Strengthening Enforcement in Unemployment Insurance. A Natural Experiment

Patrick Arni [*] Amelie Schiprowski [†]

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Abstract

Imposing benefit cuts to job seekers who do not comply with rules and requirements has become common practice in unemployment insurance (UI) systems. This paper provides first estimates of how non-compliant job seekers react when confronted with a more or less strict enforcement mechanism. We exploit an administrative reform which induced a sharp and unanticipated increase in the probability of receiving a benefit cut in response to the failure of documenting job search effort. Our difference-in-difference framework uses as a control group job seekers with other types of non-compliances, whose enforcement rules stayed constant. We find that the probability of job finding within the three months following non-compliance detection increases by 5 percentage points in reaction to the reform. This effect is however purely driven by exits to unstable jobs. Increased enforcement strictness thus appears to pressure job seekers into accepting job matches of lower quality. Estimating the effects on post-unemployment earnings is work in progress.

Keywords: Unemployment Insurance, Job Search, Enforcement

JEL Codes: J64, J65

1 Introduction

Unemployment Insurance (UI) systems in the U.S. and Europe increasingly use enforcement mechanisms to ensure that job seekers comply with rules and requirements. Most often, enforcement takes the form of benefit cuts that UI authorities impose in reaction to a non-compliance. The aim is to ensure that job seekers actively search for work, participate at job search counseling and accept available job offers. Thereby, policy makers aim at counteracting potential moral hazard problems associated with the provision of UI benefits. Given the central role that enforcement plays in modern UI regimes, its design is important. The key choice variable is here the strictness of enforcement, which varies largely across OECD countries (Venn 2012). For instance, policy makers can decide to follow a "no excuse" rationale or allow for exceptions and second chances.

[*] IZA Bonn, University of Lausanne (DEEP) and Aarhus University (CAFE)

[†] IZA Bonn, DIW Berlin and University of Potsdam, schiprowski@iza.org

Depending on this choice, the job seeker will face a more or less high probability of sanction given non-compliance.

In case the only aim is to maximize compliance with rules and obligations, the policy maker has a rather unambiguous incentive to choose a strict enforcement regime. However, the enforcement regime can have important side effects on job search and job acceptance behavior: by the threat and incidence of benefit sanctions, a strict enforcement mechanism is expected to lower the value of being unemployed among non-compliant job seekers. This is expected to increase both search effort and the attractiveness of accepting lower quality jobs, possibly raising the rate of job finding and lowering the quality of post-unemployment jobs. Empirical evidence on these effects is however rare, mainly because reforms that introduce or increase the enforcement strictness tend to come along with other changes in the job search monitoring regime, making it impossible to isolate the effect of one policy parameter.

This paper provides first estimates of how enforcement strictness in UI affects job finding and post-unemployment outcomes of non-compliant job seekers. We use variation from a reform which occurred in the Swiss UI. It induced a change from a second chance policy to the direct imposition of benefit cuts in reaction to a job seeker's failure to document search effort by the official deadline. The reform did not explicitly aim at sharpening the enforcement regime, but rather at reducing its administrative burden.[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 of setting second deadlines: a job seeker's only way to avoid a benefit cut was now to have a special reason or circumstance that excused the non-compliance. Due to its unintended nature and sudden implementation, the reform generated a sharp quasi-experimental jump probability of being sanctioned in case of non-compliance detection (from around 30% to 70%). As this jump only affected job seekers with one particular type of non-compliance, job seekers having received another type of non-compliance detection constitute a natural control group. The reform thus generates an ideal setting to evaluate the effect of a strict versus mild response to a first non-compliance by the job seeker.

We use this setting to investigate how the outcomes of job seekers having received a non-compliance notification are affected by the increase in enforcement strictness. To this purpose, we set up a difference-in-difference framework: we compare the outcomes of job seekers who received a treated non-compliance detection after the reform to the outcomes of job seekers having received the same type of detection before the reform. To avoid that confounding time trends drive this difference, we add as a second dimension the pre-post difference in outcomes of the

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

control group, i.e. of job seekers with another type of non-compliance detection. For them, the enforcement mechanism stayed constant around the reform date. Importantly, the reform did in the first months following its implementation not involve any anticipatory behavior, as the share of non-compliance detections of the treatment and control group stayed constant. In addition, we can show that the reform was not associated with any differences in observable characteristics between treated and non-treated job seekers and that there were no time-varying differences in outcomes between treatment and control group prior to the reform. Conditional on our difference-in-difference specification, the reform thus lead to a change in the enforcement regime which quasi-randomly affected some but not other job seekers.

The analysis is carried out using exhaustive administrative data on the population of job seekers entering the Swiss UI during the years 2010 and 2011. Our main analysis focuses on job seekers with non-compliance detections close to the reform date (four pre- and four post-reform months in our baseline specification). We identify substantial effects of the increase in enforcement strictness on job finding. For instance, the linear probability of job finding within the two months after non-compliance detection increased by 5 percentage points. However, the positive job finding effects are purely driven by unstable job matches, which result in the recurrence to unemployment within twelve months after the exit from unemployment. On the contrary, the probability of accepting a stable job does not react to the reform. The results thus confirm that the strictness of enforcement substantially reduces the value of remaining unemployed and thereby increases the rate of exit into temporary jobs. A heterogeneity analysis shows that point estimates are highest for female job seekers and for job seekers in low-skilled occupations. Our estimated coefficients are robust to running proportional hazard regressions, changes of the sampling restrictions, additional time controls and covariates and alternative definitions of the control group. We do not find any effects if we run placebo regressions that artificially re-place the reform date. Estimating the effects on post-unemployment earnings is work in progress.

