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ABSTRACT

Strengthening Enforcement in Unemployment Insurance: A Natural Experiment*

Enforcing the compliance with job search obligations is a core task of conditional benefit systems like unemployment insurance (UI) or welfare. For targeted policy design, it is key to understand how the enforcement regime affects job search outcomes. This paper provides first estimates that separately identify the effects of increasing enforcement strictness in UI. As a natural experiment, we exploit a reform which induced a sharp and unanticipated increase in the probability of being sanctioned after the failure to document job search effort. Using a difference-in-differences design, we find that the probability of job finding within six months increases by 6 percentage points in response to the policy change. This effect comes at the cost of lower job stability. As a consequence, early job finders experience losses in total earnings driven by fewer months in employment within the considered post-unemployment period. We use these estimates to quantify the elasticities to changes in enforcement strictness, trading off the short-run gains (job finding) against the mid-run costs (job quality).

JEL Classification: J64, J65, J68

Keywords: unemployment insurance, job search, natural experiment, enforcement

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1 Introduction

Modern unemployment insurance (UI) and welfare regimes are designed as conditional benefit systems, which actively support the job search process but also require compliance with rules and obligations. In the U.S. as well as in Europe, monitoring and enforcing the compliance with job search obligations has become a popular tool to credibly impose the conditionality of benefit receipt. The key choice variables for designing such a regime are the monitoring intensity and the probability of enforcement. Both parameters vary largely across OECD countries (Venn 2012). However, empirical studies which separately identify the causal elasticities of these policy parameters with respect to labor market outcomes are still broadly missing. As a primary reason, policy-driven changes in the enforcement strictness tend to come along with other reforms in the UI regime, making it impossible to isolate the effect of one single policy parameter.

This paper addresses this gap by estimating how an unanticipated change in enforcement strictness affects the outcomes of job seekers who were detected not complying. Our analysis comprises both job finding and post-unemployment outcomes. It offers for the first time, to our knowledge, *quasi-experimental* evidence on the effects of varying the design of enforcement regimes. We exploit a natural experiment in the Swiss UI which sharply increased enforcement strictness while keeping job search obligations and the monitoring intensity constant.

Enforcement strictness, i.e., the probability that a detected non-compliance leads to a temporary benefit cut, is a key ingredient for precise policy design. UI authorities can directly target a certain level of enforcement intensity. If the only aim is to reduce moral hazard, there is a rather unambiguous incentive to choose a strict enforcement regime. However, the strictness of enforcement can have important effects on individual job search and job acceptance behavior, as predicted by standard job search theory: by increasing the threat and incidence of benefit sanctions, enforcement is expected to lower the value of being unemployed. It thereby increases the speed of job finding and lowers the reservation value. This study directly estimates the effect of a change in the enforcement parameter as represented in job search models.

For identification, we use variation from an unexpected policy change in the Swiss UI, which affected job seekers who had failed to deliver a list of their monthly applications by the official deadline. The change did not explicitly aim at sharpening the enforcement regime, as the intention was to reduce the administrative burden faced by the local authorities.[1] Nevertheless, it substantially affected the way a non-compliant job seeker was treated by the enforcement regime: before the reform, the job seeker would receive a rather "mild" notification, defining a second deadline until which the documentation of search effort could be re-submitted. The reform abolished this practice and turned to a "no excuse" policy. A detected job seeker now had to expect a benefit cut with a high degree of certainty, in case she had no special reason or circumstance that excused the non-compliance. Due to its unintended nature and sudden implementation, the reform generated

[1] Source: own inquiries at the federal UI authorities.

a sharp quasi-experimental jump in the probability of being sanctioned in case of non-compliance detection (from around 30% to 70%). As a natural control group, we use job seekers detected with a different type of non-compliance, such as the provision of too few applications or the failure to show up at a caseworker meeting.[2] While this group is similarly affected by aggregate conditions and has similar characteristics as the treatment group, its enforcement rules stayed constant. We can set up a difference-in-differences framework to evaluate the effect of a strict versus mild intensity of enforcement, *given* a detected non-compliance. Importantly, we show that the reform did not involve any anticipatory behavior in the short run, as the share of non-compliance detections for both the treatment and the control group stayed constant. In addition, the reform was not associated with any differences in observable characteristics between treated and non-treated job seekers.

The main analysis is carried out using administrative data on the population of job seekers entering the Swiss UI during the year 2011. It focuses on job seekers with non-compliance detections close to the reform date (April 2011). We find substantial effects of the increase in enforcement strictness on job finding. For instance, the probability that the job seeker finds a job within 6 months increased by 6 percentage points. However, the positive job finding effects are predominantly driven by unstable job matches, which result in the recurrence to unemployment within 12 months after the exit from unemployment. On the contrary, the probability of accepting a stable job does not react. The results thus confirm that the strictness of enforcement substantially reduces the value of remaining unemployed and thereby increases the rate of accepting unstable jobs.

We further identify a small average increase in earnings when looking at a long-run post-unemployment window of 18 months. In turn, individuals who accept a job within 6 months experience strong losses in total earnings, mainly due to being fewer months in employment. It thus appears that there are "winners and losers": some job seekers feel pressured to accept the first job available, while others are able to increase their job search effort in a more sustainable way and generate higher wage offers. Overall, the post-unemployment analyses suggest that job quality is significantly affected by enforcement strictness, and job stability is the main channel through which reductions in job quality are realized.

This paper contributes to two main strands of the literature. First, it relates to the empirical evidence on the effects of benefit sanctions in UI and welfare regimes. The existing empirical studies on UI benefit sanctions are largely dominated by the timing-of-events approach. These papers do not identify the effects of policy changes in the enforcement probability, but the ex-post treatment effects of an imposed benefit sanction and/or the warning that a sanction might be imposed in the future (e.g. van den Berg et al. 2004, Abbring et al. 2005, Lalive et al. 2005, Rosholm and Svarer 2008, Arni et al. 2013, Van den Berg and Vikstroem 2014). In turn, this

[2] Over the unemployment spell, a job seeker can become non-compliant for several reason. We use the first non-compliance to define the treatment status.

study quasi-experimentally identifies the effect of changing the sanction probability, which is the parameter directly regulated by the policy maker.[3] Our theory section discusses the mechanisms through which this effect operates, as well as their relation to the existing literature.

Second, the estimates presented in this paper add to a recent discussion on the effect of UI benefits on reservation wage behavior. Schmieder et al. (2016) use German data and find negative wage effects of an extended potential benefit duration. They derive from this that reservation wages do not react to benefit generosity. On the contrary, Nekoei and Weber (2016) find positive earnings effects in a similar analysis for Austria. In this paper, we consider an alternative context of benefit generosity, as we do not analyze the effects of potential benefit duration, but look at benefit cuts that incur in reaction to a non-compliance. Our findings on job quality effects suggest that the most responsive dimension of post-unemployment job quality is job stability, which has been neglected by the previous literature.

This paper is structured as follows: in section 2, we discuss how the probability of enforcement affects job search behavior and show how this relates to the ex-post and ex-ante effects identified in the previous literature. Section 3 lays out the institutional framework of the Swiss UI, the enforcement regime and the reform we exploit. In section 4, we describe the data sources and sampling criteria. Section 5 presents the econometric analysis. In Section 6 we discuss results, quantify the main trade-off and provide tests on identification and robustness. Section 7 concludes.

2 Theoretical Discussion

Framework This study aims at identifying the effect of a policy-driven increase in the enforcement strictness. The outcomes of interest are labor market outcomes of job seekers who were detected not to comply with a UI rule. We thus want to identify how a change in the conditional probability of sanction affects behavior.

In the following, we discuss conditions under which the empirical analysis can identify this effect and derive theoretical predictions. To this purpose, we analyze how the present discounted value ρR of a job seeker changes when going through different states in a UI benefit regime with monitoring and enforcement of obligations. The individual can be in three different states: (i) an initial state n with no detected non-compliance (ii) a state d, in which a non-compliance has been detected, and (iii) a state s with an enforced benefit sanction. Closely related to prior work by Abbring et al. (2005) and Lalive et al. (2005), we write these states as:

$$\rho R_n = \max_{s_n} \left[b - c(s_n) + \lambda(s_n) \int_{\phi_n}^{\infty} (\frac{w}{\rho} - R_n) dF(w) + \mathbb{1}_{nc} p_d (R_d - R_n) \right], \qquad (1)$$

[3] Besides the literature on benefit sanctions, a branch of quasi-experimental and experimental studies assesses, among other components, monitoring practices in UI (e.g. Black et al. 2003, Ashenfelter et al. 2005, Van den Berg and Van der Klaauw 2006, McVicar 2008, Petrongolo 2008, Cockx and Dejemeppe 2012). These studies evaluate a whole “package” of measures, like monitoring and job search assistance or monitoring and enforcement.

where $\mathbb{1}_{nc} = 1$ if a non-compliance occurs, and $p_d \in [0,1]$ is the probability that the non-compliance is detected. In case of detection, the expected value of unemployment $R_d < R_n$ decreases to:

$$\rho R_d = \max_{s_d} \left[b - c(s_d) + \lambda(s_d) \int_{\phi_d}^{\infty} (\frac{w}{\rho} - R_d) dF(w) + p_s(R_s - R_d) \right]. \tag{2}$$

p_s is the probability of sanction conditional on detection. It is communicated to job seekers in state d through a written notification by the UI authority, informing about the enforcement process. If the sanction gets enforced, benefits are cut and the expected value of unemployment $R_s < R_d$ further reduces to:

$$\rho R_s = \max_{s_s} \left[b - sanction - c(s_s) + \lambda(s_s) \int_{\phi_s}^{\infty} (\frac{w}{\rho} - R_s) dF(w) \right], \tag{3}$$

where *sanction* denotes the amount by which benefits are reduced in case of enforcement. [4]

In equations (1) to (3), b denotes the unemployment benefit, s the search effort chosen by the job seeker, w the wage of the final job match and ϕ the reservation wage, which equals the present discounted value ρR in equilibrium. The job seeker chooses the search effort s by maximizing ρR. The choice of effort s thus depends on the marginal effort cost $c'(s)$ and the marginal benefit of effort, which is composed of an increase in the job arrival rate, $\lambda'(s)$, and the associated value differential between employment and unemployment, $\int_{\phi}^{\infty}(\frac{w}{\rho} - R)dF(w)$.

Conditions for the Empirical Analysis We want to identify the effect of a policy change $\Delta p_s > 0$ on the outcomes of job seekers in state d. $\Delta p_s > 0$ is generated by a natural experiment, which is described in the next section.

