0.038
	(0.083)	(0.076)		(0.100)		(0.068)	
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. Δ_i is the difference between the requirement and the pre-requirement effort choice. The reference category is $\Delta_i \epsilon [-2, 2]$. Columns (1), (2), (4) and (6) estimate Equation 4 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 on regressions that include all identifying dummy variables and 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.3.

Figure 11: 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))

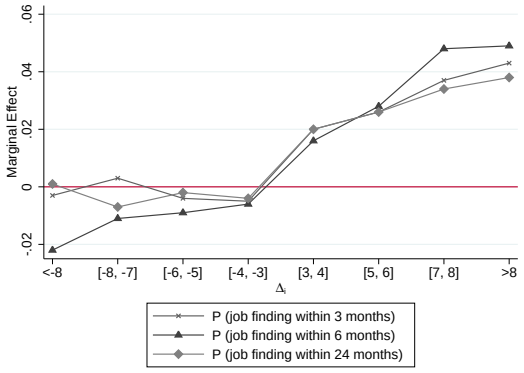


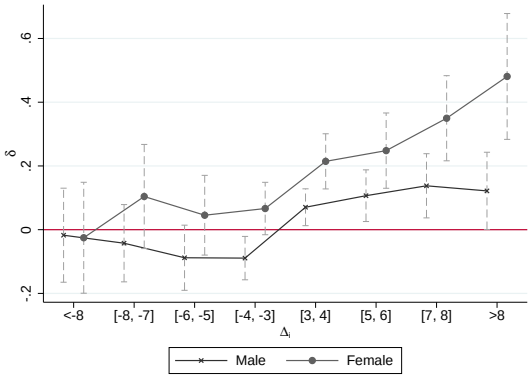
Table 5: Effects on Job Finding Hazard, Subgroup Analysis: Gender and Skill Level of Function in Last Job

	(1) Female	(2) Male	(3) Support	(4) Professional
$\Delta_i < -8$	-0.025	-0.017	-0.025	0.046
	(0.106)	(0.090)	(0.111)	(0.080)
$\Delta_i \epsilon [-8, -7]$	0.104	-0.042	-0.009	0.082
	(0.099)	(0.074)	(0.108)	(0.065)
$\Delta_i \epsilon [-6, -5]$	0.045	-0.088	-0.072	0.036
	(0.076)	(0.062)	(0.081)	(0.053)
$\Delta_i \epsilon [-4, -3]$	0.066	-0.089**	-0.076	0.009
	(0.050)	(0.041)	(0.053)	(0.033)
$\Delta_i \epsilon [3, 4]$	0.214***	0.070**	0.178***	0.074**
	(0.053)	(0.035)	(0.044)	(0.035)
$\Delta_i \epsilon [5, 6]$	0.248***	0.107**	0.223***	0.083*
	(0.072)	(0.049)	(0.066)	(0.045)
$\Delta_i \epsilon [7, 8]$	0.350***	0.138**	0.333***	0.110**
	(0.081)	(0.061)	(0.077)	(0.053)
$\Delta_i > 8$	0.481***	0.122*	0.433***	0.088
	(0.120)	(0.074)	(0.111)	(0.069)
Observations	30890	45514	34169	42235
Exits	20520	13368	19786	14102

$*p < 0.10, * * p < 0.05, * * *p < 0.01$. Standard errors are clustered at the caseworker level. Δ_i is the difference between the requirement and the pre-requirement effort choice. The reference category is $\Delta_i \epsilon [-2, 2]$. Regressions estimate equation 4 using Maximum Likelihood, with durations censored after 180 days of unemployment. They include all identifying dummy variables and 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.

Figure 12: Illustration of Results from Table 5 (with 90% CIs)

