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# Strictness in the Evaluation of Job Search Effort and Employment Outcomes

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# Strictness in the Evaluation of Job Search Effort and Employment Outcomes*

## Abstract

Unemployed job seekers must comply with job-search requirements to be eligible for unemployment insurance benefits. We exploit variation in strictness across caseworkers who conduct monthly reviews of these requirements to examine the effects of stricter monitoring on job search behavior and labor market outcomes. This variation arises from central-office caseworkers who are quasi-randomly assigned to review job seekers' activity reports and whose only interaction with job seekers occurs through this process. We find that stricter monitoring increases the likelihood of reported violations and benefit sanctions, leading to persistently higher job-search intensity, shorter unemployment durations, and higher employment rates. We find no evidence of negative effects on job quality, as measured by subsequent earnings and wages in the first job. The positive employment effects are more persistent among job seekers from low-growth industries and those with weak labor market attachment.

## JEL classification

J64, J68

## Keywords

unemployment insurance, job search, monitoring, sanctions, caseworkers

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# 1 Introduction

Job-search requirements, the monitoring of claimants' search efforts, and sanctions for noncompliance are key features of unemployment insurance (UI) systems in many countries. These measures aim to mitigate moral hazard by encouraging more intensive job search and higher job-finding rates, thereby enabling higher benefit replacement rates and improved consumption smoothing among unemployed job seekers. However, monitoring and job-search requirements may also adversely affect job quality and shift some informal job search toward formal, monitored search. The effects of monitoring may further vary across groups and labor market conditions.

Beyond supporting job seekers, caseworkers typically play a central role in monitoring job-search effort and enforcing job-search requirements. Caseworkers often differ in how they apply and combine these elements, implying that a job seeker's employment outcomes may depend on the specific caseworker to whom they are assigned (Le Barbanchon et al., 2024). This paper leverages such caseworker variation.

For identification, we exploit an institutional setting in Sweden where UI recipients are required to submit monthly reports on their job-search activities. These reports are allocated quasi-randomly to caseworkers in centralized units for assessment, and these central-office caseworkers have no direct contact with the job seekers. Exploiting variation in caseworker strictness, this setting provides credible evidence on the impacts of stricter enforcement of UI requirements. Rich administrative data provide information on job search and job finding over the unemployment spell, and the quality of the first job. We also investigate heterogeneity by labor market attachment and prior employment in growing versus declining industries. The quasi-random assignment of reports to caseworkers allows us to implement a leniency design, also commonly referred to as a judge or examiner design (Chyn et al., 2025; Goldsmith-Pinkham et al., 2026).

Specifically, unemployed job seekers in Sweden are required to submit monthly *activity reports*. Caseworkers review these reports and determine whether they meet the required standards. We exploit the fact that during our observation period (2014–2017), these reports were evaluated by caseworkers in a small number of central offices during the first four months of unemployment, while caseworkers in local offices maintained regular contact with job seekers, providing job-search assistance, counseling, and assigning them to active labor market programs (ALMPs). The allocation of reports to central-office caseworkers was effectively quasi-random. Caseworkers review reports drawn from the top of a common list and have no information about a report or the job seeker before selecting it for review. This process constitutes the only interaction between central-office caseworkers and job seekers. The setting therefore provides a clear source of exogenous variation in caseworker strictness while reducing the scope for confounding caseworker practices, addressing two common threats to identification in studies exploiting variation across caseworkers in UI systems (e.g. Arni and Schiprowski, 2019; Humlum et al., 2025; Klaeui, 2026). We also verify empirically that caseworker strictness is unrelated to job seeker characteristics.

We begin by documenting substantial variation across caseworkers in the enforcement of UI requirements. Stricter caseworkers disapprove significantly more activity reports. Since non-approvals are rare, being assigned to a stricter caseworker and receiving a disapproved report likely constitute a substantial monitoring shock. A disapproved report also initiates an entire monitoring chain, resulting in more registered violations of the UI rules and more benefit sanctions. This stricter monitoring translates into shorter unemployment durations and higher employment rates. Moving from the 10th to the 90th percentile of the caseworker strictness distribution reduces the probability of remaining unemployed after 120 days by around 1.1 percentage points, corresponding to a relative effect of 3%. Our design allows us to use the caseworker strictness as an IV for having a disapproved first activity report when we consider short-run outcomes.[1] The results suggest a strong positive effect of a disapproval of the first report on transitions to employment.

We next shed light on the underlying mechanisms. First, using data on wages and long-run earnings, we find no negative effects on job quality. Second, the positive effects on job finding are reflected in an increase in job applications, both immediately after the assessment of the first report and several months later. Overall, moving from the 10th to the 90th percentile of caseworker strictness increases the number of job applications by around 4.6% within the first 120 days of unemployment. Third, we also observe more phone and in-person meetings for job seekers assigned to stricter caseworkers. This reflects the institutional setting, in which caseworkers contact job seekers following a non-approved report. These contacts are part of the monitoring process and may contribute to the observed increase in job finding.

The effects of stricter enforcement of UI requirements are heterogeneous across job seekers and labor market conditions. In the short run, the employment effects of being assigned to a stricter caseworker are more positive for job seekers with a low risk of long-term unemployment, while those with weaker employment prospects

[1] Since the same central-office caseworkers review the first four reports, multiple channels arise once the second activity report is assessed, likely violating the exclusion restriction. This is why we focus on short-run outcomes in the IV analysis.

experience larger effects in the longer run. Hence, tighter monitoring can bring job seekers with strong labor market attachment quickly back to work, while generating a more sustained improvement in employment rates among harder-to-place job seekers.

Labor market conditions also play a role in shaping the effects of monitoring.[2] For job seekers previously employed in high-growth industries, we find a positive short-run effect on job-finding that dissipates over time. In contrast, for job seekers from low-growth industries, the effects are more persistent. This suggests that stricter monitoring is particularly effective at preventing prolonged unemployment among workers from low-growth industries. This pattern is not driven by higher mobility across industries under stricter monitoring, but rather by sustained increases in job-search intensity.

There is a broad empirical literature on job-search requirements and monitoring. Several studies find that additional job-search requirements shorten unemployment durations (Arni and Schiprowski, 2019; Bolhaar et al., 2019; Chan et al., 2024; Johnson and Klepinger, 1994). Evidence on the effects of monitoring is more mixed. Based on field experiments, Ashenfelter et al. (2005) show that increased monitoring of job-search effort reduces the duration of UI benefit receipt in the U.S., and Micklewright and Nagy (2010) find similar effects in Hungary, albeit only for certain subgroups. In contrast, van den Berg and van der Klaauw (2006) find no effect of increased monitoring on re-employment rates, which they attribute to a shift from informal to formal job-search methods.[3] Non-experimental studies exploiting policy changes over time (McVicar, 2008, 2010; Petrongolo, 2009) or age-based discontinuities (Cockx and Dejemeppe, 2012) generally find that stricter monitoring increases job-finding rates and reduces welfare receipt. A related strand of the literature examines the effects of benefit sanctions, typically using timing-of-events models (Abbring and van den Berg, 2003). These studies consistently document strong positive effects of imposed sanctions on re-employment rates (Abbring et al., 2005; Lalive et al., 2005), though sometimes at the cost of lower job quality (Arni et al., 2013; van den Berg et al., 2019; van den Berg and Vikström, 2014).[4]

[2]Recent evidence suggests that training programs may be particularly effective for unemployed workers exposed to offshoring or automation risk, who are therefore more likely to experience persistent employment losses (Humlum et al., 2025; Schmidpeter and Winter-Ebmer, 2021). Athey et al. (2026) report that earnings losses following job loss vary substantially across industries and local labor market conditions, suggesting that enforcing job search requirement may have heterogeneous effects along these dimensions.

[3]van den Berg et al. (2025) show that integration agreements, which are written contracts specifying rights and obligations of UI recipients, increase re-employment rates, particularly among job seekers with rather adverse employment prospects.

[4]For structural analyses of the interplay between monitoring and sanctions, see Cockx et al.

We are the first to provide causal evidence on how caseworker strictness in the evaluation of job-search requirements affects job seekers' search behavior and labor market outcomes. Our setting combines credible identification, based on quasi-random assignment of activity reports to caseworkers who differ in strictness, with a non-experimental setting that enhances external validity. Moreover, our large sample and rich administrative data allow us to study how the impact of monitoring varies with job seekers' employment prospects and labor market conditions. We also exploit unique longitudinal data on job-search activity, enabling us to study the dynamics of search intensity throughout the unemployment spell.

Another strand of the literature focuses more directly on the role of UI caseworkers. Exploiting unplanned caseworker absences, Schiprowski (2020) shows that missing a scheduled meeting with a caseworker reduces re-employment rates on average, with substantial heterogeneity across caseworkers. Cederlöf et al. (2026) estimate caseworker value-added in Swedish local employment offices and document considerable heterogeneity, which affects both unemployment duration and the quality of subsequent jobs. Similarly, Hervelin and Villedieu (2026) analyze French counseling offices and document substantial variation across caseworkers in training placements and employment outcomes.[5] A related set of studies exploits variation in specific caseworker interventions. Arni and Schiprowski (2019) use caseworker variation in Switzerland to document that increasing the number of required job applications shortens unemployment durations. Humlum et al. (2025) and Hyman (2018) provide evidence that retraining programs can improve employment outcomes.[6]

Our contribution to this strand of the literature is the focus on the enforcement margin: how variation in caseworker strictness in monitoring job-search requirements affects job seekers' behavior and labor market outcomes. Our setting identifies the impact of stricter enforcement of existing UI requirements, abstracting from broader caseworker, counseling, and training effects. We exploit a clear institutional source of variation: central-office caseworkers review reports drawn in quasi-random order from a common list. Because these caseworkers have no direct contact with job seekers, we can rule out alternative channels – such as differential access to ALMPs–ensuring that our estimates capture the effects of stricter monitoring.

This paper proceeds as follows. Section 2 describes the institutional background.

(2018) and van den Berg et al. (2026).

[5]Bolhaar et al. (2020) report substantial heterogeneity in how caseworkers assign welfare-to-work programs, and Schiprowski et al. (2024) provide evidence that caseworker meetings are more effective when accompanied by an integration agreement.

[6]Klaeui (2026) shows that encouraging broader job search reduces unemployment duration for job seekers in slack labor markets, while narrowing job search reduces unemployment duration for those in tight labor markets.

Section 3 introduces the data, details the sample selection criteria and presents some descriptive statistics on activity reports. Section 4 presents the empirical approach and provides evidence supporting the assumptions underlying our identification strategy. Section 5 presents our main results, and Section 6 provides evidence on effect heterogeneity. Section 7 concludes.

# 2 Institutional Background

This section describes the UI system, focusing on the rules governing benefit eligibility, monitoring of job-search requirements, and sanctions in place during our observation period from 2014 to 2017. To better understand how these rules were implemented in practice, we conducted several interviews with individuals who worked as caseworkers during our observation period.

## 2.1 Unemployment Insurance

Sweden is one of the few countries where UI coverage is voluntary. UI benefits are administered by 24 independent UI funds. Workers can choose which fund to join, but since UI funds are linked to labor unions, membership is typically determined by industry, occupation, or a combination of the two.[7] UI fund membership is high in Sweden: approximately 70% of the labor force was enrolled in a UI fund between 2010–2020. Each UI fund sets its own membership contribution, which finances both administrative costs and a fixed government fee based on the number of members.[8]

A job seeker must meet several requirements and conditions to qualify for UI benefits. First, upon becoming unemployed, the individual must have been a member of a UI fund and paid contributions for at least 12 months.[9] Second, the job seeker must have been employed for at least six of the previous 12 months. Third, the job seeker must register at the Public Employment Service (PES) and be able and willing to work at least three hours a day and a minimum of 17 hours per week. Fourth, the unemployed must actively search for a job throughout their unemployment spell. Our analysis focuses on this fourth condition.

[7] Some individuals qualify for more than one fund, but most join the one aligned with their occupation or industry. Selective entry across UI funds is therefore unlikely to be a concern.

[8] In 2014, the average monthly contribution was around SEK 115 (€10.29), with modest variation across funds, ranging from SEK 85 (€7.60) to SEK 160 (€14.31). These differences were generally not a decisive factor in fund choice (IAF Database).

[9] Landais et al. (2021) analyze selection into UI-membership in Sweden. They provide evidence that unemployment risk is higher among workers who buy supplemental coverage, driven partly by risk-based selection and partly by moral hazard.

Job seekers meeting these conditions are entitled to UI benefits for up to 60 weeks. During our observation period, benefits replaced 80% of previous earnings up to a ceiling. Individuals who do not meet the membership requirement may instead qualify for Unemployment Assistance (UA), which provides a flat-rate benefit and is less generous than UI. We restrict our analysis to UI recipients because monitoring rules were different for UA recipients during our observation period.

## 2.2 Activity Plans and Activity Reports

UI benefit recipients are required to actively search for a job. The Swedish PES is responsible both for monitoring job seekers' job-search activities and for assisting them in their search for suitable jobs. A central element of this process is the *activity plan*, which is developed jointly by a PES caseworker and the job seeker. The activity plan outlines the support that the PES will provide and specifies the activities expected from the job seeker. These are organized into eight broad support categories rather than a detailed individualized plan.[10] The activity plan does not set a minimum level of required activities. For example, there is no fixed requirement for the minimum number of job applications per month.

