

Threat Effects of Monitoring and Unemployment Insurance Sanctions: Evidence from Two Reforms*

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Abstract

This paper provides the first quasi-experimental estimates of the threat of unemployment insurance (UI) benefit sanctions on job exit rates. Using a difference-in-differences design, I exploit two reforms of the Swedish UI system that made monitoring and sanctions considerably stricter at different points in time for i) UI claimants and ii) jobseekers who exhausted their UI benefits and therefore receive alternative “activity support” benefits instead. Results show that men (in particular if long-term unemployed) respond to monitoring and the threat of sanctions by finding jobs faster. I find no significant responses for women. In contrast to this analysis, the existing literature has almost exclusively focused on estimating how job-finding rates respond for those actually receiving a sanction. I estimate such “sanction imposition effects” and find that they are similar in size for men and women. I further show that properly aggregated sanction imposition effects explain very little of the overall reform effects for men, and that they are sufficiently small to be consistent with the small and insignificant reform effects found for women. The fact that most of the effects of the reforms arise through the threat component and not through the sanction imposition effects implies that the total impact of monitoring and sanctions may be severely underestimated when focusing solely on the effects on those actually receiving sanctions.

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1 Introduction

Unemployment Insurance (UI) systems provide an important safety net in all developed countries by replacing forgone labor earnings for workers who involuntarily lose their jobs. However, just as for any insurance scheme, UI systems may induce moral hazard.¹ In the case of UI systems, moral hazard may arise in the form of reduced job search. In order to reduce moral hazard, and thereby be able to provide more insurance without adverse labor market consequences, many countries have resorted to the use of monitoring and sanction schemes. In this paper, I provide what I argue to be the most comprehensive set of estimates yet on the effectiveness of such policies.

A useful starting point when thinking about monitoring and sanctions policies is to note that attempts to deter the misuse of the UI systems through such policies closely resemble attempts to prevent crime through punishment within criminal prosecution systems. In the crime literature, the terms *deterrence* or *threat effect* refer to the change of behavior due to the fear that a given conduct will be sanctioned; *sanction imposition effect*, instead, denotes the change of behavior deriving from the actual experience of punishment.² This literature has found that policies based on deterrence can be effective in reducing crime, especially in the case of swift-and-certain punishment regimes that provide salient incentives (see e.g., Weisburd, Einat, and Kowalski, 2008, and Hawken and Kleiman, 2009).³ Most attention has been paid to deterrence for two main reasons: first, deterrence can directly modify the behavior of *all* individuals eligible for sanctions (not just of those actually sanctioned); second, deterrence has substantial cost-saving potential compared to the actual sanction imposition, since it does not even require that offenders are identified.

Deterrence is however absent from almost all studies in the context of UI systems. These have instead analyzed the effect of imposing monetary fines (benefit sanctions) on the individuals actually sanctioned. This paper brings together the insights of the crime and UI literatures. Starting from the idea that deterrence is central also in UI systems, I study both threat and sanction imposition effects in the same policy setting. In doing so, I provide the first estimates of threat effects of stricter monitoring and UI benefit sanctions through a quasi-experimental design.

¹ UI systems may also be associated with adverse selection problems, although this has been emphasized less in the literature (for an example of this see Landais, Nekoei, Nilsson, Seim, and Spinnewijn, 2017).

² Conceptually, crime deterrence can be seen in an expected utility framework where higher probability of apprehension and higher sanction size reduce the value of misbehaving (Becker, 1968). A similar setting applies to UI systems where jobseekers are monitored and a lack of job search leads to monetary fines.

³ For reviews on crime deterrence, see Chalfin and McCrary (2017), Nagin (2013a), Nagin (2013b).

In the context of UI systems, benefit sanctions are used to correct moral hazard problems arising when unemployed individuals are granted UI benefits. While jobseekers can insure themselves against unexpected income losses due to job separations, the UI benefits receipt is made conditional on exerting sufficient a job search effort, which is monitored by caseworkers at public employment (PES) offices. Inactivity and lack of cooperation may lead to UI benefit sanctions, corresponding to temporary suspensions of benefits.

Monitoring and UI benefit sanctions can be theoretically justified as being welfare enhancing (Boone, Fredriksson, Holmlund, and van Ours, 2007). In practice, however, efficiency gains can be reached either by modifying the behavior of the UI recipients actually sanctioned (sanction imposition effect) or by modifying the UI recipients' search effort through the threat of sanction imposition (threat effect). From the policy-maker's perspective, if monitoring is costly and imperfect, it is the threat effect that really matters. This is because the main objective is to diminish moral hazard in the entire population of jobseekers exerting low search effort, not just among those actually sanctioned. Despite their relevance, however, empirical evidence on threat effects is extremely scarce.

The main contribution of this paper is to fill this gap by providing the first credible evidence of threat effects in UI monitoring and sanctions systems. I exploit variation induced by two reforms of the Swedish monitoring and sanctions system that substantially increased the strictness of the pre-existing policy setting. For each of the two reforms, I compare the job exit rates of two groups of jobseekers before and after the policy change in a difference-in-differences (DID) setting. The jobseekers that I compare are the unemployed individuals receiving UI benefits (UI group) and the longer-term unemployed that have exhausted their UI benefits and receive activity support benefits (AS group). Individuals in these two groups are similar to each other, compete for jobs in the same labor market, and all start their unemployment spell by receiving UI benefits. The main difference between the two groups is that AS recipients, by definition, have been unemployed for longer.

In September 2013, following a pre-reform period where sanctions were almost non-existent and the monitoring intensity was moderate, the stock of UI recipients started being subjected to a considerably stricter policy regime. The reform resulted in a substantial increase in the number of UI sanctions issued. Moreover, monitoring became stricter due to the mandatory requirement of submitting monthly reports of the job search activity. In January 2014, a second reform introduced the same monthly activity reporting tool for the stock of AS recipients. Therefore, the first reform allows me to estimate the effect of

stricter monitoring and sanctions on the UI group job exit rate (using the AS jobseekers as controls), while the second reform allows me to estimate the effect of stricter monitoring on the AS group (using the UI jobseekers as controls). Importantly, the two reforms give me the chance of studying the two relevant policy margins in this context: the joint introduction of monitoring and sanctions, and the introduction of monitoring only.⁴

Identification of the policy parameters of interest is facilitated by the fact that, as mentioned, the two groups are composed of similar individuals. In order to take into account the fact that AS recipients are longer-term unemployed, I estimate DID-duration models where I control for duration dependence non-parametrically. I additionally adjust for a rich set of time and seasonality fixed effects in order to control for differential trends that would otherwise invalidate identification. For estimation, I use rich administrative data providing information on individual-level unemployment histories at the daily level, daily benefit payments and sanctions information, and background characteristics for the entire population of jobseekers.

I find large and significant reform effects for male jobseekers, and especially for the long-term unemployed affected by the second reform (21 percent increase in the job exit rate). The fact that jobseekers tend to respond later during their unemployment spell is in line with existing evidence on active labor market policies (ALMPs) (Card, Kluve, and Weber, 2017). Conversely, I do not find significant reform effects for women, which is also consistent with some existing evidence on ALMPs (Card, Kluve, and Weber, 2017; Bergemann and Van Den Berg, 2008). I run several checks to corroborate these findings. First, I rule out the existence of differential trends by performing placebo exercises where I shift the reform dates back in time and, separately, move forward the duration threshold for the UI individuals' eligibility to transition to the AS group. Moreover, I check for and find no support for group compositional differences across the reform dates (which would confound the reform estimates). Furthermore, I perform a number of robustness checks that support the findings in the main analyses.

The second main contribution of this paper is a decomposition of the estimated total effect of the first reform into its threat and sanction imposition effects. In order to estimate sanction imposition effects, I follow the convention in the existing literature and use a flexible bivariate duration model where I jointly model the exit to job rate and the sanction

⁴ A monitoring regime with no consequences in the form of monetary fines can still provide job search incentives. For instance, jobseekers may dislike being caught breaking the job search rules. Moreover, they may value the use of activity reports if they believe that this improves the quality of the job search assistance provided by the caseworker.

process semi-parametrically (Abbring and van den Berg, 2003). I find a 34 percent increase in the job exit rate of UI recipients as a consequence of sanction imposition. This result is in line with previous evidence on sanction effects. Moreover, results are similar in size when splitting the sample based on gender. This shows that the heterogeneous total reform effects do not arise because of different sanction imposition effects. Instead, they must be driven by differences in threat effects.

In order to quantify the size of threat effects, I perform a decomposition exercise where I subtract the sanction imposition component from the estimated total reform effect. To make these two quantities comparable, I adjust for the probability of being sanctioned and for the proportion of the spells duration that on average is covered by a sanction. I find that for male UI jobseekers, most of the total reform effect is attributable to the threat component, which accounts for a 9.2 percent increase in the job exit rate out of the total 11 percent increase due to the reform. For women, the weighted sanction effect is even smaller in size, and accounts for a negligible part of the (insignificant) total reform effects. This is consistent with the fact that for this group the total reform effect was not found to be significantly different from zero. All in all, the results from the decomposition exercise suggest that the sanction imposition effects emphasized in the literature explain very little of the overall effects of sanctions. As a consequence, the total impact of sanctions may be severely underestimated when focusing solely on sanction effects.

