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Job search requirements, effort provision and labor market outcomes☆

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ABSTRACT

How effective are effort targets? This paper provides novel evidence on the effects of job search requirements on effort provision and labor market outcomes. Based on large-scale register data, we estimate the returns to required job search effort, instrumenting individual requirements with caseworker stringency. Identification is ensured by the conditional random assignment of job seekers to caseworkers. We find that the duration of un- and non-employment both decrease by 3% if the requirement increases by one monthly application. When instrumenting actual applications with caseworker stringency, an additionally provided monthly application decreases the length of spells by 4%. In line with theory, we further find that the effect of required effort decreases in the individual's voluntary effort. Finally, the requirement level causes small negative effects on job stability, reducing the duration of re-employment spells by 0.3% per required application. We find a zero effect on re-employment wages.

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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). Evidence based on large-scale representative data is scarce, as standard data sources rarely report the individual target, the provided effort and the economic outcome simultaneously.

To address this gap, we exploit novel register data to estimate the effects of intensive-margin changes in effort requirements. We consider the context of job search, where unemployment insurance (UI) regimes systematically use search requirements to regulate the provision of effort. Requirements, which usually define a minimum number of monthly job applications enforced through the threat of benefit sanctions, are increasingly used both in Europe and in the United States (see, e.g. Venn, 2012). Nevertheless, it is unknown how job finding reacts to changes in the number of required applications. We provide first evidence by analyzing how the level of the search requirement affects the provision of effort, the duration of unemployment and re-employment outcomes.

The study is based on unique register data from the Swiss UI system, reporting required and provided monthly job applications on an individual level. Our empirical design exploits the conditional random assignment of caseworkers to job seekers, which is based either on caseload or on observable job seeker characteristics. Within a public employment service (PES) office, some caseworkers tend to set higher requirement levels than others, inducing conditionally exogenous variation in the job seeker's expected requirement. We exploit this feature by using caseworker stringency as an instrument for individual requirement levels, conditional on PES × year

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

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fixed effects.[2] Formally, the instrument is the caseworker's average requirement level, excluding the individual's own requirement (leave-out mean). The approach is inspired by an increasing number of studies exploiting judge or caseworker leniency as an instrument for individual treatments (e.g. Aizer and Doyle, 2015; Kling, 2006; French and Song, 2014; Dahl et al., 2014; Autor et al., 2017; Bhuller et al., 2017; Maestas et al., 2013).

Caseworker stringency shows a strong first stage relation with individual requirements: when the caseworker requests one application more from her other cases, the individual's predicted requirement rises by 0.7 applications. In turn, caseworker stringency is hardly related to observed job seeker characteristics and unrelated to other actions taken by the caseworker (assigned training programs, referred vacancies, enforced sanctions). We further find large support of the monotonicity assumption, as caseworker stringency shows a strong first stage for a large set of different subgroups, even when being constructed based on out-of-subgroup individuals.[3]

As a main result, the study shows that the duration of un- and non-employment both decrease by 3% when the requirement increases by one monthly job application (effect of 7 and 10 days, respectively).[4] This estimate is robust to a large number of specification tests and additional controls for actions taken by the caseworker. We also use the identifying variation to estimate the returns to an actually provided search effort (as measured by reported applications). As the elasticity of search effort to the requirement is imperfect (0.67), an additionally provided monthly application decreases the length of spells by 4%.

Furthermore, results reveal that changes in the requirement mostly affect lower-skilled job seekers. For skilled job seekers, targeting the quantity of job applications appears to be less effective. Moreover, we find larger effects among individuals who exhibit low levels of voluntary effort, which we proxy by the number of applications provided prior to the first caseworker meeting. This is in line with theory, as the requirement level imposes stronger effort changes on these individuals.

In a final step, we assess the effect of requirement levels on additional outcome dimensions. First, the willingness to become non-compliant appears to increase with the requirement: the number of imposed benefit sanctions rises by 12% per required monthly application. Second, an additional required monthly application causes a modest reduction in job stability: the length of subsequent re-employment spells decreases, on average, by 0.3%. In turn, we find a zero effect on the re-employment wage.

Our findings provide first estimates of the elasticity to additional search effort imposed by requirements. This has relevant implications for the understanding of job search behavior and the design of modern UI systems. 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 an increased generosity of UI benefits prolongs unemployment spells (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 how marginal changes in required effort translate into job search outcomes, as individual effort is not reported in standard UI registers.

A relatively large strand of the literature analyzes the extensive margin effects of introducing a system of monitored job search requirements. Keeley and Robins (1985) present a seminal paper using individual-level U.S. data to compare the re-employment outcomes of individuals with and without job search requirements. Quasi-experimental evidence includes McVicar (2008, 2010), Petrongolo (2009), Manning (2009) and Bloemen et al. (2013), who study variation arising from the introduction (or temporary abolishment) of systems with different types of monitored requirements.[5] Johnson and Klepinger (1994), Meyer (1995), Klepinger et al. (2002), Ashenfelter et al. (2005), Van den Berg and Van der Klaauw (2006) and Lachowska et al. (2016) analyze field experiments where monitored work search requirements were included.[6] Our approach differs strongly from these studies, as we analyze intensive-margin variation in the quantity of required applications at the individual level. Based on this variation, we can estimate the effect of an additional required application on the provision of effort, the duration of un- and non-employment, as well as re-employment outcomes — within an implemented system of job search monitoring. Thereby, the data in this study allow for novel estimates of the individual-level reaction to a specific, quantitative application requirement.

More generally, this paper makes a novel contribution by estimating the returns to job applications, which represent the most direct form of search effort. Both structural and reduced form analyses commonly assume search effort to be the main source of variation in the duration of unemployment spells, without actually observing effort provision. Recent exceptions include reduced form studies by Marinescu (2017), Baker and Fradkin (2017) and Lichter (2017), who estimate the effects of UI benefit generosity on effort provision using online search data and survey data, respectively. Further, Faberman and Kudlyak (2017) exploit data from an online job search engine to examine the relationship between search intensity and search duration. We contribute with direct evidence from register data on the elasticity of labor market outcomes to job applications that are induced by exogenous variation in requirement levels. The use of high-quality register data ensures full coverage of reported application effort — on online as well as offline search channels.

The paper is structured as follows. We begin by discussing theoretical predictions on the effects of changes in the requirement level (Section 2). Section 3 presents the data sources. In Section 4, we discuss the empirical design. Section 5 presents the results and Section 6 concludes.

[2] PES × year fixed effects account for office- and time-specific policies regarding requirement levels, which may for instance be endogenous to local labor market conditions.

[3] Testing monotonicity based on "reverse-sample instruments" is inspired by Bhuller et al. (2017).

[4] The unemployment duration is defined as the number of days between entry into and de-registration from unemployment (based on UI data). The non-employment duration is defined as the number of days between entry into unemployment and re-employment (based on social security records).

[5] McVicar (2008, 2010) exploits periods where monitoring was temporarily withdrawn during a series of benefit office refurbishments in the UK and Northern Ireland, respectively. Petrongolo (2009) and Manning (2009) analyze the UK JSA reform in 1996, which required benefit recipients to commit to a number of employer and job center contacts. Bloemen et al. (2013) study a Dutch reform requiring the elderly to report their job search efforts to the employment office. Non-experimental evidence comes from Borland and Tseng (2007), who assess the effect of a work search verification program in Australia using a matching approach. In addition to these studies, another strand of literature studies search monitoring systems that operate without explicitly defined requirements. One example is Cockx and Dejemeppe (2012), who exploit an age cutoff in the assignment to monitoring.

[6] Meyer (1995) summarizes lessons from the U.S. Unemployment Insurance Experiments, featuring elements of both job search verification and assistance. Johnson and Klepinger (1994) study the Washington Alternative Work-Search Experiment, which introduced different degrees of requirements. Klepinger et al. (2002) use an experiment run in Maryland to estimate the effects of different requirements (additional employer contacts and participation at a job search workshop). Lachowska et al. (2016) follow up on this study by investigating the experiment's effects on employment outcomes up to nine years later. Ashenfelter et al. (2005) report results from experiments realized in Connecticut, Massachusetts, Virginia and Tennessee introducing stricter verification of job search requirements. Van den Berg and Van der Klaauw (2006) study a Dutch field experiment introducing counseling and monitoring of requirements.

2. Theoretical discussion

In the following, we provide basic theoretical intuition on the expected effects of the individual requirement level. We further discuss factors which are expected to influence the elasticity of individual outcomes with respect to requirement changes. Throughout the discussion, 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.[7]

2.1. Setup

We consider a simple, single-period job search model with requirements and benefit sanctions, similar to Abbring et al. (2005) and Lalive et al. (2005). The value function and its implications for the provision of job search effort are described in Appendix A.1. In absence of a requirement policy, the unemployed individual chooses a voluntary level of search effort e^0, which trades off marginal costs and benefits of effort. The requirement policy introduces a minimum effort level r to be provided by the job seeker. Through the threat of a benefit cut in case of non-compliance, the policy introduces an implicit cost of providing less applications than r. This cost equals the amount of the benefit cut times the enforcement probability, where the benefit cut can depend on the severity of non-compliance (i.e., it decreases in the ratio of provided over required applications).

2.2. Effect of requirement level on effort provision & the duration of non-employment

We are interested in the intensive margin effect of an increase in the requirement level, $\partial r > 0$. Provided that the voluntary effort e^0 is smaller than r, $\partial r > 0$ causes the job seeker to raise provided effort e up to the requirement r if the cost of risking a sanction exceeds the additional effort's net cost (cost of the effort increase minus the benefit from possible additional job offer arrival). For job seekers to whom this applies, we expect to observe bunching at r, and we expect the average provision of effort to increase in the level of r. Job seekers who prefer the risk of a sanction to the provision of r may still – less-than-proportionally – increase their effort in response to $\partial r > 0$, to minimize the expected amount of the sanction (c.f. the discussion in Appendix A.1 on the different types of effort reactions). We therefore expect $\frac{\partial e}{\partial r} > 0, \forall e^0 < r$.

Under the assumption that effort increases the job offer rate λ ($\lambda'(e) > 0$), we expect the duration of non-employment D to decrease in r $\left(\frac{\partial D}{\partial r} < 0, \forall e^0 < r\right)$.[8] The responsiveness of e and D to r can be influenced by different factors. In the following, we describe three factors and provide testable implications for an empirical analysis of their relevance.

1. ***Imperfect Compliance:*** First, the possibility of non-compliance ($e < r$) limits the amount of search effort which policy makers can induce with requirements. Non-compliance arises when individuals expect the cost of non-compliance to be lower than the net cost of an additional application. In these cases, individuals prefer the risk of a benefit cut to an increase in effort. A testable implication of non-compliance is that an increase in the requirement induces a less-than-proportional increase in effort $\left(\frac{\partial e}{\partial r} < 1\right)$. As a consequence, the elasticity of non-employment with respect to a required application should, in absolute terms, be smaller than its elasticity with respect to a provided application $\left(\left|\frac{\partial D}{\partial r}\right| < \left|\frac{\partial D}{\partial e}\right|\right)$. We also expect the incidence of non-compliance to increase in r — as for a given level of e^0, compliance is costlier to achieve for higher levels of r.
2. ***Level of Voluntary Effort:*** Second, the change in effort provision imposed by the requirement depends on whether, and by how much, the requirement exceeds the voluntary effort e^0. The smaller e^0 is relative to the requirement, the larger the incentive for the individual to increase effort provision.
   As a testable implication for the empirical analysis, we should observe heterogeneous effects when interacting the effect of the requirement level with a proxy of voluntary effort. Our data source reports the number of applications which individuals provide in the month before their first caseworker meeting, i.e., before learning about their individual requirement. We use this information as a proxy for e^0, to test whether the effects of requirement increases differ between individuals with high versus low levels of e^0.
3. ***Responsiveness of Job Finding Rate to Application Quantity:*** Third, the responsiveness of the job offer arrival rate to the number of applications may vary according to the individual's labor market characteristics. For instance, the application quantity may have little effects when the application quality matters substantially. As a consequence, we expect lower effects of the requirement level on high-skilled individuals, who have to signal their skills through a high-quality application. The testable implication is that the effect of the requirement level is heterogeneous across skill groups.

2.3. Effect of requirement level on re-employment quality

The effect of the requirement level on re-employment quality is of ambiguous sign. On the one hand, a higher requirement can induce increased sampling of low quality job offers. Furthermore, the reservation wage may reduce due to the disutility associated with being required additional effort. On the other hand, the boost in search effort can extend the scope of search and thereby lead to the sampling of more high quality job offers. Further, the reduction in non-employment duration induced by additional applications can reduce the depreciation of wage offers (see, e.g., the discussion by Schmieder et al., 2016). We will empirically assess these ambiguous predictions by estimating the effect of the requirement level on re-employment job stability and wages.