To enhance the monitoring of job search efforts, monthly *activity reports* were introduced in 2013. Between the 1st and 14th of each month, job seekers are required to submit an activity report detailing their job search activities from the previous month. Most importantly, job seekers must report the positions for which they have applied. Additionally, the reports can include other activities such as job interviews and job-shadowing visits. Appendix B shows the form that the unemployed must use to submit their activity reports.

Activity reports serve as the primary tool for monitoring job-search behavior. Caseworkers review these reports and decide whether to approve them. Although general guidelines exist, assessments rely heavily on caseworker discretion. The fact that the activity plan does not specify quantitative targets, such as a minimum number of job applications, leaves substantial scope for individual judgment. According to our interviews with PES caseworkers, reviewing a report typically takes about ten minutes for an experienced caseworker. In addition to the number of job applications, caseworkers assess whether these jobs align with the job seeker's education and occupational skills. They should also take local labor market conditions into account when evaluating search efforts. This combined assessment of the

[10] The categories are: search for jobs, improve your search, guidance to work, education to work, start a new business, clarify your qualifications for work, adapt your work situation, and work-preparatory measures.

quantity and relevance of job applications is a key reason why the PES does not impose a fixed minimum number of required applications per month.

## 2.3 Allocation of Activity Reports to Caseworkers

The Swedish PES is organized into two types of offices: *local offices* and *central offices*. At local offices, job seekers register, meet with caseworkers, receive job-search assistance, and may be assigned to ALMPs. Central offices were introduced in 2010 to improve efficiency and accessibility by offering information and support to job seekers by phone. During our observation period, there were approximately 200 local offices across the country and seven central offices. Each central office was responsible for handling activity reports from a specific geographical area.[11]

When activity reports were first introduced in 2013, they were assessed by caseworkers at local PES offices. To reduce the workload of local caseworkers, responsibility for evaluating most reports was gradually transferred to the central offices. This shift began in 2013 and was fully implemented by February 2014. Central-office caseworkers were responsible for assessing activity reports but had no direct contact with the job seekers. Local caseworkers retained responsibility for meetings, job-search assistance, and program assignments.

Activity reports in central offices were managed through an IT system called *The Assistant*. In this system, the *first* activity report submitted by each newly registered job seeker appeared on a *common reports-list* accessible to all caseworkers within the office. Reports were ordered by submission date, with the oldest on top, and caseworkers processed them sequentially by selecting reports from the top. At this stage, the only information visible to the caseworker was an anonymous identification number. Caseworkers therefore had no prior information about either the content of the report or the job seeker. As a result, conditional on office and time, the allocation of reports to caseworkers was effectively quasi-random.[12]

The common reports list was strictly followed for the first report submitted by each job seeker. Once a caseworker reviewed the first report, that caseworker was typically responsible for reviewing the same job seeker's next three reports.

[11] In 2018, the central offices were restructured and reorganized. Caseworkers in central offices now also began holding meetings with job seekers and assigning them to programs, tasks that had previously been carried out by local offices.

[12] Around 15% of the job seekers receive vacancy proposals from central office caseworkers before submitting their first activity report. These proposals are sent without prior contact and, unlike vacancy referrals, are non-binding suggestions rather than mandatory applications. To isolate the impact of caseworker strictness, we exclude all job seekers who received a vacancy proposal from central-office caseworkers before the assessment of the first report. In robustness analyses, we show that our results are unaffected by this restriction.

Thereafter, activity reports were assessed by caseworkers in local offices. We therefore exploit the quasi-random allocation of the first report and primarily estimate reduced-form effects of being assigned to a stricter caseworker for the initial report.

## 2.4 Benefit Sanctions and Monitoring Chain

Violations of the conditions for retaining UI benefits can result in sanctions, which consist of temporary suspensions of UI benefits. Since UI funds administer UI benefits, they are responsible for imposing sanctions based on information provided by the PES.

The sanction system follows a staircase model in which penalties increase in severity with each subsequent violation of the rules (IAF, 2014c). Sanctions are categorized into three groups: (1) job-search and related misconduct, (2) rejection of job offers, and (3) quitting a job without valid cause. If a caseworker disapproves a report due to insufficient job search, it falls into the first category. The staircase model for sanctions due to insufficient job search consists of five steps.[13] The first violation results in a warning. The second leads to a one-day suspension of benefits, the third to a five-day suspension, the fourth to a ten-day suspension, and the fifth to a complete suspension of UI benefits.[14] The job-search and related misconduct category also includes other types of misconduct, such as failure to submit activity reports, absence from a scheduled meeting with a PES caseworker, and failure to apply for a referred job vacancy.

As described above, monitoring of job search begins with the assessment of activity reports. On average, about 7% of reports are disapproved. If a report is deemed insufficient, a sequence of actions takes place – a process we refer to as the *monitoring chain*. Local caseworkers are responsible for these actions, while central-office caseworkers are not involved. If a report is disapproved by a central-office caseworker, the job seeker is informed promptly through the online activity reporting portal and typically also by a phone call from a local caseworker. During this call, the caseworker discusses the reported job-search activities, indicates whether a violation will be registered, and may provide guidance on how to improve the job search. According to our interviews with caseworkers, these feedback calls usually last between 10 and 15 minutes. The job seeker can also view any disapproval

[13] Sanctions for rejecting job offers follow another staircase model that is independent of the staircase for insufficient job search. It involves suspensions of 5, 10, and 45 days for the first, second, and third violation, followed by complete suspension after the fourth violation. Quitting a job without valid cause leads to a 45-day suspension.

[14] In principle, individuals can appeal a sanction, but this is rare. Of all decisions, only a negligible fraction (2%) is partially or fully reversed.

through the website used to submit activity reports.

In the next step of the monitoring chain, the local-office caseworker decides whether to register a *violation*. It is not uncommon for an activity report to be disapproved without a violation being registered. In fact, only around 30% of all disapproved reports result in a registered violation.[15] If a violation is registered, this is usually done on the same day the report is disapproved; the median time between disapproval and registration is zero days. Once a violation has been registered, the job seeker is automatically informed by email. After a violation is registered, a *notification* is automatically sent to the relevant UI fund. In about 95% of cases, this occurs on the same day that the violation is registered. Finally, the UI fund decides whether to impose a *sanction* based on the information contained in the notification. Sanction decisions are usually made within two to three weeks of the notification. In our sample, 67% of notifications result in a sanction.

In *summary*, job search in Sweden is monitored through monthly activity reports. The centralized review process generates quasi-random variation in report assessments that can be exploited using a judge-design framework. A disapproved report may have several consequences. First, repeated violations will lead to benefit sanctions. Second, the absence of specific quantitative search requirements and the relatively low rate of disapproved reports suggest that a disapproval may constitute a substantial monitoring shock for job seekers. Third, a disapproval is typically followed by contact with a local-office caseworker, which may strengthen monitoring through discussions of planned search activities and provide guidance on how to improve job search. These consequences are inherent to the institutional setting, and our estimates capture their combined reduced-form effect. From a policy perspective, this corresponds to the relevant effect of stricter monitoring.

# 3 Data and Sample Selection

## 3.1 Data Sources

We use detailed register data from the Swedish PES on employment status, program participation, meetings, and all stages of the monitoring process. These data are complemented with data on labor earnings, wages, and occupations.

The PES data provide daily information on the unemployment status of all registered job seekers, eligibility for UI benefits, characteristics recorded at the start

[15] The remaining 70% are split evenly between cases where caseworkers choose not to register a violation after contacting the job seeker and cases in which the PES takes no action at all following a disapproved report (IAF, 2015a).

of the unemployment spell (such as age, gender, and unemployment history), and entries into and exits from ALMPs.[16] The data also contain records of interactions between job seekers and caseworkers, including meetings and vacancy referrals. Additionally, the PES data contain information on each activity report, including the submission date, the date it was first accessed, the caseworker who evaluated it, the number of reported job applications and other activities, and the outcome of the assessment (either "approved" or "disapproved"). Job applications include responses to advertised vacancies, unsolicited applications, and applications submitted in response to vacancy referrals and vacancy proposals. Other activities include interviews, recruitment meetings, and additional search-related activities.

We also observe all recorded violations of the UI benefit rules, including the date, reason, reporting caseworker, and whether a notification was sent to the relevant UI fund. The violation reasons allow us to distinguish between cases of insufficient job search and other violations unrelated to disapproved activity reports. The data also contain records of all sanction decisions made by the UI funds. These include the date, the type of sanction,[17] and the corresponding step on the sanction ladder, indicating the individual's position within the sequence of previous sanctions in the same category.

The PES data are merged with population registers and wage statistics from Statistics Sweden. The population registers contain additional information, including the country of origin and the level of education. We also use matched employer-employee data to obtain information on labor earnings. These data are used to construct annual measures of labor earnings and monthly earnings for the months following the assessment of the first activity report.[18]

The wage statistics contain annual data on wages,[19] occupations, and contracted hours. The data cover the entire public sector, all large private firms, and a random sample of smaller firms. In total, the data cover about 50% of private-sector employees and all public-sector employees. Information is collected each fall and refers to all employees working at sampled firms at the time of data collection.

[16] In our sample, the most common reason for exiting unemployment is a transition to employment (74.8%). Other exit reasons–such as entering education, receiving social assistance, or enrolling in alternative insurance schemes–are less common.

[17] There exist three types of sanctions, related to the reason for the sanction: (1) job search and related misconduct, (2) rejection of job offers, and (3) quitting a job without valid cause.

[18] Monthly earnings are constructed using annual earnings and monthly employment indicators. For months in which an individual is employed, earnings are calculated as annual earnings divided by the number of months employed. For months in which an individual is not employed, earnings are set to zero.

[19] Wages are recorded as monthly full-time equivalents, and are not derived from annual earnings and hours worked.

## 3.2 Sample Selection

Our analysis focuses on job seekers who entered as full-time unemployed between 1 February 2014 and 31 December 2017. The centralized assessment was fully implemented in February 2014, and we end the sample in 2017 because the central offices were restructured in 2018. Our identification strategy relies on the quasi-random allocation of each job seeker's first activity report to caseworkers in central offices. Therefore, we restrict the analysis to job seekers whose first activity report was reviewed by a central office.[20]

We focus on UI recipients, as the monitoring rules differ for non-recipients. We further restrict the sample to individuals aged 25 or older and exclude those with previous sanctions (around 4% of the sample) to focus on job seekers at the first step of the sanction ladder. We impose no restrictions on prior employment or unemployment history. We exclude job seekers who received a vacancy proposal from a central-office caseworker before submitting their first activity report. We also exclude job seekers who failed to submit their first report on time. The precision of each caseworker's strictness measure depends on the number of cases they handle. To ensure reliable measures, we restrict our main sample to caseworkers who review at least 75 first activity reports during our observation period. This restriction excludes around 5% of unemployment spells. Our results are robust to these sampling restrictions.

The final sample consists of 390 caseworkers across six central offices and 109,866 unemployment spells among 100,703 job seekers. The median caseload is 375, and 5% of caseworkers handle more than 804 job seekers. Figure A1 shows the distribution of the number of first activity reports reviewed by each caseworker during the observation period.

## 3.3 Reported Activities and Approval of Activity Reports

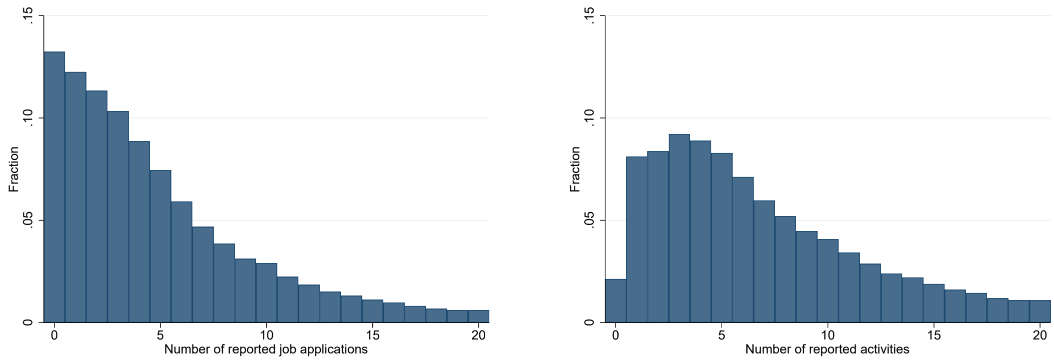
Figure 1 presents descriptive statistics on the number of job applications and total job-search activities reported in the first activity report. Panel A shows that approximately 13% of job seekers do not report any job applications, while a majority (56%) report fewer than five applications.[21] Panel B indicates that nearly all job

[20] One of the seven offices opened in 2017 and handled very few activity reports prior to September 2017. We exclude cases handled by this office, which account for approximately 1.5% of all reports reviewed by central offices.

[21] The duration of the reporting period for the first report is determined by the date of entry into unemployment, and job seekers entering in the middle of the month have a reporting period shorter than a month. However, even when restricting the sample to job seekers who report for a full month, the share with zero job applications remains substantial (12%).

seekers report at least one activity. In addition to job applications, these activities include, for example, job interviews and job-shadowing visits. Figure A2 in the Appendix shows that the number of reported job applications increases slightly in the second and third activity reports. On average, job seekers report 6 applications in the first report, increasing to 6.6 in the second and 6.7 in the third. The average number of reported activities remains relatively stable at 9.0, 9.4, and 9.2 across the three reports.