A core condition for identification is that the non-compliance decision, which drives the selection into state d, was unaffected by the policy change. The value functions above show that p_s affects the difference in value between R_n and R_d and thus the decision of non-compliance. We therefore need to assume that, in the short run, job seekers did not anticipate the policy-driven change in p_s when deciding whether to comply. The empirical analysis shows that indeed, during the considered short time interval after the reform date, neither the probability of non-compliance nor the composition of non-compliant job seekers changed (c.f. sections 3 and 6.4). This is in line with anecdotal evidence reporting that the policy change was not communicated to job seekers prior to non-compliance detection.

Main Predictions For job seekers in state d, who were detected not for a non-compliance, the following predictions on the effects of $\Delta p_s > 0$ arise: first, the present value of non-compliant

[4] Equation 3 implies the symplifying assumption that sanctions last forever. In reality, this is not the case. However, as the reform did not affect the amount and length of sanctions, the assumption does not affect our qualitative predictions.

individuals, ρR_d, decreases, as the probability of facing a future reduction in benefits increases. Second, the number of job seekers actually receiving a benefit sanction and motiving to state s increases.

These two mechanisms lead to an unambiguous decrease in the current discounted value of the non-compliant job seeker. As a consequence, the marginal benefit of search effort increases and the reservation wage decreases. This results in an increased job finding probability, as individuals search more and are ready to accept lower quality jobs. The effect on the quality of accepted jobs is ambiguous: on the one hand, job seekers are predicted to have a lower unemployment duration, which reduces potential human capital depreciation and therefore raises the average wage offer.[5] On the other hand, decreased reservation wages may lead job seekers to accept lower quality jobs. In the empirical analysis, we consider job match stability and earnings as dimensions of job quality to shed light on these effects.

Relation to Existing Estimates While this study focuses on identifying the impact of changing the sanction probability p_s – a parameter that policy makers can directly affect – the existing empirical literature commonly focuses on estimating the ex-post treatment effects of benefit sanctions.[6] The ex-post treatment effect of a sanction is the observed change in outcomes due to the job seeker dropping from status d to s. It is thus driven by the change in the expected value from (2) to (3), implying that sanction imposition drives the ex-post effect. The ex-ante effect, in turn, is determined by reactions of individuals in states n or d who expect being sanctioned.[7]

In turn, if policy makers decide to implement a *policy change*, they can choose to shift p_s or p_d (abstracting from changes in the sanction size). We consider an increase in p_s, whose effect will contain two components: first, an increase in p_s reduces the expected value of being in the detected non-compliance state R_d in (2). This effect is related to the ex-ante effect of sanction enforcement. Second, the policy change also affects the incidence and size of the ex-post treatment effect: the number of sanctioned individuals is expected to increase, while the value differential between R_d and R_s when switching from (2) to (3) decreases. Thus, the measured effect of an enforcement policy change is a combination of individual ex-ante- and ex-post reactions.

[5]Thus, we assume a non-stationary job search environment (like e.g. in van den Berg, 1990), which is realistic in a standard European labor market.

[6]e.g. van den Berg et al. 2004, Abbring et al. 2005, Lalive et al. 2005, Rosholm and Svarer 2008, Arni et al. 2013

[7]In practice, it is difficult to separately identify ex-ante effects of monitoring and enforcement. Lalive et al. (2005) and Arni et al. (2013) study the correlation between PES fixed effects in the estimated hazards of receiving a warning (i.e. of being detected not to comply) and job search outcomes.

3 Institutional Setting and Reform

Rules and Requirements Claiming UI benefits in Switzerland[8] entails a number of obligations. These include the provision of sufficient search effort, the regular appearance at caseworker meetings, the participation in active labor market programs prescribed by the caseworker and the acceptance of "suitable" job offers. The local Public Employment Service (PES) is obliged by law to monitor the job seeker's compliance with these requirements and rules.

In this study, we analyze a reform in the enforcement of job search requirements. During their first contact with the caseworker, job seekers are informed about the monthly number of applications they have to send out to comply with their individual job search requirement. Job seekers document this application activity in a "protocol of search effort", which they have to submit up to the 5th day of the following month. The PES have to monitor whether the protocol is sent in by the deadline and whether the realized number of applications fulfills the requirement.

Reform in the Enforcement Regime The enforcement regime becomes operative if a job seeker does not comply with one of the UI rules. If a non-compliance is detected, it is registered in the computer system. A registered incidence of non-compliance opens an enforcement process that can lead to the imposition of a benefit sanction. Sanctions cut benefit levels to zero for a limited number of days (usually around 5-10 days).

We exploit a policy change in the process that links the detection of non-compliance to the imposition of a sanction. The policy change abolished the accordance of a second chance to job seekers who have not handed in their "protocol of search effort" by the official deadline. In the pre-reform regime, these job seekers received a notification which defined a second deadline. They could submit the missing protocol up to this second deadline and thereby avoid a benefit sanction. Alternatively, they could also state the reasons for not submitting the protocol in order to reduce the risk of being sanctioned. The pre-reform enforcement process is illustrated in Figure 1a.

[Insert Figures 1a and 1b]

In April 2011, the federal ministry abolished the practice of setting second deadlines. The intention of this regime change was of purely administrative nature: the cantonal authorities had complained about the organizational burden of the enforcement process.[9] The reform became effective for protocols reporting on job applications submitted in April 2011 or later. This implies that from May 2011 onward,[10] non-compliance notifications would no longer set a second deadline. Instead, they would only give job seekers the possibility to state the reasons behind their non-compliance and informed them that a sanction would be imposed if no excusable reason could be

[8]For fully eligible prime age individuals, potential benefit duration is 400 working days. For young or only partially eligible workers, benefit duration is reduced by 140 or 200 days. For older workers it is topped up by 120 days. The replacement ratio is 80% or 70%, depending on the family status and on previous earnings.

[9]Source: inquiries at the state secretary for economic affairs (SECO)

[10]May 5th was the deadline for protocols referring to April.

stated (c.f. Figure 1b).

Figure 2 shows that the abolition of second chances had a large effect on the enforcement strictness faced by job seekers notified for not having submitted their protocol (T=1). The dashed vertical lines denote the baseline sampling window. The probability of receiving a benefit sanction conditional upon receiving a notification jumped sharply by more than 100%, from 0.3 to 0.7.[11] At the same time, the probability of sanction for all other types of non-compliance notifications (T=0) remained stable. For these other types, a second chance policy had never existed and the enforcement process followed the procedures documented in Figure 1b.[12]

[Insert Figure 2]

Stability of Detection Regime When analyzing the reform effect on outcomes of job seekers with a detected non-compliance, it is important to assume that the reform did not affect the job seekers' compliance behavior itself (c.f. discussion in Section 2). As a first empirical test, Figure 3 documents that the propensities of non-compliance detection did not evolve differently for the treatment and the control group in the months around the reform date. Although there is a slight decrease in the probability of detection, this decrease also occurred in the control group, suggesting that the selection into non-compliance evolved equally in both groups within the short sample window (dashed lines). There is a slight divergence of the trends in September/October. We therefore choose to end the sample period in August. Additional evidence will be presented with the identification tests in section 6, where we show that there was no change in observable job seeker characteristics associated with the policy change.

[Insert Figure 3]

There are several practice-related reasons why the enforcement policy change was unanticipated by job seekers: first, the reform aimed at reducing the bureaucratic burden of the enforcement regime and was therefore of a purely administrative nature. It was not considered as a true political reform and therefore not announced as a policy change with the intention of generating additional benefit sanctions. Second, the final enforcement decision is not taken by the caseworkers themselves, but by a higher authority in the PES. As a consequence, the caseworkers were not responsible for executing the policy change, which makes it less likely that they actively advised job seekers to change their compliance behavior around the reform date. Third, the change occurred within a larger reform package whose principal element was to reduce the potential duration of

[11] Recall that job seekers can after the reform still avoid being sanctioned by stating an "excusable reason" (e.g. sickness or an accident) for not having submitted the protocol. This is why the probability does not increase to 1.

[12] This standard procedure is also described in Lalive et al. (2005) and Arni et al. (2013), who estimate the effects of non-compliance notifications and sanctions using a timing-of-events framework.

benefit payments for certain job seekers.[13] Compared to these reforms, the practice change in the enforcement rules was of minor nature. For instance, it did not appear in the presentation that was used to communicate the political reform package to caseworkers.[14]

4 Data and Descriptive Statistics

4.1 Data Sources and Sampling

Data Sources We use administrative data of the Swiss UI on the full population of job seekers entering formal unemployment. The data base includes extensive information on entry into and exit from unemployment, individual socio-demographic characteristics and employment history. Most importantly for our purpose, the date and reason of each non-compliance detection by the caseworker are reported. We observe if and when the job seeker submitted a statement on the reasons for the non-compliance as well as the time and result of the enforcement process, i.e., the final decision on sanction imposition. To track mid-run employment outcomes, we use data from the social security register, which were linked to the UI register at the individual level. The data report information on employment status and earnings on a monthly basis. The data are available until the end of 2014. This allows us to observe the individual post-unemployment job and earnings paths up to 18 months after unemployment exit.[15] Moreover, the social security data are used to control for the full path of past earnings during the 24 months prior to unemployment.

Sampling The official procedure for imposing benefit sanctions requires entering three dates: (i) the date at which an incidence of non-compliance was detected and communicated to the job seeker, (ii) the date at which the job seeker gave a statement and (iii) the date at which the final enforcement decision was made. In practice, not all cantons appear to respect this procedure, which leads to systematically missing dates of job seeker statements and systematically coinciding dates of notification and final sanction decisions. This implies that we do not know whether and when the job seeker was informed about the non-compliance detection. As this information is crucial for the analysis, we need to exclude cantons who do not report full information on enforcement processes. By excluding cantons for which more than a quarter of enforcement cases do not report a job seeker statement,[16] we end up including 14 out of 26 cantons in our data set,

[13] Note that the political reform package does not confound with the natural experiment that we consider: the exposure to the political reform is determined by socio-demographic eligibility variables (in particular a new threshold at age 25), whereas the change in enforcement practice affects different non-compliance subgroups differently, independent of their socio-demographic variables. Thus, the treatment and control groups of the two natural experiments are independent of each other.

[14] The only official channel in which caseworkers were informed about this change of enforcement practice was within the delivery of the updated collection of practice ordinances ("Kreisschreiben"); this collection features several hundred pages.

[15] In 1.6%, the observation of the longer-run earnings outcome is not observed for the full 18 months post-unemployment. In these cases, the average is taken over the available observation window until end of 2014.