Our evaluation of a policy change in enforcement rules relates to a set of studies that use timing-of-events techniques to study how the imposition of an individual benefit sanction affects search outcomes (Abbring et al. 2005, Lalive et al. 2005, Van den Berg and Van der Klaauw 2013 Arni et al 2013). By using variation induced from a policy change, we are able to quantify the elasticity of job search outcomes to a change in the probability of enforcement induced by the policy maker. Our analysis also relates to evidence on the effects of introducing or abolishing an entire job search monitoring regime, i.e. the "package" of search requirements, their monitoring and their enforcement (Meyer 1995, Ashenfelter 2005, Van den Berg and Van der Klaauw 2006, McVicar 2008, Petrongolo 2008, Manning 2009). In contrast to these studies, we identify the effect of one policy parameter within the regime.

Further, the estimates in this paper add to a recent discussion on the effect of benefit reductions on reservation wages. Schmieder et al. (2015) find that reservation wages do surprisingly not react to benefit generosity. This result raises the question whether non-binding reservation wages are a general rule, making it relevant to evaluate the effects of benefit cuts on post-unemployment earnings in different policy contexts. We consider such an alternative context, as we do not analyze the effects of general benefit generosity, but those of benefit cuts that incur in reaction to a non-compliance. Our estimates of wage effects are work in progress.

This paper is structured as follows: section 2 lays out the institutional framework and enforcement regime of the Swiss UI. It also describes the reform which we use for identification. In section 3, we describe our data sources and sampling criteria. Section 4 presents the econometric analysis, including a discussion of estimation results. We provide tests of our identifying assumption and robustness of results in Section 5. Section 6 concludes.

2 Institutional Setting and Reform

The Swiss Unemployment Insurance (UI) System In Switzerland, job seekers are entitled to UI benefits if they meet two main prerequisites. First, they must have contributed for at least six months during the two previous years. To be eligible for the full benefit period, the contribution period extends to 12 or 18 months, depending on the individual situation. Second, job seekers must be able to be "employable" in a regular job. If these criteria are not met, there is the possibility to collect social assistance. The potential duration of unemployment benefits is two years for eligible job seekers. The replacement ratio is between 70% and 80% of previous earnings, depending on the individual family situation and the level of past earnings.

The organization of counseling and monitoring is ensured by the Public Employment Service (PES) offices, which are the organizational unit of caseworkers.

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

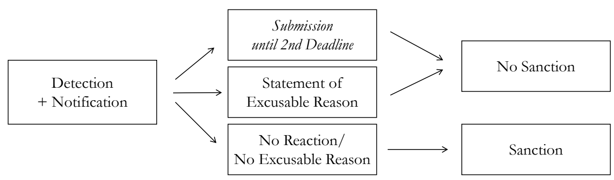
We will analyze a reform in the enforcement of search requirements. During their first contact with the caseworker, job seekers are informed about the number of monthly applications they have to send out in order to comply with their individual job search requirement. Job seekers document this monthly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

Reform in the Enforcement Regime The enforcement regime enters into place if a job seeker does not comply with one of the obligations foreseen by the UI rules. If the caseworker detects a non-compliance, he/she has to register it in the computer system. This opens an enforcement process that can lead to the imposition of a benefit sanction. Sanctions cut benefit levels to zero during a limited number of days (usually around 5-10 days, c.f. section 3).

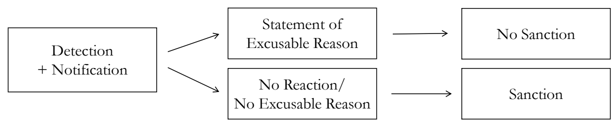
We will analyze a policy change in the process that links the detection of a non-compliance to the imposition of a sanction. This policy change abolished the accordance of second chances to job seekers who have not handed in their “protocol of search effort” by the official deadline. In the pre-reform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

Figure 1: Enforcement Process Pre and Post Reform

(a) Pre Reform



(b) Post Reform



In April 2011, the federal ministry abolished the practice of setting second deadlines. The intention of this regime change was of purely administrative nature: it aimed at reducing the organizational burden of the enforcement regime.[2] The reform became effective for protocols reporting on job applications submitted in April 2011 or later, which implied that from May 2011 (i.e. the deadline for protocols referring to April) onward, non-compliance notifications did

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

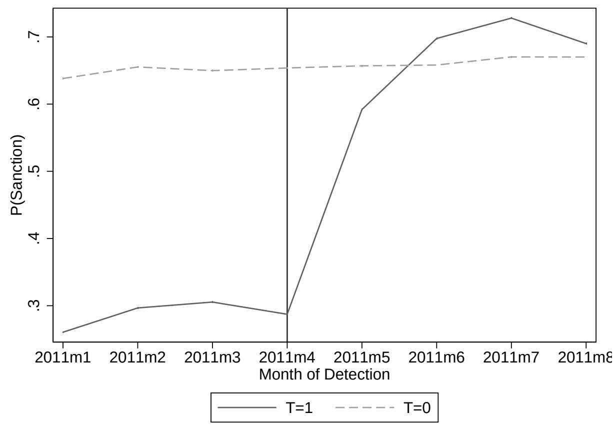
no longer set a second deadline. Instead, they only gave job seekers the possibility to state the reasons of their non-compliance and informed them that a sanction would enter into place if no excusable reason could be stated (c.f. Figure 1b).

Figure 2a shows that the abolishment 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 probability of receiving a benefit sanction conditional upon receiving a notification jumped sharply by around 100%, from 0.35 to 0.7.[3] 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 had always taken the form illustrated in Figure 1b.[4] Note from Figure 2b that the share of job seekers in the treatment group among all non-compliance notifications stayed constant around the reform date.