Figure 1: Number of job applications and activities in first activity report



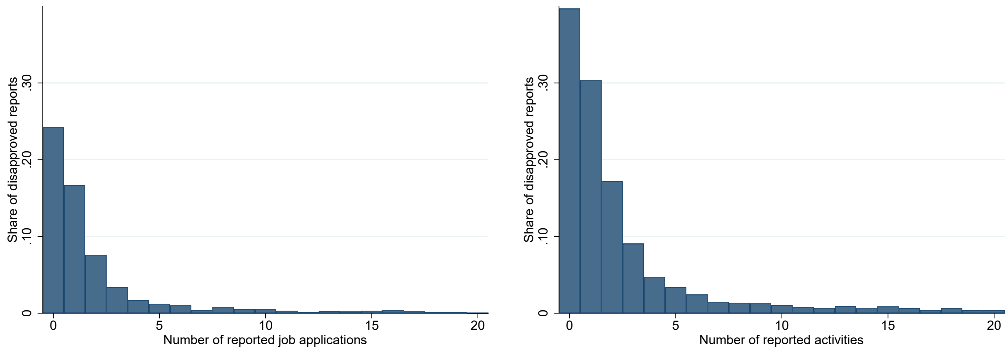

Panel A: Job Applications Report 1 Panel B: Total Activities Report 1

*Notes*: Statistics for first activity report for the analysis sample described in Section 3.2. 109,866 cases and 390 caseworkers. Job applications include standard applications, unsolicited applications, and applications for vacancy referrals or proposals. Total activities additionally include interviews, recruitment meetings, other activities, and activities specified in the action plan. Figure A2 shows equivalent statistics for reports 2 and 3.

Focusing on the first activity report, Figure 2 shows a strong negative relationship between the number of reported job applications and the probability that a report is disapproved. The figure also highlights both the comparatively lenient nature of the Swedish monitoring system and the substantial variation in report assessments. Approximately 24% of job seekers who report no job applications receive a disapproval, whereas this share falls to about 1% among those reporting five applications. When considering all job-search activities rather than job applications alone, we observe a similar pattern, but also evidence that caseworkers take other activities into account. About 40% of reports with zero activities are disapproved, compared with a disapproval rate of 3% among those reporting five activities.[22]

[22]Figure A3 in the Appendix presents the corresponding distributions for the second and the third report. Although the relationship is somewhat weaker than for the first report, we continue to observe a negative association between both the number of reported job applications and the total number of reported activities and the likelihood of disapproval.

Figure 2: Share of disapproved reports, by number of reported job applications and activities

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# 4 Empirical Approach

We are interested in estimating the causal effect of stricter monitoring of job-search requirements on unemployed job seekers' employment outcomes, using variation in caseworkers' propensity to disapprove monthly job-search reports as our measure of monitoring strictness. Identification relies on the quasi-random allocation of job seekers' activity reports to caseworkers in central offices. Caseworkers differ systematically in their strictness when evaluating these reports, yet have no direct contact with the job seekers whose reports they review. This institutional feature rules out confounding caseworker behaviors, such as counseling and meeting intensity, or assignment to ALMPs.

## 4.1 Caseworker Strictness

For each job seeker $i$, we estimate the strictness of the central-office caseworker $j$ who reviews the job seeker's first activity report. Caseworker strictness, $CS_{j(i)}$, is defined as the leave-one-out share of disapproved first activity reports among all other first reports assessed by the same caseworker during the observation period

(Chyn et al., 2025; Goldsmith-Pinkham et al., 2026):

$$CS_{j(i)} = \frac{\sum_{k \neq i} D_{jk}}{n_j - 1}, \tag{1}$$

where $D_{jk} = 1$ if caseworker $j$ disapproves the first activity report of job seeker $k$, and $D_{jk} = 0$ otherwise. $n_j$ denotes the total number of first reports reviewed by caseworker $j$.[23]

On average, caseworkers in our sample disapprove 7% of first activity reports, but there is substantial heterogeneity in strictness. The 10th and 90th percentiles of the strictness distribution are 0.020 and 0.130, respectively. Figure 3 illustrates this variation and shows that caseworker strictness strongly predicts the probability that the first activity report is disapproved. Caseworker strictness is also persistent across reports. Measures based on the first and the second reports are highly correlated, and around 75% of job seekers are assigned to the same caseworker in the first and the second report. Figure A4 in the appendix further shows that caseworkers with above-median strictness exhibit higher disapproval rates across all levels of reported job applications.

## 4.2 Empirical Specification

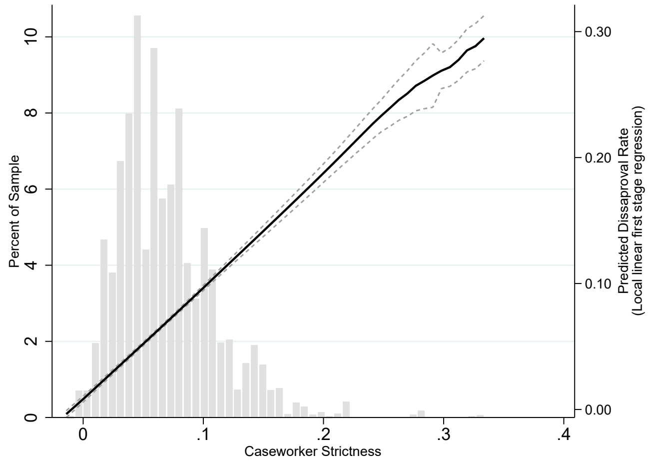
Our main specification is a reduced-form regression of a job seeker's employment outcome in period $t$ after entry into unemployment, $Y_{it}$, on caseworker strictness $CS_{j(i)}$:

$$Y_{it} = \beta_1 CS_{j(i)} + \gamma_{q(i)} + \varepsilon_{it}. \tag{2}$$

$\beta_1$ captures the reduced-form effect of caseworker strictness. The term $\gamma_{q(i)}$ denotes central-office–by–month fixed effects, ensuring that we exploit variation in caseworker strictness within a given office and month. This allows us to isolate the quasi-random variation generated by the common reports lists used in the central offices. In robustness analyses, we also consider specifications with finer fixed effects, using interacted calendar-day and year fixed effects within each central office, which does not affect our results. For parsimony, we therefore report estimates based on the central-office–by–month fixed effects as our main specification. Given that the allocation of reports to caseworkers is quasi-random conditional on time and central

[23] We also explore a residualized strictness measure, which is relevant for IV models with covariates (Chyn et al., 2025). In Section 5.6, we show that using a residualized strictness measure, adjusted for calendar month and office fixed effects, yields very similar results.

Figure 3: Distribution of caseworker strictness and the first stage




*Note*: Statistics for caseworker strictness, defined as the leave-one-out share of activity reports marked not approved for each caseworker, in the analysis sample of first reports described in Section 3. The sample consists of 109,866 cases and 390 caseworkers. The black line plots a local linear regression of the rejection rate on the strictness measure, based on an Epanechnikov kernel (with bandwidth 0.1). Strictness is demeaned using office-month fixed effects, with the global mean added back for interpretability.

office, standard errors are clustered at the office-by-month level.[24]

We additionally estimate short-run effects of receiving a disapproved first activity report. Following the standard judge-design approach (Chyn et al., 2025; Goldsmith-Pinkham et al., 2026), we use caseworker strictness, $CS_{j(i)}$, as an instrument for a disapproved activity report. The first- and second-stage equations are:

$$D_i = \theta_1 CS_{j(i)} + \gamma_{q(i)} + \zeta_{it}. \quad (3)$$
$$Y_{it} = \alpha_1 D_i + \gamma_{q(i)} + \nu_{it}. \quad (4)$$

The coefficient $\theta_1$ captures the first-stage effect of caseworker strictness on the probability of receiving a disapproved first activity report, while $\alpha_1$ captures the local average treatment effect of receiving a disapproved first activity report on the employment outcome. For this approach to be valid, the caseworker strictness must affect employment outcomes only through its effect on the approval status of the first activity report. This condition is likely to hold in the short run. However, once

[24] Alternative clustering levels, such as clustering at the office or caseworker level, yield similar results. If anything, clustering at the office-by-month level appears to be the most conservative approach.

the second activity report is assessed, the exclusion restriction is violated because the same caseworker often reviews subsequent reports. As a result, job seekers assigned to stricter caseworkers are also more likely to have their second and third activity reports disapproved. Therefore, we focus on short-run effects, measured as the probability of leaving unemployment between the assessment of the first report and the date when the job seeker is expected to submit the second activity report (approximately one month later).

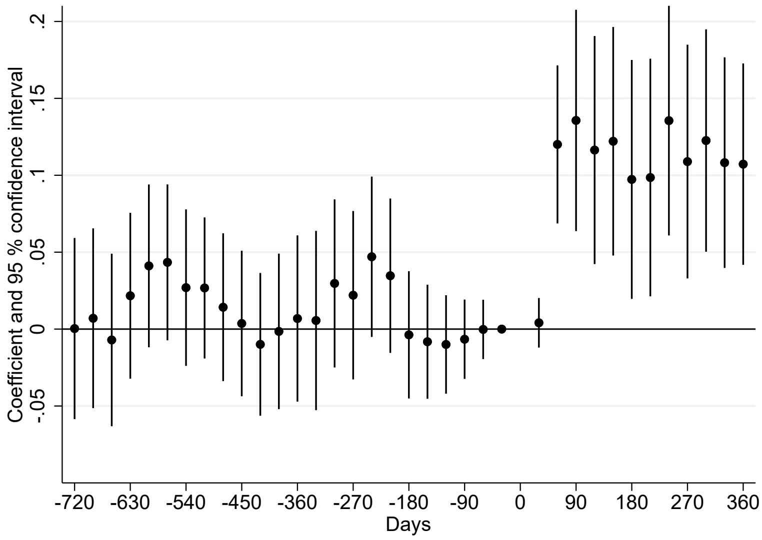
Our caseworker strictness measure is leave-one-out and therefore excludes the job seeker's own assessment. However, with covariates, conventional leave-one-out leniency measures can still suffer from own-observation bias through the adjustment for controls. Following Goldsmith-Pinkham et al. (2026), we therefore also estimate our short-run IV effects using the unbiased jackknife instrumental variables estimator (UJIVE), which constructs leave-out relative leniency while accounting for the controls. The results are robust to using UJIVE, as shown in Table A10.

## 4.3 Independence: Quasi-Random Assignment

We next show that the assignment of the first activity report to a caseworker is independent of predetermined job seeker characteristics. To do so, we regress (i) an indicator for having left unemployment within 90 days, (ii) an indicator for receiving a disapproved first activity report, and (iii) the strictness of the assigned caseworker on predetermined job seeker characteristics, controlling for central-office–by–month fixed effects. As expected, Table A1 shows that both the probability of remaining unemployed and the probability of receiving a disapproved report are significantly associated with nearly all observed characteristics (Columns 1–2). In contrast, and consistent with the quasi-random allocation mechanism based on common report lists, caseworker strictness is unrelated to predetermined characteristics. Only the coefficients for being married and having less than a high school education are statistically significant at the 10% and 5% levels, respectively (Column 3). We also perform joint F-tests, regressing the three outcomes on the full set of characteristics, excluding predicted reemployment probability. These tests confirm that the observed characteristics are jointly related to both the probability of leaving unemployment within 90 days and the probability of receiving a disapproved report, but not to the strictness of the assigned caseworker.

Figure 4 presents coefficients from regressions of employment status, measured at different points before and after entry into unemployment, on caseworker strictness. None of the 23 coefficients associated with lagged employment outcomes, observed up to 720 days before the current unemployment spell, is statistically significant at the 5% level. This provides further support for the quasi-random assignment of job seekers to caseworkers. After the assessment of the first activity report, employment rates begin to diverge according to the strictness of the assigned caseworker. We return to this figure below.

Figure 4: Effects of caseworker strictness and the probability of being employed




*Notes:* Regression coefficients from regressions of employment status at $t$ days relative to the start of the unemployment spell on caseworker strictness. All job seekers are employed at $t = -30$. Point estimates and 95% confidence intervals shown on vertical axis. Regressions include month–agency fixed effects with standard errors clustered at the month–agency level. N = 109,866; 390 caseworkers.

As further support of quasi-random assignment, Columns (1)–(2) of Table A2 show that caseworker strictness is uncorrelated with the content of the first activity report, which was submitted before any caseworker assessment. This holds both for the number of reported job applications and the total number of activities. Column (3) shows no association between strictness and the time elapsed between unemployment entry and submission of the first report. However, Column (4) reveals a small positive correlation between caseworker strictness and the time between report submission and evaluation. Moving from the 10th (0.020) to the 90th (0.130) percentile of the strictness distribution increases strictness by 0.11, implying an increase in evaluation time by 0.18 days. Given that the average evaluation time is approximately three days, this minor delay is unlikely to have a meaningful effect on labor market outcomes.

## 4.4 Caseworker Strictness and Monitoring Decisions

Figure 3 shows that caseworker strictness is positively associated with the probability that the first activity report is disapproved. This finding is confirmed by regressing an indicator for a "disapproved" first activity report on caseworker strictness while controlling for office-by-month fixed effects (Column 1, Table 1). The coefficient of 0.842 implies that moving from the 10th percentile (0.020) to the 90th percentile (0.130) of the strictness distribution increases the probability of receiving a disapproved report by 9.3 percentage points—more than doubling the mean probability of 6.9%.