3. Institutional background & data

3.1. Institutional background

3.1.1. The Swiss Unemployment Insurance (UI) system

In Switzerland, individuals are entitled to UI benefits if they contributed for at least twelve months during the two years prior to unemployment. To be eligible for the full benefit period, the contribution period extends up to 18 months for job seekers up to 55. The potential duration of unemployment benefits is usually 1.5 years for fully eligible prime age individuals. It varies, however, by the job seeker's contribution period, age and family situation. The replacement ratio ranges between 70% and 80% of gross previous earnings, depending on the individual family situation and the level of past earnings.

3.1.2. Caseworkers

To claim benefits, individuals register at the local Public Employment Service (PES) office. As in most OECD countries, the registration is followed by the assignment to a caseworker. According to survey results reported by Behncke et al. (2010), the most common

[7] In most countries, monitoring of compliance with the requirement also includes guaranteeing a minimum quality standard, as caseworkers can for instance check the content of application letters. This is also the case in Switzerland (c.f. Section 3).

[8] In the empirical analysis, we study both the un- and the non-employment durations as an outcome.

assignment criteria in the Swiss UI are caseload, occupation or industry sector (all mentioned by about 50% of surveyed caseworkers and PES officials) and randomness (mentioned by 24%). Caseworkers are in charge of assigning individual requirement levels to the job seekers, in the form of a monthly number of job applications. They also monitor and counsel benefit recipients in their search for work, and refer them to labor market programs. Caseworkers meet with job seekers approximately once every month.

3.1.3. Job search requirements

The first caseworker meeting usually takes place around two to three weeks after registration. At this meeting, the caseworker sets the requirement, i.e., the minimum number of monthly job applications which the job seeker must submit to avoid benefit cuts. Job seekers document their application activity in a monthly "protocol of search effort", which includes all types of applications made. A copy of the official form in German is included in Fig. A.1 of Appendix A.3. In this form, individuals fill in the date of each application, the name and address of the potential employer, the mode of application (written, personal, via phone) and the status of the application. If the application was rejected, job seekers have to fill in the reason of rejection. The protocols are submitted on a monthly basis to the canton or at the PES office (depending on the canton), where they are collected and registered centrally. Job seekers are required to send in copies of their applications together with the protocols. Upon receiving the protocol, cantons or PES offices record the total number of applications in the central database. Further information on the applications are not stored.

Caseworkers are legally obliged to assess whether the provided number of applications satisfies the requirement. They also check whether a minimum quality standard is met. Moreover, caseworkers occasionally verify the truthfulness of reported applications by calling the prospective employer. Once non-compliance with the search requirement is detected, the job seeker is notified and a sanction can be imposed if the job seeker had no special reason or circumstance justifying the non-compliance. The final decision of a sanction is taken by the head of the PES office. In the estimation sample, the unconditional probability of a requirement-related benefit sanction is 0.11, and the probability conditional on not complying at least once is 0.21 (c.f. Appendix Table A.3). The average size of a sanction is the monetary equivalent of 5.5 days of UI benefits (i.e., on average around 900 CHF).[9] Individuals remain registered as unemployed after receiving a sanction.

3.2. Data

3.2.1. Data source and sampling

We base the empirical analysis on individual-level data from the Swiss UI registers, merged to social security records.[10] Our sample covers all individuals who entered UI between 2010 and 2014 in the cantons of Bern, Fribourg, Solothurn and Tessin.[11] In these cantons, job search monitoring is systematically reported in the central database, to which we have access.[12] The four cantons cover around 22% of UI benefit recipients and three different geographic and language regions in Switzerland. The database reports for each calendar month individual-level information on required and provided job applications. In addition, it includes exhaustive information on other treatments assigned over the unemployment spell, socio-demographics, benefit payments as well as employment and unemployment histories. Summary statistics on these variables are reported in Appendix Tables A.1 and A.2.

We define the duration of unemployment as the duration between the registration and de-registration from UI (UI records) and the duration of non-employment as the duration between entry into UI and the first month with earnings from employment (social security records).[13] The duration of unemployment is capped at 520 days (17% of the sample), which is the maximum benefit duration.[14] The duration of non-employment is capped after 900 days (10% of the sample) or at the maximum observation period of the social security data, which is December 2015 (6% of the sample).[15]

We limit the estimation sample to individuals who are aged between 20 and 55, full-time unemployed and not eligible for disability insurance. We further require that the individual was not registered as unemployed during at least one month prior to the current registration. To reduce noise in the instrument, we drop observations whose caseworker has less than 30 cases over the sample period (2.9%, the median caseworker has 191 cases). Dropping caseworkers with few cases is in line with, e.g., French and Song (2014), Maestas et al. (2013) and Dahl et al. (2014). Results are unaffected by modifying the cutoff (c.f. sensitivity analysis in Section 5.4).

Our analysis is at the intensive margin, as we are interested in the effect of marginal changes in the number of required applications, within the population of eligible individuals. Therefore, we exclude individuals who did not receive a requirement during the first six months after their entry into unemployment. By law, every benefit recipient has to receive a requirement. Nevertheless, individuals may not become eligible if they do not stay unemployed until their first meeting with a caseworker, or if their individual situation (e.g. parental leave or participation at an active labor market program) exempts them from the requirement policy. In total, 16% of individuals are excluded because they do not become eligible for a requirement. As individuals cannot anticipate their exact requirement level ex ante, out-of-sample selection with respect to the treatment is highly unlikely. Indeed, Appendix Table A.4 shows that the probability of not having a requirement, i.e., being out-of-sample, is completely unrelated to the assigned caseworker's stringency.

3.2.2. Measurement of individual requirements and effort

The requirement level, r_i, denotes the monthly number of applications to be submitted by individual i. We measure r_i as the default (modal) requirement assigned to an individual during the first six calendar months following the month of entry into unemployment.[16] There is a large stability of requirement levels over the spell. 77% of individuals only have one requirement level during the first six months of unemployment. 92% experience at most one change, and 98% at most two changes. In the sensitivity analysis, we show that results remain unaffected when excluding individuals whose requirement level changes, or when defining r_i as the modal requirement over the first three months of unemployment (c.f. Section 5.4). Fig. 1 plots the sample distribution of requirement levels r_i. Requirements range roughly between 5 and 12 monthly job applications, with a mean of 8.3. The overall standard deviation is 2.4, the between-caseworker

[9] 1 CHF ≈1 USD.

[10] Sources: Swiss State Secretariat for Economic Affairs SECO (for UI register); Central Compensation Office CCO (for social security records).

[11] Prior to 2010, search requirements were not systematically registered in the data.

[12] Federal Swiss law prescribes the enforcement of job search requirements. Therefore, it is ensured that all cantons, including those outside the estimation sample, participate in the requirement policy. Anecdotal evidence suggests that these cantons have their own system of requirement registration rather than employing the central data base.

[13] We require an employment spell to last at least 2 months to define a non-employment exit. We do not count subsidized employment occurring during the formal unemployment spell as employment, unless it directly translates into an un-subsidized employment spell.

[14] The UI data are observed until January 2017.

[15] Results are insensitive to the exact choice of the cap (available upon request).

[16] 5% of individuals have more than one mode. In these cases, the highest mode is used. Results are robust to using the lowest mode instead (available upon request).

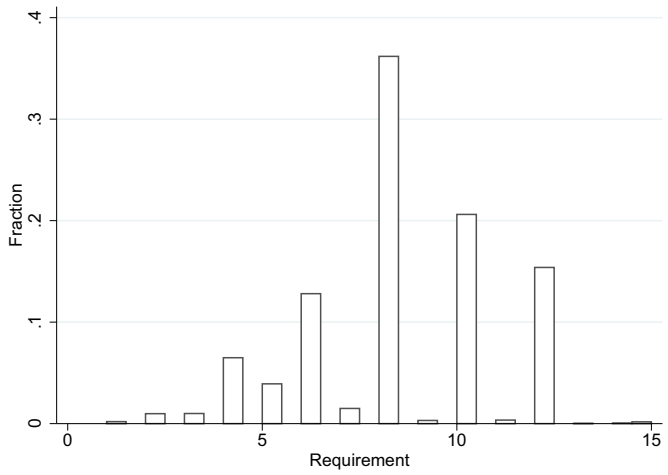

Fig. 1. Distribution of requirement levels. Requirements refer to monthly job applications. Data sources and sampling choices are described in Section 3.2. The mean requirement is 8.3. The overall standard deviation is 2.4. The between-caseworker standard deviation is 1.7 and the within-caseworker standard deviation is 1.8. N = 96,833.

standard deviation 1.7 and the within-caseworker standard deviation 1.8.

The provided effort, e_i, is measured as the reported number of monthly job applications (using either the first month with a requirement or the average over the first six months). In the heterogeneity analysis, we will also exploit the number of applications provided in the month prior to the first caseworker meeting as a proxy for voluntary effort.[17] This information is available for 82,297 individuals (85% of the full sample). Fig. 2 plots the distribution of the two effort variables. As shown in panel a, voluntary effort is broadly spread, and around 15% of individuals provide zero applications voluntarily. The requirement compresses the distribution of effort and induces peaks at the typical requirement levels 6, 8, 10 and 12 (panel b). As shown in panel c, this results from substantial bunching of effort provision at the requirement level, which is in line with the theoretical intuition on how effort responds to the requirement (c.f. Section 2 and Appendix A.1). In the first month under the requirement, around 42% of individuals provide exactly the required number of applications. Around 15% of job seekers provide less applications than required and around 43% provide more.

4. Empirical design

Our empirical design exploits the conditional random assignment of caseworkers to job seekers. Within a PES office, some caseworkers tend to set higher requirement levels than others, inducing conditionally exogenous variation in the job seeker's expected requirement.[18] We exploit this feature by using caseworker stringency (leave-out mean in requirements, excluding individual i's observation) as an instrument for individual requirement levels. The approach is inspired by an increasing number of studies exploiting judge or caseworker leniency as an instrument for individual treatments (e.g. Aizer and Doyle, 2015; Kling, 2006; French and Song, 2014; Dahl et al., 2014; Autor et al., 2017; Bhuller et al., 2017).

In the following, we present the empirical model and assess the instrument.

4.1. Estimation model

We exploit variation in caseworker stringency within PES offices as an instrument for the required and provided number of monthly job applications. We estimate the following baseline model using 2SLS:

$$s_i = \alpha + \gamma\, \bar{r}_{c,-i} + \pi_{o,t} + x_i'\beta + \nu_i \tag{1}$$

$$y_i = \alpha + \delta\, s_i + \eta_{o,t} + x_i'\theta + u_i \tag{2}$$

$$s_i \in (r_i, e_i)$$

The endogenous variable s_i denotes either the search requirement r_i or the provided effort level e_i, both measured in terms of monthly job applications. s_i is instrumented in the first stage Eq. (1) with the average requirement assigned by i's caseworker c to job seekers other than i, $\bar{r}_{c,-i}$ (leave-out mean). $\bar{r}_{c,-i}$ is thus a measure of caseworker c's stringency in the requirement-setting process. The parameter of interest is the coefficient δ in the second stage Eq. (2). It estimates the marginal effect of one additional required/provided job application, induced by caseworker c's stringency, on the outcome y_i. We also estimate reduced form effects of $\bar{r}_{c,-i}$ on y_i.

The terms $\pi_{o,t}$ and $\eta_{o,t}$ include interacted PES office × year fixed effects. They ensure that we only exploit variation in caseworker stringency within offices and time periods. Thereby, office-time specific policies regarding requirement levels, which could for instance be influenced by local labor market conditions, are accounted for. In the sensitivity analysis (Section 5.4), we show that results are unaffected when we include PES office × calendar quarter fixed effects instead. x_i includes job seeker i's socio-demographic characteristics and labor market history (summary statistics reported in Appendix Table A.1). Results show that estimates of δ are invariant to the introduction of x_i. In a sensitivity check, we include further controls for other policy choices made by the caseworker (using corresponding leave-out means) and for caseworker experience into the model. Results remain unaffected.

y_i features the linear durations of un- and non-employment as the main outcomes. Further outcomes include measures of the individual non-compliance propensity, as well as re-employment stability and earnings.

4.2. Assessment of the instrument

In the following, we first provide evidence on the strength of the first stage relationship and then test two conditions regarding instrument validity. The assessment is closely related to Bhuller et al. (2017), who test whether judge stringency is an appropriate instrument for incarceration.

4.2.1. Relevance of the first stage

To illustrate the identifying variation, Fig. 3 plots a histogram of the residual variation in caseworker stringency $\bar{r}_{c,-i}$, conditional on interacted PES office × year fixed effects and individual covariates. There is a considerable amount of residual variation in the average requirement level which caseworkers tend to assign (standard deviation of 0.8 applications).

In addition, the figure illustrates how the residual variation in $\bar{r}_{c,-i}$ relates to the residual variation in required applications, r_i, and provided applications, e_i. The two lines plot local linear regressions of the residual variation in r_i and e_i, respectively, on the one in $\bar{r}_{c,-i}$.[19]

[17] Caseworkers are asked to register this information in the database.