Despite the fact that the objective of monitoring and sanctions is to deter moral hazard in the form of violations of job search requirements, almost all studies of UI sanctions (see below for details) have focused on estimating the effect of sanction imposition on the individuals actually sanctioned. A likely reason for the lack of evidence on deterrence effects is that their identification is challenging. It requires for the researcher to compare counterfactual outcomes under different policy settings characterized by different sanctions schedules and/or probabilities of apprehension. Moreover, in order for the policies to change the job search behavior of UI claimants, it is crucial that the policy differences are substantial and salient. These are core aspects of the two reforms considered in this paper.

One exception is Boone, Sadrieh, and van Ours (2009), who provide direct evidence of the threat effects of benefit sanctions. Through a small-scale laboratory experiment, the authors compare two systems characterized by identical expected benefits, one with constant benefits and the other with higher baseline benefits and a positive probability of being sanctioned. They find that the threat of introducing the sanctions system is equal to 14.1 percentage points, while the sanction effect equals 10 percentage points. Note, however,

that it is unclear to what extent these results translate into the real-world environment and incentives faced by jobseekers.

The only two other papers studying threat effects of sanctions are Lalive, van Ours, and Zweimüller (2005) and Arni, Lalive, and Van Ours (2013), which exploit within-regional differences in the rate at which warnings are issued. They show a positive correlation between the cross-PES offices variation in the job finding rate and the variation in the propensity of issuing warnings. Lalive, van Ours, and Zweimüller (2005), in particular, find an elasticity of the job exit rate with respect to the warning rate of 0.13. In a simulation exercise, they show both relevant sanction effects (with unemployment duration reduced by almost 3 weeks for the sanctioned) and substantial threat effects (with a reduction of the unemployment rate of about 7 days for all jobseekers).

This paper also relates to a broad empirical literature on the effect of sanction imposition mentioned above. Taken together, papers in this field (almost) unambiguously find that sanction imposition increases job exit rates through increased search effort and/or reduced reservation wage,⁵ whereas the quality of the jobs found is persistently worsened.⁶ Moreover, since sanctions are coupled with monitoring, and often with elements of job-search assistance, the literature on benefit sanctions partly overlaps with that on ALMPs.⁷

The remainder of the paper is structured as follows. Section 2 outlines the institutional background. Section 3 describes the identification of the causal parameters of interest, the sampling criteria and the data used. Sections 4 and 5 present the main analyses results and the comparison between threat effects and sanction imposition effects, respectively. Finally, section 6 summarizes and concludes.

⁵ van der Klaauw, van den Berg, and van Ours (2004) and Abbring, van Ours, and van den Berg (2005) find large re-employment effects after sanction impositions for UI and welfare recipients in the Netherlands, respectively. Similar results have been found in many other settings, such as Switzerland (Lalive, van Ours, and Zweimüller, 2005), Denmark (Svarer, 2011), Germany (Hofmann, 2008; van den Berg, Uhlendorff, and Wolff (2013); Müller and Steiner, 2008), and Norway (Røed and Westlie, 2012).

⁶ See e.g., Arni, Lalive, and Van Ours (2013) and van den Berg and Vikström (2014). Other studies have also found differential effects of sanctions and financial bonuses (van der Klaauw and van Ours, 2013), and for different types of unemployment benefits (Busk, 2016).

⁷ For exhaustive reviews on ALMPs see Card, Kluve, and Weber (2017), Card, Kluve, and Weber (2010), Kluve (2010) and Caliendo and Schmidl (2016).

2 Institutional background

2.1 Unemployment Insurance and activity support entitlement

Monitoring and sanctions are a central part of the Swedish Unemployment Insurance system. UI benefit sanctions are intended to be a correctional tool for moral hazard problems (the lack of job search) arising when unemployed individuals are granted UI benefits.

In Sweden, UI benefit sanctions rules apply to all UI recipients. Jobseekers older than 20 can be eligible for either basic UI compensation or income-related UI compensation (IAF, 2014b). The entitlement conditions for basic UI benefits are registering at a PES office, actively seeking work, and being able and willing to work at least three hours each working day and 17 hours per week.⁸ Individuals eligible for basic UI benefits gain the right to income-related UI benefits under two additional conditions. First, the newly unemployed person needs to have been a voluntary member of a UI fund for at least 12 months (*membership condition*).⁹ Second, the person must have worked for at least 6 out of the 12 months prior to unemployment, at least 80 hours per month (*work condition*). Full-time unemployed UI recipients are entitled to a full 300-day period of daily cash transfers paid at most 5 times per week, which corresponds to 420 calendar time days. In the time frame considered, the size of UI payments is 320-680 Swedish Crowns (SEK) per day (\approx €35-75). The lower bound corresponds to the basic UI. Jobseekers eligible for income-related benefits are entitled to 80 percent of their former salary for the first 200 days of unemployment and 70 percent for the remaining 100 days, capped at SEK 680 per day.¹⁰

In my main analyses, I restrict the sample to full-time unemployed individuals that start their unemployment spell with a full 300-day UI period. This allows me to know at which duration time the individuals exhaust their UI benefits. I refer to this first group of jobseekers as the *UI group*.

After exhausting their UI benefits, jobseekers become eligible to receive activity support (AS) upon enrolling in the so-called *Job and Development Program*. The daily transfers are equal to 65 percent of the previous earnings, with the same minimum and maximum levels as for the UI benefits.¹¹ Since I restrict my attention to jobseekers with full UI replacement

⁸ People not satisfying these conditions may be eligible to receive means-tested social assistance benefits (SFS, 2001). These are smaller in size compared to both UI and activity support benefits described below.

⁹ According to the official statistics provided by the Swedish Unemployment Insurance Board (IAF), in the period considered about 80 percent of workers were affiliated with a UI fund.

¹⁰ By international comparison, the Swedish system is relatively generous. See Immervoll and Knotz (2018) and Grubb (2000) for cross-countries job search requirements and UI eligibility criteria.

¹¹ The program provides long-term unemployed with targeted activities corresponding to 75 percent of the

period at the inflow, in my sample, people that reach 420 unemployment duration days are eligible to transition to the Job and Development Program. I refer to this second group of jobseekers as the *AS group*.¹²

2.2 Monitoring and sanctions before the reforms

A central feature of the system is that benefit recipients need to actively search for a new job. Newly unemployed individuals that register at a PES office are required to agree on a personalized plan of action decided together with a caseworker, with the goal of exiting from unemployment. This makes the right to receive UI compensation *conditional* on exerting a given level of search effort.

The jobseeker's activities are monitored by a caseworker at the PES office. Caseworkers inform jobseekers about the conditions for UI entitlement, the requirement of looking for a suitable job, the importance of meetings at the PES, and the underlying reasons for being sanctioned (that is, mishandling the job search process and prolonging or causing unemployment). After the initial creation of the action plan, which in most cases takes place within one month since the PES registration (Arbetsförmedlingen, 2014), caseworkers have meetings with the unemployed individuals. During these meetings, caseworkers propose ALMPs, refer appropriate vacancies, and provide counseling. Meetings are also used to monitor the jobseekers' compliance with the UI rules. Before the 2013 and 2014 reforms that I study, the meetings were the only form of monitoring.

Benefit sanctions are monetary fines corresponding to a suspension of the UI benefits. Inactivity, refusal of job offers, and job quits are valid reasons for a sanction. In case the rules are violated, the caseworker sends a notification to the UI fund, which carries out an investigation and decides whether to impose a sanction.¹³

The Swedish sanctions system is characterized by a staircase model, with increasing sanction size for each violation of the rules (IAF, 2014b). Overall, sanctions are grouped into three categories: job offer rejections, lack of compliance with the general UI eligibility

individual's potential labor supply. After 450 days in the program, participants enter into a workfare scheme and are assigned to full-time work in low-qualified occupations.

¹² Note that jobseekers ineligible for UI benefits become eligible to receive activity support if they have been registered as unemployed or enrolled in a labor market policy program for 14 months. This group of unemployed people is excluded from my analyses since everyone in the sample starts with 300 days of UI benefits. Moreover, special eligibility rules apply to former youth guarantee participants, which are also excluded from my analyses as I focus on jobseekers older than 24.