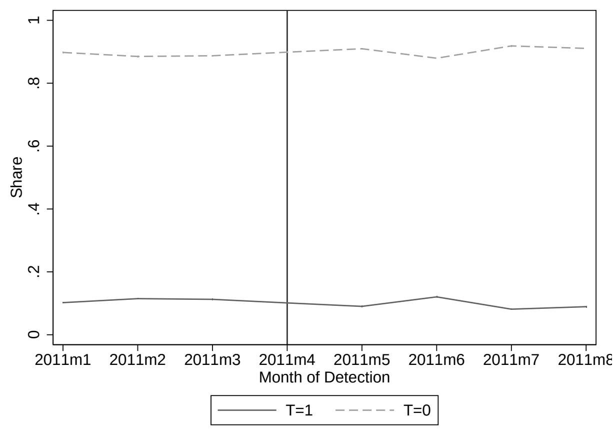
[3] Recall that job seekers can post-reform still avoid being sanctioned by stating an "excusable reason" for not having submitted the protocol. This is why the probability does not jump to 1.

[4] 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.

(a) Probability of Sanction after Notification for Treatment and Control Group



(b) Shares of Non-Compliance Notifications, Treatment and Control Group



In the context of our analysis, it will be crucial to assume that the reform did not affect the job seekers' compliance behavior itself and thus did not change the composition of non-compliant job seekers. Several reasons make us believe that job seekers did not anticipate the reform when making their compliance decision: first, the reform aimed at reducing the bureaucratic burden of the enforcement regime and was therefore of purely administrative nature. It therefore did not raise any announcements concerning a regime change that would generate additional benefit sanctions. Second, the change occurred within a larger reform package whose principal element was to reduce the potential duration of benefit payments for certain job seekers. Compared to these reforms, the policy change in the enforcement rules was of minor nature. The PES and caseworkers were thus occupied with more pressing issues. We have access to the powerpoint presentation that was used to communicate the reform package to caseworkers. The reform we analyze did not even appear there [CHECK!]. Third, the final enforcement decision is not taken by the caseworkers themselves, but by a higher authority in the PES. As a consequence, the policy change was not very present to the caseworkers and it is unlikely that they actively advised job seekers to change their compliance behavior around the reform date.

As a consequence, we retain the assumption that the policy change came as a surprise to job seekers with a detected non-compliance shortly after the reform date. In Section 5, we will test whether the reform induced any changes in observable job seeker characteristics.

3 Data and Descriptive Statistics

3.1 Data Sources and Sampling

Data Sources We use administrative data on the full population of Swiss job seekers entering into formal unemployment. The data base includes extensive information on entry and exit into unemployment, socio-demographic characteristics and employment history. We further know which Public Employment Service (PES) and caseworker the job seeker was assigned to. 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 made 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 whether a sanction was imposed. Our analysis will be concerned with the behavior of job seekers who receive at least one non-compliance notification. This is the case for about 40% of the full population of job seekers registered in UI.

Sampling Criteria The official procedure for imposing benefit sanctions asks the caseworker to enter the date at which a non-compliance was detected and communicated to the job seeker, the date at which the job seeker gave a statement and 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. While there may be cases in which the job seeker refuses to make a statement, these should not be systematic according to the procedures prescribed by the federal ministry. For cantons that do not follow these procedures, we are not able to extract the information necessary for our analysis and we do not know whether the federal prescriptions on the enforcement process are respected. We exclude cantons for which more than a quarter of enforcement cases do not report a job seeker statement.[5] According to this sample restriction, we include 14 out of 26 cantons in our data set, which corresponds to 65% of registered enforcement

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

cases.[6]

We further make standard sampling restrictions by excluding job seekers younger than 20 or older than 55 years, part-time unemployed job seekers and job seekers eligible for disability insurance benefits.

The sample period is defined based on the month of notification, which also defines the job seeker's treatment status. We want to avoid that anticipatory behavior affects the selection into non-compliance and therefore use in our baseline analysis a sample of notifications that were issued close to the reform date, during four pre- and four post-reform months. In Section 5.1, we use additional pre-reform months to document the absence of any diverging pre-trends. In Section 5.3, we show that the baseline results are robust to modifications of the sample period.

Most first non-compliance notifications occur during the beginning of the unemployment spell. In order to achieve a relatively homogeneous sample, we include in our baseline sample only job seekers who received their first notification during the first 120 days of formal unemployment (80% of all first notifications). We show in Section 5.3 that our results are invariant to modifying this sampling cutoff.

Finally, we exclude from the sample non-compliances that concern the refusal of acceptable job offers (3% of notifications in our sample) because they generate on average a benefit sanction that is four times higher than for the other enforcement types. They are thus likely to concern special cases and not suitable as part of the control group.

3.2 Treatment Status Definition and Data Description

Job seekers are assigned to the treatment group if they receive a notification that their search protocol has not been submitted by the deadline. Job seekers with another type of non-compliance notification are assigned to the control group. A considerable share of non-compliant job seekers receives at least one additional notification during their spell (39.4%). We assume that a job seeker's first notification determines his/her perception of the enforcement regime and define the treatment status based on this first notification. Table 1 shows how the different types of non-compliance notifications are distributed in the estimation sample, pre and post the reform date. The share of the treatment group is about 10%, both before and after the reform. 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 – thus even before receiving a fixed search requirement. At the first official meeting, the caseworker asks for the job seeker's previous job search activities.