Table 1: Effects of caseworker strictness on the monitoring chain

| Panel A: Monitoring chain | (1) Disapproved First report | (2) Violation Job search | (3) Violation Other | (4) Notification Sent to UI | (5) Any Sanction |
|---|---|---|---|---|---|
| CW strictness | 0.842*** | 0.209*** | -0.015 | 0.143*** | 0.126*** |
| | (0.028) | (0.016) | (0.024) | (0.026) | (0.021) |
| Mean depvar | 0.069 | 0.022 | 0.122 | 0.124 | 0.083 |
| N | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 |
| **Panel B: Sanctions Ladder 1** | **(6) Zero-day Sanction 120 days** | **(7) Zero-day Sanction 240 days** | **(8) Higher-order Sanction 120 days** | **(9) Higher-order Sanction 240 days** | |
| CW strictness | 0.110*** | 0.076*** | 0.022*** | 0.013 | |
| | (0.020) | (0.023) | (0.008) | (0.011) | |
| Mean depvar | 0.072 | 0.113 | 0.011 | 0.024 | |
| N | 109,866 | 109,866 | 109,866 | 109,866 | |

*Notes:* We control for month-office fixed effects. Panel A: dependent variables in columns 2–5 are indicators for at least one event within 120 days from the start of the unemployment spell. Columns 2—3 report registered violations for insufficient job search (Column 2) and other reasons including failure to set up an activity report, failure to submit an activity report, missed contact or visit, and failure to apply for a referred job (Column 3). Columns 4–5 report notifications sent to the UI funds and imposed sanctions due to both job search violations and other violations. Panel B: columns 6–7 include sanctions at the first step of the ladder, which is a zero day sanction. Columns 8–9 include higher-order sanctions, i.e. 1 day, 5 days, 10 days and suspension. In both cases, within 120/240 days the start of the unemployment spell. Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively.

As described in Section 2.4, a disapproved report is the first step in the monitoring chain but does not automatically result in a benefit sanction. Columns 2–5 of Table 1 show the effects of caseworker strictness on subsequent monitoring steps. Since it may take time for a disapproved report to translate into a registered violation and a benefit sanction, we examine monitoring-chain actions occurring within 120 days after entry into unemployment. This allows us to capture the full reduced-form effect on the monitoring chain, although it complicates the comparison of effect estimates across columns since it includes consequences of both the first and subsequent activity reports.[25]

The probability of a registered job-search-related violation increases with caseworker strictness (Column 2), whereas the probability of violations unrelated to job search remains unaffected (Column 3). Comparing the estimates in Columns 1 and 2, while keeping the caveats mentioned above in mind, suggests that roughly one fourth of disapproved reports lead to registered job-search violations. The likelihood that a notification is sent to the UI fund and that this results in a sanction also increases with caseworker strictness (Column 4–5). Moving from the 10th percentile to the 90th percentile of the strictness distribution increases this probability of a sanction by 1.4 percentage points, corresponding to an increase of about 17% relative to the mean.

All these estimates discussed above refer to any type of sanction, including zero-day sanctions. Panel B of Table 1 distinguishes between zero-day sanctions and those involving an actual suspension of UI benefits. We find that the likelihood of both types of sanctions increases with caseworker strictness. This holds both when we look within 120 and 240 days, although the estimated coefficients decline somewhat in magnitude and become insignificant for sanctions involving benefit suspensions within 240 days.

Taken together, these results show that stricter caseworkers lead to more disapproved activity reports, and initiate an entire monitoring chain, resulting in more registered violations, more notifications to UI funds, and ultimately more benefit sanctions.

## 4.5 Exclusion Restriction and Monotonicity

To identify the effects of caseworker strictness and, in the IV specification, the short-run effect of receiving a disapproved first report, caseworker strictness must not affect job seekers through other channels. Our institutional setting with central caseworkers being responsible for reviewing reports without having direct contact with job seekers provides strong support for this exclusion restriction.

The IV specification further requires that caseworker strictness has a monotonic effect on the probability that an activity report is disapproved. Following Frandsen et al. (2023), we jointly test the pairwise monotonicity and strict exclusion restrictions. Pairwise monotonicity assumes that all caseworkers rank job seekers in the

[25] Since time elapses between steps in the monitoring chain, some violations captured in Column 2 toward the end of the window may not yet appear in Columns 4–5.

same order of compliance. Under this assumption, a job seeker whose report would be disapproved by a relatively lenient caseworker would also have the report disapproved by any stricter caseworker. We reject the joint null that both pairwise monotonicity and the strict exclusion restriction hold.

The IV estimator nevertheless identifies a causal local average treatment effect under the weaker assumption of *average monotonicity*. This assumption requires that, for any given job seeker, the average strictness of caseworkers who would disapprove a report is weakly greater than that of those who would approve it (Chyn et al., 2025). This assumption has testable implications. First, the first-stage effect—the effect of caseworker strictness on the probability of report disapproval—should be non-negative across all observable subpopulations (Autor et al., 2019). Table A3 supports this: we find positive first-stage effects across all subgroups constructed based on observed characteristics in our data.[26] Second, caseworkers who are stricter toward one group—for example, younger individuals—should also be stricter toward the complementary group—in this case, older individuals. Table A4 shows that this holds. Recalculating the strictness measures within each subgroup and correlating them with disapproval rates in the complementary group yields consistently positive and statistically significant correlations.

# 5 Effects of Having a Stricter Caseworker

## 5.1 Employment Probability

Table 2 reports the reduced-form effects of caseworker strictness on the probability of leaving unemployment for a part- or full-time job within different time horizons. As expected, we find no effect during the first 30 days of unemployment, consistent with the institutional setting in which job seekers submit their first activity report after an average of 33 days. After 60 days, approximately one month after the first report, we find a statistically significant increase in the probability of leaving unemployment. The employment effect is largest at the beginning and then declines somewhat, but remains sizable and statistically significant throughout the first 360 days. Quantitatively, moving from the 10th percentile to the 90th percentile of the distribution of caseworker strictness increases the probability of leaving unemployment within 120 days by around 1.1 percentage points, corresponding to roughly a 3% increase relative to the mean. These results show that stricter monitoring

[26]This includes men and women, married and unmarried individuals, younger and older job seekers, those with and without college education, individuals at high and low risk of long-term unemployment, and across regions with low and high unemployment rates.

substantially shortens unemployment durations. The effects persist well beyond the first four months of unemployment, when responsibility for monitoring is transferred to local offices and no longer depends on the strictness of the initially assigned central-office caseworker.

Table 2: Effects of caseworker strictness on the probability of finding a job

| Outcome: finding a job within X days | | | | | | |
|---|---|---|---|---|---|---|
| | | After assessment of the first report | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | 30 days | 60 days | 90 days | 120 days | 180 days | 360 days |
| CW strictness | 0.008 | 0.114*** | 0.114*** | 0.103*** | 0.076** | 0.064* |
| | (0.009) | (0.027) | (0.035) | (0.038) | (0.038) | (0.033) |
| Mean depvar | 0.011 | 0.131 | 0.269 | 0.380 | 0.528 | 0.704 |
| N | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 |

*Notes:* Outcome is defined as leaving unemployment for a part- or full time job. We control for month-agency fixed effects. Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively.

The estimated effects are economically meaningful. Several features of the institutional setting may help explain their magnitude. First, because there are no explicit quantitative job-search requirements and disapprovals are relatively rare, receiving a disapproved report likely constitutes a substantial monitoring shock for job seekers. As we show below, this is reflected in large effects on job-search intensity. Second, Table A5 reports results from a complier analysis, showing that initial search intensity prior to the assessment of the first report is particularly low among compliers. Monitoring and the resulting intensified search may be particularly important for this group of inactive job seekers.[27]

These effects on exits from unemployment do not take recurring unemployment spells into account. One concern is that job seekers who leave unemployment sooner because they are assigned to stricter caseworkers may subsequently return to unemployment more quickly. We find no evidence of such an effect. When examining employment probabilities over time, taking both the current and recurring unemployment spells into account, we obtain a very similar pattern: positive effects emerge after 60 days and remain statistically significant for approximately 18 months. The estimated effect then declines and is no longer statistically significant after around 24 months (Table A6 and Figure 4).

[27] Compliers report searching for, on average, 1.5 jobs (3.4 activities), compared to the sample average of 6 jobs (9 activities). Along other dimensions, compliers are similar to the full sample, including in terms of gender, age, unemployment history, unemployment risk, and the growth rate of their previous industry. Complier means are constructed following Goldsmith-Pinkham et al. (2026); details are provided in the note to Table A5.

## 5.2 Labor Earnings and Job-Quality

Having a stricter caseworker evaluate the activity reports leads to higher job-finding rates. In a standard job-search model, more intensive monitoring lowers the value of unemployment and may therefore reduce job seekers' reservation wages. This could lead job seekers to accept lower-paying jobs or jobs with other less desirable characteristics. Stricter monitoring may thus increase job finding at the cost of lower job quality. We use wage data and longer-run earnings measures to examine whether this is the case.

We consider monthly earnings in the months following the first report and annual labor earnings up to three years after entry into unemployment (see Section 5.1 for details). These outcomes are based on tax records and therefore provide a useful complement to the job-finding data from the Swedish PES.[28] The estimated earnings effects may reflect both changes in employment and changes in job quality through employment duration and wages. To isolate the effect on wages, we additionally use data from the wage statistics on the wage in the first job after exit from unemployment. As noted above, these wages are recorded as monthly full-time equivalents and are not derived from annual earnings and hours worked.[29] Since caseworker strictness affects the transition to employment, the wage estimates are based on a selected sample and should therefore be interpreted with caution.

The results for monthly and annual labor earnings in Table 3 confirm the patterns observed for exits from unemployment. In the first month after entry to unemployment–that is, before the first report was submitted and reviewed by the caseworker–we find, as expected, no effect on monthly earnings (Column 1). Following the assessment of the first report, however, monthly earnings increase, consistent with the rise in job finding documented above (Columns 2–5). The magnitude of these earnings effects is broadly consistent with the job-finding estimates based on the PES data. For example, moving from the 10th to the 90th percentile of the strictness distribution increases job finding within 120 days by 3% and monthly earnings three months after entry increases by around 4.1%.

Columns 6–8 report results for longer-run earnings and the wage in the first job after unemployment. The estimates suggest that the positive effects on earnings are persistent, although they are estimated less precisely than the effects on exits from unemployment reported in Table 2. The point estimates for annual earnings in the

[28] One concern with the job-finding data from the Swedish PES is that they may not perfectly capture employment as they rely partly on information reported by job seekers. It is therefore reassuring that we obtain very similar results using earnings data based on tax records.

[29] This dataset covers only a subsample of all employees, which explains why the sample size is lower for this wage outcome.

second and third years after entry into unemployment are sizeable and positive, but only the estimate for the second year is statistically significant at the 10% level. The estimate for the wage in the first job is also positive but statistically insignificant. Taken together, the positive effects on earnings in the first year, the positive but imprecisely estimated effects in years two and three, and the absence of a negative effect on the wage in the first job provide no evidence that intensified monitoring induces job seekers to accept lower-quality jobs.

Table 3: Effects of caseworker strictness on labor earnings and wages

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Monthly earnings: | | | | | Yearly earnings: | | |
| | Entry month | Entry month +1 | Entry month +3 | Entry month +6 | Entry month +12 | Entry year +2 | Entry year +3 | Wage first job |
| CW strictness | -1,968 | 3,001** | 4,025** | 531 | 2,503* | 19,000* | 16,207 | 990 |
| | (2,046) | (1,361) | (1,647) | (1,198) | (1,299) | (11,130) | (11,461) | (935) |
| Mean depvar | 16,629 | 7,338 | 10,709 | 14,630 | 17,499 | 199,128 | 207,973 | 24,326 |
| N | 109,865 | 109,865 | 109,865 | 109,865 | 109,865 | 108,543 | 108,238 | 44,098 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. Columns 1–5 show monthly labor earnings in SEK in calendar months relative to the start of the spell. Columns 6 and 7 show yearly labor earnings in SEK in calendar years two and three relative to the start of the spell. All labor earnings are based on tax records. Column 8 reports the monthly wage in the first job after the unemployment spell, based on the wage statistics.

## 5.3 Job-Search Intensity

Table 4 reports the effects of caseworker strictness on job-search intensity, measured using data from the activity reports. The first report is submitted before job seekers are assigned to a caseworker; accordingly, we find no statistically significant effect for this report. Subsequent reports are submitted after the assessment of the first report: Report 2 roughly corresponds to days 31–60, Report 3 to days 61–90, and so forth. Starting with the second report, we find sizable and statistically significant increases in the number of job applications. The effects are largest for the first four reports and then decline somewhat, coinciding with the transfer of responsibility for report assessments from central to local offices. These patterns indicate that the positive effects on exits from unemployment are accompanied by an increase in job applications, both immediately after the assessment of the first report and over the subsequent months.

The effect for Report 2 implies that moving from the 10th to the 90th percentile in the distribution of caseworker strictness increases the monthly number of applications by 0.36, or 5.4%. The cumulative effect over the first four reports, corresponding to the first 120 days of unemployment, amounts to approximately 1.2 additional applications, or 4.6%.