(a) Gender



(b) Function in Last Job

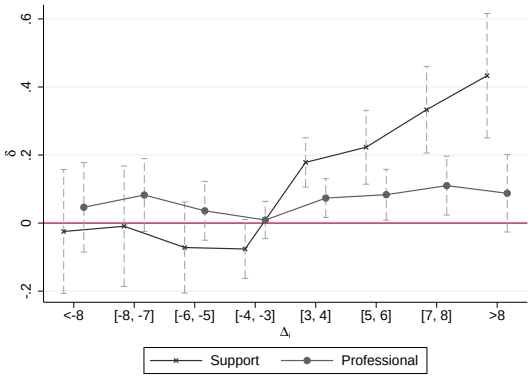


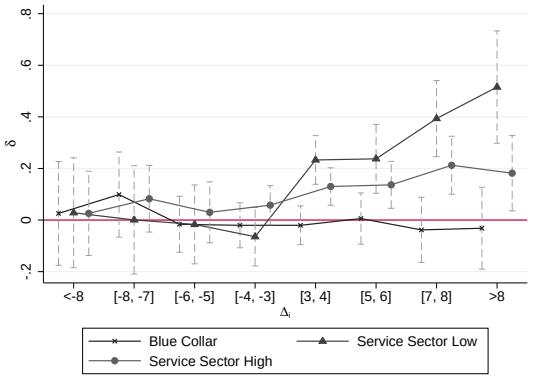
Table 6: Effects on Job Finding Hazard, Subgroup Analysis: Occupation and Regional Vacancy Rate

	(1) Blue Collar	(2) Service Low	(3) Service High	(4) Low Vac. Rate	(5) Medium Vac. Rate	(6) High Vac. Rate
$\Delta_i < -8$	0.026	0.028	0.026	0.039	-0.143	-0.103
	(0.122)	(0.129)	(0.099)	(0.133)	(0.131)	(0.112)
$\Delta_i \epsilon [-8, -7]$	0.099	0.001	0.083	0.036	-0.024	-0.033
	(0.100)	(0.128)	(0.079)	(0.109)	(0.115)	(0.084)
$\Delta_i \epsilon [-6, -5]$	-0.016	-0.017	0.030	-0.040	-0.147	0.001
	(0.066)	(0.093)	(0.072)	(0.075)	(0.099)	(0.082)
$\Delta_i \epsilon [-4, -3]$	-0.020	-0.064	0.058	-0.046	-0.082	-0.032
	(0.053)	(0.069)	(0.046)	(0.057)	(0.065)	(0.052)
$\Delta_i \epsilon [3, 4]$	-0.020	0.233***	0.130***	0.101*	0.129**	0.142***
	(0.046)	(0.057)	(0.044)	(0.052)	(0.056)	(0.046)
$\Delta_i \epsilon [5, 6]$	0.006	0.237***	0.137**	0.065	0.160**	0.252***
	(0.060)	(0.081)	(0.055)	(0.065)	(0.073)	(0.068)
$\Delta_i \epsilon [7, 8]$	-0.038	0.393***	0.212***	0.111	0.242**	0.336***
	(0.077)	(0.090)	(0.068)	(0.079)	(0.098)	(0.079)
$\Delta_i > 8$	-0.032	0.516***	0.182**	0.107	0.320**	0.387***
	(0.096)	(0.132)	(0.089)	(0.105)	(0.127)	(0.115)
Observations	26382	22477	27545	25416	25428	25560
Exits	12135	9251	12502	10484	10240	13164

$*p < 0.10, * * p < 0.05, * * *p < 0.01$. Standard errors are clustered at the caseworker level. Δ_i is the difference between the requirement and the pre-requirement effort choice. The reference category is $\Delta_i \epsilon [-2, 2]$. Regressions estimate equation 4 using Maximum Likelihood, with durations censored after 180 days of unemployment. They include all identifying dummy variables and 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. "Low", "medium" and "high" vacancy rates are understood in relative terms, i.e. we divide the distribution of vacancy rates (per region and month) by 3 to assign job seekers to one of these categories. The mean vacancy rate in our sample is .086.

Figure 13: Illustration of Results from Table 6 (with 90% CIs)

(a) Occupation



(b) Vacancy Rate

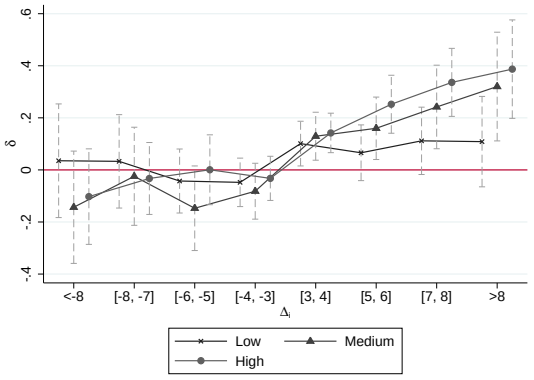


Table 7: Probability of Recurrence to Unemployment within 6/12 Months after Job Finding