¹³ The proportion of notifications leading to a sanction for the 2013-2014 period is close to 80 percent (IAF, 2014c). Individuals can in principle appeal to a sanction, but this rarely happens. The decision is taken quickly, in most cases within 2 or 3 weeks since the notification.

rules, and job quits with no valid cause. In the pre-reform period, the refusal of suitable job offers without an acceptable reason is punished with a 25 percent benefits reduction for 40 days at the first offense, with a 50 percent reduction for 40 days the second time, and with benefit suspension until a new work condition is fulfilled the third time. UI recipients can also be sanctioned for infringements related to violations of the UI entitlement conditions. These include unreported employment, failure to actively search for a job, not showing up at meetings, not signing the action plan, and failing to apply for assigned jobs. In these cases UI benefits are suspended until a new work condition is fulfilled.

Two main aspects characterize the monitoring and sanctions system before September 2013. First, the per-jobseeker number of sanctions imposed was close to zero (see Figure 1 below). In this period, Sweden was among the EU countries with the lowest sanction rate (see also Gray, 2003). As discussed by van den Berg and Vikström (2014), who study the pre-reform sanctions regime, the main reason for such a low sanction rate is that the system was perceived as too harsh by caseworkers, who were reluctant to use this policy instrument. The second feature of the pre-reform UI system is that monitoring intensity was rather low. Monitoring occurred only through meetings with the caseworker, which on average took place less than once a month (0.8 jobseeker meetings per month; Liljeberg and Söderström, 2017). Thus, the pre-reforms period is characterized by moderate monitoring and extremely low sanction rate.

During the time frame of the analyses, AS recipients are not subject to any sanctions. In principle, the right to receive activity support benefit is lost in case of expulsion from the Job and Development Program (due to unreported employment or other gross violations of entitlement conditions; IAF, 2014b), but this happens very rarely.

2.3 Two reforms of the monitoring and sanctions system

2.3.1 The September 2013 reform for the UI recipients

In September 2013 a reform of the system was implemented for the stock of UI recipients. Its objective was to improve the job search incentives of the unemployed through enhanced monitoring technology and increased sanction rate (IAF, 2014a; Arbetsförmedlingen, 2014).

A first main policy change was the introduction of a new monitoring system based on *monthly activity reports*. Latest the 14th of each month, UI recipients now have to hand in a summary of all job search activities of the last 30 days. Typically, the reports are submitted electronically, and caseworkers should use them to monitor the UI recipients'

job search effort and to provide job search assistance.¹⁴ Recall that in the pre-reform period the monitoring activity of the caseworkers was exclusively carried out during meetings with the jobseekers. Importantly, the stated policy purpose of the activity reports was *not* to replace meetings (IAF, 2014a). This is confirmed by the observed meetings intensity, that did not change after the reform (Liljeberg and Söderström, 2017). Thus, the new activity reports provided caseworkers with a new and improved way of detecting violations of the rules, and led to tighter monitoring of the jobseekers.

A second major policy change was a quick and substantial increase in the number of sanctions imposed. Different factors contributed to the sharp increase in the sanction rate. First, the sanctions schedule was made less punitive with the purpose of encouraging caseworkers to use this policy instrument.¹⁵ Second, failing to submit an activity report was included among the reasons for being sanctioned. Third, some notifications started being sent automatically to the UI funds (failure to show up at meetings or to submit an activity report), and caseworkers were no longer able to withdraw notifications sent to UI funds. Overall, the aim was to make the sanction process more efficient and less arbitrary.

The changes of the sanctions system had a tremendous impact on the number of sanctions. Figure 1 shows the total number of sanctions per jobseeker. Before the reform, the sanction rate was very close to zero; after the reform, the number of sanctions increased dramatically.

As mentioned above, the reform also introduced a less harsh sanction schedule. Figure 2 shows that before the reform, the average sanction size was around 20 days of suspension. After the September 2013 reform, the average sanction size decreased to roughly 2.5 days. To compare the relative importance of the drastically increased sanction rate and the reduced sanctions size, Figure 3 shows the average number of UI suspension days within the first 12 months of unemployment. This provides a measure of the expected sanction cost, which reflects changes in both the rate and the size of the sanctions.¹⁶

The figure shows that the expected sanction cost increased dramatically as a result of the new rules. This is because before the reform the sanction rate is virtually zero, and after the

¹⁴ According to survey evidence, in about 80 percent of the cases the activity reports are inspected by the caseworker within 14 days (Arbetsförmedlingen, 2014).

¹⁵ Under the new rules, job offer refusal sanctions correspond to 5, 10, 45 days of suspension for the first 3 times, and to the loss of entitlement until new work requirement (capped at 112 days) for the fourth time. UI eligibility sanctions (including the failure to submit an activity report) correspond to a first time warning, 1 day, 5 days, 10 days, and loss of entitlement for the subsequent infringements.

¹⁶ To keep the two periods separated, I stop summing up the sanctions one year before the September 2013 reform. Sanctions of an “indefinite” length – in practice capped at a higher bound number of days – are assumed to last their maximum possible duration.

Figure 1 – Number of sanctions per jobseeker

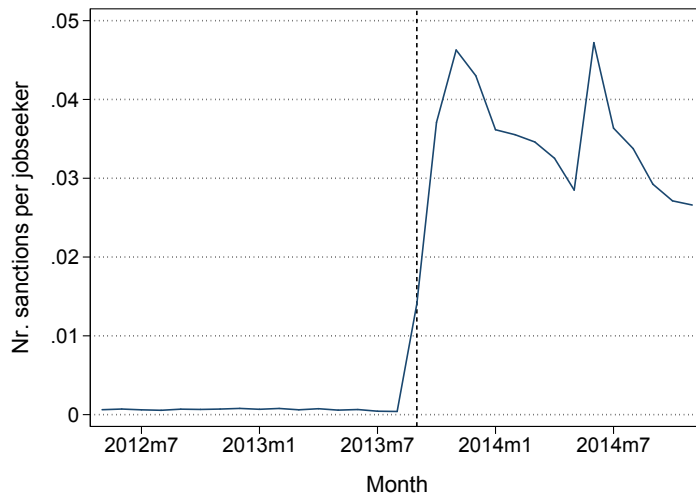
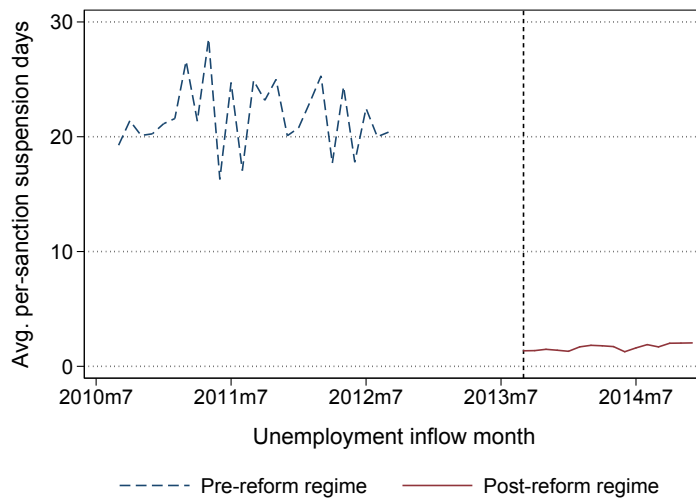
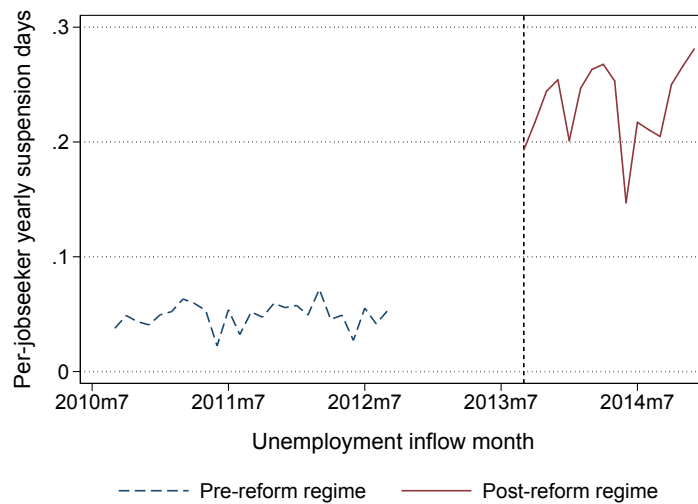


Figure 2 – Average length of the sanctions, in days



reform the increase in the number of sanctions more than outweighs the decrease in their size. Thus, unless jobseekers are extremely risk averse, the new stricter system provides greatly enhanced job search incentives compared to the old one. Moreover, van den Berg and Vikström (2014) find that the size of the sanction imposed is secondary compared to the shock of being sanctioned, which suggests that the increased sanction rate is relatively more important than its decreased size. This has also been confirmed outside UI systems (e.g., Weisburd, Einat, and Kowalski, 2008; Hawken and Kleiman, 2009). In sum, the new monthly activity reports and the sharp increase in the number of sanctions implied a substantially

Figure 3 – Expected sanction cost per-newly unemployed



stricter monitoring and sanctions system. No other changes to the UI system were made.