Table 4: Effects of caseworker strictness on the monthly number of reported job applications

| Outcome: Number of reported job applications | | | | | | |
|---|---|---|---|---|---|---|
| | | After assessment of the first report | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Report 1 | Report 2 | Report 3 | Report 4 | Report 5 | Report 6 |
| CW strictness | 0.013 | 3.242*** | 3.462*** | 4.124*** | 2.193** | 2.491* |
| | (0.593) | (0.656) | (0.788) | (0.976) | (1.106) | (1.277) |
| Mean depvar | 6.022 | 6.625 | 6.679 | 6.778 | 6.846 | 6.939 |
| N | 109,866 | 83,123 | 62,422 | 44,484 | 35,365 | 28,220 |

*Notes:* Outcomes are monthly number of job applications based on data from the activity report. We control for month-agency fixed effects. Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively.

One potential concern is that we observe job-search intensity only for individuals who remain unemployed, while the probability of remaining unemployed depends on caseworker strictness. This will lead to dynamic selection. To assess the importance of this concern, Table A7 reports results for a balanced panel of job seekers who submitted the first two reports. The results show a similar pattern to that in Table 4: caseworker strictness has a positive effect on the number of job applications in the second report, but no effect on the number reported in the first.

## 5.4 Labor Market Programs and Meetings

Besides affecting job-search intensity, a disapproved report and the ensuing monitoring chain may influence participation in ALMPs and contact with caseworkers. For instance, local office caseworkers often contact the job seeker following a disapproved report, which may lead to follow-up meetings or enrollment in an ALMP. Local caseworkers may also interpret a disapproved report as a signal that the job seeker needs additional support.

To examine these channels, we analyze the relationship between caseworker strictness and participation in the three most common Swedish ALMPs (work practice, vocational training, and preparatory training) as well as the number of meetings with local-office caseworkers.[30] Note that the job seekers have no meetings with the central-office caseworkers. Table 5 shows no statistically significant correlation between caseworker strictness and the probability of participating in any of the three major ALMPs within 120 days after entry into unemployment (Columns 1–4). We obtain similar results when considering alternative time horizons.

Table 5: Caseworker strictness, labor market programs and meetings

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Labor market programs | | | | Meetings | |
| | Any Program | Work Practice | Vocational Training | Preparatory Training | Face to Face Meetings | Phone Meetings |
| CW strictness | -0.002 | -0.004 | 0.002 | -0.000 | 0.163* | 1.231*** |
| | (0.011) | (0.005) | (0.005) | (0.006) | (0.087) | (0.096) |
| Mean depvar | 0.015 | 0.004 | 0.005 | 0.005 | 1.917 | 1.066 |
| N | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 |

*Notes:* The program-participation outcomes are constructed from the search-category codes (*skat koder*) in the PES dataset *sokatper*, while the meeting outcomes are based on the PES dataset *Histaktso*. All specifications include month-agency fixed effects. Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively.

In contrast, we find a positive relationship between the strictness of the central-caseworker and the number of face-to-face and telephone meetings with local-office caseworkers (Columns 5–6). The effect on telephone meetings is particularly sizable. The estimated effect of caseworker strictness on disapproved reports in Table 1 is 0.84. Comparing this with the estimate for telephone meetings (1.231) suggests that each disapproved report generates roughly 1.5 additional telephone contacts. This finding is consistent with the institutional setting, in which job-seekers often discuss a disapproved report with their local-office caseworker by telephone.

Taken together, these additional contacts appear to be a natural component of the intensified monitoring process. Several mechanisms may therefore contribute to the positive effects on job finding, including the monitoring shock associated with receiving a disapproved report, subsequent warnings and benefit sanctions, and increased contact with the PES. We cannot disentangle the relative importance

[30] Work practice involves participation in workplace-based activities in both the private and public sectors (Forslund et al., 2013). Vocational training consists of training aimed at improving the skills of unemployed workers, focusing on occupations where skills are (expected to be) in short supply (see, e.g., van den Berg and Vikström, 2022 for a recent evaluation). Preparatory training includes educational and other activities intended to prepare unemployed job seekers for participation in other labor market programs.

of these mechanisms. Nevertheless, the rapid employment response shown in Figure 4, together with the sustained increase in job applications documented above, is consistent with the initial disapproval prompting an early and persistent increase in job-search effort.

## 5.5 Causal Effects of Disapproved Activity Reports

Since stricter caseworkers affect the entire monitoring chain and trigger additional meetings, we cannot use caseworker strictness as an instrument for a benefit sanction or for the longer-run effects of receiving a disapproved report.

It can, however, be used as an instrument for receiving a disapproved first activity report when the analysis is restricted to outcomes observed before the second report is assessed. This restriction is necessary because the same caseworker typically assesses the first four reports. Outcomes observed after the assessment of the second report may therefore reflect the effects of stricter assessments of both the first and second reports, which cannot be separately identified.

We analyze job finding during the second reporting period, defined as the calendar month immediately following the first reporting period. The results in Table A8 indicate sizable positive effects of receiving a disapproved first activity report on both job finding and job-search intensity. The probability of finding a job between the first and the second report increases by 11 percentage points, and the number of job applications increases by 3.9 during this period. Under the assumption of average monotonicity, these estimates identify a positively weighted local average treatment effect of receiving a disapproved first activity report, with the weights reflecting how strongly each job seeker's probability of receiving a disapproval varies with the strictness of the assigned caseworker (Chyn et al., 2025).

As mentioned above, several factors may contribute to a sizable effect of a disapproved report. First, there are no explicit search requirements and disapprovals are rare, implying that a disapproval likely constitutes a substantial monitoring shock. Second, initial search intensity prior to the first report assessment is low among compliers and a "monitoring push" may be particularly important for relatively inactive job seekers (see Table A5).

## 5.6 Robustness analyses

Table A9 reports results from several robustness analyses. First, our main specification includes month-by-office fixed-effects, assuming that, conditional on the month in which a job seeker submits an activity report to a given central office, assignment to a caseworker within that office is quasi-random. Since we observe the exact submission date, we can instead include calendar-day-by-office fixed-effects. While this increases the number of fixed effects from 276 in the baseline specification to 3,857, this specification is based on the weaker assumption that conditional on the day the unemployed job seeker submits the activity report, the assignment to a caseworker is quasi-random. The estimates remain very similar to those from the main specification (Panel A of Table A9).

Second, the main specification does not control for observed characteristics of the job seekers. Given the quasi-random assignment mechanism, the inclusion of such controls should not substantially affect the results. Panel B confirms this: adding a standard set of predetermined characteristics yields results that are very similar to the baseline estimates. Third, our main sample also includes only caseworkers who assess at least 75 first activity reports. Relaxing this restriction to include caseworkers who assess at least 50 reports produces very similar results (Panel C).

Fourth, in Section 2.3, we noted that some job seekers receive vacancy proposals from the central office caseworkers before submitting their first activity report. These proposals are voluntary and are sent without prior contact with the job seeker. In the main analyses, we exclude these job seekers, but Panel D shows that including them leaves the estimates virtually unchanged. For the IV approach, we additionally residualize caseworker strictness with respect to month-office fixed effects prior to constructing the instrument.

Finally, we implement the UJIVE estimator recommended by Goldsmith-Pinkham et al. (2026). While our baseline caseworker strictness measure already excludes the job seeker's own assessment, UJIVE additionally accounts for the potential bias arising from the interaction between leave-out leniency measures and the fixed effects included in the IV specification. The results are reported in Table A10 and are consistent with the baseline IV estimates, indicating that our findings are not sensitive to how month-office variation is absorbed.

# 6 Heterogeneity by individuals and markets

The consequences of job loss and the effectiveness of labor market policies may differ substantially across job seekers and labor market conditions. Recent evidence shows that earnings losses following job displacement vary systematically with workers' characteristics, industries, and labor market conditions (Athey et al., 2026). The effectiveness of interventions for unemployed job seekers may likewise depend on workers' employment prospects. van den Berg et al. (2025) find that mandatory integration agreements increase employment primarily among job seekers with adverse employment prospects, while Humlum et al. (2025) show that classroom training is particularly effective for workers exposed to adverse labor-demand shocks and persistent employment losses. Motivated by these findings, we examine whether the employment effects of stricter monitoring vary with job seekers' risk of long-term unemployment and with labor market conditions in their industry of origin.

## 6.1 Individual Risk of Long-term Unemployment

We begin by constructing a measure of each job seeker's risk of long-term unemployment. Specifically, we predict the probability of finding a job within 360 days using standard machine-learning methods and information on individual characteristics and labor market history.[31] To avoid overfitting, we train the model on observations excluded during sample selection and evaluate its performance in the final analysis sample.[32] The final specification uses gradient-boosted decision trees and achieves a ROC–AUC of 0.7.[33] We classify job seekers as being at high risk of long-term unemployment if their predicted probability of finding a job within 360 days is below the sample median.

Table 6 shows the estimated effects of caseworker strictness on the probability of finding a job.[34] The estimates suggest different patterns early and later in the unemployment spell, although the differences between the two groups are not statistically significant. Between 60 and 120 days after entering unemployment, the employment effect is larger for job seekers with a relatively low risk of long-term unemployment. Beyond 120 days, this pattern reverses, with a larger estimated effect for job seekers at high risk of long-term unemployment. After 360 days, the effect is statistically insignificant for the low-risk group but remains sizeable and statistically significant for the high-risk group. For the latter, moving from the 10th

[31] We estimate several candidate models (Lasso, ridge regression, random forest and gradient-boosted decision trees) and select the specification with the best out-of-sample predictive performance. The prediction model uses information on age, gender, disability, marriage, level of education, region of birth, prior employment, previous earnings, unemployment and program-participation histories, and search occupation.

[32] The training data include observations excluded because of the caseworker case-load restriction and job seekers who received a vacancy proposal before submitting their first activity report.

[33] The ROC–AUC (area under the receiver operating characteristic curve) measures the model's ability to rank individuals according to their probability of exiting unemployment. A ROC–AUC of 0.7 implies that the model ranks a randomly selected individual who exits unemployment within 360 days above a randomly selected individual who does not in 70 percent of cases. A random classifier would obtain a ROC-AUC of 0.5.

[34] For both groups, we find no impact on employment rates within the first 30 days of unemployment. This period precedes the assessment of the first activity report and is therefore consistent with the results for the full sample.

to the 90th percentile of the caseworker strictness distribution increases the probability of finding a job within 360 days by approximately 1.3 percentage points. This longer-run pattern is consistent with the evidence in van den Berg et al. (2025), who find that mandatory integration agreements are particularly effective for job seekers with relatively adverse employment prospects.

We also re-estimate the effects of caseworker strictness on the monitoring chain (Table A11) and on participation in ALMPs and meetings (Table A12) separately for the two groups. The estimates are similar across groups, indicating that the differences in employment effects are unlikely to be explained by differences in the first-stage effects on report disapproval or in the number of telephone contacts following a disapproved report.

Table 6: Heterogeneous effects by individual risk of long-term unemployment: effects on job-finding

| Outcome: finding a job within X days | | | | | | |
|---|---|---|---|---|---|---|
| | | After assessment of the first report | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | 30 days | 60 days | 90 days | 120 days | 180 days | 360 days |
| Panel a: Low risk | | | | | | |
| CW strictness | 0.015 | 0.152*** | 0.170*** | 0.131** | 0.046 | 0.029 |
| | (0.013) | (0.038) | (0.049) | (0.053) | (0.051) | (0.041) |
| Mean depvar | 0.014 | 0.168 | 0.339 | 0.473 | 0.647 | 0.821 |
| N | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 |
| Panel b: High risk | | | | | | |
| CW strictness | 0.001 | 0.081*** | 0.071 | 0.087* | 0.118** | 0.114** |
| | (0.009) | (0.031) | (0.044) | (0.049) | (0.051) | (0.050) |
| Mean depvar | 0.008 | 0.094 | 0.199 | 0.287 | 0.410 | 0.588 |
| N | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 |
| P-value diff | 0.293 | 0.133 | 0.119 | 0.525 | 0.288 | 0.199 |

Notes: Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. High unemployment risk is defined as below median probability of exiting unemployment within 360 days. Low unemployment risk is defined as above median probability of exiting unemployment within 360 days. Prediction of exiting unemployment within 360 days is based on Gradient Boosted Decision Trees, using the following input variables: age, gender, disability, marriage, level of education, region of birth, and employment history: employed t-180 days,employed t-360 days,employed t-540 days, employed t-720 days, number of days of UE last 5 years, number of programs last 5 years, and number of UE spells last 5 years, previous income, and search occupation. P-value diff shows p-values for tests of differences in coefficients between the high- and low-risk individuals.

The results indicate that stricter monitoring has a larger effect on job seekers with a low long-term unemployment risk in the short run, while the effect appears larger and more persistent for high-risk job seekers at longer horizons. This pattern suggests that intensified monitoring can move individuals with relatively strong labor market attachment back into work quickly. For job seekers with weaker labor market attachment, intensified monitoring does not generate the same short-run increase in job finding, likely reflecting greater barriers to immediate re-employment. However, for this group, stricter monitoring—and the associated increase in job-search intensity—appears to generate more persistent effects on job finding.