[6] 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 by Lalive et al. (2005) and Arni et al. (2011), who cover respectively 3 and 7 cantons.

and assesses whether they were sufficient. If previous job search activities were insufficient from the caseworker's point of view, a non-compliance is registered. It is mechanical that these cases dominate the distribution of first non-compliance notifications, as they are usually registered at the first caseworker meeting, i.e. about three weeks after registration. Our robustness analysis in section 5.3 will show that results are unaffected when we exclude this type of notification from the estimation sample.

Table 1: Non-Compliance Notifications Pre and Post Reform

Reason of Non-Compliance Notification	N_{pre}	% of sample pre	N_{post}	% of sample post
Search protocol absent at deadline (T=1)	1023	10.75%	642	9.44%
Insufficient search effort before first meeting	5219	54.86%	3946	58.01%
Non-compliance with search requirement	1079	11.34%	492	7.23%
Delay or absence at caseworker meeting	1745	18.34%	1322	19.44%
Other	447	4.70%	400	5.88%
Total T=0	8490	89.25%	6160	90.56%
Total	**9513**		**6802**	

"Other" reasons of a non-compliance notification are the non-participation at an active labor market program or the failure to comply with orders by the PES.

Table 2 shows how different features of the enforcement regime changed in response to the reform. It shows simple differences-in-differences 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 case of enforcement.

Clearly, the only substantial effect of the reform on enforcement practices concerns the sanction probability of a notification. 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 negative difference-in-difference in the number of days between registration into unemployment and the incidence of the first notification. Our econometric framework will take this into account and ensure that the timing of notification does not influence the estimates. The reform is associated with a slight decrease in the sanction amount by the equivalent of .67 of a day of UI The duration from notification to sanction in case of enforcement stayed stable.

Table 2: Enforcement Regime Pre and Post Reform

		Before	After	Difference
P(Sanction)	T=1	0.285	0.673	0.387
	T=0	0.648	0.664	0.016
	Difference	-0.363	0.009	**0.371**
Days to Notification	T=1	63.607	65.883	2.276
	T=0	35.051	32.297	-2.755
	Difference	28.556	33.587	**5.031**
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	T=1	6.880	6.157	-0.723
	T=0	7.141	7.094	-0.047
	Difference	-0.260	-0.936	**-0.676**

4 Econometric Analysis

We exploit the reform described in Section 2 as a local and unanticipated shock that affected job seekers whose first non-compliance detection concerned a job search protocol not handed in by the deadline: these job seekers suddenly faced a no-excuse policy and were most likely going to receive a benefit sanction. The control group are job seekers with another type of non-compliance, where enforcement strictness stayed constant. Our baseline specifications include job seekers with notifications issued in one of the four pre- and post-reform months, as job seekers close to the reform date are most likely to experience the policy change as a surprise (c.f. discussion in section 2). In Section 5, we will extend the pre-reform period by two years to check the assumption of common pre-trends.

4.1 Econometric Framework

We set up a difference-in-difference specification, essentially 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. We estimate the reform effect on a set of linear outcomes using OLS and on a set of duration outcomes using proportional hazards regressions. The underlying identifying assumption of the difference-in-difference specification –in particular common time trends and the absence of compositional changes– in Section 5.

Linear Outcomes In our baseline approach, we are interested in the reform effect on a linear outcome y of individual i. The main outcomes of interest are the probabilities of realizing certain unemployment exits (job finding, stable job finding or unstable job finding) within 1/2/3 months after receiving a non-compliance notification. We also estimate effects on the probability of realizing these exits within 6 months after registration into unemployment. We first specify these outcomes as linear probabilities and estimate the following difference-in-difference equation using OLS:

$$y_i = \alpha + \delta^* reform_i + \gamma^* T_i + \eta_t + \xi_{t_w} + \lambda_{t,t_w} + \pi_{PES} + x_i'\beta + u_i \tag{1}$$

The indicator variable $reform_i$ is set equal to one if the job seeker's first non-compliance detection was affected by the reform. This is the case if it concerned the absence of the search protocol at the deadline and was registered after April 2011. The coefficient of interest $\hat{\delta}$ measures the effect of being affected by the reform.

T_i and η_t are the basic second order terms of the difference-in-difference framework. The indicator T_i is set equal to one if an individual is in the treatment group; $\hat{\gamma}$ thus measures time-constant differences between the treatment and the control group. η_t is a vector of fixed effects for the job seeker's month of notification. It holds constant all group-constant time effects.

Two additional vectors of time controls address that not all job seekers receive their non-compliance notification at the same moment during their 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 job seeker's 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 is not confounded by changes in the duration to notification. Further, the interaction between the month of notification and the time from registration to notification determines a job seeker's calendar time of registration. λ_{t,t_w} thus also controls for potential compositional effects due to different inflow dates.

π_{PES} includes fixed effects for the Public Employment Service (PES) which the individual registered at. In parts of our regressions, we control for a substantial set of individual-specific covariates x_i (socio-demographics, education, employment and unemployment history). We show however that our results are not significantly affected by the introduction of x_i.

Duration Model In order to take duration dependence into account, we run a second set of estimations on the log hazard ln θ to exit destination e, ln θ^e. θ^e can be the hazard from registration to destination e, or the hazard from non-compliance notification to destination e.

We specifiy ln θ^e as:

$$ln\ \theta^e = ln\ \lambda(t_e) + \delta * reform_i + \gamma * T_i + \eta_t + \xi_{t_w} + \lambda_\tau + \pi_{PES} + x_i'\beta \quad (2)$$

The identifying variables are as in the OLS specification. We add the log of $\lambda(t_e)$, which models duration dependence as piece-wise constants, using a step function:

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

where k(= 1, . . . , K) is a subscript for time-intervals and $I_k(t)$ are time-varying dummy variables for time intervals of 30 days each.[7] Depending on the outcome and on the censoring choice, K takes different values. As we estimate a constant term, we always normalize $\lambda(t_{e,1})$ to be 0.