[18] The exogenous assignment criteria are described in Section 3.1. In total, the sample includes 506 caseworkers. A PES office has on average 15 caseworkers over the sample period.

[19] The illustration of the first stage relationship through local linear regressions is inspired by Dahl et al. (2014) and Bhuller et al. (2017).

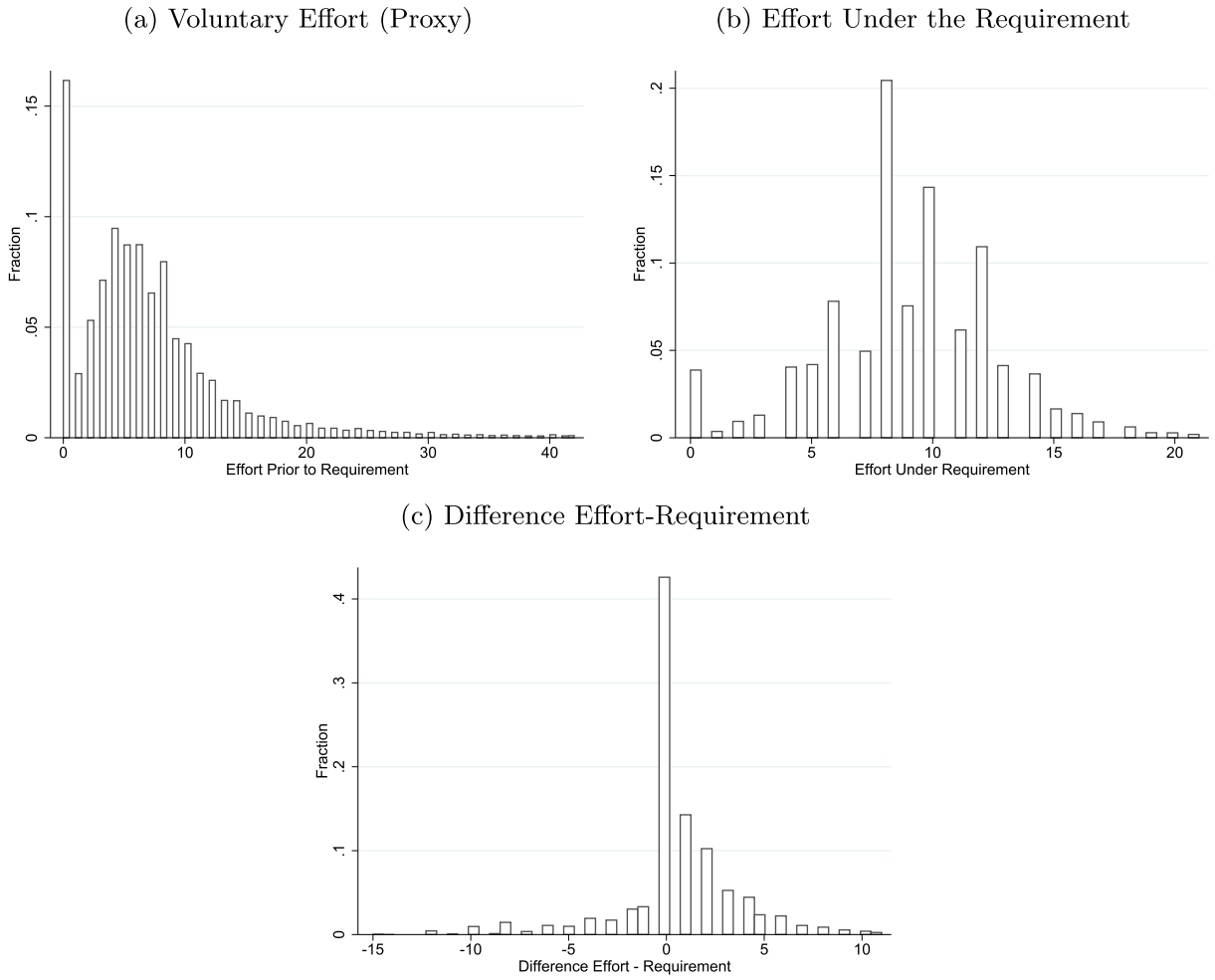

Fig. 2. Distribution of effort levels. Effort levels refer to monthly job applications. Panel a plots the number of applications provided before announcement of the requirement at the first caseworker meeting. Panel b plots the number of applications in the first month under the requirement. Panel c plots the difference between applications provided in the first month under the requirement and the requirement. Data sources and sampling choices are described in Section 3.2. In all three panels, the top 1% are excluded.

The solid line illustrates how individual requirements respond to caseworker stringency along the distribution of caseworker stringency. The line documents a first stage relationship slightly below one. The dashed line plots the response of effort provision. It clearly ranks below the solid line, reflecting that requirements induced by caseworker stringency do not translate one-to-one into increases in effort. The figure thus provides first empirical evidence on the relevance of non-compliance as discussed in Section 2, by showing that $\frac{\partial e}{\partial r} < 1$.

Table 1 reports linear first stage estimates based on regression Eq. (1). Columns 1 and 2 show first stage effects of caseworker stringency on the individual requirement, with and without covariates.[20] When caseworker stringency raises by one monthly application, the individual's requirement raises by 0.7 applications (column 2). The F-statistic of 79 documents the strength of the instrument.

Columns 3 and 4 report effects of caseworker stringency on the average number of monthly job applications provided over the first six months of unemployment. In line with the graphical evidence, results confirm that the reaction of provided effort to caseworker stringency is substantially below its effect on required applications: when caseworkers assign on average one monthly application more to their other job seekers, the individual's number of provided applications only increases by 0.47 applications (column 4). This implies that one required application results, on average, in 0.67 (=0.47/0.7) provided applications. Compliance is thus clearly imperfect. In Appendix Table A.5, we find that the elasticity of effort to caseworker stringency is fairly stable over the first six months of unemployment, as it hardly decreases from 0.48 in months 1–2 to 0.45 in months 5–6(panel A). The same holds true when we regress effort on the instrumented requirement (panel B). The elasticity is 0.69 up to month 4 and decreases slightly down to 0.64 in months 5–6.

4.2.2. Instrument validity

4.2.2.1. Conditional independence. Caseworker stringency ($\bar{r}_{c,-i}$) is valid as an instrument for individual requirements if it is conditionally independent of the job seeker's characteristics. Table 2 assesses to which extent requirement levels and caseworker stringency can be explained by individual characteristics observed in the data.

In column 1, we begin by regressing requirement levels on individual covariates. Results show a large degree of correlation between requirement levels and most of the variables. For example, individuals whose pre-unemployment earnings were higher than the

[20] The fact that first stage estimates do not change strongly in response to the introduction of convariates gives first support to the conditional independence assumption, which will discuss in more detail in Section 4.2.2.

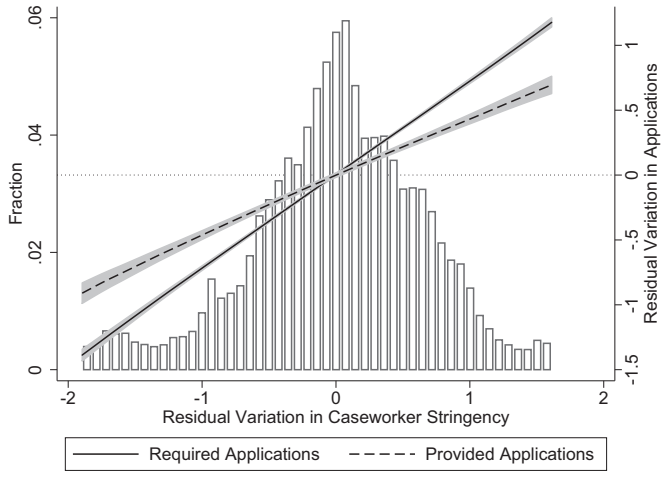

Fig. 3. Caseworker stringency, requirement levels and effort provision. Residuals stem from regressions of the respective variable (caseworker stringency/required applications/provided applications) on interacted PES × calendar year fixed effects and job seeker covariates. Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The graph is inspired by Dahl et al. (2014) and Bhuller et al. (2017). The histogram shows the density of residuals in caseworker stringency along the left y-axis (top and bottom 2% excluded). The solid line plots a local linear regression of residuals in required monthly applications on residuals in caseworker stringency. The solid line plots a local linear regression of residuals in provided monthly applications on residuals in caseworker stringency. The gray areas show 95% confidence intervals (standard errors clustered at the caseworker level).

median have to write 0.4 monthly applications less on average, and non-permanent residents have to write 0.3 applications more. The introduction of PES × year effects in column 2 hardly changes the pattern.

In columns 3 and 4, the outcome is our measure of caseworker stringency, $\bar{r}_{c,-i}$. Column 3 excludes PES office × year fixed effects from the regression. In this specification, results report a high degree of correlation between $\bar{r}_{c,-i}$ and most individual characteristics. Column 4 shows that this largely results from between-office variation, as the number and size of significant coefficients strongly reduces after the introduction of PES office × year fixed effects. Within offices, individuals aged older than the median have, on average, caseworkers who require 0.031 monthly applications less. Further, individuals previously employed in the white collar sector have caseworkers who require on average 0.063 monthly applications less, and individuals with low levels of education have caseworkers who require on average 0.041 applications more. This reflects a certain specialization of caseworkers on sectors and age profiles, as the assignment of job seekers to caseworkers may -in addition to caseload- be based on occupation or industry (c.f. Section 3.1). Nevertheless, the size of the correlation is economically small, provided that the standard deviation of $\bar{r}_{c,-i}$ amounts to 0.8 after conditioning on PES office × year fixed effects. None of the other covariates relates significantly to $\bar{r}_{c,-i}$. In particular, there remains no relation to the individual's previous earnings and labor market attachment, which likely reflect a large degree of unobserved productivity differences. Indeed, it is unlikely that unobserved characteristics influence the assignment, because the officer deciding on the assignment only disposes of information which is also contained in the administrative data. This strongly suggests that we can address the small amount of non-randomness due to observable-based assignment by controlling for a flexible vector of age dummies and detailed occupation dummies.[21]

4.2.2.2. Exclusion restriction. An additional challenge when using caseworker stringency as an instrument for requirements is the role of other, potentially related, policy choices. These could possibly affect the exclusion restriction. For instance, caseworkers who assign on average higher requirements may also enforce the compliance with rules more strictly. Similar issues are discussed in the empirical literature using judge stringency (in particular by Bhuller et al., 2017; Mueller-Smith, 2017), where judges not only decide on incarceration, but also on, e.g., fines, community service, probation, and guilt.

In Table 3, we test whether the instrument $\bar{r}_{c,-i}$ correlates with other decisions typically taken by UI caseworkers. To this aim, we regress $\bar{r}_{c,-i}$ on the probability that job seeker i experiences other treatment assignments during the first six months after entry into unemployment. We distinguish between the assignment to training programs, the referral of vacancies, and the incidence of sanctions due to the non-compliance with a rule. To measure the type of vacancies a caseworker tends to refer, we also consider the share of vacancies that are in the job seeker's prior occupation.[22] Sanctions are distinguished according to whether they relate to the requirement (the job seeker provided too little or no applications) or not (e.g., the job seeker was not present at a caseworker meeting or at a training program). Summary statistics on the variables are reported in Appendix Table A.2. In columns 1 and 2, the dependent variable is the individual's requirement level. No matter whether regressions include PES office × year fixed effects (column 2) or not (column 1), there is a large degree of correlation between the individual requirement level and all the other assignments.

In column 3, the dependent variable is the caseworker stringency measure $\bar{r}_{c,-i}$, but interacted PES office × year fixed effects are excluded from the regression. Results show a high degree of correlation, which is likely due to office-specific policy regimes. In offices where requirements are high, individuals are on average referred to more vacancies and assigned to more training programs. However, after we introduce PES × year effects in column 4, caseworker stringency only relates to sanctions which refer to a non-compliance with the requirement. This relation is most likely a mechanical result of job seeker behavior, reflecting that job seekers with higher requirements face higher costs of compliance. Indeed, sanctions which are unrelated to requirements show no significant relation to $\bar{r}_{c,-i}$. This is also the case for the probability of a vacancy referral and the type of referred vacancies, as well as for the probability of a training assignment. Finally, column 5 controls for the job seeker's

Table 1
First stage regressions.

	Requirement		Effort	
	(1)	(2)	(3)	(4)
Caseworker stringency	0.766***	0.700***	0.487***	0.465***
	(0.022)	(0.023)	(0.341)	(0.335)
F-stat (test for underidentification)	79.45	79.01	61.65	61.18
Covariates	No	Yes	No	Yes
Outcome mean	8.287	8.288	8.517	8.518
N	96,833	96,833	96,833	96,833

Requirement and effort refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

[21] Summary statistics on job seeker covariates included in the baseline regressions are reported in Appendix Table A.1.

[22] We classify jobs as being in the same occupation if the three first digits of the occupation codes coincide.