	(1) 6 Months, All	(2) 6 Months, Support Function	(3) 12 Months, All	(4) 12 Months, Support Function
$\Delta_i < -8$	-0.044**	-0.069**	0.000	-0.001
	(0.017)	(0.028)	(0.020)	(0.032)
$\Delta_i \epsilon [-8, -7]$	-0.038***	-0.052**	-0.019	-0.023
	(0.014)	(0.022)	(0.015)	(0.023)
$\Delta_i \epsilon [-6, -5]$	-0.031***	-0.051***	0.000	-0.015
	(0.012)	(0.018)	(0.012)	(0.019)
$\Delta_i \epsilon [-4, -3]$	-0.019**	-0.024*	0.003	0.002
	(0.008)	(0.014)	(0.009)	(0.015)
$\Delta_i \epsilon [3, 4]$	0.019***	0.036***	0.006	0.014
	(0.007)	(0.011)	(0.007)	(0.012)
$\Delta_i \epsilon [5, 6]$	0.020*	0.044***	-0.004	0.007
	(0.010)	(0.014)	(0.010)	(0.015)
$\Delta_i \epsilon [7, 8]$	0.044***	0.078***	0.004	0.010
	(0.014)	(0.018)	(0.013)	(0.020)
$\Delta_i > 8$	0.056***	0.096***	0.003	0.022
	(0.018)	(0.024)	(0.017)	(0.026)
Outcome Mean	0.138	0.162	0.267	0.313
N	76404	34169	76404	34169

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. Δ_i is the difference between the requirement and the pre-requirement effort choice. The reference category is $\Delta_i \epsilon [-2, 2]$. All regressions estimate equation 2 using OLS and include all identifying dummy variables and 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. 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 to employment and recurs to unemployment within 6/12 months after the exit. Job seekers with a "support function" held a previous job with supportive instead of professional tasks.

Figure 14: Illustration of Results from Table 7, Column (1) (with 90% CIs)

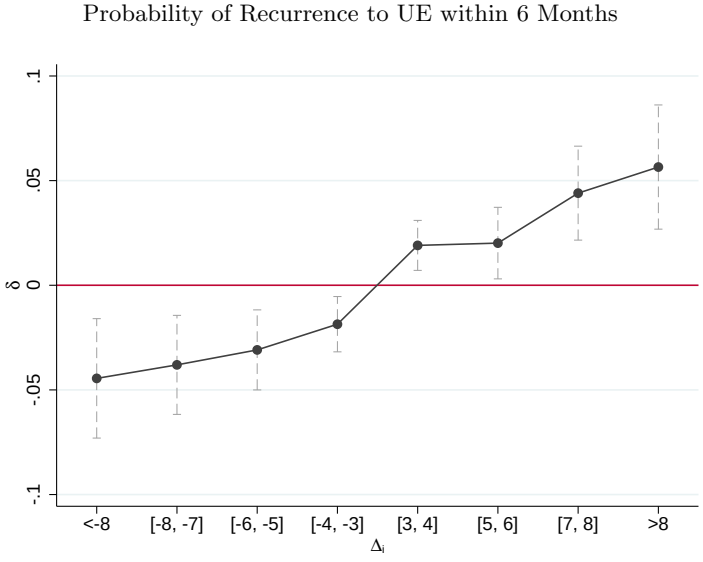


Table 8: Decomposition of Effect on Job Finding (within 6 month): Unstable (recurrence within 6 months) vs. Stable (no recurrence) Jobs

	(1) Job Finding	(2) Job Finding and Recurrence	(3) Job Finding and No Recurrence
$\Delta_i < -8$	0.034	-0.028*	0.062**
	(0.023)	(0.016)	(0.027)
$\Delta_i \epsilon [-8, -7]$	0.034*	-0.027*	0.061***
	(0.017)	(0.014)	(0.021)
$\Delta_i \epsilon [-6, -5]$	0.019	-0.026**	0.045***
	(0.013)	(0.011)	(0.015)
$\Delta_i \epsilon [-4, -3]$	0.006	-0.016**	0.022**
	(0.009)	(0.008)	(0.011)
$\Delta_i \epsilon [3, 4]$	0.021**	0.022***	-0.001
	(0.008)	(0.007)	(0.010)
$\Delta_i \epsilon [5, 6]$	0.031***	0.037***	-0.006
	(0.011)	(0.011)	(0.014)
$\Delta_i \epsilon [7, 8]$	0.027*	0.052***	-0.025
	(0.015)	(0.014)	(0.020)
$\Delta_i > 8$	0.041**	0.074***	-0.033
	(0.019)	(0.020)	(0.025)
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. Δ_i is the difference between the requirement and the pre-requirement effort choice. The reference category is $\Delta_i \epsilon [-2, 2]$. All regressions estimate equation 2 using OLS and include all identifying dummy variables and 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. In addition, the incidence of benefit sanctions and the duration of unemployment in the first spell are controlled for.