4.2 Estimation Results

4.2.1 Main Results

Probability of Job Finding Table 3 reports estimated effects of the reform on the probability of job finding within 1/2/3 months after having received a notification on non-compliance.[8] This probability reacts substantially to the reform-induced increase in enforcement strictness: for instance, treated job seekers are after the reform 5.7 percentage points more likely to exit to employment within 3 months after receiving the notification of non-compliance, relative to an outcome mean of .30.

To gain evidence on the dimension of job quality, we split the outcome into unstable and stable job finding. Unstable job finding is coded as one if the job seeker registers back into unemployment within the 12 months following exit from unemployment. Stable job finding is coded as one if the job lasts at least 12 months. The result is striking: stable job finding does not react at all to the changed enforcement strictness (Table 4). The effect is solely driven by exits to jobs which lead to the recurrence into unemployment, as shown by Table 5.

Duration to Job Finding Results on the duration to job finding the same picture. Table 6 reports regressions results on the hazard from non-compliance notification to job finding, stable job finding and unstable job finding. We right-censor durations 3 months after non-compliance. [Describe and Interpret results]

Table 7 reports coefficients on the hazard from registration at the PES to job finding, stable job

[7] When we estimate effects on the duration to a second notification, time intervals are 10 days each.

[8] Job finding is coded as one if a job seeker's reason of de-registration is the acceptance of a job found by the job seeker.

finding and unstable job finding. We right-censor durations 6 months after registration. [Describe and Interpret]

Alternative choices of exogenous right-censoring do not affect this picture.[9] Note that the duration to unstable job finding is endogenously right-censored by the competing outcome of stable job finding and vice-versa. Addressing this by specifying a competing-risks model with correlated unobserved heterogeneity terms would however require sensitive functional form assumptions.

4.2.2 Heterogeneity

To assess whether the effects of an increased enforcement strictness are driven by certain types of job seekers, we run one of our main regressions separately for different subgroups. Table 8 reports effects on the probability of job finding within the three months following non-compliance detection by gender, education and the job seeker's function in the last job. Table 10 shows the same heterogeneity analysis for the probability of unstable job finding within the three months following non-compliance detection. Point estimates are higher for female job seekers as well as job seekers with low education levels and whose previous function was a lower one.

4.2.3 Subsequent Compliance Behavior

The primary aim of an enforcement regime is to enhance the compliance with rules. It is thus informative to assess whether job seekers who experience a strict enforcement regime decide to comply more afterwards.

We can only provide tentative evidence on how the reform affected subsequent compliance behavior. The reason is that the outcome of a second non-compliance is endogenous to the job seeker's job finding behavior: we have found that the reform increases the rate with which job seekers find a job. Therefore, job seekers affected by the reform are on average less at risk of a second non-compliance. Nevertheless, we propose an indicative analysis of how the reform affected non-compliance behavior in the month following the first notification. In Table **??**, columns (1) and (2), we regress equation 1 where the outcome is the probability of job finding within the month following the first notification. For job seekers affected by the reform, this probability is reduced by around 5.7 percentage points.

In columns (3) and (4), we report coefficients on the hazard of hazard of receiving a second non-compliance notification following the first one (c.f. equation2). A job seeker's "non-compliance spell" is censored one month following the first non-compliance. If the job seeker's exit to unemployment occurs before job seekers is censored at the date of exit. Coefficients again suggest that the hazard of a second non-compliance is reduced substantially by the reform, by around 27%

[9]Documentation available upon request.

(=exp(-.322)-1).

Due to the mentioned endogeneity problem, these results however need to be interpreted with caution.

5 Identification Tests and Robustness Analysis

The key identification assumption behind our Difference-in-Difference analysis is that the difference in outcomes between treated and non-treated job seekers is solely driven by the reform-induced change in the enforcement regime. This assumption is by definition not testable, but we can perform several tentative checks and tests.

In the following, we first test whether the reform changed the selection of job seeker 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 parallel during the pre-reform period. We also report results from placebo regressions in which we artificially place the reform date one year pre and one year post the true reform date. Third, we validate the robustness of our results to alternative sample restrictions and alternative definitions of the control group.

5.1 Compositional Changes

One central concern in our context are potential differences in composition between treated and non-treated job seekers pre and post the reform date. If job seekers anticipate the reform, they may take it into account in their compliance decision. In such a scenario, different types of job seekers would select into the non-compliance.

To check whether the reform induced changes in the composition of job seekers, we regress different observable characteristics on the basic difference-in-difference framework (equation (1) 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 composition of non-compliant job seekers that bias our estimates.

5.2 Common Trend and Placebo Regressions

Our baseline sample only included job seekers who received a notification of non-compliance during the four pre- and post-reform months. To test the parallel trend assumption, we now extend the sample start up to notifications issued in January 2009. This allows checking whether the outcomes of treatment and control group evolved similarly during the two pre-reform years.