[16] This is a plausibility cutoff; our results are not affected if we shift it to the left or right. Documentation is available upon request.

which corresponds to 65% of registered enforcement cases.[17] Further, we apply standard sampling restrictions by focusing on job seekers eligible for UI benefit receipt between 20 and 55 years and by excluding part-time unemployed job seekers, as well as job seekers eligible for disability insurance.

This study analyzes the behavior of job seekers who receive at least one non-compliance notification. Most first non-compliance notifications occur during the beginning of the unemployment spell. To achieve a sample of job seekers with a rather homogeneous elapsed duration of unemployment, we include only job seekers who received their first notification during the first 120 days of unemployment. This covers 80% of all first notifications. The sample period is defined based on the month of first non-compliance notification, which also determines the job seeker's pre vs. post reform status. For the main analyses of the natural experiment, the sample is focused on unemployment spells for which the first notification is registered between the four pre- and four post-reform months, i.e., between January and August 2011. Anticipatory behavior and compositional changes are unlikely in the short-run after the policy change, which motivates the short sample period (c.f. section 3).[18] In Section 6.5, we use additional pre-reform months to document the absence of any diverging pre-trends.[19]

Finally, we exclude notifications that concern the refusal of acceptable job offers (3% of notifications), because they generate sanctions which are on average four times higher than those of the other enforcement types. They are thus likely to concern special cases and not suitable as part of the control group.

4.2 Descriptive Evidence: Notifications and Enforcement Regime

A job seeker is assigned to the treatment group if she receives a notification that her search protocol has not been submitted by the deadline. Job seekers with another type of non-compliance notification are assigned to the control group. The assignment of the treatment status is based on the first non-compliance notification event.[20] Table 1 shows how the different types of non-compliance notifications are distributed in the estimation sample before and after the policy change. The treatment group constitutes about 10% of the sample. Within the control group, the most common type of notification refers to insufficient search effort before the first meeting with the caseworker. Job seekers are obliged to actively search for a job as soon as they learn about their unemployment. These cases of non-compliance mechanically dominate the distribution of first notifications, as they are registered at the first caseworker meeting, i.e., about three weeks after registration.

[17] Note that we are able to cover substantially more cantons than previous studies on the Swiss UI benefit sanction system using data from the late nineties and early two thousands by Lalive et al. (2005) and Arni et al. (2011), who cover respectively 3 and 7 cantons.

[18] Indeed, section 6.5 shows that the composition of job seekers stayed constant during the sample period.

[19] Sensitivity analyses show that the baseline results are robust to modifications of the sample period and that they are invariant to modifications of the 120-days-cutoff for first notifications.

[20] As usual in the related literature, we assume that the first occurrence of a certain type of event determines the job seeker's perception of the respective treatment policy regime. A considerable share of job seekers receives more than one notification during the unemployment spell (39% of non-compliant job seekers).

Other important types of non-compliance are insufficient search effort and delay or absence at a scheduled meetings with the caseworker.

[Insert Table 1]

Table 2 shows how different features of the enforcement regime changed in response to the reform. It reports simple difference-in-differences (in bolt) for the average sanction probability, the average number of days to notification, the average number of days from notification to sanction in case of enforcement and the average days of benefit cuts imposed in the case of enforcement.

Clearly, the only substantial effect of the reform on enforcement practices concerns the probability of non-compliers to be sanctioned. While this probability stayed constant in the control group, it increased from .285 to .673 in the treatment group, i.e., by around 125%. There is a small difference-in-differences in the number of days between registration in unemployment and the incidence of the first notification. The econometric framework will take this into account by adding detailed controls for the duration to notification. The reform is associated with a small decrease in the amount of the imposed sanction. The duration from notification to sanction in the case of enforcement remained stable.

[Insert Table 2]

5 Econometric Framework

5.1 Main Specification

We exploit the reform described in Section 3 as a local and unanticipated shock in the enforcement probability. The shock affected job seekers who became non-compliant with the UI rules by not handing in their search protocol before the deadline. These job seekers suddenly faced a no-excuse policy and were most likely going to receive a benefit sanction. The control group consists of job seekers who became non-compliant for another reason (c.f. section 4.2).

We set up a difference-in-differences (D-i-D) specification, comparing the pre-post difference in outcomes of treated job seekers to the pre-post difference in outcomes of job seekers in the control group. The underlying identifying assumptions –in particular common time trends and the absence of compositional changes– are tested in Section 6.4. We estimate the effect of the reform on a set of main outcomes of interest, which are job finding, job stability and post-unemployment earnings. The outcome y of job seeker i is specified as follows:

$$y_i = \delta \; (post_t \times T_i) + \gamma \; T_i + \eta_t + \xi_{t_w} + \lambda_{t,t_w} + \pi_{PES} + x_i'\beta + u_i \qquad (4)$$

The D-i-D term $post_t \times T_i$ takes the value one if the job seeker's first non-compliance notification was affected by the enforcement policy change. This is the case if the non-compliance refers to not having submitted the search protocol by the deadline (T_i) and if it was registered after April 2011 ($post_t$).[21] The coefficient of interest δ thus measures the effect of the policy change.

T_i and η_t are the basic D-i-D second order terms. T_i controls for time-constant differences between the treatment and the control group. η_t is a set of fixed effects which indicate the calendar month in which the job seeker is notified about his non-compliance. It controls for group-constant time effects. The dummy $post_t$ is collinear with η_t and therefore omitted.

Two additional sets of controls address that not all job seekers receive their non-compliance notification at the same time of the unemployment spell: ξ_{t_w} contains fixed effects for the number of full weeks passed between a job seeker's registration and the date of notification. λ_{t,t_w} interacts these effects with the calendar month of notification. We thus not only control for the weeks of unemployment passed at the time of notification, but allow this effect to vary according to the calendar month of notification. This adds flexibility and ensures that the reform effect is not confounded by changes in the duration to notification.[22] Furthermore, it is important to assert that the interaction λ_{t,t_w} between the month of notification and the weeks elapsed from registration to notification also determines the calendar time of the job seeker's registration with UI. The set of indicators in λ_{t,t_w} thus also control for potential compositional effects due to different inflow dates.

π_{PES} includes fixed effects for the individual's local Public Employment Service (PES) unit. In parts of the regressions, we control for an extensive set of individual-specific covariates x_i. The majority of the regressions are estimated using OLS. For a set of duration outcomes we apply proportional hazards regressions, as further specified in the following subsection.

5.2 Further Estimation Details

When estimating effects on the duration to unemployment exit or to job finding, we specify the proportional hazard θ^e as:

$$ln\ \theta^e = ln\ \lambda(t_e) + \delta\ (post_t \times T_i) + \gamma\ T_i + \eta_t + \xi_{t_w} + \lambda_{t,t_w} + \pi_{PES} + x_i'\beta \tag{5}$$

Duration dependence takes a non-parametric form, expressed through the step function:

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

[21] The reform started to become effective for protocols that referred to the job seeker's activities in the month of April. All protocols registered as not submitted after April were thus affected.

[22] Recall, however, that the time to notification was not substantially affected by the policy change (c.f. Table 2).

where k(= 1, . . . , K) is a subscript for the time intervals and $I_k(t)$ are time-varying dummy variables for subsequent intervals. $\lambda(t_{e,k})$ contain thus the piece-wise constant levels of the baseline hazard. When we right-censor the duration of unemployment after 6 months, we distinguish the following time intervals: 1-2 months, 2-3 months, 3-4 months, 4-5 and 5-6 months. In specifications where the duration to unemployment exit is censored after two years, the intervals 6-12 months and 12-24 months are added. As we estimate a constant term, we normalize $\lambda(t_{e,1})$ to be 0. The other terms of the equation are as in the linear estimation framework (equation 4).

6 Estimation Results

6.1 Exit from Unemployment

Unemployment Duration We first assess how the quasi-experimental change in enforcement strictness affects the duration of unemployment, taking into account different destinations after unemployment exit. Table 3 presents coefficients on the unemployment exit hazard, as specified in equation 5. We show results both for a short-run window (censoring after 6 months of unemployment) and for a long-run window (censoring after 24 months). We mostly expect early reactions, as the sample contains first notifications on non-compliance detections, which are issued within the first 120 days. The first two columns present estimated effects on the overall duration of unemployment, abstracting from the exit destination. Columns (3) and (4) focus on exits to employment, implying that individuals who exit to other destinations than employment are right-censored.[23] Columns with covariates include controls for the job seeker's socio-demographics and (un-)employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3.

The first four columns hardly report any reform effect on the duration of unemployment. The short-run effect on job finding is at the margin of significance, suggesting that the reform induced an increase of 16% (=exp(.148)-1) in the hazard during the first 6 months of unemployment. Columns (5) and (6) further restrict attention to individuals who find a job through their individual effort. At a job seeker's de-registration, it is recorded whether the job match was generated by the individual herself or by a vacancy referral made by the caseworker. From the theoretical discussion (section 2), we expect job finding through individual effort to be most affected by the reform, as the threat and incidence of benefit sanctions induces job seekers to raise search effort and lower their reservation value. In turn, vacancy referrals can be hardly influenced by the job seeker. Indeed, the hazard of job finding by individual effort reacts by significant 24% (=exp(.213)-1) during the first six months. For this outcome, there is also a significant long-run effect of 14% (=exp(.134)-1). These effects will be further quantified in terms of unemployment duration and

[23]The PES registers the job seeker's reason for exiting formal unemployment. Therefore, it is possible to classify the destinations of unemployment exit.

benefits in section 6.4.

[Insert Table 3]

Short-Run Job Finding Probability The presented estimates suggest that the policy change mostly induced short-run behavioral changes. To gain more insights into these, we run OLS regressions on short-run exit probabilities. We then decompose the effect on job finding by individual effort into exits to stable and unstable jobs.

Table 4 reports the difference-in-differences (D-i-D) coefficient on the probability of job finding and the probability of exiting without job. The outcomes are measured 2 months after the time of notification (first panel) and 6 months after entry into formal unemployment (second panel). Columns (1) and (2) report effects on the probability of finding a job through individual effort. Within two months after receipt of the non-compliance notification, this probability increases by 4.8 percentage points, corresponding to an increase of around 20% relative to the mean. When looking at the outcome window of 6 months after entry into unemployment, the probability increases by 6 percentage points, i.e., by around 15% relative to the mean.

The probability of finding a job through a vacancy referral is not significantly affected by the policy change, as shown by columns (3) and (4). This is in line with the idea that vacancy referrals are not a function of the job seeker's individual effort. Finally, the last two columns show that there is no significant effect of the reform on the probability of exiting formal unemployment without a job. The reform did thus not push non-compliant job seekers into non-employment. None of the presented results are sensitive to the inclusion of control variables.