Table 9: Robustness Checks

	(1) Baseline	(2) $s_0 > 0$	(3) s_0*Occupation	(4) t_0 = Month of Registration	(5) 1st UE Spell	(6) Not Imputed
$\Delta_i < -8$	-0.021	-0.041	-0.010	-0.117	-0.054	-0.058
	(0.071)	(0.071)	(0.069)	(0.098)	(0.092)	(0.074)
$\Delta_i \epsilon [-8, -7]$	0.018	0.001	0.025	0.011	0.038	-0.020
	(0.061)	(0.061)	(0.060)	(0.085)	(0.077)	(0.062)
$\Delta_i \epsilon [-6, -5]$	-0.024	-0.038	-0.021	0.031	-0.048	-0.065
	(0.054)	(0.054)	(0.053)	(0.067)	(0.063)	(0.057)
$\Delta_i \epsilon [-4, -3]$	-0.031	-0.038	-0.032	-0.035	-0.020	-0.071**
	(0.034)	(0.034)	(0.033)	(0.046)	(0.044)	(0.036)
$\Delta_i \epsilon [3, 4]$	0.128***	0.143***	0.129***	0.119***	0.145***	0.150***
	(0.032)	(0.033)	(0.032)	(0.041)	(0.041)	(0.034)
$\Delta_i \epsilon [5, 6]$	0.164***	0.169***	0.166***	0.143**	0.187***	0.191***
	(0.046)	(0.046)	(0.045)	(0.058)	(0.056)	(0.048)
$\Delta_i \epsilon [7, 8]$	0.228***	0.207***	0.231***	0.184***	0.267***	0.256***
	(0.056)	(0.061)	(0.055)	(0.071)	(0.067)	(0.058)
$\Delta_i > 8$	0.264***	0.329***	0.268***	0.311***	0.285***	0.318***
	(0.076)	(0.080)	(0.075)	(0.090)	(0.088)	(0.076)
Observations	76404	59393	76404	32637	59989	70106
Exits	33888	27127	33888	15790	24748	29798

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the caseworker level. Δ_i is the difference between the requirement and the pre-requirement effort choice. The reference category is $\Delta_i \epsilon [-2, 2]$. All regressions estimate Equation 4 using Maximum Likelihood, with durations censored after 180 days of unemployment. They include all identifying dummy variables and 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. In Column (2), all job seekers in the estimation sample have reported $s_0 > 0$. In Column (3), γ_i^{s0} is interacted with occupation group effects (service sector low/ service sector high/ blue collar). In Column (4), s_i^0 of all job seekers in the estimation sample refers the month of registration. In Column (5), neither t_0 nor t_1 are the same as the month of the first caseworker meeting. In Column (6), all job seekers in the estimation sample have not been formally unemployed for at least 2 years previous to their current spell. Column (7) excludes job seekers with imputed requirement levels.

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A Appendix

A.1 Theory Appendix

We discuss in the following the theoretical background underlying the predictions presented in section 2. According to a slightly adopted version of Abbring, van den Berg and van Ours (2005),[45] the job seeker's value function in presence of a search requirement s^r writes:

$$\rho R = \max_s \left[b - c(s) + \lambda(s) \int_{\phi}^{\infty} (\frac{w}{\rho} - R) dF(w) + I(s < s^r)(1 - \frac{s}{s^r}) p_0 (R_{sanc} - R) \right] \tag{5}$$

p_0 denotes the probability of being sanctioned in case of non-compliance and $R_{sanc} < R$ the expected value of unemployment after benefits have been cut by the benefit sanction. b is the unemployment benefit, s the search effort measured as 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 the optimal effort level s^*. s^* results from a trade-off between the marginal cost of effort $c'(s)$ and its marginal benefit, which involves an increase in the job arrival rate $\lambda'(s)$ and the associated differential in value between employment and unemployment $\int_{\phi}^{\infty} (\frac{w}{\rho} - R) dF(w)$.