We replace the $reform$-dummy in our main equation by the interaction term $T_i^* \kappa_{per}$, which measures the effect of being in the treatment group and receiving a notification in a given four-month period. This results in the following equation (hazard regressions take the functional form specified in equation 2):

$$y_i = \alpha + \delta_\kappa^* T_i^* \kappa_{per} + \gamma^* T_i + \eta_t + \xi_{t_w} + \lambda_\tau + \pi_{PES} + u_i$$

If the assumption of parallel pre-trends hold, $\hat{\delta}_\kappa$ should only be significant for the post-reform period May-August 2011. Figure 3 plots $\hat{\delta}_\kappa$ for our main outcomes, the reference period being the four pre-reform months January-April 2011. None of the graphs suggests the presence of any diverging pre-trends between treatment and control groups during the two years preceding the reform.[10]

As an additional check, Table 12 reports results from placebo regressions, with the reform date artificially placed one/two calendar years before the actual reform date. The estimation period is thus January to August 2010/January to August 2009. Outcomes are the probability of job finding, stable job finding and unstable job finding within 3 months following notification. We do not find any effects of the "placebo reform" on these outcomes.[11]

5.3 Robustness

We now test the robustness of our estimates to alternative sampling choices. The outcome of reference is the probability of job finding within three months after notification.[12]

Column (1) recalls our baseline estimate. Column (2) extends the sample by including job seekers who received their first notification up to 180 days after the start of their unemployment spell (instead of 120). Column (3) reduces the sampling window to notifications sent out up to July 2011 and column (4) extends the sampling window to notifications sent out up to September 2011. In column (5), we exclude from the control group notifications that relate to the job seeker's effort before his/her first caseworker meeting, as these notifications are issued on average about a month earlier than the others.

None of the tests leads to significant changes in the estimated coefficients.

[10] Regressions with covariates give the same picture. Documentation available upon request.
[11] This holds for all other outcomes considered in this paper. Documentation available upon request.
[12] Results hold for all other outcomes considered in this paper. Documentation available upon request.

6 Conclusion

[Preliminary]

We provide first quasi-experimental evidence on the effects of increased enforcement strictness in UI. The studied reform increased the speed of job finding, but only into unstable job matches. This shows that a high enforcement strictness lowers job seekers' value of being enrolled into UI and thereby induces changes in search outcomes. These findings suggest that the job seeker's reservation value is affected by the potential incidence of benefit cuts induced by a stricter enforcement regime. They need to be complemented by estimates on post-unemployment earnings, but they already suggest that non-binding reservation wages, such as identified by Schmieder et al. (2015) are not a general rule. For instance, behavioral mechanisms may drive the fact that enforcement-related benefit cuts affect the reservation value of job seekers: they are unanticipated, i.e. occur as a "shock" and they may induce stigma effects as they are officially labeled as benefit sanctions.

It is important to keep in mind that the effect is driven by two changes: an increase in the number of job seekers who actually experienced a benefit cut and a change in perception of the enforcement regime for all job seekers with a non-compliance detection in the treatment group. As a limitation of our study, we cannot separately identify these two channels. However, a policy change in enforcement strictness will always entail both mechanisms; one can thus not be generated without the other.

This paper is work in progress, as we plan to add estimates on the effects on post-unemployment earnings.

Tables and Figures

Table 3: Probability of Job Finding within 1/2/3 Months after Notification

	(1) 1 Month	(2) 1 Month	(3) 2 Months	(4) 2 Months	(5) 3 Months	(6) 3 Months
reform=1	0.040**	0.037**	0.048**	0.049**	0.053**	0.057**
	(0.018)	(0.017)	(0.021)	(0.021)	(0.024)	(0.022)
T=1	0.016	0.015	0.021	0.018	0.001	-0.001
	(0.012)	(0.012)	(0.015)	(0.014)	(0.015)	(0.014)
Outcome Mean	0.107	0.107	0.220	0.220	0.301	0.301
Covariates	NO	YES	NO	YES	NO	YES
Observations	16315	16315	16315	16315	16315	16315

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 1 using OLS and include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, employment and unemployment history.

Table 4: Probability of Stable Job Finding 1/2/3 Months after Notification

	(1) 1 Month	(2) 1 Month	(3) 2 Months	(4) 2 Months	(5) 3 Months	(6) 3 Months
reform=1	0.005	-0.001	0.001	-0.005	0.005	0.001
	(0.020)	(0.020)	(0.026)	(0.024)	(0.028)	(0.026)
T=1	0.042***	0.039***	0.052***	0.046***	0.040**	0.031*
	(0.014)	(0.013)	(0.016)	(0.015)	(0.017)	(0.016)
Outcome Mean	0.122	0.122	0.252	0.252	0.356	0.356
Covariates	NO	YES	NO	YES	NO	YES
Observations	16315	16315	16315	16315	16315	16315

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 1 using OLS and include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, employment and unemployment history. Stable job finding is coded as one if a job seeker finds a job and does not re-enter UE for at least 12 months.

Table 5: Probability of Unstable Job Finding within 1/2/3 Months after Notification

	(1) 1 Month	(2) 1 Month	(3) 2 Months	(4) 2 Months	(5) 3 Months	(6) 3 Months
reform=1	0.026**	0.023**	0.045***	0.041***	0.045***	0.040***
	(0.011)	(0.011)	(0.013)	(0.012)	(0.014)	(0.013)
T=1	0.007	0.005	0.000	-0.004	-0.004	-0.009
	(0.006)	(0.007)	(0.009)	(0.009)	(0.010)	(0.010)
Outcome Mean	0.037	0.037	0.074	0.074	0.101	0.101
Covariates	NO	YES	NO	YES	NO	YES
Observations	16315	16315	16315	16315	16315	16315

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 1 using OLS and include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, employment and unemployment history. Unstable job finding is coded as one if a job seeker finds a job and re-enters UE within the following 12 months.