Table 2
Relation of job seeker characteristics to requirements and caseworker stringency.

	Requirement		Caseworker stringency (leave-out mean)	
	(1)	(2)	(3)	(4)
Female	−0.126***	−0.075***	−0.060**	−0.000
	(0.037)	(0.022)	(0.030)	(0.015)
Aged > Median	−0.312***	−0.450***	0.125***	−0.031***
	(0.034)	(0.024)	(0.026)	(0.008)
Married	−0.058**	−0.099***	0.025	−0.008
	(0.024)	(0.017)	(0.018)	(0.007)
HH size > 2	−0.141***	−0.048***	−0.107***	−0.005
	(0.031)	(0.016)	(0.025)	(0.008)
Non-Permanent Resident	0.288***	0.088***	0.172***	0.003
	(0.030)	(0.018)	(0.028)	(0.010)
Low Education	0.065	0.185***	−0.051	0.041***
	(0.040)	(0.025)	(0.034)	(0.014)
Blue Collar	−0.388***	−0.096***	−0.282***	0.032
	(0.073)	(0.032)	(0.070)	(0.023)
White Collar	−0.326***	−0.312***	−0.121*	−0.063***
	(0.071)	(0.031)	(0.066)	(0.020)
No of Prev. UE Spells	−0.054	−0.041**	0.027	0.007
	(0.044)	(0.021)	(0.032)	(0.007)
Previous Earnings > Median	−0.384***	−0.174***	−0.156***	0.005
	(0.034)	(0.021)	(0.028)	(0.010)
Share Employed in 5 Yrs Pre-UE	−0.511***	−0.114***	−0.341***	0.000
	(0.057)	(0.034)	(0.047)	(0.019)
PBD < 400 days	−0.003	−0.008	0.014	0.008
	(0.029)	(0.018)	(0.024)	(0.008)
Office × Year FE	No	Yes	No	Yes
Outcome Mean	8.287	8.287	8.287	8.287
N	96,833	96,833	96,833	96,833

Requirement levels refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). UE = unemployment, PBD = potential benefit duration. Previous unemployment and employment are specified as linear variables. The sector of activity takes the three values "blue collar", "white collar" and "low-skilled service sector" (baseline). All other covariates are specified as binary variables. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

non-compliance with the requirement (through dummies for the number of incidences in which the provided number of applications was lower than the requirement). Here, the correlation between stringency and the probability of a sanction cuts by half and becomes statistically insignificant. This exercise obviously suffers from a bad control problem, as non-compliance is an outcome of the requirement level. It does, however, give additional support to the intuition that the caseworker's sanctioning behavior does not correlate systematically with her stringency in the requirement setting process. One likely reason is that the caseworker's discretion in imposing sanctions is smaller than her discretion in setting requirements, as the final sanctioning decision is made by the PES office head.

Taken together, it appears that caseworkers who are more stringent requirement setters do not behave in a systematically different way when it comes to other observed policy choices. This evidence is further supported in Section 5.4, where we include the leave-out means of other policy choices made by the caseworker as control variables and find that results are unaffected.

As an additional check of the exclusion restriction, Table 4 tests for a relationship between the caseworker's average requirement

Table 3
Relation of other policy assignments to requirements and caseworker stringency.

	Requirement		Caseworker stringency (leave-out mean)		
	(1)	(2)	(3)	(4)	(5)
P (Sanction Related to Requirements)	0.029	0.176***	−0.085**	0.054***	0.025
	(0.043)	(0.024)	(0.038)	(0.019)	(0.018)
P (Sanction Unrelated to Requirements)	0.143***	0.207***	−0.060*	0.017	0.012
	(0.042)	(0.026)	(0.035)	(0.013)	(0.013)
P (Training Program)	0.416***	0.235***	0.168***	0.010	0.012
	(0.062)	(0.022)	(0.064)	(0.015)	(0.015)
P (Vacancy Referral)	0.987***	0.401***	0.367***	0.011	0.010
	(0.051)	(0.028)	(0.047)	(0.018)	(0.018)
Share Vacancies of Same Occupation	0.114**	0.050*	0.055	0.017	0.017
	(0.052)	(0.027)	(0.040)	(0.015)	(0.015)
Office × Year FE	No	Yes	No	Yes	Yes
Controls for Non-compliance	No	No	No	No	Yes
Outcome Mean	8.287	8.287	8.287	8.287	8.287
N	96,833	96,833	96,833	96,833	96,833

Requirement levels refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). Sanctions are classified as "related to requirements" if their stated reason is the under-provision of job applications by the job seekers, and as "unrelated" otherwise. Vacancy referrals are classified as "same occupation" if the first three digits of the vacancy's occupation code are the same as those of the job seeker's previous job. All regressions control for individual covariates (x_i). In column 5, regressions additionally include dummies for the number of non-compliances (i.e., incidences in which effort is below the requirement) realized over the first six months. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

Table 4
Relation of caseworker experience to average assigned requirement.

	Average requirement set by the caseworker			
	(1)	(2)	(3)	(4)
Pre-sample No of Cases / 100	0.004			
	(0.008)			
Pre-sample No of Yrs Worked at PES		0.003		
		(0.011)		
Sample No of Yrs Worked at PES			−0.026	
			(0.037)	
Total No of Yrs Worked at PES				0.001
				(0.009)
Outcome Mean	8.129	8.129	8.129	8.129
N	506	506	506	506

The pre-sample period is 2010–2009. As the unit of observation is the caseworker, the number of observations reduces to 506. In column 1, the explanatory variable is the number of cases (divided by 100) which the caseworker treated during the pre-sample period 2000–2009 (mean = 5.8). In column 2, it is the number of years which the caseworker worked at the PES during the pre-sample period 2000–2009 (mean = 4.6). In column 3, it is the number of years which the caseworker worked at the PES during the sample period 2010–2014 (mean = 3.8). In column 4, it is the total number of years worked at the PES from 2000 to 2014 (mean = 8.5). All regressions include PES office fixed effects, but no additional covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors.

and her on-the-job experience.[23] To this end, we regress the average requirement assigned by a caseworker on different measures of experience and cases handled. In column 1, the explanatory variable is the number of cases handled by the caseworker during the ten years prior to the sample period.[24] In column 2, we consider the pre-sample number of years worked at the PES, in column 3 the in-sample number of years, and in column 4 the total observed number of years. None of the columns reports a significant relationship between experience and the average requirement set by the caseworker.

4.2.2.3. Monotonicity. Finally, we test for the monotonicity of our instrument. In his study on the effects of incarceration, Mueller–Smith (2017) argues that monotonicity can be violated in settings when judge stringency is used as an instrument, because judges can be more stringent in some and more lenient in other cases. In our setting, monotonicity would be violated if some caseworkers systematically set higher requirements for some types of job seekers and lower ones for others.

As pointed out by Bhuller et al. (2017), the monotonicity assumption has two testable implications in the context of judge/caseworker stringency instruments. On the one hand, the first stage relationship should be non-negative for any subsample. Bhuller et al. (2017) propose to test this implication by constructing the instrument based on the entire sample, and then using it in first stage estimations on subsamples. We implement this approach in column 1 of Table 5. We first reproduce the linear first stage relationship for the entire estimation sample, which is 0.7. The sample is then split according to the job seeker's sector of activity (blue collar, low-skilled service sector and white collar), but the instrument remains the same. For all three subsamples, the first stage coefficient remains around 0.7. We perform the same exercise for a large number of additional job seeker covariates; the results are reported in column 1 of Appendix Table A.6. The first stage is positive and strong for all subsamples.

[23] We do not observe additional caseworker characteristics in the data.

[24] We do not consider the number of cases handled during the sample period, as it is a potential outcome of the average unemployment duration of a caseworker's cases, which, in turn, can depend on the caseworker's requirement setting strategy.

Table 5
Testing monotonicity.

	Requirement (first stage)	
	Baseline instrument	Reverse sample instrument
	(1)	(2)
0. Full Sample		
CW Stringency	0.700***	
	(0.023)	
Outcome Mean	8.287	
N	96,833	
1. Subsample: Blue Collar Sector		
Estimate	0.662***	0.587***
	(0.027)	(0.031)
Outcome Mean	8.181	8.181
N	34,057	34,057
2. Subsample: Low-qualif. Service Sector		
Estimate	0.721***	0.683***
	(0.035)	(0.039)
Outcome Mean	8.653	8.653
N	24,935	24,935
3. Subsample: White Collar Sector		
Estimate	0.697***	0.670***
	(0.027)	(0.032)
Outcome Mean	8.142	8.142
N	37,841	37,841

The test for instrument monotonicity is inspired by Bhuller et al. (2017). The reverse sample instrument is based on individuals excluded from the given subsample. For example, the reverse sample instrument for the white collar subsample is computed based on individuals in the blue collar or low-skilled service sector.The requirement refers to monthly job applications. All regressions include interacted PES office × year fixed effects and individual covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Results for subsamples based on other individual characteristics are provided in Appendix Table A.6.

As a second implication of monotonicity, the first stage relationship needs to be non-negative in a given subsample when the instrument is constructed based on individuals outside the subsample (c.f. Bhuller et al., 2017). This is tested in column 2 of Tables 5 and A.6. We construct a "reverse-sample instrument", which corresponds to the caseworker mean of requirements for individuals excluded from the subsample. This instrument is then used in a first stage regression run on individuals in the subsample. Results show that the coefficient size slightly decreases relative to the baseline instrument. However, it is consistently positive and ranges between 0.55 and 0.7. We thus find no evidence that the monotonicity assumption is violated in our setting.

5. Results

How does the requirement level affect labor market outcomes? We first report the requirement's average effect on the duration of un- and non-employment. We also estimate the effect of actually provided effort. In a second step, we analyze heterogeneity to understand the factors which influence the effect of search requirements. We then study how the requirement affects individual non-compliance and the incidence of benefit sanctions. Fourth, we assess effects on the quality of accepted jobs. In a final step, we provide several sensitivity analyses.

5.1. Effects on the duration of un- and non-employment

5.1.1. Average effects

5.1.1.1. OLS estimates. Before discussing the causal effect of changes in search requirements, we report OLS results as a baseline. We

Table 6
Effect on the duration of un-/non-employment: OLS estimates.

	UE duration	NE duration	UE duration	NE duration
	(1)	(2)	(3)	(4)
Requirement	3.203***	5.533***		
	(0.462)	(0.790)		
Effort			6.557***	5.801***
			(0.333)	(0.412)
Outcome Mean	257.672	302.698	257.672	302.698
N	96,833	96,833	96,833	96,833

Requirement and effort refer to monthly job applications. All regressions include interacted PES office × year fixed effects and individual covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

expect OLS estimates to be biased because caseworkers take individual labor market characteristics into account when setting requirements. Table 6 reports results from OLS estimations regressing the outcome on the individual requirement level, while controlling for PES office × year effects and individual covariates.

Results report a negative correlation between the size of the requirement and unemployment exit. Increasing the requirement by one application is associated with a 3 days longer unemployment spell (column 1) and a 6 days longer non-employment spell (column 2).[25] This may point to a selection mechanism according to which caseworkers assign higher requirements to less employable job seekers. Similarly, an individual who provides one more application per month stays, on average, about 6 days longer in un- and non-employment (columns 3 and 4). These associations are in line with Faberman and Kudlyak (2017), who study US job search data from an online job search engine and observe that job seekers who take longer to find a job send relatively more applications throughout their entire spell.

5.1.1.2. IV estimates. We now turn to the causal analysis, using caseworker stringency (leave-out mean of requirements, $\bar{r}_{c,-i}$) as an instrument for individual requirement levels. We first provide graphical evidence on the reduced-form relationship between caseworker stringency and the duration of unemployment in Fig. 4. Inspired by Dahl et al. (2014), the graph represents an analogue of Fig. 3. It plots the residual variation in the unemployment duration (conditional on PES office × year fixed effects and covariates) against the residual variation in caseworker stringency. Based on local linear regressions, the figure reveals that unemployment spells shorten when caseworkers tend to set higher requirements. This is the case along the whole support of stringency.