[Insert Table 4]

Stable vs. Unstable Job Finding From a policy perspective, it is of key interest to look beyond unemployment exit by considering the quality of job matches. As a first dimension of quality, we analyze whether the reform affected the stability of accepted jobs. To this purpose, we split the outcome into finding stable vs. unstable jobs. A job is coded as stable if the job seeker remains out of formal unemployment for at least 12 months. It is coded as unstable if the job seeker registers back into unemployment within 12 months.

Table 5 shows that finding a stable job does not react significantly to the increased enforcement strictness (columns 1 and 2). Columns (3) and (4) show that the probability of exiting to an unstable job increases by around 4 percentage points, both within 2 months after notification (50% relative to the mean) and within 6 months after entry into unemployment (30% relative to the mean). Exits to unstable jobs cover 82% and 63% of the effect on job finding (for the two outcome windows, respectively). A stricter enforcement regime thus raises the willingness to search for and to accept temporary jobs or jobs which turn out to be bad matches.

[Insert Table 5]

6.2 Earnings

Besides job stability, earnings constitute the second important dimension of post-unemployment job quality. Job search theory makes ambiguous predictions on the effects of the policy change on earnings (c.f. section 2): on the one hand, an increased sanction probability lowers the reservation value of non-compliant job seekers and can thereby raise the willingness to accept lower wage offers. On the other hand, it can alleviate the depreciation of wage offers by reducing the duration of unemploymen.

In Tables 6 and 7, total earnings take the average earnings over the respective outcome windows, including months with zero earnings. This measure captures the total effect on the individual income situation. We then decompose it into two components: first, the earnings from employment, for which the months with zero earnings are excluded.[24] Second, the share of months in employment over the considered post-unemployment period. The two reported post-unemployment outcome periods are the first six post-unemployment months (first panel) and the first 18 post-unemployment months (second panel).[25]

Table 6 shows D-i-D coefficients from estimations that are based on the full estimation sample. There is no significant effect of the policy change on post-unemployment earnings within the first 6 months after exit (first panel). Columns 7 to 9 suggest that the reform induces individuals to work fewer months during this time period. In turn, columns 5 to 6 suggest a slight positive effect on earnings from employment. However, these estimates are not statistically significant.

The positive effect of the enforcement policy on employment earnings becomes stronger and turns significant when considering the longer-run post-unemployment period of 18 months (second panel). It shows that individuals earn, on average, about 206 CHF[26] (per month; 4.5% of the outcome mean) more due to the reform. This result depends on the inclusion covariates, which control for the sample selection into employment, in particular through the individual path of past earnings.[27] It suggests that the pressure induced by an increased enforcement probability may also result in additional job search effort that generates higher wage offers. The finding can be associated with the evidence by Schmieder et al. (2016), who find positive earning effects in response to a shortened UI benefit duration.

As in the last subsection, we further decompose the analysis by focusing on reactions of early job finders, who were predominantly affected by the enforcement policy. Table 7 shows results on

[24] Therefore, individuals who never were employed over the outcome period are excluded from these regressions.

[25] Formally, the decomposition of total earnings is straightforward: $\frac{\sum^T w_t}{\sum^T e_t + \sum^T n_t} = \frac{\sum^T w_t}{\sum^T e_t} \cdot \frac{\sum^T e_t}{\sum^T e_t + \sum^T n_t}$. w_t is monthly earnings and e_t/n_t equal 1 if the individual is employed/not employed in the corresponding month t, which runs from $t = 1$ to $T = 6$ or $T = 18$.

[26] 1 CHF = 1.03 USD = 0.91 EUR.

[27] Research by Arni et al. (2013) – based on the same register data sources (but an earlier sample window) – shows that the controlling for the path of past earnings eliminates the large majority of the unobserved heterogeneity due to selection into employment.

earnings only for job seekers who find a job (through their individual effort) within six months of unemployment. These estimates are, in principle, composed of two mechanisms: first, they contain the causal reform effect on the job quality of early job finders. Second, the reform may change the composition of early job finders, thereby inducing a change in average earnings. The second mechanism is addressed by the extensive set of control variables, in particular through the path of past earnings (c.f. footnote 27). The table shows a significant negative effect on total earnings (column 1), which is predominantly driven by a reduction in the share of months in employment (column 7). This effect gets more pronounced when considering the longer post-unemployment window of 18 months. Job seekers who recur to unemployment within 12 months show the strongest reaction (columns 3 and 9). But also individuals who do not recur within 12 months incur significant earnings losses (column 2). These results are in line with the decreased job stability identified in the previous subsection. The negative effect on the job stability of early job finders may have two causes: more frequent recurrence to unemployment, but potentially also more frequent job-to-job transitions. Overall, the results provide clear evidence that job stability is an important channel through which job quality reacts to UI reforms. In many previous analyses that study the effects of UI benefits on job quality, this channel was not considered (e.g. Chetty et al. 2007, Schmieder et al. 2016, Nekoei and Weber 2016).

Moreover, the two tables 6 and 7 jointly suggest that an increased enforcement strictness in UI may induce two mechanisms that are relevant for post-unemployment outcomes. On the one hand, it pressures some individuals to directly exit unemployment in response to a large probability of sanction –these job seekers suffer by accepting jobs which push them into an employment path of low stability. On the other hand, there appear to be job seekers who turn the additional pressure into additional search effort, which generates higher wage offers. These individuals are likely to drive the positive mid-run effect on the earnings path reported in table 6.

[Insert Tables 6 and 7]

6.3 Heterogeneity

In the following, we assess the heterogeneity in the effects of enforcement strictness. We first consider job seeker characteristics and second the PES-level treatment intensity in the policy change.

6.3.1 Job Seeker Characteristics

Table 8 shows D-i-D coefficients on the main outcomes by gender, education and previous earnings. The upper panel addresses the main short-run outcome of finding a job through individual effort (within six months of unemployment). There is no heterogeneity by gender or by previous earnings. Point estimates on the job finding outcomes are higher for job seekers with *low* education levels (without professional or college degree). However, due to the rather small size of the treatment group and the large standard errors resulting from it, the differences in the effects do not reach statistical significance.

The lower panel of Table 8 considers the post-unemployment earnings and employment outcomes for the entire observed window of 18 months. For the total earnings measure we do not find effects which differ from the average effects. Considering the impacts on the earnings from employment over 18 months post unemployment, point estimates suggest that the above-reported positive effect is driven slightly more by male job seekers and individuals with higher education.[28]

Finally, we focus back on the group of early job finders (last panel). We find significant evidence that the discussed loss in total earnings and employment stability of early job seekers is mostly driven by male individuals.

[Insert Table 8]

6.3.2 PES-Level Treatment Intensity

Prior to the reform, the Public Employment Service (PES) units differed in their strictness of enforcement. As a consequence, there is substantial variation in Δ_p^{PES}, the reform-driven increase in sanction probability p_s by PES.[29] We divide the sample into low- and high-intensity PES, the cutoff being $\Delta_p^{PES} = 0.2$. The non-weighted mean Δ_p of an average PES is .09 in the low-intensity group and .4 in the high intensity group. Table 9 reports the corresponding results. It shows that the reform effects on job finding are stronger for PES with a high Δ_p^{PES}, suggesting that the average effects are mostly driven by them. Note however that the average treatment intensity is four times higher in the high-intensity group, but point estimates are only about twice as high. The results thus tentatively suggest concave marginal effects of increasing the enforcement strictness. We do not find statistically significant differences in the earnings effects on job seekers in PES with high vs. low changes in enforcement strictness.[30]

The causal interpretation of these heterogeneity results relies on the assumption that the controls for covariates appropriately take into account compositional differences between the job seeker populations by PES. Due to the very rich set of individual covariates, including two years of controls for past earnings, we believe that this assumption holds well.[31]

[Insert Table 9]

[28] We do not find substantial heterogeneity in the effects on months in employment for the full sample. Results are available upon request.

[29] Δ_p^{PES} is the difference between the average sanction probability in a PES during the four post-reform months and the average sanction probability in a PES during the four pre-reform months.

[30] This also holds for effects on employment earnings and on months employed (results available upon request).

[31] There further is anecdotal evidence that the intensity of enforcement policy is more driven by the preference of the heads of PES for high or low strictness than by job seeker characteristics.

6.4 Trade-Off between Short-run and Mid-run: Quantification

In a final step, we quantify the financial trade-off associated to an increased enforcement strictness: in the short run, higher enforcement intensity results in an increase in speed of job finding. In the mid-run, however, early job finders experience a worsened earnings and employment situation. To quantify this trade-off, we compute the financial effects of faster job finding, using predictions based on the hazard regression estimates reported in Table 3 and compare them to mid-run effects on individual earnings. Table 10 shows that the policy change ($\Delta_p = .38$) decreased the duration to job finding on average by 16 days.[32] This implies that a 10 percentage point increase in enforcement intensity speeds up job finding by about 4 days. The financial payoff of faster labor market reentry can be computed as the difference between individual (daily) earnings and benefits, multiplied by the mentioned effect on job finding. This results in a gain of almost 600 CHF for a treated individual. For all treated job seekers, the mid-run effect on total earnings, on the other hand, is insignificantly different from zero, as was discussed in section 6.3.

However, when focusing on early job finders, who reacted strongest to the policy change, we observe ambiguous net effects. Within the first six months of unemployment, the stricter enforcement policy speeds up job finding by 5 days, resulting in a short-run gain of about 200 CHF. This has to be confronted with the negative effect on earnings in the longer run. According to the estimation results, earnings after unemployment are lowered by about 400 CHF per month. Taking into account that the earnings effect accumulates over time, the conclusion of this trade-off assessment is negative for the early job finders.

We then assess how the quantifications differ according to the PES-level treatment intensity. As explained in section 6.3, the policy change had a stronger bite in some PES than in others. Table 10 shows that the effect on duration to job finding is about twice as large for job seekers confronted with a large increase in enforcement strictness, as compared to those confronted with lower changes.[33] Recognizing that the average increase in the enforcement intensity was about 4 times as big in the large Δ_p group as in the small one, these simulation results suggest that the marginal effect of strengthening enforcement is concave: it diminishes in the size of the policy increase.

Finally, we also compute the direct savings for UI induced by shorter durations of benefit payments. To this purpose, we compare the individual durations to unemployment exit (all destinations, not only job finding) with and without the policy change. In total, the UI barely saves on duration, as changes in early job finding do not translate into significant reductions in the overall unemployment duration.