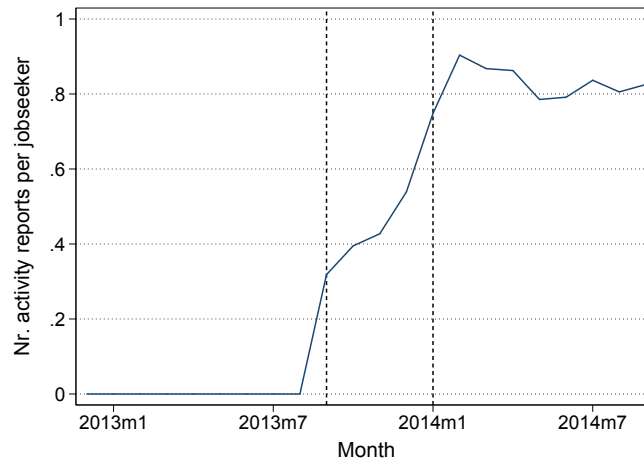
2.3.2 The January 2014 reform for the AS recipients

In January 2014, a second reform of the monitoring system was implemented for the AS group, that is the jobseekers who have exhausted their UI benefits and start receiving activity support benefits. The goal of the reform was to make the overall monitoring system homogeneous for both UI and AS jobseekers.

Before January 2014, the AS group was only subject to monitoring through caseworker meetings.¹⁷ The reform enhanced monitoring by extending the system of monthly activity reports already in place for the UI jobseekers to all AS recipients. Figure 4 illustrates this by showing the aggregate number of monthly activity reports per jobseeker. The figure shows a first increase in September 2013, due to the introduction of the activity reports for the UI recipients, and a second increase in January 2014, relative to the second reform affecting the AS group. The second reform did not change the sanction rules, so that the sanction rate remained practically zero for the AS group also after the rule changes. Moreover, the intensity of PES meetings remained the same (Liljeberg and Söderström, 2017). Thus, this second reform implied substantially tighter monitoring for the AS recipients, with no

¹⁷ The AS group was subject to benefit sanctions only in the extremely rare cases of expulsion from the Job and Development Program.

Figure 4 – Number of per-jobseeker activity reports



consequences in the form of sanctions.¹⁸

3 Empirical strategy and data

3.1 Difference-in-differences (DID) design

In order to estimate the effects of the two policy changes, I use the differential rollout of the two reforms for the UI and AS groups in a DID setting. Recall that *all* sampled individuals start in the UI group (the full-time unemployed with a complete number of UI benefit days at the inflow). Out of these, the UI jobseekers that remain unemployed after 420 days are eligible to transition to the AS group (the individuals receiving activity support upon the enrollment in the Job and Development Program). Thus, these two groups are exposed to similar business cycle conditions since they compete on the same labor market, and are composed of very similar individuals (in fact, all AS recipients *were* UI recipients originally). The main difference between the two groups is that the AS jobseekers are longer-term unemployed.

For each reform, I compare the outcomes of the two groups before and after the date of the policy change (the treatment). The estimated parameter is the *total effect* of the

¹⁸ The AS recipients may increase their job search due to the reform even in the absence of benefit sanctions. This is the case if they value the regular submission of the activity reports (e.g. because they believe that this enhances the quality of the job search assistance provided by the caseworker), or if they dislike being caught breaking the rules when not submitting the activity report.

policy shift, averaged across the treated individuals. Since the final goal is to quantify threat effects, these total reform estimates still need to be decomposed into threat effect and sanction imposition effect. The model used for estimating sanction effects is described in Section 5.1, whereas the decomposition exercise is presented in Section 5.2.

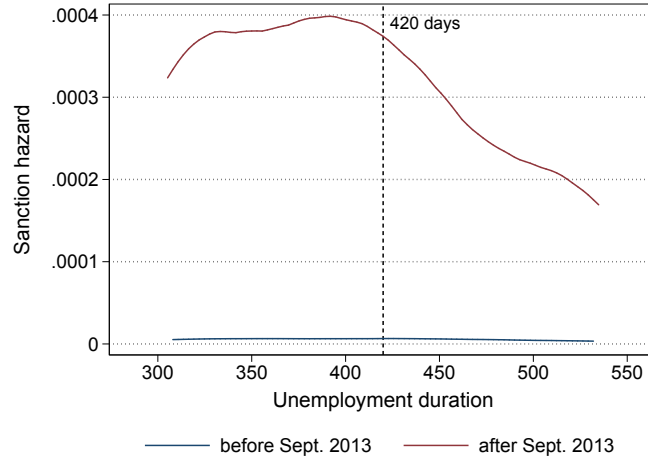
Consider the September 2013 reform that made the monitoring and sanctions regime stricter for all UI recipients without changing the existing rules for the AS group. In this case, I compare the re-employment rate of the UI recipients (the treated group) to that of the AS recipients (the comparison group), before and after September 2013. This returns the average effect of the stricter monitoring and sanctions reform on the UI recipients. I use a similar DID approach for the second reform, where the AS recipients (the treated group) are compared to the UI recipients (the controls) before and after January 2014. In this case, I estimate the average effect of the reform on the AS recipients.

Throughout the DID analyses, individuals are classified as transitioning to the AS group at 420 unemployment duration days, i.e. when they exhaust their UI benefits and are *eligible* to enroll in the Job and Development Program and collect activity support benefits. All estimates should accordingly be interpreted as Intention-to-treat estimates (ITT). The ITT strategy is motivated by the fact that the actual AS-transition is not a deterministic function of the time spent in unemployment: it usually, but not always, occurs at 420 days after the first UI payment. This is because the unemployed may not use benefits at the full speed, for instance because they have a part-time job. Hence, using the transition eligibility allows me to avoid using the actual timing of the transition, which in general is not random.

The identification strategy exploits two sources of variation: over calendar time and unemployment duration. Note that spells crossing the 420-day threshold and the reform dates contribute to the identification of the parameters of interest. Figure 5 shows the unemployment duration margin used to identify the first reform effect. The left- and right-hand sides of the graph show the predicted sanction hazard for the UI and AS group, respectively. In the pre-reform period (blue line), the sanction hazard for the UI group is nearly zero, consistent with the fact that during that time sanctions were used very rarely. After the reform (red line), the sanction hazard tends to increase with the unemployment duration, and subsequently plunges after the 420-day threshold. Note that in Figure 5 the hazard does not reach 0 for the AS group, since the ITT setting introduces some noise.

The identification of the reform effects in the DID setting requires absence of differential time trends in the two groups and no anticipatory effects of the reform. If this is the case, the observed average pre- and post-reform outcomes of the comparison group can be used to

Figure 5 – Sanction hazard in the two regimes



retrieve the counterfactual average outcome for the treated group (e.g., for the first reform, what would have happened to UI recipients in the absence of the new monitoring and sanctions rules). By design, all time-fixed differences in the two groups are netted out.

Formally, the model is the following. Let d be unemployment duration (in days), m and y calendar month and year, and $g = UI, AS$ the jobseeker group. Define $D^{(1)} \equiv D_d^{UI} \cdot D^{\text{Sept}2013}$ to be the first reform indicator, i.e. the time-varying treatment variable equal to one for the UI group after September 1st 2013, and equal to zero otherwise. Here, D_d^{UI} is a time-varying indicator for being in the UI group, and $D^{\text{Sept}2013}$ is a time-varying indicator for being in the post-September 2013 period. Moreover, $D^{(2)} \equiv (1 - D_d^{UI}) \cdot D^{\text{Jan}2014}$ is the second reform indicator equal to one for the AS group after January 1st 2014.¹⁹ I estimate the following DID-duration model for the hazard of exiting unemployment:²⁰

$$\ln \theta(d, m, y) = \ln \lambda_d + \beta_1 D^{(1)} + \beta_2 D^{(2)} + \lambda_{my} + \lambda_{mg}, \quad (1)$$

where the two parameters of interest are β_1 , the effect of being in the new monitoring and sanctions regime for the UI group, and β_2 , the effect of being subject to the activity reports monitoring regime for the AS group.²¹ Note that (i) all spells start with $g = UI$, and (ii) at

¹⁹ Hence, the two reform indicators vary over both duration and calendar time.

²⁰ Model (1) is a Cox model for the exit to job (see e.g. van den Berg, 2001). Note that the functional form of (1) implicitly assumes out any unobserved components that may differentially affect the two groups over time. In the absence of observed pre-treatment trends, the existence of such components is unlikely.

²¹ The residual variation exploited for identification of β_1 and β_2 comes from within month differences in the two groups, after netting out monthly seasonality fluctuations specific for the UI and AS recipients and

$d = 420$ the jobseekers switch to $g = AS$ (hence, $g \equiv g(d)$). Note also that over a given spell $D^{(1)}$ and $D^{(2)}$ can switch on and off depending on the group the unemployed person belongs to and on whether the given reform has been implemented or not.