## 6.2 Labor Market Conditions

We next examine whether the effect of stricter monitoring depends on labor market conditions, distinguishing between job seekers from low- and high-growth industries. The direction of this heterogeneity is theoretically ambiguous. On the one hand, stricter monitoring may be more effective for individuals from high-growth industries, where a larger number of vacancies makes it easier to translate additional search effort into employment. On the other hand, job seekers from growing industries may find jobs relatively quickly even in the absence of intensive monitoring, leaving less scope for stricter monitoring to affect their employment outcomes. Conversely, workers from low-growth industries may face greater barriers to re-employment but may also benefit more from a sustained increase in search effort.

To study this, we use matched employer–employee data and define industry growth as the change in employment between $t-5$ and $t+5$, where $t$ denotes the year in which the first activity report is assessed. Industries are classified as low or high growth depending on whether their growth rate is below or above the sample median.[35] We measure growth at the two-digit industry level, but obtain similar results using alternative industry classifications. Job seekers are assigned to industries based on their last job prior to entering unemployment. The resulting groups should therefore be interpreted as being defined by industry of origin, since some job seekers may already have redirected their search toward other industries by the time the first report is assessed.

The results are reported in Table 7. For job seekers from high-growth industries, stricter monitoring substantially increases job finding in the short run, but the estimated effects dissipate after around 120 days. In contrast, for those from low-growth industries, the initial effects are smaller but persist over a longer horizon and remain statistically significant after 360 days.[36] These findings are robust to

[35] The majority of the unemployed workers in our sample have been working in industries with a positive growth rate, but around 20% come from industries in which employment declined over the relevant period.

[36] Table A13 and Table A14 show that the effects on the monitoring chain, ALPMs, and meetings are similar across the two groups, so that these factors cannot explain the heterogeneity by industry

alternative measure of industry growth (Table A15). Overall, the results indicate that the employment effects of stricter monitoring are more persistent for job seekers originating from low-growth industries, although the differences are not statistically significant. Stricter enforcement of job-search rules may therefore be particularly important for preventing prolonged unemployment among workers from industries with weaker employment growth. This pattern is consistent with the evidence in Humlum et al. (2025), who find that workers exposed to offshoring benefit particularly strongly from classroom training. Although the intervention differs from ours, both sets of results suggest that labor market policies may generate more persistent gains for workers facing adverse labor-demand conditions.

Table 7: Heterogeneous effects for low- and high growth industries: effects on job-finding

| Outcome: finding a job within X days | | | | | | |
|---|---|---|---|---|---|---|
| | | After assessment of the first report | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | 30 days | 60 days | 90 days | 120 days | 180 days | 360 days |
| Panel a: Low Growth Industries | | | | | | |
| CW strictness | 0.012 | 0.077** | 0.115** | 0.135** | 0.114** | 0.109** |
| | (0.012) | (0.036) | (0.048) | (0.053) | (0.054) | (0.046) |
| Mean depvar | 0.011 | 0.132 | 0.266 | 0.377 | 0.524 | 0.705 |
| N | 53,021 | 53,021 | 53,021 | 53,021 | 53,021 | 53,021 |
| Panel b: High Growth Industries | | | | | | |
| CW strictness | 0.003 | 0.151*** | 0.129*** | 0.092* | 0.054 | 0.036 |
| | (0.010) | (0.037) | (0.049) | (0.051) | (0.053) | (0.046) |
| Mean depvar | 0.011 | 0.132 | 0.275 | 0.388 | 0.537 | 0.709 |
| N | 55,515 | 55,515 | 55,515 | 55,515 | 55,515 | 55,515 |
| P-value diff | 0.556 | 0.130 | 0.829 | 0.538 | 0.410 | 0.238 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. Industry growth rate is defined for 2-digit industry codes, and measures employment change between year t-5 and t+5. P-value diff shows p-values for tests of differences in coefficients between the high- and low-growth industries.

# 7 Conclusions

This paper provides new evidence on how the strictness with which job-search requirements are enforced affects the behavior and labor market outcomes of unemployed job seekers. We exploit the quasi-random assignment of monthly activity

growth.

reports to central-office caseworkers who differ in their propensity to disapprove reports. Because these caseworkers have no direct contact with job seekers and are not involved in counseling or program assignment, the setting allows us to isolate variation in the enforcement of existing job-search requirements from broader differences in caseworker practices.

We find that assignment to a stricter caseworker initiates an entire monitoring chain. It increases the probability that an activity report is disapproved, that a job-search violation is registered, and that the job seeker receives a warning or benefit sanction. Stricter monitoring subsequently raises reported job-search intensity, shortens unemployment durations, and increases employment rates. The increase in job applications appears immediately after the first report is assessed and persists for several months, including after responsibility for reviewing reports has been transferred from central to local offices. We also observe additional telephone and face-to-face contact with local-office caseworkers following stricter assessments. Thus, the effects we estimate reflect the combined consequences of a more credible monitoring threat, the possibility of sanctions, and the supportive follow-up that is triggered by a disapproved report. The employment gains do not appear to come at the cost of lower job quality. Earnings rise following the assessment of the first report, and we find no evidence of lower wages in the first job or negative effects on longer-run earnings.

The effects also differ in their timing across job seekers and labor market conditions. For individuals with relatively strong employment prospects and for workers coming from high-growth industries, the estimated employment effects are concentrated in the short run. For job seekers with weaker labor market attachment and those coming from low-growth industries, the effects emerge more gradually but are more persistent. These findings suggest that stricter monitoring plays a crucial role in preventing prolonged unemployment spells, especially among workers facing greater barriers to re-employment.

From a policy perspective, our results show that the enforcement of job-search requirements can be an effective tool for improving employment outcomes. However, the heterogeneity in effects indicates that a "one-size-fits-all" approach may not be optimal. Instead, monitoring policies could be designed to balance strict enforcement with targeted support, focusing particularly on job seekers at greater risk of long-term unemployment, such as workers from low-growth industries or those with weak labor market attachment. Strengthening monitoring in such contexts may yield the greatest returns in terms of reducing unemployment durations and preventing labor market detachment. At the same time, policymakers should be attentive to potential costs associated with stricter enforcement, including administrative burden and the risk of disproportionate hardship for vulnerable job seekers.

Overall, our findings suggest that well-designed monitoring systems, which combine credible enforcement with supportive follow-up, can improve labor market outcomes and enhance the effectiveness of unemployment insurance systems.

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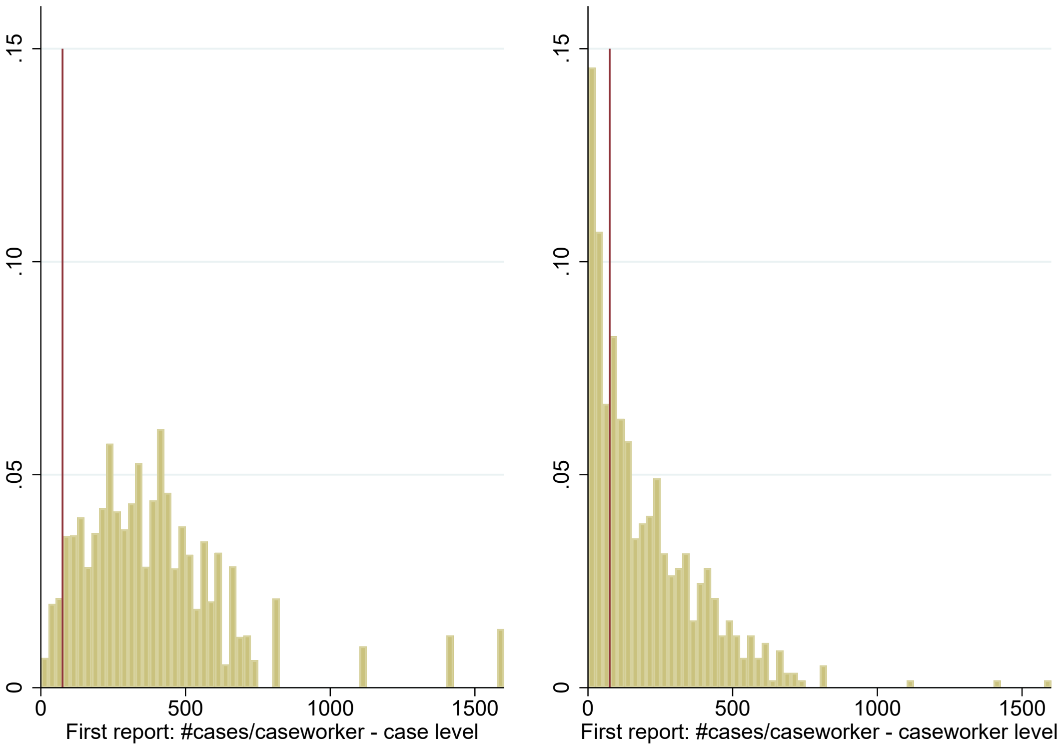
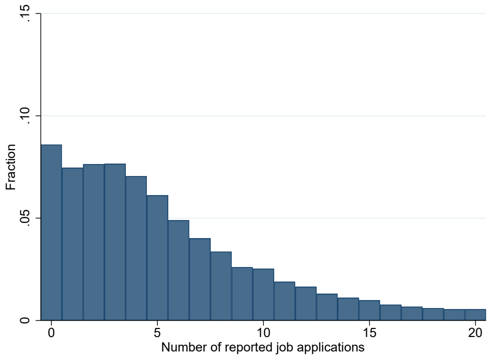
# Online Appendix A: Additional tables and figures

Figure A1: Number of reviewed reports by central-office caseworkers

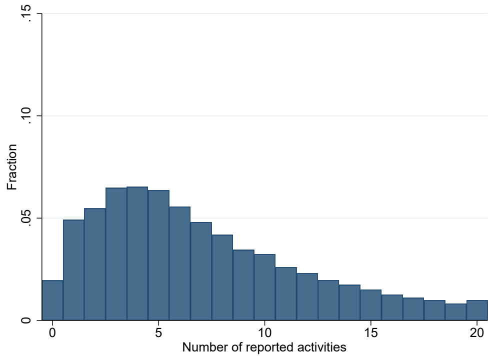



*Notes*: Distribution of number of cases reviewed by each caseworker, measured at the case level respectively the caseworker level. Number of cases corresponds to the number of unemployed handing in a first activity report during our observation period. The vertical lines indicate the threshold of 75 cases per caseworker. In the full sample there are 115,217 cases and 570 caseworkers. After restricting to caseworkers with at least 75 cases, we end up with 109,866 cases and 390 caseworkers.

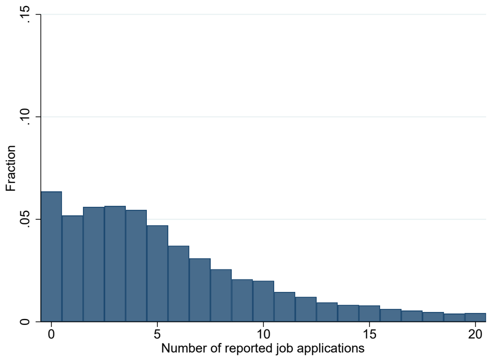
Figure A2: Number of job applications and reported activities in the second and third report



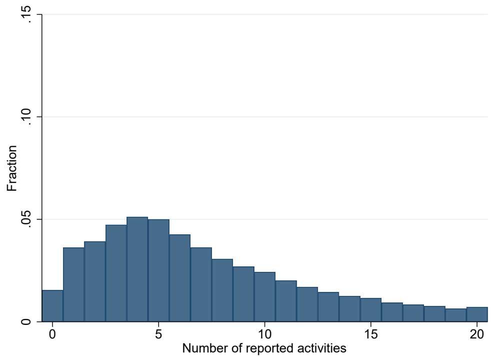

Panel A: Job Applications Report 2




Panel B: Total Activities Report 2



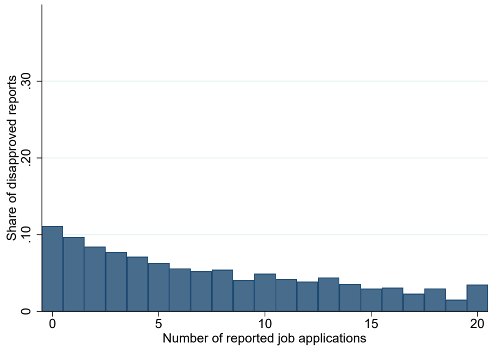

Panel C: Job Applications Report 3



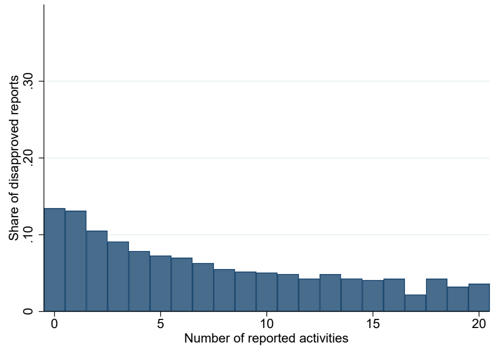

Panel D: Total Activities Report 3

*Notes*: Statistics for second (N= 83,123) and third (N=62,422) report for the analysis sample of described in Section 3. 390 caseworkers. Job applications include standard applications, unsolicited applications, and applications for vacancy referrals or proposals. Total activities additionally include interviews, recruitment meetings, other activities, and activities specified in the action plan.