In a *system with requirements*, the job seeker chooses her level of effort s by optimizing the present discounted value expressed by equation 5. In this expression, the requirement 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 ($I(s < s^r) = 1$), there is an exogenous probability p_0 that the job seeker receives a benefit sanction which reduces the value of unemployment. This probability becomes more salient when the distance from the provided to the required effort increases $(1 - \frac{s}{s^r})$.

The *difference between the requirement and the job seeker's voluntary effort*, $\Delta^* = s^r - s^*$, determines how individual search behavior is affected by the requirement policy. The following three cases, which are illustrated in Figure 1 (section 2), can be distinguished:

(I) $\Delta^* \leqslant 0$: the job seeker faces a non-binding requirement, which is lower than her voluntary effort level. In this case, all search outcomes are *unaffected by the search requirement* and the job seeker continues providing s^*.

(II) $\Delta^* > 0$ and compliance: the job seeker faces a binding requirement and has to increase search effort by Δ^* to comply. In case II, the individual cost of complying is lower than the cost of facing the risk of a future benefit sanction. *The job seeker therefore chooses to submit $s = s^r$ applications.* As this effort level is sub-optimal from the job seeker's perspective, this behavioral change decreases the present value of unemployment.

(III) $\Delta^* > 0$ and non-compliance: beyond an individual-specific threshold, the job seeker's present value of submitting less applications than required and incurring a given risk of sanction is larger than the present value of complying. In other words, the cost of compliance is too high.

[45] We introduce the term $1 - \frac{s}{s^r}$ to account for the empirical fact that the probability of sanction becomes more likely when the ratio of provided to required effort becomes high. Alternative designs of this term would be possible, without changing the qualitative predictions of the model.

The job seeker now chooses not to comply by choosing an effort level $s < s_r$. The probability of sanction is now positive and lowers the job seeker's present value of unemployment.

A.2 Data Appendix

A.2.1 Sampling criteria for job search data

The empirical analysis is done at the intensive margin and focuses on requirement effects for individuals who are eligible for the job search obligation. To identify these individuals, we make the following sampling restrictions and plausibility assumptions:

First, job seekers are by definition not eligible if they exit unemployment within one month of unemployment or before their first caseworker meeting[46] takes place (in total 8.9% of the initial sample).[47] We also exclude job seekers for whom no first caseworker meeting is reported during the first 90 days of unemployment (1.8%).

For the remaining sample, there is still the possibility that job seekers are systematically exempted from the requirement regime because they qualify for special exemption reasons (maternity, preparation of self-employment, participation at a long-run training program etc.). In these cases, search will not be monitored. To assess whether a job seeker was monitored during a given calendar month t,[48] we use a variable that reports for each month whether the effort was monitored and whether the job seeker was exempted from the requirement of active job search. We define an individual's unemployment spell as systematically affected by the requirement regime if at least two calendar months are monitored up to the third month of unemployment (one of them may refer to the month previous to the month of registration). We exclude individuals who do not meet this criterion (7.46%).

Job seekers are only relevant for our analysis if the caseworker monitored their search effort both before and after requirement imposition. We therefore exclude job seekers whose last monitored record refers to the month previous to their first meeting with the caseworker (2.06%) and job seekers whose first monitored entry refers to the month following their first meeting with the caseworker (2.2%). As the monitoring of both pre-requirement effort and compliance with the requirement are prescribed by law, these individuals are most likely exempted for special reasons and not exposed to the standard treatment.

A.2.2 Imputation of Missing Requirement Levels

Given the available data on caseworkers' requirement setting behavior, imputation of the 8.2% missing requirement entries is straightforward. In a first step, 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 $\hat{s}^r_{c(i)}$ to the next integer and adjust it 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 most plausible that job seekers with missing $\hat{s}^r_{c(i)}$ were assigned to one of these levels. To account for this, we

[46] We define the first caseworker meeting as the first completed meeting of at least 30 minutes. An exception is the canton of 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.

[47] All percentages refer to the sample that was relevant before we apply a sampling criterion.

[48] An entry in the search effort data base always refers to a full calendar month

proceed as follows:

1. We compile for each caseworker a candidate list of plausible requirement thresholds $C_{CW} := \{s^r_{c(i)} | \text{Caseworker assigns } s^r_{c(i)} \text{ in at least 10\% of her cases}\}$
2. We choose $s_C \in C_{CW}$ with $|s_c - \hat{s}^r_i|$ 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 the empirical analysis, we show as a robustness check (section 6.3) that imputed values do not drive the results, as the estimates are robust to their exclusion from the estimation sample.