The job exit hazard $\theta(\cdot)$ on the left-hand side of (1) is the instantaneous (daily) probability of exiting to job conditional on being unemployed up to duration time d . It is modeled as a function of the baseline hazard $\ln \lambda_d$, that captures non-parametrically the unemployment duration dependence; λ_{my} , a set of year-specific monthly fixed effects capturing calendar time-specific effects common to the two groups; and λ_{mg} , a set of monthly fixed effects that controls group-specific seasonality.²²

By definition, the AS recipients are similar to the the UI ones, especially since, as explained in the next section, I retain spell parts close to and centered around 420 duration days. This makes the common trends assumption more likely to hold. In order to further balance trends in the two groups, I control for monthly time fixed effects, and in robustness specifications I additionally adjust for the group-specific seasonality fixed effects. Moreover, since a main difference between the two groups is that the AS recipients are by definition longer-term unemployed (and hence potentially more negatively selected over unemployment time than the UI recipients), I also non-parametrically control for duration dependence. Finally, after setting up the estimation model, I formally test for the absence of differential group trends by estimating placebo reform effects where I anticipate the reform dates to test whether they are statistically different from zero. Moreover, in a different placebo exercise I move forward the duration threshold for the UI individuals' eligibility to transition to the AS group.

One concern is that the effect of the first reform may change the composition of the controls in the second reform (since UI jobseekers are treated during the first reform and are used as comparison group later on). To formally assess this possibility (dynamic selection), I test for changes in a range of observable characteristics between the UI and AS groups before and after the two reforms.

the duration dependence component.

²² The main effects $D^{\text{Sept}2013}$ and $D^{\text{Jan}2014}$ are implicitly controlled for through the λ_{my} terms. The main effect D_d^{UI} is omitted in (1), as in the DID specifications that I estimate I assign the transition to the AS group at $d = 420$ (ITT framework). In this case, D_d^{UI} cannot be separately identified from the baseline hazard.

3.2 Data description

3.2.1 Data sources

I use information from several Swedish administrative registers. Data from the Swedish Public Employment Service provides information on all unemployment spells (at daily level) and rich background characteristics. Population registers from Statistics Sweden (LOUISE) provide additional background characteristics. I use the register called ASTAT from the Swedish Unemployment Insurance Board (IAF) to link information on the number of UI benefit days. The same register includes daily information on all benefit sanctions.

3.2.2 Sampling and descriptive statistics

I construct the analyses sample in the following way. First, I select all unemployment spells starting with full-time unemployment. Age at inflow is restricted to be between 25 and 50. This is done because young people are subject to special eligibility rules for participation in the Job and Development Program, and older workers may be eligible for early retirement schemes and other targeted policies. The analyses also exclude all individuals with disabilities. Next, I retain only spells with a full 300 UI days at the start of the spell (equivalent to 420 calendar time days). Moreover, I focus on spells of a duration of between 280 and 560 days, i.e. close to and centered around the 420-day threshold. Shorter spells are ignored, and all ongoing spells are right-censored after 560 days. I start to sample unemployment spells two years before the first reform of September 2013, and I include spells up until March 2015, where any ongoing spells are right-censored. This ensures that enough pre-reform observations are available to capture the pre-treatment trends through the rich set of time and seasonality fixed effects.

Table 1 shows descriptive statistics. The columns report group averages in the three periods (before September 2013; between September 2013 and January 2014; and after January 2014). All characteristics are measured at the time of inflow into unemployment. The table shows that the AS group is composed of jobseekers that are less educated and more likely to be immigrants and married. Compared to the UI group, they also have a weaker attachment to the labor market and a lower income in the three years preceding the start of the spell. All this shows that the longer-term unemployed (the AS group) have less favorable characteristics than their shorter-term unemployed counterparts (the UI group). Note that this is not a problem for the identification of the reform effects, since the DID model adjusts for all time-fixed differences between the two groups. What would be problematic

Table 1 – Group averages in the three reform periods

	Before Sept. 2013		Sept. 2013– Jan. 2014		After Jan. 2014	
	UI	AS	UI	AS	UI	AS
Age	37.41	37.81	37.52	37.73	37.52	37.89
Education: compulsory	0.19	0.21	0.19	0.22	0.19	0.21
Education: secondary	0.47	0.47	0.47	0.45	0.46	0.46
Education: upper	0.34	0.32	0.34	0.33	0.35	0.33
Any child below 18	0.42	0.42	0.41	0.42	0.41	0.42
Immigrant	0.48	0.53	0.52	0.56	0.52	0.56
Married	0.40	0.42	0.41	0.43	0.40	0.42
Male	0.57	0.58	0.60	0.60	0.59	0.60
Unemployed 24 months before	0.27	0.30	0.32	0.36	0.31	0.36
Any program in last 24 months	0.03	0.03	0.03	0.04	0.03	0.03
Duration of last unemployment spell	200	221	232	253	239	260
Any program in last 4 years	0.04	0.05	0.05	0.05	0.05	0.05
Previous income (past 3 years)	1658	1543	1699	1503	1763	1624
Inflow year: 2010	0.31	0.35	0.00	0.00	0.00	0.00
Inflow year: 2011	0.33	0.40	0.00	0.00	0.00	0.00
Inflow year: 2012	0.36	0.25	0.60	1.00	0.09	0.46
Inflow year: 2013	0.00	0.00	0.40	0.00	0.91	0.54
Nr. observations	32,185	16,565	8,468	4,972	12,132	7,999

Notes: Average observables in the UI and AS groups, by reform period as defined by the two reform dates (September 2013 and January 2014). All socio-economic characteristics and previous labor market history measured at the inflow into unemployment. Previous income in 100s SEK.

are *changes* in group differences over time. However, this does not appear to be the case, since Table 1 shows that the group differences are stable over time. Later, I formally test for such dynamic selection patterns.

4 The total effects of the two reforms

4.1 Main results

I start by estimating the effects of the two reforms by gender. This has been shown to be a relevant dimension according to which ALMPs effects vary (see e.g., Card, Kluge, and Weber, 2017; Bergemann and Van Den Berg, 2008). Table 2 presents the estimates using the DID model presented in Section 3 for the exit rate to a job (re-employment rate).

Panel A shows the results for men. To start with, Column 1 presents placebo estimates where I shift the entire observation window and anticipate the reform dates by two years. Apart from this, the overall data structure, sampling criteria and estimated model are kept

exactly as in the main analyses. Any significant placebo estimates would raise doubts on the validity of the identification strategy and the parallel-trends assumption. This is not the case, since placebo estimates in Column 1 are insignificant and very close to zero.

Table 2 – Total effects of the monitoring and sanction reforms, by gender

	Placebo period		Reform period	
	(1)	(2)	(3)	(3)
<i>Panel A: Men</i>				
Reform 1: Monitoring and sanctions, UI recipients	-0.02 (0.08)	0.11* (0.07)	0.11* (0.07)	
Reform 2: Monitoring, AS recipients	0.04 (0.08)	0.21*** (0.07)	0.21*** (0.07)	
Nr. individuals	18,301	25,682	25,682	
Spell duration	X	X	X	
Calendar Time FE	X	X	X	
Covariates			X	
<i>Panel B: Women</i>				
Reform 1: Monitoring and sanctions, UI recipients	0.006 (0.09)	-0.05 (0.08)	-0.03 (0.08)	
Reform 2: Monitoring, AS recipients	0.05 (0.10)	-0.06 (0.09)	-0.03 (0.09)	
Nr. individuals	13,884	19,066	19,066	
Spell duration	X	X	X	
Calendar Time FE	X	X	X	
Covariates			X	

Notes: DID-Cox model estimates for the re-employment rate using the data described in Section 3.2. The covariates include: dummy for any children, age, migrant status, married, education. Robust standard errors in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Next, Column 2 of Panel A reports the estimates for the actual reform period. The table shows that the re-employment rate for male jobseekers is significantly affected by the first monitoring reform (11 percent increase).²³ The effect of the second reform is even

²³ Since the model coefficients measure changes in log re-employment rates, estimates are interpreted as percentage changes in the re-employment rate when the corresponding covariates are increased by one unit. In the pre-September 2013 period, the re-employment rate the month preceding the 420 duration days threshold is equal to 0.113 (and similar for men and women).

larger, with a 21 percent increase of the re-employment rate. The results are robust to the additional inclusion of socio-economic characteristics (Column 3).

These results may appear puzzling since the second reform provides individuals with stricter monitoring, while the first reform introduces both stricter sanctions and stricter monitoring. However, remember that the two reforms affect different groups of jobseekers: the second reform affects the long-term unemployed (AS group), while the first one affects more the shorter-term unemployed (UI group). If the long-term unemployed react differently to monitoring incentives, this may explain the different effects of the two reforms. In fact, a common finding in the literature is that the long-term unemployed tend to benefit more from ALMPs (Card, Kluve, and Weber, 2017). Another difference between the two reforms is that both the pre- and post-reforms strictness of the system for the two groups is different.²⁴ Hence, we should not necessarily expect the first reform to have a larger effect than the second one.