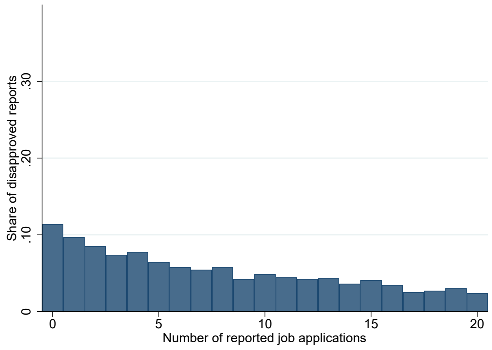
Figure A3: Share disapproved reports, by number of reported job applications and activities



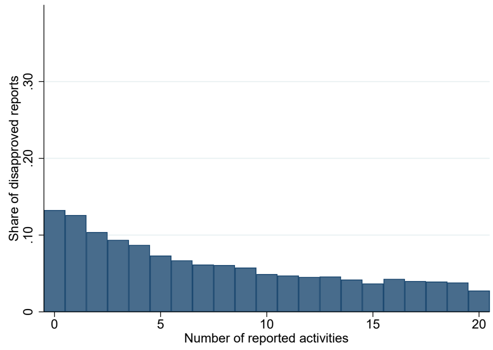

Panel A: Job Applications Report 2




Panel B: Total Activities Report 2



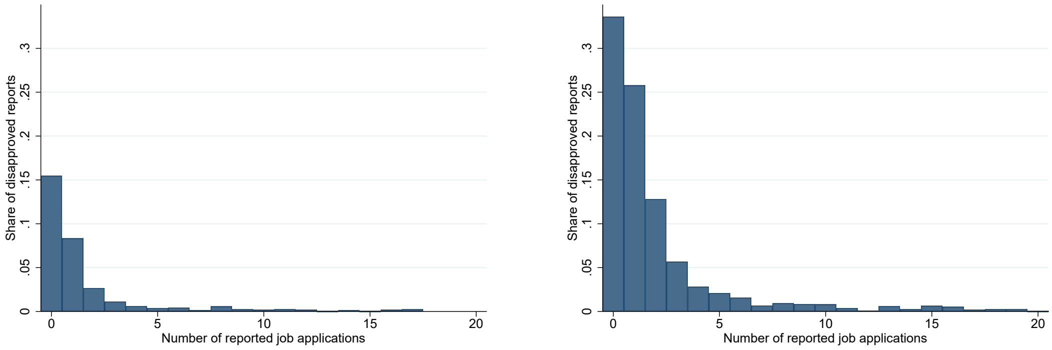

Panel C: Job Applications Report 3




Panel D: Total Activities Report 3

*Notes*: Statistics for second (N= 83,123) and third (N=62,422) report for the analysis sample of described in Section 3. 390 caseworkers. Job applications include standard applications, unsolicited applications, and applications for vacancy referrals or proposals. Total activities additionally include interviews, recruitment meetings, other activities, and activities specified in the action plan.

Figure A4: Share disapproved reports, by caseworker strictness and number of reported job applications




Panel A: Low Caseworker Strictness Panel B: High Caseworker Strictness

*Notes*: Share of disapproved activity reports by low and high caseworker strictness. Low strictness is defined as below median caseworker strictness. High strictness is defined as above median caseworker strictness. Median strictness is 5.8% disapproved activity reports. 109,866 cases and 390 caseworkers.

Table A1: Testing for random assignment of unemployed to caseworkers

| | Left UE Within 90 days | Disapproved Report | Caseworker Strictness |
|---|---|---|---|
| Age | -0.004*** | 0.001*** | -0.000 |
| | (0.000) | (0.000) | (0.000) |
| Male | -0.054*** | 0.011*** | 0.000 |
| | (0.003) | (0.002) | (0.000) |
| Disabled | -0.191*** | 0.037*** | 0.001 |
| | (0.009) | (0.007) | (0.001) |
| Married | -0.026*** | -0.000 | -0.000* |
| | (0.003) | (0.002) | (0.000) |
| *Education level* | | | |
| Less than high school | -0.059*** | 0.033*** | 0.001** |
| | (0.005) | (0.003) | (0.000) |
| High school | 0.011*** | 0.017*** | 0.000 |
| | (0.003) | (0.002) | (0.000) |
| College | 0.018*** | -0.030*** | -0.000 |
| | (0.004) | (0.002) | (0.000) |
| *Region of birth* | | | |
| Sweden | 0.080*** | -0.009*** | -0.000 |
| | (0.003) | (0.002) | (0.000) |
| Nordic countries | -0.002 | 0.013** | -0.001 |
| | (0.009) | (0.005) | (0.001) |
| EU | -0.039*** | -0.010*** | -0.000 |
| | (0.006) | (0.003) | (0.001) |
| Outside of EU | -0.089*** | 0.012*** | 0.000 |
| | (0.004) | (0.002) | (0.000) |
| *Labor market history* | | | |
| Employed t-180 days | 0.028*** | 0.003 | -0.000 |
| | (0.005) | (0.003) | (0.000) |
| Employed t-360 days | -0.006 | -0.003 | 0.000 |
| | (0.006) | (0.002) | (0.000) |
| Employed t-540 days | 0.047*** | -0.001 | 0.000 |
| | (0.004) | (0.002) | (0.000) |
| Employed t-720 days | 0.018*** | -0.004** | 0.000 |
| | (0.005) | (0.002) | (0.000) |
| Number of days UE last 5y | -0.000*** | 0.000*** | -0.000 |
| | (0.000) | (0.000) | (0.000) |
| Number of programs last 5y | 0.012*** | 0.000 | -0.000 |
| | (0.004) | (0.002) | (0.000) |
| Number of UE spells last 5y | 0.015*** | 0.000 | -0.000 |
| | (0.002) | (0.001) | (0.000) |
| Predicted employment within 90 days | 0.957*** | -0.131*** | -0.002 |
| | (0.023) | (0.009) | (0.001) |
| F-test | 144*** | 28*** | 1 |
| Mean depvar | 0.285 | 0.069 | 0.069 |
| N | 109,865 | 109,865 | 109,865 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. Each line correspond to a single regression with one predetermined job seeker characteristic as independent variable. The dependent variable is in column (1) indicator for having left unemployment within 90 days, column (2) indicator for receiving a disapproved first activity report, and column (3) caseworker strictness. The F-statistic for the joint significance of job seeker characteristics is reported at the bottom of the table. The F-test includes all variables except the predicted probability of exiting unemployment within 90 days.

Table A2: Caseworker strictness, timing of decisions and content of the first report

| | Number of job applications | Number of activities | Time until unemployed hands in the report | Time untill CW checks the report |
|---|---|---|---|---|
| CW strictness | 0.015 | -0.159 | -0.449 | 1.634** |
| | (0.593) | (0.715) | (1.432) | (0.693) |
| | | | | |
| Mean depvar | 6.02 | 8.96 | 33.06 | 3.00 |
| N | 109,865 | 109,865 | 109,865 | 109,836 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. The time is measured in days and refers to (1) the time between the entry into unemployment and the day at which the unemployed hands in the first activity report, and (2) the time between the days the unemployed hands in the report and the moment the caseworker evaluates it.

Table A3: Monotonicity check: job seeker characteristics

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Male | Female | Married | Non Married | $< 40$yrs | $\geq 40$yrs |
| CW strict | 0.911*** | 0.769*** | 0.860*** | 0.829*** | 0.771*** | 0.910*** |
| | (0.037) | (0.035) | (0.041) | (0.033) | (0.035) | (0.037) |
| Mean strict | 0.075 | 0.063 | 0.069 | 0.069 | 0.063 | 0.075 |
| N | 56,732 | 53,134 | 41,431 | 68,435 | 54,373 | 55,433 |
| | (7) | (8) | (9) | (10) | (11) | (12) |
| | College | High-school or less | Individual Low risk | Individual High risk | Industry Low growth | Industry High growth |
| CW strict | 0.612*** | 0.998*** | 0.803*** | 0.880*** | 0.842*** | 0.830*** |
| | (0.034) | (0.037) | (0.036) | (0.034) | (0.035) | (0.038) |
| Mean strict | 0.052 | 0.081 | 0.061 | 0.076 | 0.068 | 0.069 |
| N | 44,592 | 64,588 | 54,933 | 54,933 | 53,021 | 55,515 |

*Notes:* First stage regressions of caseworker strictness on the probability of getting a disapproved activity report within subgroups based on predetermined job seeker characteristics. Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects.

Table A4: Monotonicity check: across subgroups

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Male | Female | Married | Non Married | $< 40$yrs | $\geq 40$yrs |
| CW strict | 0.823*** | 0.622*** | 0.832*** | 0.695*** | 0.629*** | 0.842*** |
| | (0.036) | (0.032) | (0.042) | (0.030) | (0.031) | 0.039) |
| N | 56,732 | 53,134 | 41,431 | 68,435 | 54,433 | 55,493 |
| | (7) | (8) | (9) | (10) | (11) | (12) |
| | College | High-school or less | Individual Low risk | Individual High risk | Industry Low growth | Industry High growth |
| CW strict | 0.456*** | 0.936*** | 0.697*** | 0.828*** | 0.758*** | 0.752*** |
| | (0.027) | (0.044) | (0.034) | (0.036) | (0.034) | (0.035) |
| N | 65,274 | 45,278 | 54,933 | 54,933 | 55,515 | 53,021 |

*Notes:* First stage regressions of caseworker strictness in subgroup (A), defined as the share of disapproved activity reports within the subgroup, on the probability of getting a disapproved activity report within subgroup (B). E.g. In column (1) the dependent variable is getting a disapproved activity report for males, with strictness among females as the independent variable. Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects.

Table A5: Complier Characteristics

| | Sample Mean | Complier Mean | N |
|---|---|---|---|
| Male | 0.516 | 0.551 | 109,866 |
| | | (0.022) | |
| Married | 0.377 | 0.419** | 109,866 |
| | | (0.021) | |
| Age | 41 | 42*** | 109,866 |
| | | (0.463) | |
| Born in Sweden | 0.759 | 0.777 | 109,866 |
| | | (0.019) | |
| Number of days UE last 5y | 284 | 318** | 109,866 |
| | | (15) | |
| Number of programs last 5y | 0.081 | 0.087 | 109,866 |
| | | (0.016) | |
| Number of UE spells last 5y | 1.359 | 1.609*** | 109,866 |
| | | (0.076) | |
| High individual unemployment risk | 0.500 | 0.509 | 109,866 |
| | | (0.022) | |
| High industry growth | 0.511 | 0.511 | 108,536 |
| | | (0.021) | |
| Number of job applications report 1 | 6.022 | 1.479*** | 109,866 |
| | | (0.337) | |
| Number of total activities report 1 | 8.959 | 3.382*** | 109,866 |
| | | (0.423) | |

*Notes:* Bootstrap standard errors are reported for the complier group. ***, **, * indicate significant difference in means at significance levels 1%, 5% and 10% respectively. We control for month-agency fixed effects. Complier means are constructed following Goldsmith-Pinkham et al. (2026). First, the means among treated compliers are recovered by regressing the interaction of each covariate $x_i$ with the treatment receiving a disapproved report $D_i$ on the treatment instrumented by caseworker strictness $Z_{j(i)}$ - obtaining a weighted average of the covariate using the same weights as in the main IV specification. Second, to obtain the weighted average among non-treated compliers, the treatment variable is exchanged for $1 - D_i$. The two specifications are pooled using $\tilde{D}_i = 2D_i - 1$ as the treatment indicator. I.e. regressing $x_i \times \tilde{D}_i$ on $\tilde{D}_i$ instrumented by caseworkers strictness, and controlling for month-agency fixed effects.

Table A6: Effects of caseworker strictness on employment status

| | (1) 30 days | (2) 90 days | (3) 180 days | (4) 360 days | (5) 540 days | (6) 720 days |
|---|---|---|---|---|---|---|
| CW strictness | 0.004 | 0.135*** | 0.097** | 0.107*** | 0.105*** | 0.047 |
| | (0.008) | (0.037) | (0.040) | (0.033) | (0.031) | (0.030) |
| | | | | | | |
| Mean depvar | 0.010 | 0.283 | 0.549 | 0.710 | 0.780 | 0.813 |
| N | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects.

Table A7: Robustness analyses for dynamic sample selection

| | (1) | (2) | (3) |
|---|---|---|---|
| | All reports | Sample with two reports | |
| | Report 1 | Report 1 | Report 2 |
| CW Strictness | 0.015 | 0.809 | 3.241*** |
| | (0.593) | (0.724) | (0.657) |
| Mean depvar | 6.022 | 6.133 | 6.625 |
| N | 109,865 | 83,122 | 83,122 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. Column 1 reports estimates for the full sample of job seekers, and Columns 2–3 restricts the sample to those who submitted the second report.

Table A8: Effects of caseworker strictness and probability of leaving unemployment within end of second report period

| | (1) Reduced form | (2) IV estimates |
|---|---|---|
| Panel a: Exits from unemployment | | |
| CW Strictness | 0.093*** | |
| | (0.022) | |
| Disapproved report | | 0.110*** |
| | | (0.026) |
| Mean depvar | 0.098 | 0.098 |
| N | 109,866 | 109,866 |
| Panel b: Job-search intensity | | |
| CW Strictness | 3.242*** | |
| | (0.656) | |
| Disapproved report | | 3.893*** |
| | | (0.805) |
| Mean depvar | 6.625 | 6.625 |
| N | 83,123 | 83,123 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. In the IV estimation the dependent variable receiving a disapproved first report is instrumented by caseworker strictness from the first report. The outcome is measured on the inferred last day of the second reporting period—specifically, the last day of the month after the first reporting period ends. The number of days from the start of the unemployment spell to this inferred last day of the second reporting period ranges from 40 to 100 days, with an average of 56 days.