Table 7 presents the linear regression results. In panel A, only interacted PES office × year fixed effects are included in the regressions. We add individual covariates (x_i) in panel B.[26] Column 1 shows the reduced-form effect of $\bar{r}_{c,-i}$ on the duration of unemployment. It reports that the duration decreases by 5 days when $\bar{r}_{c,-i}$ increases by one monthly application (panel B). As shown by column 2, the non-employment duration (i.e., the duration until reentering employment) decreases by 7 days. The coefficient signs thus switch compared to the OLS estimates, confirming the intuition that requirement levels are endogenous to the job seeker's employability.[27]

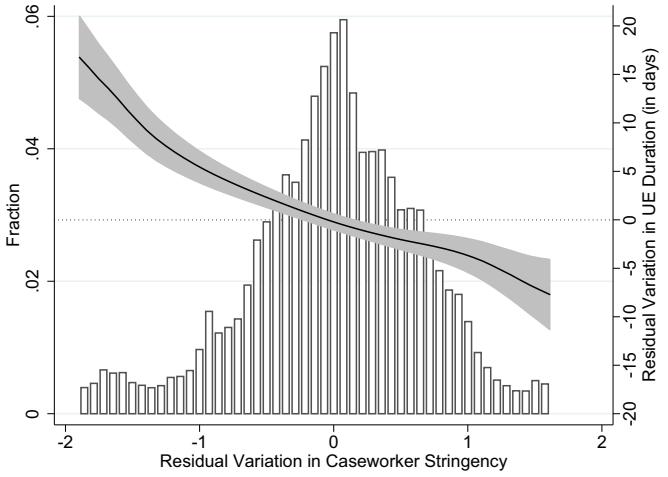

Fig. 4. Caseworker stringency and unemployment duration. Residuals stem from regressions of the respective variable (caseworker stringency/unemployment duration) on interacted PES × calendar year fixed effects and job seeker covariates. Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The graph is inspired by Dahl et al. (2014). The histogram shows the density of residuals in caseworker stringency along the left y-axis (top and bottom 2% excluded). The solid line plots a local linear regression of residuals in the duration of unemployment on residuals in caseworker stringency. The gray areas show 95% confidence intervals (standard errors clustered at the caseworker level).

To estimate the return to an additional required application, columns 3 and 4 scale up the coefficients by the first stage (c.f. Section 4.2 and Table 1 for the first stage estimates). The IV estimates show that individuals reduce unemployment by 7 days and non-employment by 10 days when they are required to send one more application per month. Both estimates correspond to an effect of 3% relative to the mean. Appendix Table A.8 shows that the total number of months during which individuals receive UI during their first year decreases by 0.12 months (2%) per additional required application, while the total number of months in employment increases by 0.11 months (2%). Further, Appendix Table A.9 reports effects on the probability to find a job (i.e., to exit non-employment) after 3, 6, 12 and 18 months. The effect in terms of percentage points is roughly stable over the outcome periods, implying that the percentage effect relative to the outcome mean decreases over the spell. The effect thus operates mostly at early stages.

As a further analysis, Appendix Table A.10 reports how the probability to enter a PES-posted job is affected by the requirement. Coefficients for individuals exiting after different periods of non-employment all show an increase by around 2 percentage points (20% relative to the mean). This suggests that increased requirement levels also foster the use of PES-posted vacancies as application channel. One explanation is that applications to PES-posted vacancies are easier to monitor by the caseworker and presumably impose low search costs to the job seeker. However, it has to be noted that PES-posted vacancies only cover a small proportion of all job findings (around 9% of exits from non-employment).[28]

Finally, we use the identifying variation to estimate the returns to provided search effort. In columns 5 and 6, we report IV estimates on the elasticity of un- and non-employment durations to the number of provided monthly job applications (first stage reported

[25] The duration of unemployment is capped at 520 days, while the duration of non-employment is capped after 900 days or at the maximum observation period of the social security data (December 2015).

[26] Summary statistics on covariates are reported in Appendix Table A.1.

[27] Appendix Table A.7 shows that the introduction of PES office × year fixed effects are central for causal identification, as coefficients are similar to the biased OLS estimates when fixed effects are excluded. This shows that a PES' average requirement level correlates with the average unemployment duration of job seekers in a PES area. The use of between-PES variation in requirement levels would thus suffer from an endogeneity problem.

[28] An open question is whether applications to PES-posted vacancies explicitly come at the expense of job finding through other channels. For instance, Van den Berg and Van der Klaauw (2006) show that individuals tend to substitute informal by formal job search when they are monitored by the PES. We leave this question for future research which disposes of data on search channels.

Table 7
Effect on the duration of un-/non-employment: IV estimates.

	Reduced form		2SLS: requirement		2SLS: effort	
	UE duration	NE duration	UE duration	NE duration	UE duration	NE duration
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Without Controls						
CW Stringency	−5.675***	−7.839***				
	(1.321)	(2.158)				
Requirement			−7.412***	−10.237***		
			(1.778)	(2.893)		
Effort					−11.654***	−16.096***
					(2.935)	(4.724)
Panel B: W/ Controls for Individual Covariates						
CW Stringency	−5.044***	−6.950***				
	(1.226)	(1.919)				
Requirement			−7.205***	−9.927***		
			(1.792)	(2.806)		
Effort					−10.842***	−14.938***
					(2.861)	(4.403)
Outcome Mean	257.672	302.698	257.672	302.698	257.672	302.698
N	96,833	96,833	96,833	96,833	96,833	96,833

Requirement and effort levels refer to monthly job applications. They are instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). All regressions include interacted PES office × year fixed effects. In panel B, regressions additionally include individual covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

in Section 4.2). As a consequence of imperfect effort compliance, the effects of an induced application exceed those of a required one: the duration of unemployment decreases by 11 days (4% relative to the mean), and the duration of non-employment by 15 days (5% relative to the mean). These estimates correspond to the marginal effect of a monthly application on job finding, keeping labor market tightness constant.[29]

Overall, the coefficients do not change significantly from panel A to B, suggesting a minor influence of job seeker covariates. We will document the robustness to additional control variables and sample changes in Section 5.4.

5.1.2. Heterogeneous effects

We now assess the underlying effect heterogeneity, to analyze the factors which might influence the responsiveness of unemployment durations to required job applications. First, we study how the effect differs by the individual effort chosen before the first caseworker meeting, to proxy heterogeneity by voluntary effort. In a second step, we explore heterogeneity by the job seeker's individual labor market characteristics.

5.1.2.1. Voluntary effort. In Table 8, we use the number of applications provided before the first caseworker meeting as a source of heterogeneity.[30] Since individuals do not know their caseworker or their exact requirement ex-ante, we interpret their pre-meeting application effort as a proxy for voluntary effort.[31] Recognizing that the proxy is possibly noisy and can be mis-reported by the job seeker, we use it to test whether the effect of requirement changes depend on the individual's initial voluntary effort.[32] The theoretical intuition is that requirement increases are less costly for individuals whose voluntary effort is already at a high level. Therefore, the effect of an increased requirement is expected to be larger for individuals who have low levels of voluntary effort.

The empirical results support this intuition. Individuals whose initial effort level ranks below the median (5 monthly applications) show significantly stronger reactions to the requirement. As reported in column 1, a one-unit increase in caseworker stringency induces a decrease of 7.5 days of unemployment for this group. One additional required application reduces unemployment by 10 days (4% relative to the mean, column 2). Both effects are half as large for individuals with above-median initial effort levels (effect difference significant at the 5% level). When using non-employment as an outcome in columns 3 and 4, point estimates are 3 times larger for the group with lower initial effort. Fig. 5 additionally shows that the coefficient size decreases with the individual's quartile in the distribution of initial effort.[33] It also reveals a concave pattern in the effect heterogeneity, as individuals in the lowest quartile show by far the strongest reaction. This may reflect (i) convex effort costs and (ii) the fact that the share of individuals who do not have an incentive to react to the instrument increases in e_0.

5.1.2.2. Individual labor market characteristics. As a second heterogeneity analysis, we explore whether the effect of required job applications differs by skill levels and socio-demographics. We thereby test whether the elasticity of job finding to the number of applications depends on the individual's labor market characteristics. From a policy perspective, this question is relevant for the design of targeted requirement levels.

Columns 1 and 2 of Table 9 reveal that the effect of an additional required application on the duration of unemployment is stronger for individuals with relatively low educational attainment, as well as for individuals who were previously employed in the low-qualified service sector (mostly cleaning and restaurants). For both groups, the duration of unemployment decreases significantly more than for individuals with relatively high skills. A plausible explanation is

[29] Note that an extrapolation of these results to imposing more applications to all job seekers would need to account for equilibrium effects, which can be significant, as the recent literature has shown for the cases of UI benefit extensions and training programs (Crépon et al., 2013; Lalive et al., 2015; Gautier et al., 2018).

[30] This information is only available for 82,297 individuals (85%). Caseworkers are asked to register this information in the database, since individuals are required to actively search for work as soon as they learn about the termination of their job.

[31] Appendix Table A.11 shows that there is no relation between the proxy of voluntary effort and caseworker stringency.

[32] Our focus is on effort at the beginning of the unemployment spell, and we take no stance on whether our measure of e_0 proxies voluntary search in later periods of the spell.

[33] The average initial effort is 0.5 applications in the first quartile, 3.9 applications in the second quartile, 6.9 applications in the third quartile and 16.6 applications in the fourth quartile.

Table 8
Effect heterogeneity: job seeker's voluntary effort.

	Unemployment duration		Non-employment duration	
	Reduced form	2SLS		
	(1)	(2)	(3)	(4)
CW Stringency × Low e_0	−7.477***		−11.658***	
	(1.475)		(2.859)	
CW Stringency × High e_0	−3.618**		−3.291*	
	(1.464)		(1.927)	
Requirement × Low e_0		−10.211***		−15.784***
		(1.990)		(3.868)
Requirement × High e_0		−5.473***		−5.280*
		(2.096)		(2.769)
p-Value for H_0: coeff (high) = coeff (low)	0.027	0.035	0.005	0.006
Outcome Mean	249.660	249.660	297.859	297.859
N	82,297	82,297	82,297	82,297

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). An individual is classified as "low e_0" if she provided less than 5 applications (sample median) in the month preceding her first caseworker meeting. The sample size differs from the baseline sample because information on prior application activity is not available for 14,536 individuals (15%). All regressions include interacted PES office × year fixed effects and individual covariates. PES office × year fixed effects are fully interacted with the dummy classifying individuals as "low e_0". $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

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

Column 3 further shows that the effect of one required application is slightly higher for female job seekers. This estimate likely reflects that women are more often employed in the restaurant and cleaning sector. Finally, column 4 shows that individuals aged higher than the median (35) react slightly more. One among many possible interpretations is that older individuals have more financial commitments and therefore a higher incentive to comply and to provide additional effort. Appendix Table A.12 shows the same pattern of heterogeneity when instead using the duration of non-employment as an outcome.

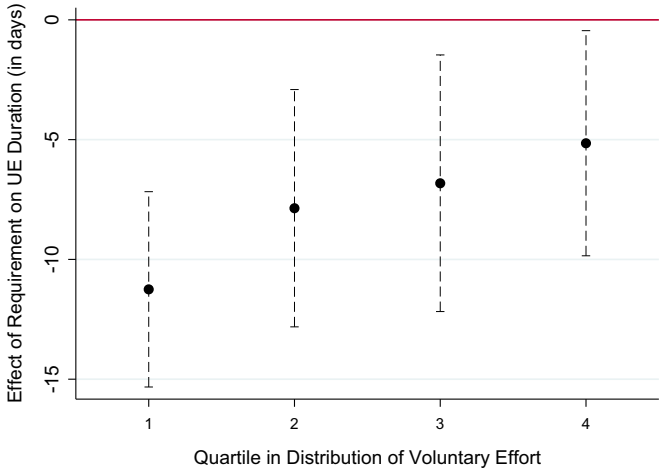

Fig. 5. Effects of requirements on unemployment duration, by level of voluntary effort. The figure plots coefficients from a 2SLS regression, in which the instrumented requirement is interacted with the individual's quartile in the distribution of voluntary effort. Voluntary effort is measured as the number of applications provided by the individual in the month preceding her first caseworker meeting. Information on prior application activity is not available for 14,536 individuals (15%). PES office × calendar year fixed effects are fully interacted with the quartile of voluntary effort. The average voluntary effort is 0.5 applications in the first quartile, 3.9 applications in the second quartile, 6.9 applications in the third quartile and 16.6 applications in the fourth quartile. The dashed lines show 95% confidence intervals (standard errors clustered at the caseworker level).

5.2. Effects on non-compliance

The requirement level may affect further dimensions of individual behavior, such as the individual compliance decision. From a theoretical perspective, higher requirements increase the cost of compliance. As a consequence, increases in the requirement level may increase non-compliance and thus trigger a higher incidence of benefit sanctions. In Table 10, we test for this mechanism. Column 1 reports that one additional required application per month increases the number of non-compliances – i.e., incidences in which the job seeker provides less applications than required – during the first six months of unemployment by 0.08 (15% relative to the mean). Higher requirements thus indeed cause non-negligible increases in non-compliance. Column 2 reveals that the effect is significantly stronger for individuals with lower voluntary effort e_0 (point estimate 0.1, versus 0.07 for individuals with high e_0, difference significant at the 5% level). This is in line with the theoretical intuition that lower voluntary effort reflects higher marginal effort costs. As a consequence, compliance with the requirement is more costly for them.