Interestingly, Panel B shows no significant effects of any of the two reforms for women. One way of interpreting the heterogeneous effects for men and women is that gender differences may reflect differences in labor market attachment. In fact, the existing evidence suggests that ALMPs are more effective for more disadvantaged groups, as the long-term unemployed (Card, Kluve, and Weber, 2017; Bergemann and Van Den Berg, 2008). If men are relatively more disadvantaged on the labor market, and the more disadvantaged groups on average benefit the most from monitoring and sanctions policies, then we can expect to find the heterogeneous effects observed. This is supported by Table A.1 in the Appendix, which shows that men are characterized by worse previous labor market history.

To better understand the heterogeneity of the total reform effects for men and women, the next section reports estimates of the sanction imposition effects separately by gender. This may reveal whether the gender differences are due to differential threat effects or differential sanction effects.²⁵

Table 3 shows the effects of the two reforms when pooling men and women. As before, the placebo estimates in Column 1 are insignificant. Column 2 shows a 10 percent increase in the exit to job rate of AS jobseekers due the monitoring reform. The point estimate for

²⁴ The UI group was subject to sanctions already before the rules changed (although the sanction rate was very low). Hence, UI jobseekers pass from a moderate monitoring and sanctions system to a stricter one. Instead, AS jobseekers pass from an even milder pre-reform period with no sanctions to a stricter monitoring-only one.

²⁵ Moreover, I plan to more directly inspect the mechanisms behind the heterogeneous effects according to gender. In particular, by inspecting the actual and predicted unemployment duration for the two groups, and by looking at results according to type and level of education and previous industry.

Table 3 – Total effects of the monitoring and sanction reforms

	Placebo	Reforms period	
	-1 Year (1)	Main (2)	With covariates (3)
Reform 1: Monitoring and sanctions, UI recipients	-0.006 (0.06)	0.05 (0.05)	0.05 (0.05)
Reform 2: Monitoring, AS recipients	0.05 (0.06)	0.10* (0.06)	0.12** (0.06)
Nr. individuals	32,185	44,748	44,748
Spell duration	X	X	X
Calendar Time FE	X	X	X
Covariates			X

Notes: DID-Cox model estimates for the re-employment rate using the data described in Section 3.2. The covariates include: dummy for any children, age, migrant status, gender, married, education. Robust standard errors in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

the first reform is also positive, but not significantly different from zero.

4.2 Robustness Analyses

This section presents three sets of robustness analyses. First, I test for the lack of changes in compositional differences between the UI and AS groups before and after the reforms (dynamic selection). Second, I present the results from alternative model specifications to test the robustness of the main analyses estimates. Finally, I implement additional placebo checks to test the parallel trends assumption.

4.2.1 Dynamic selection

Identification in the main DID model relies on a comparison of the re-employment rates around the AS threshold after 420 days of unemployment. Thus, different spell segments are compared to each other (early parts being UI, later parts being AS). A potential concern, is that any treatment effects during the first part of the spells (i.e. for the first reform, the effect of stricter monitoring and sanctions for the UI recipients) may change the composition of jobseekers that remain in the second part of the spells. This creates the so-called dynamic selection problem, which may confound the estimated effects due to the changes in the

composition of the groups.

To address this, I replace the outcome (re-employment rate) with observed characteristics (such as socio-economic variables) measured at the unemployment inflow. Otherwise, I estimate the DID model as in the main analyses. This offers one way of studying the assumptions underlying the DID model, since significant estimates for these observed variables would indicate problems with dynamic selection. Specifically, I regress each observed characteristic on the two reform indicators, the AS group indicator and the interaction between the two. This DID exercise allows me to compute the outcome averages for the UI and AS groups in the three calendar time periods defined by the two reforms (see Columns 1-3 of Table 4). For each reform, the regression coefficients on the interaction term return the difference in the two groups averages across the given reform date (Columns 4 and 6).

Reassuringly, Table 4 reveals no significant estimates and all point estimates are very close to zero (see the p -values in Column 5 and 7). This is true also when considering the entire set of covariates in a joint test. I also construct a measure of predicted unemployment duration using all the observed covariates, and use this as an outcome in the same DID regression framework.²⁶ The group differences in predicted unemployment across the reforms are also insignificant. All this suggests that dynamic selection is not an issue, hence supporting my main results and identification strategy.

4.2.2 Robustness analyses

Table 5 presents results from several robustness analyses with the baseline results in the first column for comparison.

In the main analyses, I adjust for general seasonal variation through time-varying calendar time indicators. Here, Column 2 of Table 5 reports model estimates where I add group-specific monthly dummy variables, which additionally adjust for different seasonal dynamics in the UI and AS groups. The main estimates are very robust to the inclusion of these seasonality controls, hence, ruling out that the observed effects are due to group-specific seasonality effects.

As explained above, in the main analyses the transition of the UI jobseekers to the AS group is assigned at 420 days, without using the actual transition date (which is potentially endogenous). This comes at the cost of increasing noise, since some unemployed individuals transition to the AS group already before this threshold, and others do so after the

²⁶ Here, I use the pre-September 2013 spell parts to predict unemployment duration (for all reforms periods) as a function of all observables.

Table 4 – Robustness analyses: Dynamic selection and compositional differences

Outcome	AS and UI mean differences			Period 2 vs 1		Period 3 vs 2	
	Period 1	Period 2	Period 3	Est.	p-value	Est.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	0.392	0.205	0.363	-.187	0.214	.158	0.354
Education: compulsory	0.021	0.022	0.017	.002	0.783	-.005	0.552
Education: secondary	-0.001	-0.016	-0.001	-.015	0.133	.015	0.189
Education: upper	-0.020	-0.007	-0.017	.013	0.175	-.01	0.374
Any child below 18	0.002	0.005	0.003	.002	0.857	-.002	0.846
Immigrant	0.048	0.044	0.039	-.005	0.599	-.004	0.695
Married	0.014	0.023	0.020	.008	0.409	-.002	0.836
Male	0.013	-0.001	0.017	-.014	0.150	.019	0.102
Unemployed 24 months before	0.029	0.039	0.044	.01	0.271	.005	0.636
Any program in last 24 months	0.003	0.007	0.005	.004	0.246	-.002	0.667
Duration of last unemployment spell	20.934	21.019	21.366	.084	0.989	.348	0.959
Any program in last 4 years	0.004	0.004	0.001	.001	0.864	-.003	0.591
Past average income (in last 3 years)	-0.146	-0.188	-0.133	-.043	0.315	.056	0.246
<i>Joint significance p-value</i>					0.421		0.892
Predicted unemployment duration	1.557	1.384	1.449	-.172	0.431	.065	0.793

Notes: DID regressions for AS and UI compositional differences across regimes. Columns (1)–(3): average outcome differences between the two groups in the policy regimes defined by the two reforms dates. Columns (4) and (6): DID estimates when respectively comparing (2) to (1) and (3) to (2). Previous income averaged in the three years preceding the unemployment inflow (in log-scale). Predicted unemployment duration computed by: (i) regressing unemployment duration on all observables using period 1 spell parts; and (ii) using the estimated model to predict for all periods. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Table 5 – Robustness analyses for the total reform effects

	Baseline	Group-specific seasonality	Control for month post UI exhaustion	Control for month before UI exhaustion	Duration 250-590	Duration 310-530
	(1)	(2)	(3)	(4)	(5)	(6)
Reform 1: Monitoring and sanctions, UI recipients	0.05 (0.05)	0.05 (0.06)	0.04 (0.06)	0.05 (0.05)	0.03 (0.05)	0.04 (0.06)
Reform 2: Monitoring, AS recipients	0.10* (0.06)	0.10 (0.07)	0.13* (0.07)	0.11* (0.06)	0.08 (0.05)	0.09 (0.06)
Nr. individuals	44,748	44,748	44,748	44,748	51,607	39,253
Spell duration	X	X	X	X	X	X
Calendar Time FE	X	X	X	X	X	X

Notes: Robustness estimates of the main results when using the full sample. Column 1: baseline results (spells range between 280 and 560 days); Column 2: additional inclusion of group-specific seasonal dummies; Columns 3 and 4: partition out the month following and preceding the 420-day threshold, respectively; Columns 5 and 6: sampling spells ranging in 250-590 and 310-530 duration days, respectively. Robust standard errors in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

threshold. This is not problematic for identification, since the exogenous 420-day threshold is used for *all* jobseekers, but it may reduce the precision of the estimates. Therefore, in Column 3 I explore whether it is possible to obtain a stronger first stage for identification than the one shown in Figure 5. To this end, I “dummy out” the first month after the 420-day threshold, so that the spell parts immediately following the AS transition do not contribute to the estimation of the reform effects.²⁷ This procedure returns very similar estimates to the ones in the main analyses.