[Insert Table 10]

[32] The notes in Table 10 describe in detail the predictions made to compute effects on the duration to job finding.

[33] To ensure that the effect differences between the two heterogeneity groups are not driven by compositional differences, the effects of different treatment intensities are predicted for the group of *all* treated job seekers.

6.5 Identification Tests and Robustness Analysis

In the following, we test the main assumptions behind the D-i-D analysis. First, we assess whether the reform changed the selection of job seekers into the different types of non-compliances, and thereby the composition of treatment and control group. Second, we check whether differences in outcomes between treatment and control groups evolved in parallel trends during the pre-reform period. Third, we validate the robustness of the main results to alternative sample restrictions and definitions of the control group.

6.5.1 Compositional Changes

One central concern is the presence of reform-induced changes in the composition of treated and non-treated job seekers. If job seekers anticipate the increased enforcement strictness, they may take it into account in their compliance decision. If this was the case, the types of job seekers who select into non-compliance would differ before and after the reform.

To check whether the reform induced changes in the composition of job seekers, we regress measures of past earnings, education, unemployment history, gender, marriage status and age on the basic difference-in-difference framework (equation 4) without x_i). Table 11 shows that none of the variables is significantly correlated to the reform indicator. Although we are not able to test for changes in unobservable characteristics, this supports the assumption that the reform did not induce any changes in the observable composition of non-compliant job seekers that could bias the estimates.

[Insert Table 11]

6.5.2 Common Pre-Reform Trends

The baseline sample included job seekers who received a notification of non-compliance during the four pre- and post-reform months. To test the parallel trend assumption, which is central to the validity of D-i-D estimations, we now extend the sample back to notifications issued in January 2009. We can thereby assess whether the differences in outcomes of the treatment and control group were stable during the two pre-reform years.

We replace the $post_t \times T_i$-interaction in our main equation by the term $\kappa_\tau \times T_i$, which measures the effect of being in the treatment group and receiving a notification in a given four-month period τ. This results in the following equation:

$$y_i = \sum_{\kappa=0}^{K} \delta_\kappa (\kappa_\tau \times T_i) + \gamma\, T_i + \eta_t + \xi_{t_w} + \lambda_{t,t_w} + \pi_{PES} + x_i'\beta + u_i$$

If the assumption of parallel pre-trends holds, $\hat{\delta}_\kappa$ should only be significant for the post-reform period of May-August 2011.

Figure 4 plots $\hat{\delta}_\kappa$ for the main job finding outcomes, the reference period being the four pre-reform months January-April 2011. None of the graphs reveal any significant pre-trends between treatment and control groups during the two years preceding the reform.[34] In Figure 5 we show the plotted $\hat{\delta}_\kappa$ for the earnings regressions. Again, no pre-trends are visible from the graphs.[35]

[Insert Figures 4 and 5]

6.5.3 Robustness Tests

We now test the robustness of our estimates to alternative sampling choices. The outcomes of reference are the probability of job finding and of unstable job finding within 6 months after entry into unemployment.[36]

In Table 12, column (2) extends the sample by including job seekers who experience their first detection up to 150 days after the start of their unemployment spell (instead of 120 in the baseline). The motivation to exclude job seekers whose notification occurred later than 120 days after entry into unemployment was to achieve a homogeneous sample of elapsed duration at the time of notification. Column (3) reduces the sampling window to detections between January 15 and August 14, 2011 and column (4) extends the sampling window to detections between December 2010 and September, 2011 (instead of January to August 2011 in the baseline). Column (5) excludes from the control group detections that refer to the non-compliance with job search requirements, as these relate to the same general topic as the notifications of the treatment group. Changes to the sampling window partly lead to small losses in the statistical precision of the estimates; however, none of the tests leads to significant changes in the estimated coefficients.

[Insert Table 12]

7 Conclusion

This paper presents first quasi-experimental evidence on the effects of increased enforcement strictness in UI. Enforcement strictness, i.e. the probability to enforce benefit sanctions, is a policy parameter that can directly be targeted and modified by policy makers. When increasing the probability of enforcement, the policy maker raises both the threat effect on job seekers and the incidence of sanctioned individuals. This study estimates this composite effect of enforcement on the labor market outcomes of non-compliant job seekers.

[34] Regressions without covariates give the same picture. Documentation is available upon request.

[35] Additional outcomes are omitted for space reasons. None of them shows any pre-trend. Documentation is available upon request.

[36] The robustness results hold for the other outcomes, which are omitted for space reasons. Documentation is available upon request.

We find that strengthening enforcement strictness (conditional on constant monitoring intensity) increases the speed of job finding among non-compliant job seekers – but this effect results predominantly in unstable job matches. Early job finders, the majority group in UI, experience a negative effect on post-unemployment earnings, due to more frequent recurrence to UI and transitions to less stable jobs. These findings show that job stability is a highly relevant channel through which policy-induced reductions in job match quality may occur. This channel is understudied in the existing literature. For instance, studies on the effects of benefit generosity in UI on job quality have generally focused on wage effects (e.g. Schmieder et al. 2016, Nekoei and Weber 2016).

Besides these employment stability effects, our findings reveal that a stricter enforcement policy has ambiguous effects on earnings. For the average job seeker, we find that long-run earnings improve in response to the policy change. For a subset of individuals, stricter enforcement seems to increase job search effort or search efficiency, resulting in slightly increased earnings from employment. At the same time, early job finders experience a loss in total earnings, driven mostly by a negative effect on job stability.

Through the lens of job search theory, the results suggest that early job finders react to stricter enforcement by adapting their reservation wage. Had the policy effect only affected search effort, we would not have found a worsened job match quality. Thus, enforcement policy seems to affect the job acceptance rate and not only the job offer arrival process.

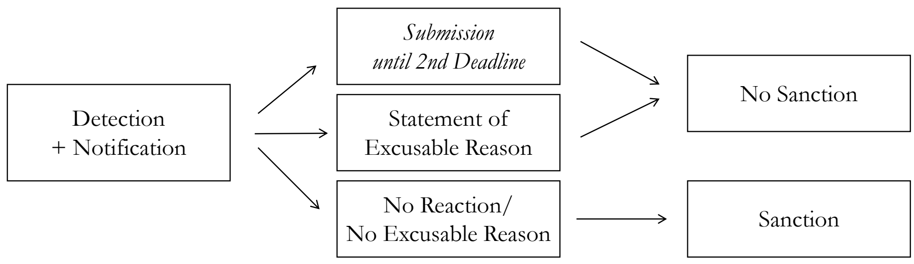
We use the estimates from the natural experiment to quantify the elasticity of job search outcomes to an increased enforcement strictness in financial terms. On average, a 10 percentage point increase in the sanction probability reduces the duration to finding a job (through individual effort) by 4 days. This translates into 3.4% of a monthly salary. At the same time, early job finders experience a loss of 2.3% in total earnings per month (considering 1.5 years after UE). Thus, this core subgroup is confronted with a negative trade-off between the short and longer run. The quantification further reveals that the marginal effect of enforcement strictness tends to be concave in enforcement intensity.

The presented results deliver a reduced form estimate of the enforcement policy parameter as used in standard job search models. The estimates are thus suitable to improve the empirical basis for optimal policy models or structural general equilibrium approaches. For policy makers, it is of substantial interest to gain further empirical knowledge on enforcement mechanisms in systems like UI or welfare, because the enforcement of job search obligations is a key strategy to counteract moral hazard. It is more targeted than adapting general benefit generosity, which affects compliers and non-compliers in the same extent. Evidence on how enforcement affects job search behavior thus provides important inputs for the elaboration of more specific policy designs.

8 Tables and Figures

Figure 1: Enforcement Process Pre and Post Reform

(a) Pre Reform



(b) Post Reform

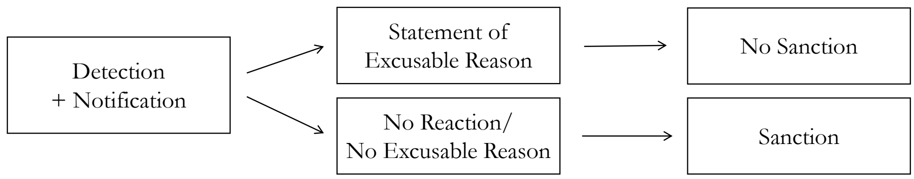
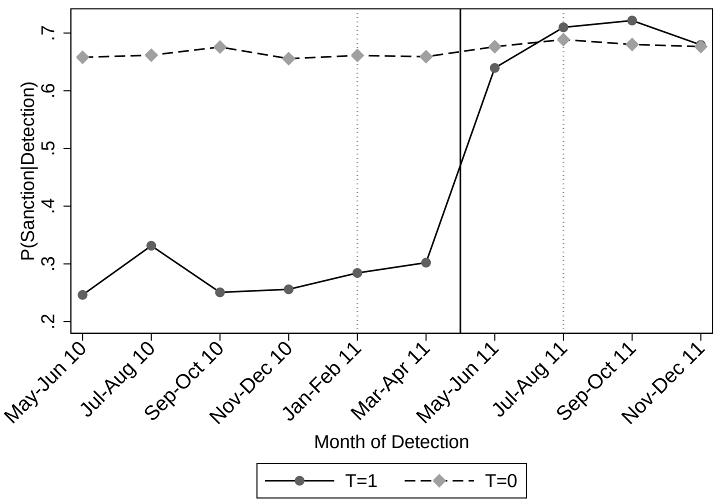
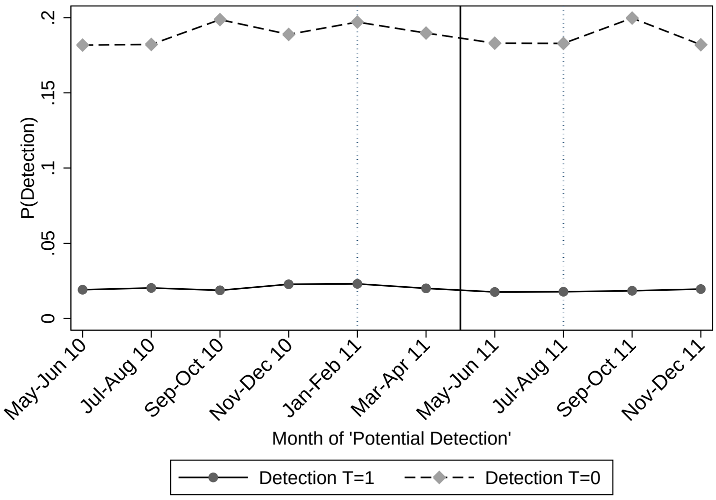


Figure 2: Probability of Sanction after Detection



N=16218. The dashed vertical lines delimit the baseline sample window. The solid vertical line indicates the reform date. T=1 are job seekers who receive a non-compliance notification in the treatment group (for not having submitted the job search protocol). T=0 are job seekers who are notified about a non-compliance in the control group (other reasons of non-compliance). The underlying data sources and sampling choices are described in section 4.2.