Table A9: Robustness analyses

| | (1) 30 days | (2) 60 days | (3) 90 days | (4) 120 days | (5) 180 days | (6) 360 days |
|---|---|---|---|---|---|---|
| Panel A: Day-agency fixed effects | | | | | | |
| CW strictness | 0.008 | 0.115*** | 0.121*** | 0.111*** | 0.088** | 0.072** |
| | (0.008) | (0.026) | (0.033) | (0.036) | (0.036) | (0.033) |
| Panel B: Controlling for observable jobseeker characteristics | | | | | | |
| CW strictness | 0.008 | 0.116*** | 0.117*** | 0.107*** | 0.082** | 0.072** |
| | (0.009) | (0.027) | (0.035) | (0.038) | (0.038) | (0.033) |
| Mean depvar | 0.011 | 0.131 | 0.269 | 0.380 | 0.528 | 0.704 |
| N | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 | 109,866 |
| Panel C: Caseload min 50 cases | | | | | | |
| CW strictness | 0.007 | 0.115*** | 0.120*** | 0.109*** | 0.087** | 0.086** |
| | (0.008) | (0.025) | (0.034) | (0.037) | (0.038) | (0.033) |
| Mean depvar | 0.011 | 0.131 | 0.269 | 0.380 | 0.528 | 0.704 |
| N | 112,290 | 112,290 | 112,290 | 112,290 | 112,290 | 112,290 |
| Panel D: Including JS with vacancy proposals | | | | | | |
| CW strictness | 0.006 | 0.119*** | 0.120*** | 0.120*** | 0.103*** | 0.069** |
| | (0.007) | (0.023) | (0.031) | (0.034) | (0.033) | (0.029) |
| Mean depvar | 0.011 | 0.127 | 0.264 | 0.376 | 0.524 | 0.702 |
| N | 147,137 | 147,137 | 147,137 | 147,137 | 147,137 | 147,137 |

*Notes:* ***, **, * indicate significance at 1%, 5% and 10% respectively. Panel A: control for day-agency fixed effects. Standard errors are clustered at the day-agency level and reported in parentheses. Panel B: Control for month-agency fixed effects and observable characteristics: Level of education (compulsory school, high-school, college), male, immigrant, married, divorced, disability, days unemployed last 2 years, days unemployed last 5 years, number of unemployment spells last 2 years, number of unemployment spells last 5 years, number of programs last 5 years. Standard errors are clustered at the month-agency level and reported in parentheses. Panel C: Include all caseworkers with at least 50 cases, controlling for month-agency fixed effects. Standard errors are clustered at the month-agency level and reported in parentheses. Panel D: Include both job seekers who did and did not receive any vacancy proposal from a central office caseworker prior to submitting the first activity report. Controlling for month-agency fixed effects. Standard errors are clustered at the month-agency level and reported in parentheses.

Table A10: Robustness: UJIVE estimates of short-run effects

| | (1) Reduced form | (2) IV estimates |
|---|---|---|
| Panel a: Exits from unemployment | | |
| CW Strictness | 0.097*** | 0.110*** |
| | (0.023) | (0.026) |
| Mean depvar | 0.098 | 0.098 |
| N | 109,866 | 109,866 |
| Panel b: Job-search intensity | | |
| CW Strictness | 3.481*** | 3.973*** |
| | (0.670) | (0.784) |
| Mean depvar | 6.625 | 6.625 |
| N | 83,123 | 83,123 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. In the IV estimation the dependent variable receiving a disapproved first report is instrumented by caseworker strictness from the first report. Residual caseworker strictness is demeaned by month times agency fixed effects. The outcome is measured on the inferred last day of the second reporting period—specifically, the last day of the month after the first reporting period ends. The number of days from the start of the unemployment spell to this inferred last day of the second reporting period ranges from 40 to 100 days, with an average of 56 days.

Table A11: Heterogeneous effects by individual risk of long-term unemployment: monitoring chain

| | (1) Disapproved First Report | (2) Violation Job Seach | (3) Violation Other | (4) Notification Sent to UI | (5) Warning/ Sanction |
|---|---|---|---|---|---|
| Panel a: Low risk | | | | | |
| CW strictness | 0.803*** | 0.195*** | 0.020 | 0.146*** | 0.116*** |
| | (0.036) | (0.018) | (0.030) | (0.031) | (0.026) |
| Mean depvar | 0.061 | 0.020 | 0.108 | 0.111 | 0.064 |
| N | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 |
| Panel b: High risk | | | | | |
| CW strictness | 0.880*** | 0.222*** | -0.053 | 0.136*** | 0.103*** |
| | (0.034) | (0.020) | (0.034) | (0.037) | (0.027) |
| Mean depvar | 0.076 | 0.025 | 0.137 | 0.137 | 0.081 |
| N | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 |

Notes: Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. High unemployment risk is defined as below median probability of exiting unemployment within 360 days. Low unemployment risk is defined as above median probability of exiting unemployment within 360 days. Prediction of exiting unemployment within 360 days is based on Gradient Boosted Decision Trees, using the following input variables: age, gender, disability, marriage, level of education, region of birth, and employment history: employed t-180 days,employed t-360 days,employed t-540 days, employed t-720 days, number of days of UE last 5 years, number of programs last 5 years, and number of UE spells last 5 years, previous income, and search occupation. P-values for tests of differences in coefficients between the high- and low-risk individuals (by column): (1) p = 0.067, (2) p = 0.238, (3) p = 0.087, (4) p = 0.818, (5) p = 0.713 .

Table A12: Heterogeneous effects by individual risk of long-term unemployment: labor market programs and meetings

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Labor market programs | | | | Meetings | |
| | Any Program | Work Practice | Vocational Training | Preparatory Training | Face to Face Meetings | Phone Meetings |
| Panel a: Low risk | | | | | | |
| CW strictness | -0.000 | 0.002 | -0.000 | -0.002 | 0.159 | 1.171*** |
| | (0.013) | (0.006) | (0.007) | (0.006) | (0.116) | (0.132) |
| Mean depvar | 0.012 | 0.003 | 0.005 | 0.004 | 1.793 | 1.051 |
| N | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 |
| Panel b: High risk | | | | | | |
| CW strictness | -0.005 | -0.010 | 0.005 | 0.001 | 0.140 | 1.278*** |
| | (0.016) | (0.007) | (0.008) | (0.009) | (0.129) | (0.130) |
| Mean depvar | 0.018 | 0.005 | 0.005 | 0.007 | 2.041 | 1.082 |
| N | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 | 54,933 |

Notes: Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. High unemployment risk is defined as below median probability of exiting unemployment within 360 days. Low unemployment risk is defined as above median probability of exiting unemployment within 360 days. Prediction of exiting unemployment within 360 days is based on Gradient Boosted Decision Trees, using the following input variables: age, gender, disability, marriage, level of education, region of birth, and employment history: employed t-180 days,employed t-360 days,employed t-540 days, employed t-720 days, number of days of UE last 5 years, number of programs last 5 years, and number of UE spells last 5 years, previous income, and search occupation. P-values for tests of differences in coefficients between the high- and low-risk individuals (by column): (1) p = 0.823, (2) p = 0.193, (3) p = 0.614, (4) p = 0.826, (5) p = 0.911, (6) p=0.544 .

Table A13: Heterogeneous effects by industry growth: monitoring chain

| | (1) Disapproved First Report | (2) Violation Job Seach | (3) Violation Other | (4) Notification Sent to UI | (5) Warning/ Sanction |
|---|---|---|---|---|---|
| Panel a: Low growth industries | | | | | |
| CW strictness | 0.842*** | 0.211*** | 0.016 | 0.166*** | 0.109*** |
| | (0.035) | (0.019) | (0.034) | (0.034) | (0.027) |
| Mean depvar | 0.068 | 0.022 | 0.117 | 0.119 | 0.069 |
| N | 53,021 | 53,021 | 53,021 | 53,021 | 53,021 |
| Panel b: High growth industries | | | | | |
| CW strictness | 0.830*** | 0.205*** | -0.067* | 0.106*** | 0.102*** |
| | (0.038) | (0.021) | (0.034) | (0.038) | (0.028) |
| Mean depvar | 0.069 | 0.022 | 0.127 | 0.129 | 0.076 |
| N | 55,515 | 55,515 | 55,515 | 55,515 | 55,515 |

Notes: Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. Industry growth rate is defined for 2-digit industry codes, and measures employment change between year t-5 and t+5. P-values for tests of differences in coefficients between the high- and low-risk individuals (by column): (1) p = 0.796, (2) p = 0.828, (3) p = 0.098, (4) p = 0.243, (5) p = 0.850 .

Table A14: Heterogeneous effects by industry growth: labor market programs and meetings

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Labor market programs | | | | Meetings | |
| | Any Program | Work Practice | Vocational Training | Preparatory Training | Face to Face Meetings | Phone Meetings |
| Panel a: Low growth industries | | | | | | |
| CW strictness | 0.008 | -0.002 | 0.009 | 0.000 | 0.234** | 1.210*** |
| | (0.016) | (0.007) | (0.008) | (0.009) | (0.118) | (0.126) |
| Mean depvar | 0.015 | 0.004 | 0.006 | 0.005 | 1.867 | 1.035 |
| N | 53,021 | 53,021 | 53,021 | 53,021 | 53,021 | 53,021 |
| Panel b: High growth industries | | | | | | |
| CW strictness | -0.016 | -0.007 | -0.005 | -0.003 | 0.089 | 1.231*** |
| | (0.013) | (0.006) | (0.006) | (0.007) | (0.128) | (0.125) |
| Mean depvar | 0.014 | 0.004 | 0.004 | 0.005 | 1.960 | 1.099 |
| N | 55,515 | 55,515 | 55,515 | 55,515 | 55,515 | 55,515 |

Notes: Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects. Industry growth rate is defined for 2-digit industry codes, and measures employment change between year t-5 and t+5. P-values for tests of differences in coefficients between the high- and low-risk individuals (by column): (1) p = 0.236, (2) p = 0.596, (3) p = 0.160, (4) p = 0.724, (5) p = 0.397, (6) p=0.894 .

Table A15: Robustness analyses and the measurement of industry growth: Employment 90 days after unemployment entry

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Growth t-5, t+5 | | Growth t-5, t | |
| | 2-digit code | 3-digit code | 2-digit code | 3-digit code |
| Panel a: Low growth industries | | | | |
| CW strictness | 0.115** | 0.127** | 0.161*** | 0.120** |
| | (0.048) | (0.049) | (0.048) | (0.048) |
| Mean depvar | 0.266 | 0.268 | 0.260 | 0.266 |
| N | 53,021 | 53,666 | 53,857 | 53,747 |
| Panel b: High growth industries | | | | |
| CW strictness | 0.129*** | 0.111** | 0.072 | 0.116** |
| | (0.049) | (0.046) | (0.044) | (0.047) |
| Mean depvar | 0.275 | 0.273 | 0.281 | 0.275 |
| N | 55,515 | 54,863 | 54,679 | 54,782 |

*Notes:* Standard errors are clustered at the month-agency level and reported in parentheses. ***, **, * indicate significance at 1%, 5% and 10% respectively. We control for month-agency fixed effects.

# Online Appendix B: English translation of the monthly activity report submitted to the Swedish Public Employment Service

ARBETSFÖRMEDLINGEN
SWEDISH PUBLIC EMPLOYMENT SERVICE

Sida 1 (2)

Send the form to:

Arbetsförmedlingen
107 67 Stockholm

## Activity report

**Name and personal identity or co-ordination number**

**Personal identity (YYYYMMDD-XXXX)**
**Co-ordination number (YYMMDD-XXXX)**

| First name | Surname |
|---|---|
| | |

**Activity report for**

Month Year

Exemple:
01 = January

Fill in your applied jobs and completed activities during the previous month or period.

**Have you applied for an advertised job vacancy?**

Please give information about if and when you have applied for job vacancies that have been advertised in a newspaper, Platsbanken or on other internet sites.

MB0001381

Af 00332 6.0

| Date | What job have you applied for? | Employer | Town/City |
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**Have you expressed interest in a job that was not advertised?**

Please give information about if and when you have expressed interest or handed in your CV to an employer.

| Date | What job did you express interest in | Employer | Town/City |
|---|---|---|---|
| | | | |
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**Have you been to a job interview?**

Please give information if you have been to a job interview, and the date of the interview.

| Date | What job was the interview for? | Employer | Town/City |
|---|---|---|---|
| | | | |
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**Have you done anything else to increase your chances of getting employment?**

Please give information about any activities that you have carried out or participated in to increase your chances of getting employment, for example improved your application documents, applied for a training course, or been to a recruitment event. Please also enter the date of the activity.

| Date | Description of activity |
|---|---|
| | |
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| | |

**Do you not have anything to report?** Check the box if you do not have anything to report.

☐ I do not have any job applications, interviews or activities.

**Signature**

| Date | Signature |
|---|---|
| Name in block letters | |