The UI recipients that remain unemployed and exhaust their UI benefits eventually transition to the AS group. Here, one concern is that workers may increase their search effort just before exhausting their UI benefits (see e.g., Card, Chetty, and Weber, 2007). However, note that the DID model flexibly adjusts for duration dependence (through the baseline hazard), and this also controls for increased exit rates just before benefit exhaustion. However, one may worry that these anticipatory effects change in correspondence with the two reforms. To check for this, Column 4 reports estimates where the period before the AS transition is “dummied out” in a similar way as above (with the pre-420 month indicator interacted with the two reforms variables). The estimates are robust to this exercise.

Next, I report robustness analyses with respect to the sampling window. In the main analyses, all spells range between 280 and 560 days. The last two columns of Table 5 show results when varying the size of the duration window. Specifically, I extend this window (Column 5) and tighten the window (Column 6) around the 420-day threshold. In both cases the results are very similar to those in the main analyses.

Finally, Table 6 reports the same robustness checks separately for men and women. The estimates show that also the main results for these two groups are robust.

4.2.3 Extended placebo analyses

Identification of the reform effects is based on variation across calendar time and spell duration. Table 7 shows the results from extended placebo analyses where I misplace the reform dates and the duration time thresholds, respectively.

First, I study placebo effects for different placebo reform dates. To this aim, I show results when moving the entire sampling window back in time one and two years, respectively in Columns 1 and 2. The dates are moved by exactly one or two years to preserve the same seasonal structure that characterizes the sampling window of the main analyses. The

²⁷ Specifically, I add a time-varying indicator switching to one during the 30 days following the 420-day threshold and I interact it with the two reforms indicators.

Table 6 – Robustness analyses for the total reform effects, by gender

	Baseline (1)	Group-specific seasonality (2)	Control for month post UI exhaustion (3)	Control for month before UI exhaustion (4)	Duration 250-590 (5)	Duration 310-530 (6)
<i>Panel A: Men</i>						
Reform 1: Monitoring and sanctions, UI recipients	0.11* (0.07)	0.09 (0.08)	0.12 (0.08)	0.12* (0.07)	0.10 (0.06)	0.08 (0.08)
Reform 2: Monitoring, AS recipients	0.21*** (0.07)	0.17* (0.10)	0.24*** (0.09)	0.20** (0.08)	0.20*** (0.07)	0.19** (0.08)
Nr. individuals	25,682	25,682	25,682	25,682	29,475	22,687
Spell duration	X	X	X	X	X	X
Calendar Time FE	X	X	X	X	X	X
<i>Panel B: Women</i>						
Reform 1: Monitoring and sanctions, UI recipients	-0.05 (0.08)	-0.009 (0.10)	-0.08 (0.09)	-0.04 (0.09)	-0.07 (0.08)	-0.03 (0.09)
Reform 2: Monitoring, AS recipients	-0.06 (0.09)	-0.02 (0.12)	-0.06 (0.11)	-0.03 (0.09)	-0.12 (0.09)	-0.05 (0.10)
Nr. individuals	19,066	19,066	19,066	19,066	22,132	16,566
Spell duration	X	X	X	X	X	X
Calendar Time FE	X	X	X	X	X	X

Notes: Robustness estimates of the main results when splitting the sample by gender. Column 1: baseline results (spells range between 280 and 560 days); Column 2: additional inclusion of group-specific seasonal dummies; Columns 3 and 4: partition out the month following and preceding the 420-day threshold, respectively; Columns 5 and 6: sampling spells ranging in 250-590 and 310-530 duration days, respectively. Robust standard errors in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Table 7 – Placebo analyses for the total reform effects

	Placebo calendar time		Placebo spell duration	
	-2 Years (1)	-1 Year (2)	480-760 (3)	580-860 (4)
Reform 1: Monitoring and sanction, UI recipients	-0.001 (0.06)	-0.006 (0.06)	-0.09 (0.08)	-0.08 (0.10)
Reform 2: Monitoring, AS recipients	-0.07 (0.07)	0.05 (0.06)	-0.08 (0.08)	-0.06 (0.11)
Nr. individuals	40,184	32,185	19,319	13,358

Notes: Placebo estimates when anticipating the reform dates (Columns 1 and 2), and when delaying the transition to the AS group (Columns 3 and 4). Spells used in Columns 1 and 2 range between 280 and 560 duration days. Robust standard errors in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

resulting placebo estimates are always insignificant.

Second, in the main analyses, the duration of all sampled spells ranges between 280 and 560 days, with the UI to AS threshold at 420 days. In Columns 3 and 4 of Table 7, this sampling window is shifted to 480-760 days and 580-860 days, with placebo thresholds at 620 and 720 days. Otherwise, the model structure is the same with reform dates at September 2013 and January 2014. Since at these thresholds there are no reform changes, I expect the corresponding placebo estimates to be zero. From the table we see that the point estimates are negative but insignificant, supporting the main results. The only potential issue is the size of the placebo estimates. However, their negative sign indicates that, if anything, the positive estimates from the real period should be biased towards zero.

5 The relationship between threat and sanction effects

To obtain the threat effect of the monitoring and sanctions regime, it is necessary to decompose the reform effect into a threat effect component and a sanction imposition component.

In order to estimate sanction imposition effects, I focus on the sanctions imposed during the new monitoring and sanctions regime, when the large increase in the sanctions rate took place. To this aim, I sample unemployment spells starting after September 2013 and merge UI benefit sanctions to the spells. Durations are right-censored at the end of 2015. I proceed as in the main analyses, and select only spells of full-time and non-disabled unemployed, aged between 25 and 50 at the inflow. I sample only spells of UI recipients.²⁸ I do not distinguish between the different types of UI benefit sanctions, and I focus on the first sanction during unemployment spells. To avoid misclassification, I restrict the spells so that they are least 15 days long.

5.1 Sanction imposition effects

5.1.1 Identification of sanction effects

To estimate the effect of a sanction I use a flexible bivariate duration model commonly referred to as the *Timing-of-Events* (ToE) model (Abbring and van den Berg, 2003). This model is the standard approach for the estimation of sanction effects (see e.g. Arni, Lalive, and Van Ours, 2013; van den Berg and Vikström, 2014).

²⁸ Being more restrictive by selecting only those with the full amount of UI benefits at the inflow does not qualitatively change the results.

In this framework, the goal is to identify the causal effect of a sanction on the re-employment rate (θ_e , the outcome of interest). The challenge is that sanctions are not random events. Many observable and unobservable factors may influence the sanction rate, and these factors are likely to also affect the re-employment rate. Hence, I jointly model the re-employment rate and the sanction rate, θ_s . Let d be time in unemployment, λ_{ed} and λ_{sd} are baseline hazard functions capturing duration dependence, x is a set of determinants observable to the researcher, and D_d is a time-varying treatment indicator taking the value one after a sanction has been imposed. The model includes the unobserved heterogeneity terms $v = (v_e, v_s)'$, that are allowed to be correlated; each captures the effect of unobserved determinants respectively on the re-employment rate and the sanction rate. The model is:

$$\ln \theta_e(d, x, D, v_e) = \ln \lambda_{ed} + x' \beta_e + \delta D_d + v_e \quad (2)$$

$$\ln \theta_s(d, x, v_s) = \ln \lambda_{sd} + x' \beta_s + v_s, \quad (3)$$

where δ represents the treatment effect of interest (here assumed to be constant, but it can be allowed to vary with duration d , time since treatment, or observed characteristics x).

In this setting identification relies on the following assumptions (Abbring and van den Berg, 2003). First, individuals must not be able to anticipate the *exact* timing of the sanction (*no anticipation*). In this setting, several aspects of the sanction assignment process are unknown to the jobseeker, for instance because the actual decision is taken by the UI fund. Moreover, even if some jobseekers might anticipate the timing of a sanction, UI funds typically decide upon imposing a sanction soon after they are notified. Hence, the sanction imposition estimates are unlikely to be characterized by strong anticipatory effects. A second assumption is the Mixed Proportional Hazard (MPH) structure in (2) and (3) (*MPH assumption*). Third, x and v should be independently distributed (*random effects assumption*). The last two assumptions can be relaxed if multiple-spell data is used (Abbring and van den Berg, 2003).

If these and some additional regularity conditions hold, the model is non-parametrically identified. Note that identification does not require exclusion restrictions (the x vector is the same in the two hazard rates). This makes the model particularly appealing in this setting, since quasi-experimental variation in the assignment of sanctions is not available and exclusion restrictions would be hard to justify. Intuitively, identification is achieved by a quick succession of events. If a sanction is rapidly followed by a transition from unemployment to employment, this is evidence of a causal effect. Instead, any selection effects do not give rise to the same type of quick succession of events.

5.1.2 Estimation of sanction effects

In order to estimate the ToE model, it is necessary to specify the baseline hazards, the distribution of the unobserved heterogeneity and select the covariates. I follow the common practice in the literature and use a discrete support point distribution for the unobserved heterogeneity (see Lindsay, 1983; and Heckman and Singer, 1984). To select the number of support points, I rely on the evidence in Gaure, Røed, and Zhang (2007) and Lombardi, van den Berg, and Vikström (2018).