Columns 3 and 4 report that the effects on non-compliance behavior translate into a higher incidence of benefit sanctions (summary statistics on sanctions are included in Appendix Table A.3). A one-application higher requirement increases the number of sanctions imposed over the first six months of unemployment by 0.02 (12% relative to the mean). Again, the effect is stronger for individuals with lower voluntary effort (c.f. column 4). As additional evidence on how sanction imposition responds to the behavior of job seekers, column 1 of Appendix Table A.13 reports that the number of sanctions increases in the number of non-compliances. Further, column 2 shows that there remains no association between a caseworker's requirement stringency and the number of sanctions after conditioning on the job seeker's compliance behavior. This supports the idea that the effect of the requirement level on sanctions is indeed driven by the job seeker's compliance behavior, and not confounded by the caseworker's enforcement strictness.[34] Columns 3 and 4 of Appendix

[34] This intuition is further supported by column 4 of Table 3 (Section 4.2), where we found no association between the caseworker's requirement setting stringency and the probability of being sanctioned for a reason that does not relate to the requirement. Further, column 5 showed that the association between the caseworker's requirement setting stringency and the probability of a sanction that relates to the requirement becomes insignificant after conditioning on the job seeker's compliance behavior.

Table 9
Effect heterogeneity: individual labor market characteristics.

	Duration of unemployment (2SLS)			
	(1)	(2)	(3)	(4)
Requirement × (Low Education = 1)	−13.104***			
	(2.024)			
Requirement × (Low Education = 0)	−5.357***			
	(1.847)			
Requirement × (Low-qualif. Service Sector = 1)		−11.924***		
		(1.951)		
Requirement × (Low-qualif. Service Sector = 0)		−5.460***		
		(1.858)		
Requirement × Female			−8.152***	
			(1.805)	
Requirement × Male			−6.571***	
			(1.860)	
Requirement × (Age > Median)				−9.048***
				(1.891)
Requirement × (Age ≤ Median)				−5.487***
				(1.777)
p-Value for H_0: coeff equality	0.000	0.000	0.047	0.000
Outcome Mean	257.672	257.672	257.672	257.672
N	96,833	96,833	96,833	96,833

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office ×year fixed effects and individual covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

Table 10
Effect on non-compliance and the incidence of benefit sanctions.

	Number of non-compliances (2SLS)		Number of sanctions (2SLS)	
	(1)	(2)	(3)	(4)
Requirement	0.077***		0.015***	
	(0.022)		(0.005)	
Requirement × Low e_0		0.097***		0.029***
		(0.025)		(0.008)
Requirement × High e_0		0.065***		0.010**
		(0.023)		(0.004)
p-value for H_0: coeff (high) = coeff (low)		0.052		0.001
Outcome Mean	0.512	0.524	0.121	0.130
N	96,833	82,297	96,833	82,297

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The number of non-compliances (columns 1–2) and the number of sanctions (columns 3–4) are computed over the first six months of unemployment. An individual is classified as "low e_0" if she provided less than 5 applications (sample median) in the month preceding her first caseworker meeting. The sample size differs from the baseline sample because information on prior application activity is not available for 14,536 individuals (15%). All regressions include interacted PES office × year fixed effects and individual covariates. In columns 2 and 4, PES office × year fixed effects are interacted with the dummy classifying individuals as "low e_0". $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

Table A.13 further reveal that the amount of the sanction is positively related to the degree of the non-compliance.[35]

Taken together, the evidence clearly suggests that higher requirement levels make compliance harder to achieve. As a consequence, policy makers need to be aware that higher requirement levels induce a higher incidence of non-compliance. Further, the result calls for a discussion on the channels through which the effect of requirements on unemployment exit operates. Given that previous research has shown that benefit sanctions increase job finding (e.g., Abbring et al., 2005; Lalive et al., 2005; Van den Berg and Van der Klaauw, 2006; Van der Klaauw and Van Ours, 2013; Van den Berg et al., 2004; Arni et al., 2013), an increased incidence of sanctions could be a relevant channel through which the effects of requirements operate.

We perform a back-of-the envelope calculation to get a tentative sense of the importance of benefit sanctions as an effect channel. Our results show that the number of sanctions raises by 0.02 in response to one additional required application. In their study based on Swiss data, Arni et al. (2013) find that the announcement and enforcement of a sanction reduce the duration of unemployment by 27 days. [36] The effect of an additional required application operating through benefit sanctions would thus be of around $27 \times 0.02 = 0.54$ days. This only corresponds to around 8% of the main effect (7 days, c.f. panel B of Table 7). It therefore appears that only a minor part of the effect on unemployment duration operates through additional sanctioning. The sensitivity analysis of Section 5.4 confirms this intuition by showing that results are unaffected when we introduce sanctions (through a caseworker-level leave-out mean, excluding individual i) as an endogenous control variable in the regression.

[35] There is a small negative correlation between caseworker stringency and the amount of the sanction. As this outcome is only available for the selective sample which gets at least one sanction, it is difficult to interpret this relationship.

[36] Lalive et al. (2005) find an effect of 20 days.

Table 11
Effect on job stability.

	Duration of re-employment spell (2SLS)			
Sample:	*All Job Finders*		*Job Finders with NE Duration < 1 Yr*	
	(1)	(2)	(3)	(4)
Requirement	−0.649*		−0.929**	
	(0.388)		(0.414)	
Requirement × Low e_0		−0.852		−1.160**
		(0.595)		(0.587)
Requirement × High e_0		−0.359		−0.726
		(0.503)		(0.554)
p-Value for H_0: coeff (high) = coeff (low)		0.508		0.566
Outcome Mean	269.388	268.362	274.889	273.761
N	82,839	70,486	65,287	56,137

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The duration of re-employment is measured as the difference between the first exit from non-employment and the first return to non-employment in the social security data. We cap it at 300 days because of our limited time window. The duration of re-employment is available if the individual exited from non-employment within the observation window. An individual is classified as "low e_0" if she provided less than 5 applications (sample median) in the month preceding her first caseworker meeting. The sample size in columns 2 and 4 differs because information on prior application activity is not available for all individuals. All regressions include interacted PES office ×year fixed effects and individual covariates. In columns 2 and 4, the fixed effects are interacted with the dummy classifying individuals as "low e_0". *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

Table 12
Effect on re-employment wages.

	Log monthly re-employment wage (2SLS)			
Sample:	*All job finders*		*Job finders with NE duration < 1 Yr*	
	(1)	(2)	(3)	(4)
Requirement	0.002		−0.002	
	(0.006)		(0.005)	
Requirement × Low e_0		−0.000		−0.003
		(0.007)		(0.008)
Requirement × High e_0		−0.000		−0.002
		(0.007)		(0.006)
p-Value for H_0: coeff (high) = coeff (low)		0.983		0.892
Outcome Mean	8.117	8.107	8.185	8.171
N	82,839	70,486	63,778	55,326

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). The outcome is the average monthly log wage obtained during the first three months after exit from non-employment. It is available if the individual exited from non-employment within the observation window. An individual is classified as "low e_0" if she provided less than 5 applications (sample median) in the month preceding her first caseworker meeting. The sample size in columns 2 and 4 differs because information on prior application activity is not available for all individuals. All regressions include interacted PES office × year fixed effects and individual covariates. In columns 2 and 4, the fixed effects are interacted with the dummy classifying individuals as "low e_0". *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

5.3. Effects on job quality

We next explore whether additional required applications affect the quality of accepted job offers. As noted in Section 2, the expected direction of a potential effect is ambiguous: on the one hand, individuals may expand their search to worse wage offers to comply with the requirement. On the other hand, the additional applications may increase the chance of a good offer and counteract the depreciation of job offers over the non-employment spell.

In Table 11, we assess how the requirement level affects the duration of re-employment spells.[37] Column 1 reports that the duration of the re-employment spell decreases by 0.6 days in response to an additional required monthly application. This effect can be considered as being small, as it only corresponds to a 0.3% change relative to the mean. Column 2 reports that the effect is only insignificantly stronger for individuals with relatively low voluntary effort.[38] In columns 3 and 4, we consider the more homogeneous sample of individuals with a non-employment duration of less than 360 days. For this group, statistical precision is larger, while the effects remain of similar size.

Next, we analyze how the requirement level affects re-employment wages. Table 12 reports effects on the average monthly log earnings obtained during the first three months after exit from non-employment. We find a zero effect in all specifications from columns 1 to 4. There may be two explanations for this zero effect. It may be that the two theoretical channels discussed above counteract each other, or that marginal requirement changes are too small to affect job quality. It is interesting to compare this result to studies evaluating the introduction of a job search monitoring regime (i.e., the extensive margin). These studies tend to find negative effects on the quality of accepted jobs (e.g. Petrongolo, 2009; Manning, 2009). In turn, our results show

[37] We consider the first re-employment spell: it starts at exit from initial non-employment and ends at a subsequent return to non-employment, measured on the basis of the social security data. It is available for individuals with an observed exit from non-employment (i.e., an exit before the maximum observation period of Dec. 2015). We cap re-employment spells at 300 days because of our limited observation window. In all analyses of job quality, we control for the duration of non-employment through monthly dummies.

[38] We also find no significant heterogeneity for the characteristics considered in Table 9.

Table 13
Sensitivity analysis for the influence of other policy choices.

1. Baseline	−7.205***	96,833
	(1.792)	
2. With caseworker experience	−7.279***	96,833
	(1.783)	
3. With leave-out mean of other assignments	−5.699***	96,833
	(1.786)	
4. With leave-out mean of endogenous sanctions	−8.663***	96,833
	(1.887)	

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). All regressions include interacted PES office × year fixed effects and individual covariates. In row 2, the additional control variables are measures of caseworker experience (cf. independent variables in Table 4). In row 3, the additional control variables are the caseworker's leave-out mean probabilities of assigning a training program, of referring a vacancy and of imposing an unrelated sanction during the first six months of unemployment. In row 4, the additional control variable includes the caseworker's leave-out mean probability of a requirement-related benefit sanction during the first six months of unemployment. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level. Further estimation details can be found in Section 4.

Table 14
Additional sensitivity analyses.

	Effect of Requirement on UE Duration (2SLS)	N
1. Baseline	−7.205***	96,833
	(1.792)	
2. Control for PES office × calender quarter FE	−7.466***	96,833
	(1.768)	
3. Cluster S.E.s at PES level	−7.205***	96,833
	(2.015)	
4. Exclude individuals with requirement changes	−7.612***	74,546
	(1.932)	
5. Use modal requirement over first 3 months of UE	−6.905***	93,399
	(1.794)	
6. Use alternative measure of caseworker stringency	−6.950***	96,833
	(1.962)	

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). All regressions include interacted PES office × year fixed effects and individual covariates. In row 6, caseworker stringency is measured as the share of cases in which the caseworker assigns a requirement which exceeds the PES office mean (excluding individual *i*). $^{*}p < 0.10$, $^{**}p < 0.05$,$^{***}p < 0.01$. Standard errors are clustered at the caseworker level. Further estimation details can be found in Section 4.

that small intensive margin increases in the requirement level do not induce substantial additional harm to employment quality.

5.4. Sensitivity analysis

5.4.1. Influence of other policy choices

As discussed in Section 4.2, one possible threat to the validity of the instrument is the influence of other, possibly related, policy choices made by the caseworker, or of other caseworker characteristics. While we did not find a relationship between caseworker stringency and individual-level policy assignments as well as caseworker experience in Tables 3 and 4, we provide here an additional check. Similar to Bhuller et al. (2017), we include the leave-out means of other policy choices made by the caseworker (sanctions unrelated to the requirement, training programs and vacancy referrals) as control variables. Results are reported in Table 13. The first row reproduces, for convenience, the baseline 2SLS estimate of the effect of an additional required application on the duration of unemployment.[39] In row 2, we control for different measures of caseworker experience and number of cases (all variables included in Table 4). This hardly changes the coefficient. In row 3, we include as controls the leave-out means of other policy choices made by the caseworker, except for sanctions related to requirements, which are an outcome of the requirement level (c.f. Section 5.2). The coefficient only changes by a small, insignificant amount (−0.8 days).

In row 4, we introduce the probability to be sanctioned due to the non-compliance with a requirement as an (endogenous) control variable. We again specify this variable as a leave-out mean on the caseworker level, excluding individual *i*'s observation. While the result of this "bad control" exercise has to be interpreted with caution, we observe that the coefficient hardly reacts. This supports the idea that sanctions are not the main mechanism underlying the effects of requirement changes (c.f. discussion in Section 5.3).

5.4.2. Additional checks

In a final step, we check the main result's sensitivity along additional dimensions. In Table 14, the first row again recalls the baseline 2SLS estimate on the effect of an additional required application on the duration of unemployment. Row 2 shows that the coefficient does not change when controlling for PES office × quarter instead of year effects. Further, the effect significance remains unaffected when we cluster at the office level (N = 35), instead of the caseworker level (row 3). In row 4, we exclude individuals whose requirement levels change over the first six months of unemployment. Row 5 re-defines the individual requirement as the modal requirement assigned over the first three (instead of six) months of unemployment. Both changes do not affect the estimates significantly. Finally, we use in row 6 an alternative specification of caseworker stringency as the instrument. Instead of the caseworker's leave-out mean, we define the instrument as the share of cases in which the caseworker assigns a requirement which exceeds the PES office mean (excluding individual *i*). The coefficient is quasi-identical to the baseline.