Figure 3: Probability of Detection



N=16218. The dashed vertical lines delimit the baseline sample window. The solid vertical line indicates the reform date. T=1 are job seekers who receive a non-compliance notification in the treatment group (for not having submitted a job search protocol by the deadline). T=0 are job seekers who are notified about a non-compliance in the control group (other reasons of non-compliance). The underlying data sources and sampling choices are described in section 4.2. Job seekers who never committed a detected non-compliance do not have any "actual" date of detection. For them, we calculate a month of "potential detection": it is the month of the date of registration +30 days (as the median lag between registration and the first detection is 30 days).

Table 1: Non-Compliance Notifications Before and After the Policy Change

Reason of Non-Compliance Notification	N_{pre}	% of sample pre	N_{post}	% of sample post
Search protocol not submitted by deadline (T=1)	1015	10.73%	637	9.42%
Other Reasons (T=0):	8443	89.27%	6123	90.58%
- Insufficient search effort before registration	5609	59.30%	4256	62.96%
- Protocol submitted, but insufficient effort	1352	14.29%	719	10.64%
- Delay or absence at caseworker meeting	1164	12.31%	868	12.84%
- Other	170	1.80%	160	2.37%
Total	**9458**		**6760**	

"Other" contains the non-participation at an active labor market program or the failure to comply with orders made by the PES.

Table 2: Enforcement Regime Pre and Post Reform

		Before	After	Difference
P(Sanction)	T=1	0.292	0.672	0.380
	T=0	0.660	0.683	0.022
	Difference	-0.369	-0.011	**0.358**
Days to Notification	T=1	63.492	65.656	2.165
	T=0	35.061	32.316	-2.745
	Difference	28.431	33.340	**4.909**
Days Notification to Sanction	T=1	18.644	20.317	2.498
	T=0	19.567	21.142	0.751
	Difference	-0.923	-0.825	**0.098**
Amount of Sanction (days)	T=1	6.880	6.157	-0.723
	T=0	7.141	7.094	-0.047
	Difference	-0.260	-0.936	**-0.676**

N=16218. The bolt numbers are the difference-in-differences in the respective parameter. The amount of benefit sanction and the number of days between notification and sanction are computed based on the unmerged unemployment insurance register data, as they are available with less precision in the merged data. The difference between the two samples is 97 individuals.

Table 3: Duration of Unemployment (Proportional Hazard)

Outcome Window		All Exits		Job Finding		Job Finding (Indiv. Effort)	
		(1)	(2)	(3)	(4)	(5)	(6)
24 months after UE entry							
	D-i-D	0.049	0.018	0.060	0.067	0.136*	0.134*
		(0.067)	(0.056)	(0.078)	(0.071)	(0.079)	(0.076)
	no. of exits	15717	15717	10737	10737	8699	8699
6 months after UE entry							
	D-i-D	0.141	0.118	0.131	0.148*	0.197**	0.212**
		(0.087)	(0.077)	(0.096)	(0.087)	(0.100)	(0.094)
	no. of exits	9646	9646	7367	7367	6147	6147
	Covariates	no	yes	no	yes	no	yes
	N	16218	16218	16218	16218	16218	16218

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors (in brackets) are clustered at the PES level. Regressions estimate equation 5 using maximum likelihood. They include all fixed effects specified in equation 5. Columns with covariates additionally control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3. The reported coefficient is the difference-in-differences parameter δ from equation 5. Job finding (indiv. effort) turns one if the job seeker found the job through her individual search effort, without a vacancy referral from the PES.

Table 4: Probability of Job Finding and Exit without Job

Outcome Window	Job Finding (indiv. effort)		Job Finding (placement)		Exit Without Job	
	(1)	(2)	(3)	(4)	(5)	(6)
2 months after notification						
D-i-D	0.048**	0.048**	-0.015	-0.016	0.010	0.005
	(0.022)	(0.021)	(0.016)	(0.016)	(0.010)	(0.009)
outcome mean	0.220	0.220	0.057	0.057	0.031	0.031
6 months after UE entry						
D-i-D	0.054**	0.062***	-0.022	-0.023	0.017	0.009
	(0.026)	(0.023)	(0.017)	(0.017)	(0.013)	(0.012)
Mean	0.379	0.379	0.083	0.083	0.069	0.069
Covariates	no	yes	no	yes	no	yes
N	16218	16218	16218	16218	16218	16218

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors (in brackets) are clustered at the PES level. Regressions estimate equation 4 using OLS. They include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3. The reported coefficient is the difference-in-differences parameter δ from equation 4. Job finding (indiv. effort) turns one if the job seeker found the job through her individual search effort. Job finding (placement) turns one if the job seeker found the job through a vacancy referral from the PES.

Table 5: Probability of Stabls vs. Unstable Job Finding

Outcome Window	P(Stable Job Finding)		P(Unstable Job Finding)	
	(1)	(2)	(3)	(4)
2 months after notification				
D-i-D	0.004	0.008	0.046***	0.040***
	(0.017)	(0.017)	(0.013)	(0.012)
Mean	0.146	0.146	0.074	0.074
6 months after UE entry				
D-i-D	0.011	0.023	0.047***	0.039***
	(0.021)	(0.019)	(0.015)	(0.013)
Mean	0.254	0.254	0.125	0.125
Covariates	no	yes	no	yes
N	16218	16218	16218	16218

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors (in brackets) are clustered at the PES level. Regressions estimate equation 4 using OLS. They include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3. Stable job finding is coded as one if the job seeker finds a job (through her individual search effort) and does not re-enter formal unemployment for at least 12 months. Unstable job finding is coded as one if the job seeker finds a job (through her individual search effort) and re-enters formal unemployment UE within the following 12 months.

Table 6: Effects on Post-Unemployment Earnings: Full Sample

	Total Earnings			*Employment Earnings*			*Months Employed (Share)*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-UE Average over 6 Mon.									
D-i-D	-98.228	76.969	32.363	-12.892	132.498	117.629	-0.018	-0.003	-0.010
	(138.413)	(125.119)	(125.401)	(145.132)	(111.988)	(112.443)	(0.024)	(0.023)	(0.023)
Mean	3233.3	3233.3	3233.3	4112.9	4112.9	4112.9	0.757	0.757	0.758
Post-UE Average over 18 Mon.									
D-i-D	-80.023	84.540	39.564	45.237	199.853**	176.943*	-0.017	-0.004	-0.011
	(139.193)	(120.609)	(120.010)	(129.867)	(98.639)	(97.238)	(0.021)	(0.021)	(0.020)
Mean	4150.9	4150.9	4150.9	4556.2	4556.2	4556.2	0.892	0.892	0.893
Covariates	no	yes	yes	no	yes	yes	no	yes	yes
conditional on UE duration	no	no	yes	no	no	yes	no	no	yes
N	16218	16218	16218	14524	14524	14524	16218	16218	16218

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors (in brackets) are clustered at the PES level. Regressions estimate equation 4 using OLS. They include all fixed effects. Columns with covariates additionally additionally control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3. Controls for the duration of unemployment in columns 3, 6 and 9 are specified as dummies for each 10-day interval. "Total Earnings" are the average monthly earnings obtained during the outcome window, including months with zero earnings. "Earnings from Employment" exclude months with zero earnings; job seekers with no earnings during the outcome window are excluded from these regressions. "Months Employed (Share)" is the ratio of months in employment over months out of employment during the outcome window.

Table 7: Effects on Post-Unemployment Earnings: Sample of Early Job Finders (within 6 months, through individual effort)

	Total Earnings			*Employment Earnings*			*Months Employed (Share)*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Stable	Unstable	All	Stable	Unstable	All	Stable	Unstable
Post-UE Average over 6 Mon.									
D-i-D	-368.421**	-485.615**	3.724	-112.187	-265.194	163.668	-0.043*	-0.025	-0.032
	(182.488)	(236.855)	(246.932)	(145.339)	(200.343)	(242.992)	(0.024)	(0.030)	(0.040)
Mean	4232.9	4599.2	3489.8	4596.9	4867.7	4051.7	0.903	0.934	0.841
Post-UE Average over 18 Mon.									
D-i-D	-409.578**	-432.137*	-440.604**	-30.263	-218.809	190.898	-0.058**	-0.015	-0.138***
	(183.343)	(230.093)	(220.181)	(131.392)	(182.902)	(189.711)	(0.026)	(0.030)	(0.038)
Mean	4150.9	4645.9	3147.1	4686.0	5007.8	4041.9	0.860	0.909	0.760
Covariates	yes	yes	yes	yes	yes	yes	yes	yes	yes
conditional on UE duration	no	no	no	no	no	no	no	no	no
N	6147	4117	2030	5961	3983	1978	6147	4117	2030

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 4 using OLS. They include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3. "Total Earnings" are the average monthly earnings obtained during the outcome window, including months with zero earnings. "Earnings from Employment" exclude zero earnings from the average; job seekers with no earnings during the outcome window are excluded from these regressions. "Months Employed (Share)" is the ratio of months in employment over months out of employment during the outcome window. "All" refers to all early job finders. "Stable" refers to early job finders staying out of formal unemployment for at least 12 months. "Unstable" refers to early job finders recurring to formal unemployment within 12 months.