Table 6: Duration from Notification to Job Finding

	(1) All	(2) All	(3) Unstable	(4) Unstable	(5) Stable	(6) Stable
treat	0.264***	0.260***	0.594***	0.487***	0.099	0.102
	(0.101)	(0.093)	(0.144)	(0.138)	(0.117)	(0.116)
typtr	0.033	0.034	-0.017	-0.022	0.058	0.082
	(0.058)	(0.059)	(0.092)	(0.095)	(0.077)	(0.079)
Covariates	NO	YES	NO	YES	NO	YES
Observations	16315	16315	16315	16315	16315	16315
Exits	4917	4917	1659	1659	3292	3292

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 2 and include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, employment and unemployment history. UE spells censored 90 days aftere notification. Stable job finding is coded as one if a job seeker finds a job and does not re-enter UE for at least 12 months. Unstable job finding is coded as one if a job seeker finds a job and re-enters UE within the following 12 months.

Table 7: Duration from Registration to Job Finding

	(1) All	(2) All	(3) Unstable	(4) Unstable	(5) Stable	(6) Stable
treat	0.209**	0.212**	0.490***	0.404***	0.072	0.077
	(0.099)	(0.093)	(0.139)	(0.128)	(0.116)	(0.113)
typtr	0.027	0.017	-0.013	-0.053	0.045	0.067
	(0.052)	(0.053)	(0.081)	(0.088)	(0.071)	(0.073)
Covariates	NO	YES	NO	YES	NO	YES
Observations	16315	16315	16315	16315	16315	16315
Exits	6174	6174	2038	2038	4136	4136

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 2 and include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, employment and unemployment history. UE spells are censored after 180 days since registration. Stable job finding is coded as one if a job seeker finds a job and does not re-enter UE for at least 12 months. Unstable job finding is coded as one if a job seeker finds a job and re-enters UE within the following 12 months.

Table 8: Heterogeneity: Job Finding within 3 Months after Notification

	(1) Male	(2) Female	(3) Low Education	(4) High Education	(5) Low Function	(6) High Function
reform=1	0.053*	0.081**	0.063*	0.047	0.055*	0.045
	(0.027)	(0.038)	(0.032)	(0.030)	(0.032)	(0.032)
T=1	0.014	-0.039	-0.014	0.012	-0.027	0.028
	(0.018)	(0.024)	(0.020)	(0.019)	(0.024)	(0.018)
Outcome Mean	0.309	0.289	0.253	0.330	0.250	0.332
Covariates	YES	YES	YES	YES	YES	YES
Observations	10264	6051	6071	10244	6164	10151

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 1 using OLS and include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, employment and unemployment history.

Table 9: Heterogeneity: Unstable Job Finding within 3 Months after Notification

	(1) Male	(2) Female	(3) Low Education	(4) High Education	(5) Low Function	(6) High Function
reform=1	0.031*	0.048*	0.041*	0.032*	0.053*	0.021
	(0.017)	(0.025)	(0.023)	(0.018)	(0.027)	(0.020)
T=1	-0.004	-0.006	-0.015	-0.002	-0.024	0.007
	(0.011)	(0.019)	(0.016)	(0.013)	(0.019)	(0.014)
Outcome Mean	0.114	0.079	0.119	0.090	0.113	0.094
Covariates	YES	YES	YES	YES	YES	YES
Observations	10264	6051	6071	10244	6164	10151

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions estimate equation 1 using OLS and include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, employment and unemployment history. Unstable job finding is coded as one if a job seeker finds a job and re-enters UE within the following 12 months.

Table 10: Second Non-Compliance Notification within 1 Month

	(1) Linear Probability	(2) Linear Probability	(3) Duration	(4) Duration
reform=1	-0.048*	-0.057**	-0.274*	-0.322**
	(0.024)	(0.024)	(0.153)	(0.156)
T=1	0.031*	0.030**	0.163*	0.157*
	(0.016)	(0.015)	(0.092)	(0.086)
Covariates	NO	YES	YES	YES
Observations	16315	16315	16315	16315
Outcome Mean	0.170	0.170		
Exits			2770	2770