To assess the sensitivity of the results related to potential small sample issues in caseworker cells, Fig. 6 assesses whether the main estimate changes when restricting the sample to caseworkers

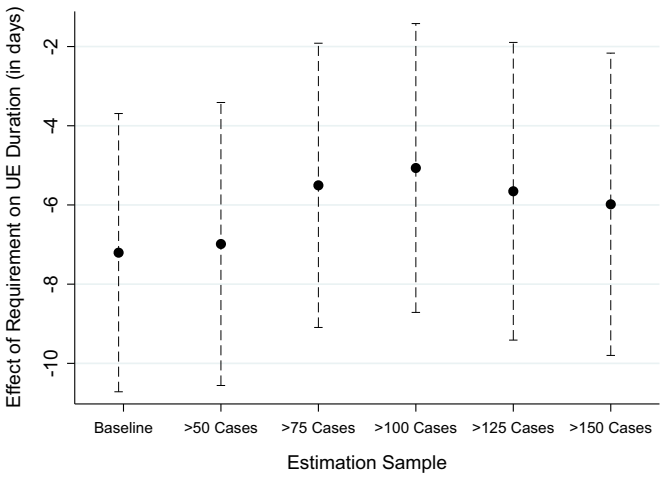

Fig. 6. How do the effects vary by caseload? The figure shows how the main coefficient (effect of the instrumented requirement on the duration of unemployment) changes when restricting the sample to caseworkers with more than 50, more than 75, …, more than 150 cases. The baseline sample includes caseworkers with more than 30 cases. 90% of caseworkers have more than 50 cases (corresponding to 98% of job seekers, N = 94,833) and 67% have more than 150 cases (corresponding to 82% of job seekers, N = 79,028). The dashed lines show 95% confidence intervals (standard errors clustered at the caseworker level).

[39] A sensitivity analysis with respect to the non-employment duration leads to the same conclusions (available upon request).

with more than 50/75/…/150 cases. The absolute coefficient size decreases slightly, but not monotonically, in these restrictions. Further, none of the differences in the coefficient sizes is statistically significant. Caseload does thus not appear to substantially influence the estimates.

6. Conclusion

We provide first empirical estimates of the individual returns to job search effort imposed by requirements. Exploiting conditionally random variation in caseworker stringency, we find that, on average, one additional required application reduces the duration of un- and non-employment spells by 3% (7 and 10 days, respectively). The corresponding effect of an additional provided application amounts to 4%. These results show that the setting of individual effort targets to steer job search can be a successful strategy for labor market policy. Search requirements induce additional applications which are indeed relevant for the success of job search. However, policy makers need to be aware that required search effort does not translate one-to-one in provided search effort. We quantify the elasticity of effort provision to the requirement to be 0.67. In line with theory, it further turns out that higher requirements also induce higher rates of non-compliance and benefit sanctions.

When considering the longer run, we find modest reactions of re-employment job quality to requirements. An additionally required job application causes re-employment spells to shorten by 0.3%; the effects on wages are zero. Strengthening the requirement regime thus seems only to marginally reduce job quality. One has to be aware, however, that this study quantifies the intensive margin effect of an additional job application. The introduction of a requirement policy, compared to the counterfactual of no such regime, could well induce stronger impacts on job quality outcomes (c.f. the evidence by, e.g., Petrongolo, 2009; Manning, 2009).

As a further important result, we find substantial heterogeneity in how individuals react to requirements. Effects of effort targets are strongest among lower-skilled job seekers. Furthermore, individuals who start off their unemployment spell with a low level of voluntary effort show stronger reactions to requirements. This suggests that there is substantial between-individual variation in effort cost and the returns to effort. Knowing these heterogeneous patterns can help policy makers to improve the design of requirement policies.

As a cautionary note, it is important to keep in mind that the estimates in this study correspond to the marginal effect of an application on job finding, keeping labor market tightness constant. Large-scale increases in the average job search requirement faced by job seekers may well induce job search externalities. Such externalities have been shown to be significant in the cases of UI benefit extensions (Lalive et al., 2015) and training programs (Crépon et al., 2013; Gautier et al., 2018). It is beyond the scope of this study to separately quantify the impact of externalities arising from large-scale requirement changes.

Altogether, our estimates contribute to the scarce empirical evidence on how individual outcomes react to explicit targets and effort 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 is that policy makers can afford to set higher benefit levels when monitoring counteracts moral hazard. Our findings show how marginal changes in 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.

Appendix A

A.1. Theory appendix

The job seeker's value function describing her problem of choosing effort e in the presence of a search requirement r writes:[40]

$$\rho R = \max_e \left[b - c(e) + \lambda(e) \int_{\phi}^{\infty} \left(\frac{w}{\rho} - R \right) dF(w) - I(e < r)\, p_0\, c\left(\frac{e}{r}\right) \right] \tag{A.1}$$

p_0 denotes the probability of being sanctioned in the case of a non-compliance, i.e., when $I(e < r) = 1$.[41] The benefit cut imposed in the case of a sanction is represented by $c\left(\frac{e}{r}\right)$, with $c'\left(\frac{e}{r}\right) < 0$, implying that the cut increases in the severity of the non-compliance.[42] b is the unemployment benefit and w the wage of the final job match. ϕ denotes the reservation wage, which equals ρR after optimization.

In the absence of a requirement, the job seeker chooses the optimal effort level e^0, solving the decision problem Eq.(A.1) without the last term. e^0 results from a trade-off between the marginal cost of effort $c'(e)$ and its marginal benefit, which involves an increase in the job offer arrival rate $\lambda'(e)$ and the associated differential in value between employment and unemployment, $\int_{\phi}^{\infty} \left(\frac{w}{\rho} - R \right) dF(w)$.

In a system with requirements, the job seeker chooses her level of effort e by optimizing Eq. (A.1). In this expression, the requirement enters through the term $I(e < r)\, p_0\, c\left(\frac{e}{r}\right)$: in case the job seeker provides a search effort that is lower than the requirement ($I(e < r) = 1$), there is an exogenous probability p_0 that the job seeker sees her benefit cut by the amount c, whose level decreases in the ratio of provided over required applications, $\frac{e}{r}$.[43] There is thus an implicit cost of providing less applications than required, which increases in the gap between provided effort and the requirement.

The reaction of individual effort to the requirement can be divided into three segments: (1) If $e_0 \geq r$, the job seeker does not change her behavior compared to a world without requirements. (2) If $e_0 < r$, and if the net cost of providing the differential $r - e^0$ (i.e., cost of the effort

[40] Abbring et al. (2005) and Lalive et al. (2005) set up similar value functions when discussing the effects of benefits sanctions.

[41] In principle, p_0 could be decomposed into two components: the probability that the non-compliance is detected and the probability that the detected non-compliance leads to the imposition of a benefit sanction. This decomposition would, however, not change the insights of our theoretical discussion.

[42] This modeling choice is motivated by the empirical fact that the amount of benefit cuts decreases in $\frac{e}{r}$.

[43] The job seeker's reaction with respect to policy-induced changes in p_0 and $c\left(\frac{e}{r}\right)$ is not within the scope of this paper. It is in more detail discussed by Abbring et al. (2005) and Lalive et al. (2005), who estimate the effect of UI benefit sanctions on the exit from unemployment.

increase minus the benefit from possible additional job offer arrivals) is smaller than the cost associated to the risk of (any) sanction, the job seeker chooses $e = r$. (3) If the net cost of providing the differential $r - e^0$ exceeds the cost associated to the risk of a sanction, the job seeker does not comply and provides $e < r$. In this scenario, $e < r$ may still exceed e_0, because individuals reduce the expected size of the benefit cut by reducing the distance between e and r.

In the presence of heterogeneous effort costs, we can expect to observe all three types of reactions in the data. We expect some job seekers to provide more applications than required, a certain mass of job seekers bunching at the requirement level and some job seekers providing less applications than required.

A.2. Additional tables

Table A.1
Summary statistics on individual covariates.

Variable		Mean	Std. Dev.	Min	Max
Female		0.389	0.487	0	1
Married		0.392	0.488	0	1
Non-Swiss national		0.443	0.497	0	1
Non-permanent resident		0.181	0.385	0	1
Previous UE in yrs		0.598	0.808	0	4
Share employed in 5 yrs prior UE		0.752	0.257	0	1
Potential benefit duration < 400		0.283	0.450	0	1
Age group (omitted baseline: 20–24)					
	25–29	0.177	0.382	0	1
	30—34	0.160	0.367	0	1
	35–39	0.384	0.486	0	1
	40–44	0.127	0.333	0	1
	45–49	0.127	0.333	0	1
	50–55	0.118	0.322	0	1
Additional household members (omitted baseline: 0)					
	1	0.124	0.330	0	1
	2 to 3	0.120	0.325	0	1
	4 and more	0.008	0.090	0	1
Position in last job (omitted baseline: professional or self-empl.):					
	Manager	0.033	0.177	0	1
	Support	0.429	0.495	0	1
Level of education (omitted baseline: apprenticeship):					
	Minimum education	0.227	0.419	0	1
	Short further education	0.058	0.233	0	1
	High school	0.056	0.229	0	1
	Professional diploma	0.061	0.240	0	1
	Applied university	0.041	0.199	0	1
	University	0.066	0.248	0	1
	Missing	0.023	0.150	0	1
Domain of occupation in last job (omitted baseline: admin and office):					
	Food and raw materials	0.040	0.196	0	1
	Production (blue collar)	0.132	0.338	0	1
	Engineering	0.031	0.173	0	1
	Informatics	0.020	0.139	0	1
	Construction	0.130	0.336	0	1
	Sales	0.116	0.321	0	1
	Tourism, transport, communication	0.039	0.193	0	1
	Restaurant	0.174	0.379	0	1
	Cleaning and personal service	0.036	0.186	0	1
	Management and HR	0.036	0.185	0	1
	Journalism and arts	0.013	0.112	0	1
	Social work	0.012	0.110	0	1
	Education	0.011	0.102	0	1
	Science	0.011	0.104	0	1
	Health	0.035	0.184	0	1
	Others (skilled)	0.050	0.219	0	1
Sector of activity (omitted baseline: unskilled service sector)					
	Blue Collar	0.352	0.478	0	1
	White Collar	0.391	0.488	0	1
Average earnings in 2 yrs prior UE, Decile (omitted baseline: 1st)					
	2nd Decile	0.100	0.300	0	1
	3rd Decile	0.100	0.300	0	1
	4th Decile	0.100	0.300	0	1
	5th Decile	0.100	0.300	0	1
	6th Decile	0.100	0.300	0	1
	7th Decile	0.100	0.300	0	1
	8th Decile	0.100	0.300	0	1
	9th Decile	0.100	0.299	0	1
	10th Decile	0.100	0.300	0	1
N			96,833		

Table A.2
Summary statistics on treatments assigned over the first six months of UE.

Variable	Mean	Std. Dev.
P (Sanction Related to Requirements)	0.111	0.314
P (Sanction Unrelated to Requirements)	0.055	0.228
P (Training Program)	0.318	0.466
P (Vacancy Referral)	0.205	0.404
Share of vacancies in prior occupation	0.167	0.348
N	96,833	

Sanctions are classified as related to requirements if their stated reason is the under-provision of job applications by the job seekers. Vacancy referrals are classified as being in the job seeker's prior occupation if the first three digits of the occupation code coincide with those of the job seeker's previous job.

Table A.3
Summary statistics on sanctions.

	Mean	Std. Dev.	N
Overall			
Sanction Probability	0.111	0.314	96,833
Number of Sanctions	0.121	0.357	96,833
Conditional on ≥ 1 Non-compliance			
Sanction Probability	0.205	0.404	33,368
Number of Sanctions	0.227	0.469	33,368
Conditional on ≥ 1 Sanction			
Sanction Amount (Days of 0 Benefits)	5.487	3.436	10,718

The table only includes sanctions which relate to a non-compliance with the requirement, realized during the first 6 months of unemployment. The panel named "Conditional on ≥1 Non-Compliance" only includes job seekers who at least once report less applications than required during their first six months of unemployment. Sanctions can be imposed in other cases as well, for instance if the reported applications do not meet minimum quality standards.

Table A.4
Test for out of sample selection.

	P (out of sample)	
	(1)	(2)
CW Stringency	−0.004	−0.003
	(0.011)	(0.010)
Covariates	No	Yes
Outcome Mean	0.161	0.161
N	115,446	115,446

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). All regressions include interacted PES office × year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

Table A.5
Effort provision over the first 6 months of unemployment (UE).