A.3 Additional Figures and Tables

Figure 15: Descriptive Job Finding Hazard (monthly intervals)

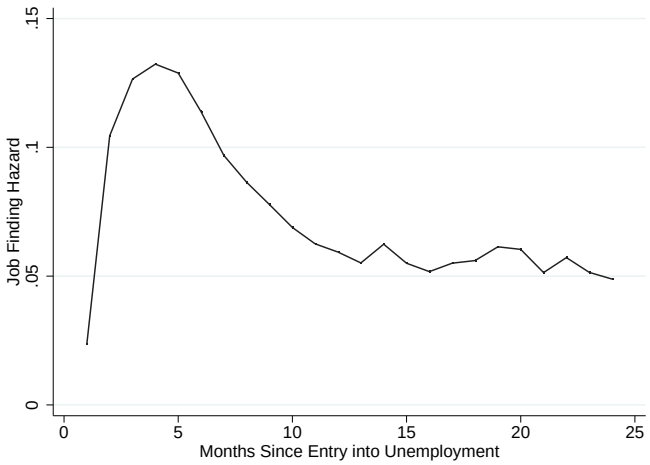
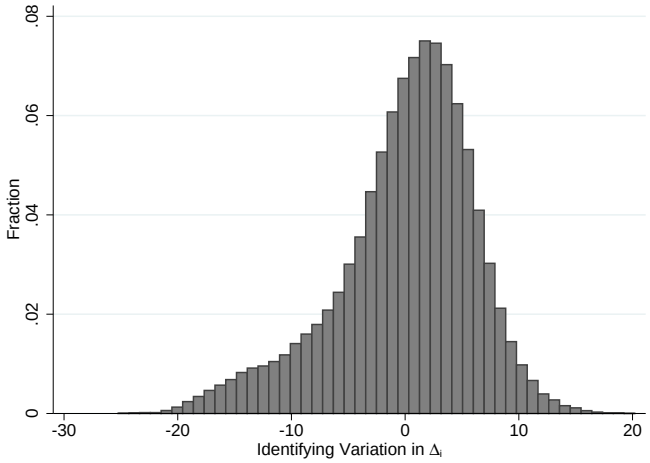


Figure 16: Variation in Δ_i Used for Identification



The graph illustrates the predicted residuals from an OLS regression of Δ_i on γ_i^{s0}, π_c,$r_{c(i)}^{sr}$ and x_i such as defined in section 5.2.

Table 10: Summary Statistics: Requirement and Effort Variables

Variable	Mean	Std. Dev.	Min.	Max.
Δ_i	0.993	7.435	-24	15
s_i^0	7.507	7.114	0	25
Share of job seekers with higher s_i^r (within caseworker)	0.274	0.33	0	0.999
Share of job seekers with lower s_i^r (within caseworker)	0.274	0.225	0	0.999
Cardinal rank of requirement (within caseworker)	5.632	2.045	1	13
N		76404		

Table 11: Summary Statistics: Timing of Requirement Policy

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
Weeks between registration and 1st meeting (rounded):2	0.253	0.435	0	1
Months between t_0 and month of registraion (rounded):-1	0.427	0.495	0	1
Months between t_0 and month of registraion (rounded): 1	0.104	0.305	0	1
Months between t_0 and month of registraion (rounded): 2	0.014	0.119	0	1
Months between t_0 and month of registraion (rounded): 0	0.454	0.498	0	1
N		76404		

Table 12: Summary Statistics: Covariates

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
Civil status: married	0.386	0.487	0	1
Civil status: widowed	0.005	0.071	0	1
Civil status: divorced	0.097	0.295	0	1
Civil status: single	0.513	0.5	0	1
Household members: 1	0.194	0.395	0	1
Household members: 2-3	0.184	0.387	0	1
Household members: 4+	0.013	0.115	0	1
Household members: 0	0.609	0.488	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
Prior UE spells during past 730 days: 0	0.554	0.497	0	1
Prior UE spells during past 730 days: 1	0.28	0.449	0	1
Prior UE spells during past 730 days: 2	0.129	0.335	0	1
Prior UE spells during past 730 days: 3	0.024	0.153	0	1
Prior UE spells during past 730 days: 4+	0.013	0.112	0	1
Length of last UE spell (in days)	76.289	134.55	0	730
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
N		76404		