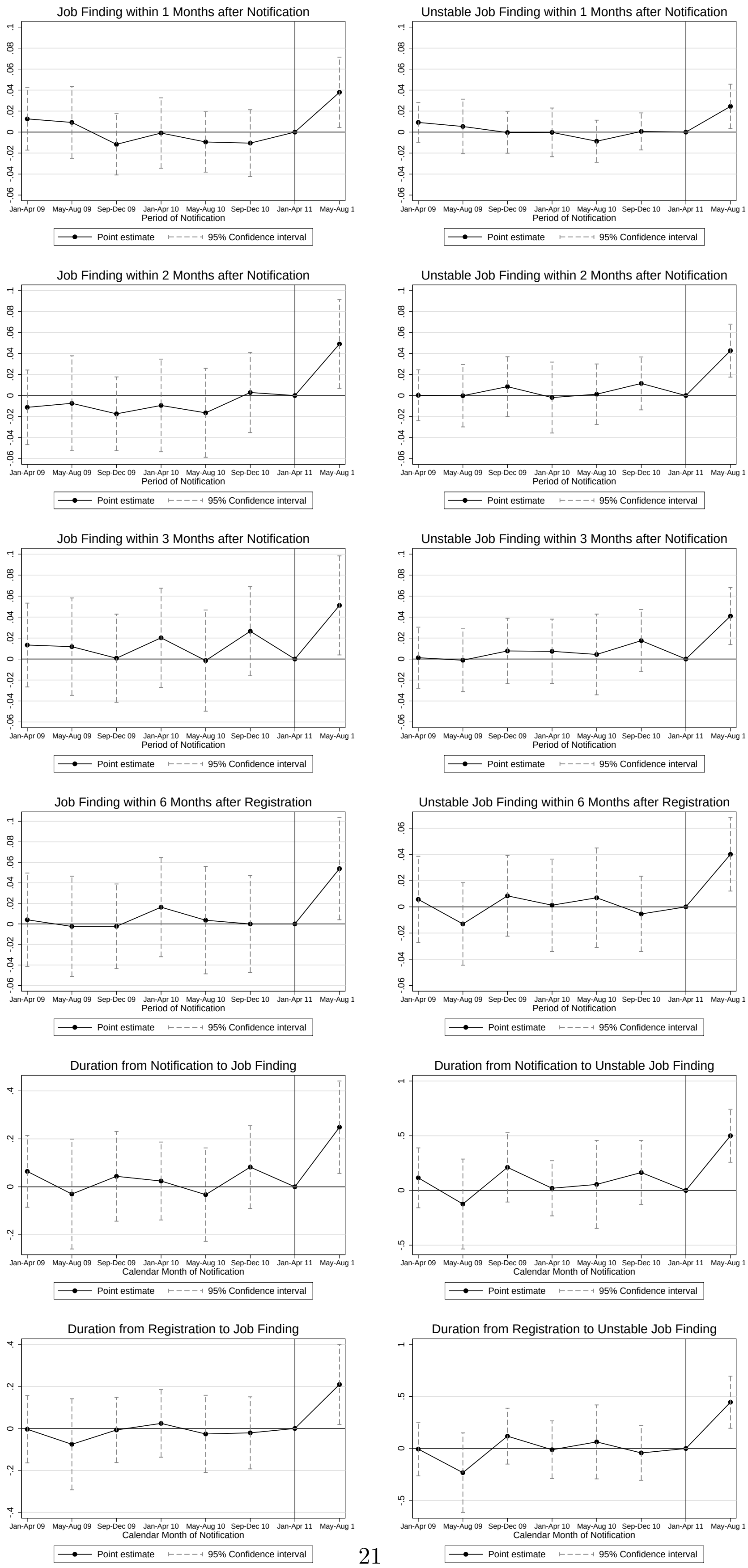
$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. Columns (1) and (2) are based on estimates of equation 1 using OLS and including all fixed effects. Columns (3) and (4) are based on estimates of equation 2 and include all fixed effects. Columns with covariates additionally control for the job seeker's socio-demographics, employment and unemployment history.

Table 11: Testing for Composition Changes

	(1) Log Prev Earnings	(2) Unlearned	(3) UE within last 12 mon	(4) Female	(5) Married	(6) Age
reform=1	-0.028	-0.020	0.027	-0.015	0.007	-0.909
	(0.026)	(0.031)	(0.027)	(0.026)	(0.024)	(0.563)
T=1	0.023	-0.023	0.123***	-0.025	-0.000	0.329
	(0.015)	(0.015)	(0.020)	(0.017)	(0.015)	(0.424)
Outcome Mean	8.294	0.372	0.329	0.371	0.336	32.896
Covariates	NO	NO	NO	NO	NO	NO
Observations	16315	16315	16315	16315	16315	16315

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions include all fixed effects included in equation (1). In Columns (2) to (5), the outcome is specified as a 0/1 variable.

Figure 3: Common Trend



(Graphs display the estimated difference in outcomes between treatment and control group for four-months intervals between January 2009 and August 2011. Variables are as specified in equations 1 and 2, excluding covariates.)

Table 12: Probability of Job Finding (All/Unstable/Stable) within 3 Months after Notification; "Placebo Reform" in 2009 and 2010

	(1) 2009, All	(2) 2009, Unstable	(3) 2009, Stable	(4) 2010, All	(5) 2010, Unstable	(6) 2010, Stable
treat	-0.011	-0.027	-0.007	-0.009	-0.001	-0.019
	(0.024)	(0.017)	(0.030)	(0.025)	(0.021)	(0.022)
typtr	0.024	0.018	0.030*	0.013	0.005	0.030*
	(0.015)	(0.013)	(0.015)	(0.016)	(0.012)	(0.017)
Outcome Mean	0.251	0.090	0.272	0.282	0.091	0.322
Covariates	YES	YES	YES	YES	YES	YES
Observations	17689	17689	17689	17255	17255	17255

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions include all fixed effects included in equation (1). Columns with covariates additionally control for PES effects as well as for the job seeker's socio-demographics, employment and unemployment history. The estimation period is respectively January to August 2009/ January to August 2010; the "placebo" reform dates are May 1 2009/ May 1 2010.

Table 13: Probability of Job Finding within 3 Months after Notification; Robustness Analysis

	(1) Baseline	(2) Time to Notification	(3) Smaller Sampling Window	(4) Larger Sampling Window	(5) Altern. Control Group
reform=1	0.057**	0.044**	0.049*	0.046**	0.065***
	(0.022)	(0.020)	(0.025)	(0.021)	(0.022)
T=1	-0.001	0.002	-0.006	-0.000	-0.002
	(0.014)	(0.013)	(0.014)	(0.014)	(0.015)
Outcome Mean	0.301	0.297	0.307	0.295	0.286
Covariates	YES	YES	YES	YES	YES
Observations	16315	17623	14401	18427	7150

$*p < 0.10, **p < 0.05, ***p < 0.01$. Standard errors are clustered at the PES level. All regressions include all fixed effects included in equation (1) and control for PES effects as well as for the job seeker's socio-demographics, employment and unemployment history. Column (2) extends the sample by including job seekers who received their first notification up to 180 days after the start of their unemployment spell (instead of 120). Column (3) reduces the sampling window to notifications sent out up to July 2011 and column (4) extends the sampling window to notifications sent out up to September 2011. Column (5), excludes from the control group notifications that relate to the job seeker's effort before his/her first caseworker meeting.

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