	Provided applications per month, average over:		
	Months 1–2 of UE	Months 3–4 of UE	Months 5–6 of UE
	(1)	(2)	(3)
Panel A: Reduced Form Estimates			
CW Stringency	0.479***	0.484***	0.445***
	(0.032)	(0.035)	(0.038)
Panel B: IV Estimates			
Requirement	0.685***	0.692***	0.644***
	(0.041)	(0.048)	(0.050)
Outcome Mean	8.926	8.552	8.336
N	87,465	74,968	56,838

Requirements refer to monthly job applications. In panel B, the individual requirement is instrumented by caseworker stringency, which is measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). All regressions include interacted PES office × year fixed effects and individual covariates. The sample size decreases from column 1 to 3 because individuals exit unemployment. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

Table A.6
Testing monotonicity: additional job seeker characteristics.

	Requirement (first stage)	
	Baseline instrument	Reverse sample instrument
	(1)	(2)
A. Gender		
1. Subsample: Male		
Estimate	0.689***	0.589***
	(0.023)	(0.029)
Outcome Mean	8.266	8.266
N	59,195	59,195
2. Subsample: Female		
Estimate	0.725***	0.671***
	(0.031)	(0.036)
Outcome Mean	8.320	8.320
N	37,638	37,638
B. Age		
1. Subsample: Age≤Median		
Estimate	0.695***	0.612***
	(0.027)	(0.031)
Outcome Mean	8.521	8.521
N	51,001	51,001
2. Subsample: Age>Median		
Estimate	0.704***	0.645***
	(0.027)	(0.031)
Outcome Mean	8.026	8.026
N	45,832	45,832
C. Marriage Status		
1. Subsample: Unmarried		
Estimate	0.705***	0.638***
	(0.024)	(0.028)
Outcome Mean	8.344	8.344
N	58,888	58,888
2. Subsample: Married		
Estimate	0.690***	0.664***
	(0.027)	(0.029)
Outcome Mean	8.198	8.198
N	37,945	37,945
D. Household Size		
1. Subsample: Household Size≤2		
Estimate	0.705***	0.666***
	(0.026)	(0.029)
Outcome Mean	8.418	8.418
N	51,671	51,671
2. Subsample: Household Size>2		
Estimate	0.691***	0.659***
	(0.024)	(0.027)
Outcome Mean	8.137	8.137
N	45,162	45,162
E. Residence Status		
1. Subsample: Swiss or Permanent Resident		
Estimate	0.696***	0.563***
	(0.023)	(0.030)
Outcome Mean	8.206	8.210
N	79,296	79,206
2. Subsample: Non-permanent Resident		
Estimate	0.719***	0.708***
	(0.033)	(0.035)
Outcome Mean	8.652	8.652
N	17,537	17,537

(*continued on next page*)

Table A.6 (*continued*)

	Requirement (first stage)	
	Baseline instrument	Reverse sample instrument
	(1)	(2)
F. Education		
1. Subsample: Apprenticeship or Higher		
Estimate	0.710***	0.558***
	(0.023)	(0.039)
Outcome Mean	8.234	8.237
N	69,307	69,225
2. Subsample: Unlearned		
Estimate	0.654***	0.609***
	(0.036)	(0.039)
Outcome Mean	8.420	8.420
N	27,526	27,526
G. Prior Unemployment		
1. Subsample: Without Prior Unemployment		
Estimate	0.710***	0.558***
	(0.023)	(0.039)
Outcome Mean	8.234	8.237
N	69307	69225
2. Subsample: With Prior Unemployment		
Estimate	0.654***	0.609***
	(0.036)	(0.039)
Outcome Mean	8.420	8.420
N	27,526	27,526
H. Labor Market Attachment (Share Employed in 5 Previous Yrs)		
1. Subsample: ≤Median		
Estimate	0.709***	0.657***
	(0.027)	(0.030)
Outcome Mean	8.596	8.596
N	47,549	47,549
2. Subsample: >Median		
Estimate	0.690***	0.663***
	(0.023)	(0.027)
Outcome Mean	7.989	7.989
N	49,284	49,284
I. Income in Previous Job		
1. Subsample: ≤Median		
Estimate	0.724***	0.663***
	(0.027)	(0.032)
Outcome Mean	8.619	8.619
N	48,281	48,281
2. Subsample: >Median		
Estimate	0.670***	0.622***
	(0.023)	(0.026)
Outcome Mean	7.957	7.957
N	48,552	48,552
J. Potential Benefit Duration		
1. Subsample: ≥400days		
Estimate	0.684***	0.599***
	(0.024)	(0.029)
Outcome Mean	8.172	8.172
N	69,463	69,463
2. Subsample: <400days		
Estimate	0.735***	0.719***
	(0.028)	(0.030)
Outcome Mean	8.578	8.578
N	27,370	27,370

The test for instrument monotonicity is inspired by Bhuller et al. (2017). The reverse sample instrument is based on individuals excluded from the given subsample. For example, the reverse sample instrument for the subsample of females is computed based on male individuals. Requirements refer to monthly job applications. All regressions include interacted PES office × year fixed effects and individual characteristics. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

Table A.7
Required job applications and the duration of unemployment: IV estimates without fixed effects and covariates.

	Reduced form		2SLS: requirement		2SLS: effort	
	UE duration	NE duration	UE duration	NE duration	UE duration	NE duration
	(1)	(2)	(3)	(4)	(5)	(6)
CW Stringency	4.185***	13.284***				
	(1.381)	(2.168)				
Requirement			4.216***	13.381***		
			(1.390)	(2.183)		
Effort					4.606***	14.620***
					(1.468)	(2.242)
Outcome Mean	257.672	302.698	257.672	302.698	257.672	302.698
N	96,833	96,833	96,833	96,833	96,833	96,833

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). Regressions include no fixed effects or covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

Table A.8
Effects on the total number of months in UI and employment during the first year.

	Reduced form		2SLS	
	Months in UI	Months employed	Months in UI	Months employed
	(1)	(2)	(3)	(4)
CW Stringency	−0.087***	0.079**		
	(0.027)	(0.031)		
Requirement			−0.124***	0.113**
			(0.039)	(0.045)
Outcome Mean	7.599	5.194	7.599	5.194
N	96,833	96,833	96,833	96,833

Requirements refer to monthly job applications. They are instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). Outcomes are the total number of months with UI benefits (columns 1 and 3) and the number of months employed (columns 2 and 4) during the first 12 months after unemployment entry. They can sum to more than 12 if an individual re-enters unemployment by claiming part-time UI benefits. All regressions include interacted PES office × year fixed effects and individual covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

Table A.9
Effects on job finding probability.

	Prob. of job finding w/in T months (2SLS)			
	T = 3	T = 6	T = 12	T = 18
	(1)	(2)	(3)	(4)
Requirement	0.008**	0.013**	0.015***	0.013***
	(0.004)	(0.005)	(0.005)	(0.004)
Outcome Mean	0.260	0.487	0.674	0.792
N	96,833	96,833	96,833	96,833

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). All regressions include interacted PES office × year fixed effects and individual covariates. The outcome is coded as one if an individual's social security records report positive employment earnings in month T after entry into unemployment. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Further estimation details can be found in Section 4.

Table A.10
Effects on the probability to leave from unemployment to a job posted at the PES.

	P (exit to job posted at the PES)		
Sample	*NE duration<180*	*NE duration<360*	*All*
	(1)	(2)	(3)
Requirement	0.016***	0.020***	0.018***
	(0.006)	(0.006)	(0.005)
Outcome Mean	0.084	0.096	0.085
N	47,168	65,287	96,833

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual *i*). Exit to a job posted at the PES is inferred from the reason of de-registration stated in the UI data. All regressions include interacted PES office ×year fixed effects and individual covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

Table A.11
Caseworker stringency and voluntary effort.

	Caseworker stringency	
	(1)	(2)
Voluntary Effort (e_0)	−0.001	−0.000
	(0.002)	(0.002)
Covariates	No	Yes
Outcome Mean	8.303	8.303
N	82,297	82,297

Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects and individual covariates. Voluntary effort (e_0) is measured as the number of applications provided by the individual in the month preceding her first caseworker meeting. The sample size differs from the baseline sample because information on prior application activity is not available for 14,536 individuals (15%). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

Table A.12
Effect heterogeneity: individual labor market characteristics.

	Duration of non-employment (2SLS)			
	(1)	(2)	(3)	(4)
Requirement × (Low Education =1)	−17.675***			
	(3.161)			
Requirement × (Low Education =0)	−7.500***			
	(2.900)			
Requirement × (Low-Qualif. Service Sector =1)		−16.743***		
		(3.018)		
Requirement × (Low-Qualif. Service Sector =0)		−7.406**		
		(2.932)		
Requirement × Female			−13.050***	
			(2.901)	
Requirement × Male			−7.837***	
			(2.900)	
Requirement × (Age > Median)				−12.064***
				(2.995)
Requirement × (Age ≤ Median)				−7.939***
				(2.780)
p-Value for H_0: coeff equality	0.000	0.000	0.000	0.002
Outcome Mean	302.698	302.698	302.698	302.698
N	96,833	96,833	96,833	96,833

The requirement refers to monthly job applications. It is instrumented by caseworker stringency, measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office ×year fixed effects and individual covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506). Effects on the unemployment duration are reported in Appendix Table A.12. Further estimation details can be found in Section 4.

Table A.13
Determinants of benefit sanctions.

	Number of sanctions		Amount of sanction	
	(1)	(2)	(3)	(4)
Number of Non-compliances	0.098***	0.098***	−0.013	−0.012
	(0.004)	(0.004)	(0.040)	(0.040)
Average Ratio Effort/Requirement	−0.005	−0.004	−0.926***	−0.940***
(Degree of Non-compliance)	(0.004)	(0.004)	(0.152)	(0.152)
CW Stringency		0.005		−0.111**
		(0.003)		(0.055)
Outcome Mean	0.121	0.121	5.487	5.487
N	96,833	96,833	10,718	10,718

In columns 1 and 2, the outcome is the number of sanctions imposed due to a non−compliance with the requirement over the first six months of unemployment. In column 2, it is the amount of the sanctions in terms of days without benefit receipt (only available for individuals with at least one sanction over the first six months of unemployment). Requirements refer to monthly job applications. Caseworker stringency is measured as the caseworker's leave-out mean of assigned requirements (excluding individual i). All regressions include interacted PES office × year fixed effects and individual covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the caseworker level (N = 506).

A.3. Additional background

(a) Page 1

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Arbeitslosenversicherung

Eingangsdatum / Datum des Poststempels

Nachweis der persönlichen Arbeitsbemühungen

Name und Vorname	AHV-Nr.	Monat und Jahr

Datum der Bewerbung Tag Monat	Firma, Adresse Kontaktperson, Telefon-Nr.	Stellenbezeichnung	Zuweisung RAV	Pensum		Bewerbung			Ergebnis der Bewerbung				
				Vollzeit	Teilzeit (%)	Schriftlich / elektronisch	Persönlich	Telefonisch	noch offen	Vorstellungs-gespräch	Anstellung	Absage	Absagegrund

A13

(b) Page 2

Datum der Bewerbung Tag Monat	Firma, Adresse Kontaktperson, Telefon-Nr.	Stellenbezeichnung	Zuweisung RAV	Pensum		Bewerbung			Ergebnis der Bewerbung				
				Vollzeit	Teilzeit (%)	Schriftlich / elektronisch	Persönlich	Telefonisch	noch offen	Vorstellungsg espräch	Anstellung	Absage	Absagegrund

Datum: _____ Unterschrift der versicherten Person:

Beilagen: _____ _____

Hinweis

Die versicherte Person muss alles Zumutbare unternehmen, um Arbeitslosigkeit zu vermeiden oder zu verkürzen. Insbesondere ist es ihre Sache, Arbeit zu suchen, wenn nötig auch ausserhalb ihres bisherigen Berufes (Art. 17 AVIG).

Die Pflicht, sich persönlich um Arbeit zu bemühen, gilt bereits vor Eintritt der Arbeitslosigkeit (z.B. während der Kündigungsfrist oder dem befristeten Arbeitsverhältnis).

Die versicherte Person muss der zuständigen Amtsstelle für jede Kontrollperiode (Kalendermonat) bis spätestens am 5. Tag des Folgemonats schriftliche Angaben über ihre Bemühungen um Arbeit einreichen (Art. 26 AVIV). Dazu dient dieses Formular. Schriftliche Unterlagen wie Kopien von Bewerbungsschreiben oder Absagebriefen sind beizulegen.

Nach dem 5. Tag des Folgemonats eingereichte Arbeitsbemühungen können nicht mehr berücksichtigt werden, ausser es liegt ein entschuldbarer Grund vor.

Versicherte Personen, die sich nicht genügend um zumutbare Arbeit bemühen oder eine solche ablehnen, werden je nach dem Verschulden bis zu einer Dauer von höchstens 60 Tagen in der Anspruchsberechtigung eingestellt (Art. 30 AVIG).

Mit unwahren oder unvollständigen Angaben macht sich die versicherte Person strafbar (Art. 105 ff. AVIG).

Fig. A.1. Official protocol of job search effort (in German).

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