Table 8: Heterogeneity: Job Seeker Characteristics

		(1)	(2)	(3)	(4)	(5)	(6)
		Gender		Education		Previous Earnings	
		Male	Female	Low	High	Low	High
***Job Finding** (6 months, indiv. effort)*							
All	D-i-D	0.065**	0.063	0.075**	0.034	0.081**	0.070*
		(0.029)	(0.042)	(0.032)	(0.038)	(0.033)	(0.040)
	Mean	0.386	0.366	0.411	0.321	0.337	0.420
Stable	D-i-D	0.018	0.023	0.029	0.005	0.034	0.028
		(0.025)	(0.036)	(0.029)	(0.034)	(0.026)	(0.032)
	Mean	0.245	0.269	0.295	0.180	0.226	0.280
Unstable	D-i-D	0.047**	0.040	0.047**	0.029	0.047**	0.042**
		(0.019)	(0.026)	(0.019)	(0.028)	(0.021)	(0.021)
	Mean	0.141	0.098	0.117	0.141	0.110	0.140
***Post-Unemployment Outcomes** (over 18 months): full sample*							
Total Earnings	D-i-D	103.364	-44.203	99.078	4.246	94.531	94.305
		(154.104)	(157.132)	(150.558)	(183.151)	(169.512)	(186.874)
	Mean	3420.2	2795.6	3374.5	2849.9	2479.4	3876.0
Employment Earnings	D-i-D	258.572*	103.150	132.544	236.086	190.404	188.291
		(133.016)	(140.292)	(138.158)	(148.147)	(139.802)	(144.790)
	Mean	4448.5	3616.1	4270.0	3898.5	3401.8	4824.8
***Post-Unemployment Outcomes** (over 18 months): early job finders*							
Total Earnings	D-i-D	-696.628***	-49.657	-344.074*	-623.540	-333.621	-462.079
		(262.365)	(229.829)	(197.735)	(456.658)	(236.177)	(327.290)
	Mean	4341.0	3810.8	4239.6	3943.8	3417.2	4721.5
Employment (share)	D-i-D	-0.092**	-0.056	-0.052*	-0.052	-0.064	-0.068**
		(0.036)	(0.041)	(0.027)	(0.057)	(0.039)	(0.034)
	Mean	0.850	0.877	0.871	0.834	0.836	0.878
	Covariates	yes	yes	yes	yes	yes	yes
	N	10203	6015	10468	5750	7983	8235

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 4 using OLS. They include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3. Education is coded as "low" if the job seeker has no higher education or apprenticeship degree, and "high" otherwise. Previous earnings is coded as "low" if the individual's average total earnings during the six months prior to unemployment are lower than the median, and as "high" otherwise. There is no significant effect by subgroup on the non-reported earnings outcomes (omitted to save space and available upon request). N for employment earnings for full sample (by column): 9167/5357/9499/5025/6974/7550; they are slightly smaller since some individuals do not return to employment within 18 months after unemployment. N for early job finders (by column): 3943/2204/4304/1843/2689/3458.

Table 9: Heterogeneity: PES-Level Treatment Intensity (Δ_p^{PES})

		(1) $\Delta_p^{PES} < .2$	(2) $\Delta_p^{PES} \geq .2$
***Job Finding** (6 mon, indiv. effort)*			
All	D-i-D	0.043	0.082**
		(0.038)	(0.031)
	Mean	0.395	0.364
Stable	D-i-D	0.027	0.034
		(0.032)	(0.027)
	Mean	0.268	0.241
Unstable	D-i-D	0.016	0.048***
		(0.024)	(0.017)
	Mean	0.127	0.123
***Post-Unemployment Outcomes** (over 18 mon.): full sample*			
Total Earnings	D-i-D	-67.508	237.053
		(184.879)	(149.980)
	Mean	3214.2	3163.7
***Post-Unemployment Outcomes** (over 18 mon.): early job finders*			
Total Earnings	D-i-D	-485.231	-149.438
		(315.993)	(239.147)
	Mean	4223.6	4074.6
	Covariates	yes	yes
	N	7981	8237

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 4 using OLS and include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3. Δ_p^{PES} is the PES-level change in sanction probability during the four pre- and post-reform months. The (unweighted) average Δ_p^{PES} is .09 for PES in column (1) and .4 for PES in column (2). There is no significant effect on the non-reported earnings outcomes (omitted to save space and available upon request). N for early job finders (by column): 3150/2997.

Table 10: Quantification: Policy Effects in Financial Terms

	Average Effect		Heterogenous Effects	
	$\Delta_p = .38$ (Reform)	$\Delta_p = .10$ (Marg. Effect)	$\Delta_p \approx .09$ (Reform)	$\Delta_p \approx .40$ (Reform)
Individual Trade-off				
All Treated: effect (per person)				
(1) on duration to job finding [days]	-16.1	-4.2	-22.1	-11.8
...gain through faster LM reentry [CHF]	585.2	154.0	807.4	432.9
(2) on earnings, avg 18 mt post-UE [CHF, per mt]	(38.8)	(10.2)	(237.7)	(-65.9)
Early Job Finders (w/i 6 mt): effect (per person)				
(1) on duration to job finding [days]	-5.1	-1.4	-8.4	-2.5
...gain through faster LM reentry [CHF]	193.2	50.9	315.4	94.0
(2) on earnings, avg 18 mt post-UE [CHF, per mt]	-414.4	-109.1	(-150.2)	(-478.6)
Savings for UI				
All Treated: effect (per person)				
on unemployment duration [days]	(-2.7)	(-0.7)		
...saved UI benefits [CHF]	(289.3)	(76.1)		

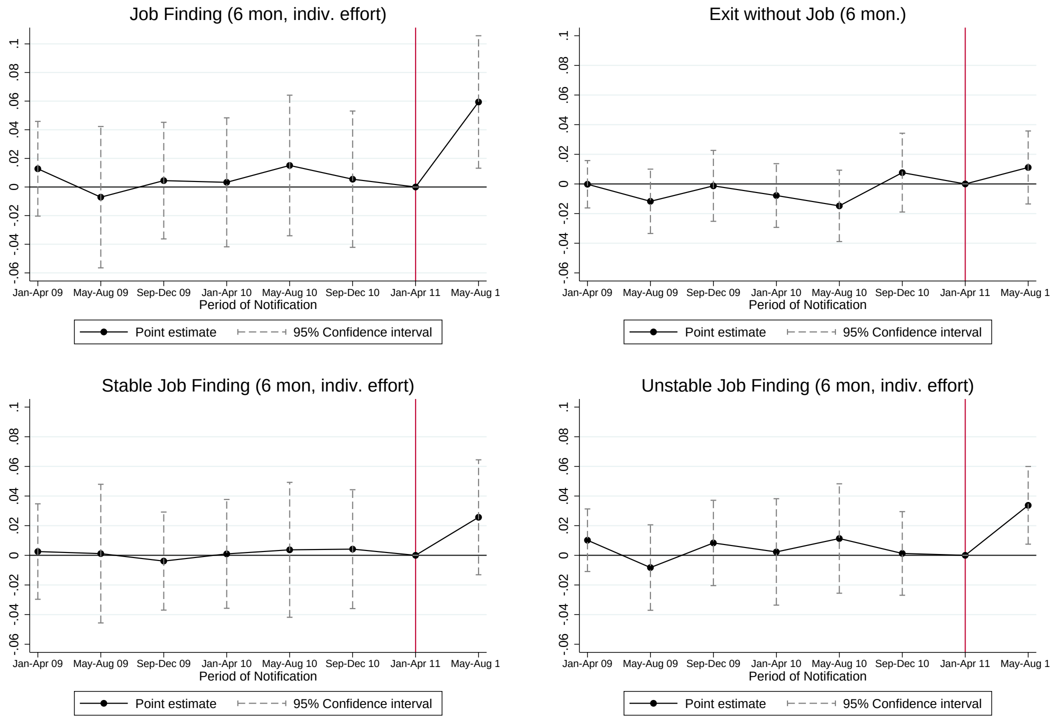
Values in parentheses indicate that the estimated (earnings- or duration-) effect is not significant. Δ_p denotes the change in enforcement probability in percentage points. "Reform" indicates that effects are computed based on the average reform-driven variation in Δ_p. "Marg. Effect" indicates the marginal effect per 10 percentage points increase in enforcement probability. It is calculated based on the average reform effect.
The quantifications of the policy effects on unemployment duration are based on the following estimates of proportional hazard regressions (using equation 5): Savings for UI: table 3, column (2), full outcome window (censoring after 24 months); individual trade-off for all treated: table 3, column (6), full outcome window; individual trade-off for early job finders: table 3, column (6), short outcome window (censoring after 6 months); heterogeneous effects for all treated/early job finders: proportional hazard models using equation 5 (not reported, available on request) for duration to finding a job (through individual effort) within the full/short outcome window (censoring after 24/6 months). Based on each of these estimated models, individual unemployment durations are predicted for two counterfactuals: in the reform case, the D-i-D coefficient is set to 1, in the case without reform, it is set to 0. The difference between these counterfactual unemployment durations quantifies the duration effect of the policy change. It is always reported for the population of the treatment group (N= 1652), as an average treatment effect on the treated. Results for the heterogeneous effects are predicted into the same population for both groups of PES. This ensures that the quantified results are not driven by potential differences in group composition. The financial gains through faster labor market (LM) reentry are obtained by multiplying the difference between individual employment earnings and daily benefits (in CHF) with the predicted duration effect. Analogously, the saved UI benefits are computed by multiplying the individual daily benefit (in CHF) with the predicted duration effect. The effects on earnings are copied from table 6, column (3) and table 7, column (1) and from table 9 for the heterogenous effects. 1 CHF = 1.03 USD = 0.91 EUR.

Table 11: Composition of Job Seekers

	(1) Prev Earnings	(2) No Degree	(3) UE within last year	(4) Female	(5) Married	(6) Age
D-i-D	-106.463	0.022	0.028	-0.015	0.011	-0.902
	(153.540)	(0.030)	(0.027)	(0.026)	(0.024)	(0.560)
T=1	-87.961	-0.002	0.123***	-0.025	-0.002	0.311
	(92.317)	(0.015)	(0.021)	(0.017)	(0.016)	(0.427)
Outcome Mean	3492.239	0.645	0.329	0.371	0.336	32.889
Covariates	NO	NO	NO	NO	NO	NO
Observations	16218	16218	16218	16218	16218	16218

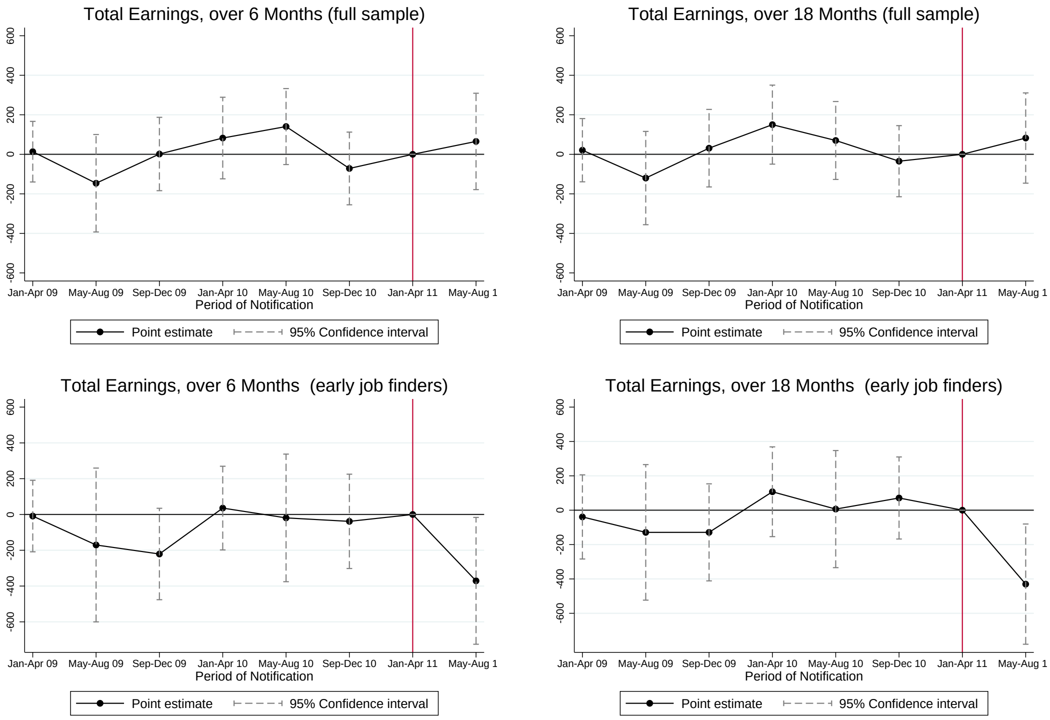
$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors (in brackets) are clustered at the PES level. All regressions estimate equation 4 using OLS. They include all fixed effects and exclude covariates. In column 1, the outcome is the average earnings in the 12 months prior to unemployment (zero for job seekers with no previous employment). In column 2, the outcome turns one if the job seeker holds no professional or academic degree. In column 3, the outcome turns one if the job seeker was unemployed during the year prior to the current spell. In columns 4 and 5, the outcomes are a female and a married dummy. In column 6, the outcome is the job seeker's age.

Figure 4: Assessment of Common Pre-Trends for Main Job Finding Outcomes



Graphs display the estimated difference-in-differences for four-months intervals between January 2009 and August 2011. Reported coefficients correspond to the vector δ_κ of equation 6. The baseline period are the four pre-reform months January to April 2011. The solid vertical line indicates the reform date. Regressions are estimated using OLS. They include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3.

Figure 5: Assessment of Common Pre-Trends for Main Earnings Outcomes



Graphs display the estimated difference-in-differences for four-months intervals between January 2009 and August 2011. Reported coefficients correspond to the vector δ_κ of equation 6. The baseline period are the four pre-reform months January to April 2011. The solid vertical line indicates the reform date. Regressions are estimated using OLS. They include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3.

Table 12: Probability of Job Finding within 6 Months (through individual effort): Robustness Analysis

		(1) Baseline	(2) Notifications <150 Days	(3) Smaller Time Window	(4) Larger Time Window	(5) Alternative Control Group
All	D-i-D	0.064***	0.048**	0.046*	0.056**	0.060**
		(0.023)	(0.021)	(0.027)	(0.022)	(0.024)
	Mean	0.379	0.370	0.376	0.377	0.383
Unstable						
	D-i-D	0.040***	0.033***	0.033**	0.030**	0.037***
		(0.013)	(0.012)	(0.015)	(0.013)	(0.014)
	Mean	0.125	0.123	0.122	0.126	0.126
	Covariates	yes	yes	yes	yes	yes
	N	16218	16982	13376	20571	15701

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors (in brackets) are clustered at the PES level. Regressions estimate equation 4 using OLS. They include all fixed effects and covariates, which control for the job seeker's socio-demographics, unemployment and employment history, including a full path of past earnings during the 24 months prior to unemployment. Summary statistics on covariates are reported in tables A.1 to A.3. Column (1) recalls the baseline estimates Column (2) extends the sample to job seekers who received their first notification up to 150 days after the start of their unemployment spell (instead of 120). Column (3) reduces the sampling window to notifications sent out between January 15 and August 14 2011. Column (4) extends the sampling window to notifications sent out between December 2010 and September 2011. Column (5) excludes from the control group notifications that refer to the compliance with job search requirements.

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A Appendix: Additional Tables

Table A.1: Summary Statistics on Covariates: UI Data (1/2)

Variable	Mean	Std. Dev.	Min	Max	Obs
Female	0.371	0.483	0	1	16218
Age	32.889	9.819	20	55	16218
Age Squared	1178.078	701.907	400	3025	16218
Non-permanent resident	0.251	0.434	0	1	16218
No of UE spells in last 3 y	0.775	1.001	0	7	16218
Log total UE duration in past 3y	2.490	2.646	0	6.980	16218
Log Dur of longest UE spell in past 3 y	2.395	2.541	0	6.980	16218
Mother tongue $\neq$ regional language	0.441	0.497	0	1	16218
Position in last job (omitted baseline: professional):					
Self-employed	0.003	0.053	0	1	16218
Manager	0.036	0.186	0	1	16218
Support	0.378	0.485	0	1	16218
Experience (omitted baseline:>3 years):					
None	0.016	0.124	0	1	16218
< 1 Year	0.075	0.264	0	1	16218
1-3 Years	0.166	0.372	0	1	16218
Missing	0.452	0.498	0	1	16218
Qualification level of last job (omitted baseline: skilled):					
Semi-skilled	0.153	0.360	0	1	16218
Unskilled	0.246	0.430	0	1	16218
Missing	0.156	0.363	0	1	16218
Civil status (omitted baseline: single):					
Married	0.336	0.472	0	1	16218
Widowed	0.094	0.291	0	1	16218
Potential benefit duration (omitted baseline: 260-520 days):					
$\leq$90 days	0.046	0.210	0	1	16218
>90, $\leq$ 200 days	0.161	0.367	0	1	16218
>200, $\leq$ 260 days	0.230	0.421	0	1	16218
>520 days	0.014	0.118	0	1	16218
Replacement rate (omitted baseline: 75%):					
0%	0.023	0.149	0	1	16218
70%	0.313	0.464	0	1	16218
71-74%	0.056	0.230	0	1	16218
75-79%	0.047	0.212	0	1	16218
Domain of occupation in last job (omitted baseline: admin and office):					
Food and agriculture	0.030	0.171	0	1	16218
Preparation of raw material	0.011	0.106	0	1	16218
Production (blue collar)	0.119	0.324	0	1	16218
Electro & watches	0.005	0.068	0	1	16218
Marketing and print	0.016	0.124	0	1	16218
Chemistry	0.004	0.065	0	1	16218
Engineering	0.017	0.128	0	1	16218
Informatics	0.024	0.152	0	1	16218
Construction	0.144	0.351	0	1	16218
Sales	0.111	0.314	0	1	16218
Tourism, transport, communication	0.045	0.207	0	1	16218
Banking, trust and insurance	0.014	0.118	0	1	16218
Restaurant	0.157	0.363	0	1	16218
Cleaning and personal service	0.042	0.201	0	1	16218
Management and HR	0.034	0.182	0	1	16218
Security and law	0.010	0.102	0	1	16218
Journalism and arts	0.014	0.118	0	1	16218
Social work	0.013	0.113	0	1	16218
Education	0.011	0.106	0	1	16218
Science	0.008	0.090	0	1	16218
Health	0.036	0.187	0	1	16218
Others (skilled)	0.067	0.249	0	1	16218
Missing	0.001	0.029	0	1	16218

Table A.2: Summary Statistics on Covariates: UI Data (2/2)

Variable	Mean	Std. Dev.	Min	Max	Obs
Level of Education (omitted baseline: apprenticeship):					
Minimum education	0.234	0.424	0	1	16218
Short further education	0.062	0.242	0	1	16218
High School	0.040	0.196	0	1	16218
Professional diploma	0.026	0.159	0	1	16218
Applied university	0.042	0.201	0	1	16218
University	0.058	0.233	0	1	16218
Missing	0.075	0.263	0	1	16218
Country of Nationality (omitted baseline: Switzerland):					
France or Italian	0.067518	0.250924	0	1	16218
Portugal, Spain or Greece	0.090085	0.286313	0	1	16218
Baltic States or Turkey	0.123381	0.328885	0	1	16218
nonEU Eastern Europe	0.007893	0.088491	0	1	16218
EU, U.S., Canada	0.091133	0.287808	0	1	16218
African countries	0.023554	0.15166	0	1	16218
Middle and South America	0.017573	0.131398	0	1	16218
Asian countries	0.026884	0.161749	0	1	16218
No of other household members (omitted baseline: none):					
1	0.167715	0.373624	0	1	16218
2	0.118449	0.323149	0	1	16218
3	0.038352	0.192052	0	1	16218
4+	0.012147	0.109545	0	1	16218
Employability assessed by caseworker (omitted baseline: missing):					
Easy	0.025466	0.157539	0	1	16218
Medium	0.12369	0.329237	0	1	16218
Difficult	0.035146	0.184155	0	1	16218
Language region (omitted baseline: German):					
French	0.120853	0.325967	0	1	16218
Italian	0.099026	0.298706	0	1	16218
Rhaeto-Romanic	0.005056	0.070929	0	1	16218
English skills (omitted baseline: none):					
Basic	0.171908	0.377312	0	1	16218
Good	0.350043	0.476998	0	1	16218
Regional mobility (omitted baseline: regional commuter):					
Not mobile	0.036934	0.188606	0	1	16218
Switzerland	0.020964	0.143269	0	1	16218
Switzerland and abroad	0.013195	0.114114	0	1	16218

Table A.3: Summary Statistics on Covariates: Social Security Data

Variable	Obs	Mean	Std. Dev.	Min	Max
Total Earnings:					
3 Months prior to Entry	16218	3265.112	2785.527	0	15000
6 Months prior to Entry	16218	3508.259	2787.299	0	15000
12 Months prior to Entry	16218	3492.239	2706.145	0	15000
18 Months prior to Entry	16218	3529.446	2748.241	0	15000
24 Months prior to Entry	16218	3422.557	2666.492	0	15000
36 Months prior to Entry	16218	3102.258	2696.861	0	15000
Share of Months in Employment:					
3 Months prior to Entry	16218	0.760246	0.389801	0	1
6 Months prior to Entry	16218	0.803305	0.361457	0	1
12 Months prior to Entry	16218	0.812831	0.324226	0	1
18 Months prior to Entry	16218	0.828863	0.318979	0	1
24 Months prior to Entry	16218	0.812379	0.296371	0	1
36 Months prior to Entry	16218	0.767172	0.348181	0	1