In the simulation study by Gaure, Røed, and Zhang (2007), the authors find that the general approach of approximating the unobserved heterogeneity through a discrete distribution performs well. However, they also highlight that unjustified restrictions, such as pre-defining a small number of support points for the discrete distribution, may result in large bias. In Lombardi, van den Berg, and Vikström (2018), the authors also study ToE specification issues, but use a different simulation approach based on actual data (the so-called Empirical Monte Carlo design; see Huber, Lechner, and Wunsch, 2013). The use of data on real outcomes and covariates to simulate placebo treatment spells has the advantage of providing evidence more closely linked to real applications and based on less arbitrarily chosen data generating processes. Both papers conclude that it is important to use information criteria to select the number of support points.

I use three information criteria. They are a function of the number of support points (W), the overall number of model parameters ($k = k(W)$), and the number of spell parts used in the estimation (N). The Akaike information criterion (AIC) penalizes over-parameterization, whereas the Bayesian information criterion (BIC) and Hannan-Quinn information criterion (HQIC) additionally penalizes increases in sample size. They are formally defined as: $AIC_W = L(\hat{\vartheta}_W) - k$, $BIC_W = L(\hat{\vartheta}_W) - 0.5k \cdot \ln N$ and $HQIC_W = L(\hat{\vartheta}_W) - k \cdot \ln(\ln N)$, where $L(\hat{\vartheta}_W)$ is the achieved maximum value of the likelihood. The number of support points is selected based on the number that maximizes the given information criterion.²⁹

For the baseline hazard functions, I use a piecewise constant distribution (8 duration pieces). The observed covariates include a rich set of baseline socio-economic characteristics (gender, age and education dummies), regional dummies, inflow month and year indicators, the regional unemployment rate at the time of inflow, and a set of variables capturing short- and long-term previous labor market history.

²⁹ To search for the support points values, I use the same search algorithm as in Gaure, Røed, and Zhang (2007) and Lombardi, van den Berg, and Vikström (2018).

5.1.3 Sanction effects estimates

In accordance with the analyses of the reforms, I estimate sanction effects both when using the full sample and separately for men and women. Table 8, Column 1, reports the sanction effect for the full sample.³⁰ Here, we see that jobseekers exit to job 34 percent faster after being sanctioned.³¹ This is consistent with the fact that sanctions decrease the value of staying unemployed, leading to increased job-search intensity and/or decreased reservation wages. Interestingly, the estimated effect is very similar in size to the baseline results in van den Berg and Vikström (2014), who study sanction effects in the Swedish setting before September 2013. Overall, the size of the estimated sanction effect is substantial, but smaller than the effect of sanctions in other countries.³²

From Columns 2 and 3 of Table 8, we see that the effects are very similar for men and women: men exit to job 33.6 percent faster after a sanction, while the same increase is 36.7 percent for women. The fact that the sanction effect is very similar for men and women is contrary to what was found in the analysis of the total reform effects, where we saw large effects for men but small and insignificant effects for women (for both reforms).

From the analyses in this section, it is clear that sanction effects do not drive the heterogeneity in the total reform effects previously found. Instead, such heterogeneous patterns must be due to differences in threat effects. This motivates the calculation of the threat effects in the following section.

5.2 The relationship between threat and sanction effects

In this section, I decompose the total effect of the first reform into the threat effect and sanction effect components. This allows me to compare the relative importance of the two elements, both for the full sample and when splitting it according to gender. Note that different aspects make the decomposition not straightforward. First, threat effects may have an impact on the sanction rate. Second, UI jobseekers were subject to sanctions already before September 2013 (although as mentioned, the sanction rate was very close to zero). Lastly, the magnitude of the threat effects may in principle change over the time

³⁰ All information criteria previously defined return 4, 5 and 4 mass points as preferred specifications for the full sample, women-only and men-only samples, respectively.

³¹ Estimates are interpreted as percentage changes in the re-employment rate. This is because δ corresponds to the change in the log re-employment hazard when the sanction indicator switches to one.

³² For instance, for the Netherlands Abbring, van Ours, and van den Berg (2005) find that a sanction doubles the job exit rate. For Switzerland, the total effect of a warning and a sanction increases the re-employment rate by around 50 percent (Lalive, van Ours, and Zweimüller, 2005).

Table 8 – Sanction effects in the new monitoring and sanctions regime

	Baseline (1)	Men (2)	Women (3)
Sanction effect	0.340*** (0.015)	0.336*** (0.019)	0.367*** (0.024)
Nr. individuals	498,066	259,616	238,450

Notes: Sanction imposition effects. Unobserved heterogeneity approximated with 4 mass points. Controls include: timing of inflow; socio-economic characteristics; local labor market (region, local unemployment rate); unemployment history (up to 2 and 10 years before the unemployment inflow). Standard errors in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

spent in unemployment. In the decomposition exercise, I simplify the analysis by assuming constant sanction rate, by not considering pre-reform sanction effects, and by assuming constant threat effects over duration time. The decomposition is performed according to the following formula:

$$\text{Threat effect} = \text{Total effect} - \text{Sanction effect} \times (p \cdot \text{coverage}), \quad (4)$$

where the threat effect on the left-hand side is computed as the difference between the total effect of the September 2013 reform and the weighed sanction effect. The size of the sanction imposition effect is rescaled to make it comparable to the total reform effect. In particular, the weighting term $p \cdot \text{coverage}$ is a function of (i) p , the share of the sanctioned individuals among those used in the sanction effect estimation; and (ii) coverage , the fraction of the spell length that on average is covered by the imposed sanctions for the subset of sanctioned individuals.

Table 9 shows that most of the total reform effects estimated with the DID model are due to threat effects, not to the actual imposition of sanctions. In fact, after rescaling the sanction effects to make them comparable to the total reform effects, their size becomes extremely small. In particular, when looking at the full sample and comparing weighted sanction effect and threat effect (Columns 5 and 6), the threat of being in a stricter system leads to a 3.5 percent increase in the exit to job rate, which is more than twice the weighted sanction effect.

An even more extreme pattern is found for male UI recipients. For them the threat effect (9.2 percent job exit increase out of the total 11 percent increase) is larger than for the full

sample. For women, sanction imposition effects are sizable and similar in size to those of men, but become extremely small after weighting them. As a consequence, for this group the weighted sanction effects account for a negligible part of the total reform effects. This is consistent with the fact that for women the total reform effects were not found to be significantly different from zero.

Table 9 – Threat and sanction imposition effects comparison

Group	Total reform effect (1)	Proportion sanctioned (2)	Spell part covered by sanction (3)	Sanction effect (4)	Weighted sanction effect (5)	Threat effect (6)
All	0.05	0.092	49.7%	0.339	0.015	0.035
Men	0.11	0.107	50.3%	0.336	0.018	0.092
Women	-0.05	0.075	48.7%	0.367	0.013	-0.06

Notes: Threat effects computed as the difference between the total effect of the September 2013 reform (Column 1) and the weighted sanction imposition effect (Column 5). The weighting factor is equal to the share of jobseekers sanctioned during the post-reform period (Column 2) multiplied by the average spell part covered by the sanction (Column 3).

6 Conclusions

In this paper I explore threat effects in the context of UI systems, where the job search behavior of jobseekers is monitored and lack of search activity is sanctioned with UI benefits suspension. Despite the goal of monitoring and sanctions is to deter lack of job search of all the unemployed, threat effects have received very limited attention in the UI literature.

In the first part of the paper, I show that male jobseekers significantly and robustly increase their job finding rates in response to a shift to a stricter monitoring and sanctions system. In line with existing evidence, I find even larger effects for the long-term unemployed and no effects for women. These overall reform effects can be the result of changes in threat effects, changes in sanction imposition effects, or a combination of the two. For this reason, I propose a decomposition exercise where I jointly consider these two effects and I show that the threat effects largely dominate the sanction imposition effects.

Overall, this study shows that the threat of sanction imposition can enhance the job search effort of the eligible jobseekers, above and beyond the effect of actual sanction imposition. Since the sanction imposition effects emphasized in the literature account for a minor part of the reform effects, my analyses suggest that the total impact of sanctions may be severely underestimated when focusing solely on sanction effects.

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Appendix A Additional Figures and Tables

Table A.1 – Group averages in the three reform periods, by gender

	Period 1		Period 2		Period 3	
	Men	Women	Men	Women	Men	Women
<i>Panel A: UI group</i>						
Age	37.35	37.50	37.38	37.74	37.48	37.59
Education: compulsory	0.20	0.17	0.21	0.17	0.20	0.17
Education: secondary	0.48	0.46	0.49	0.43	0.49	0.43
Education: upper	0.31	0.37	0.30	0.39	0.31	0.40
Any child below 18	0.34	0.52	0.34	0.52	0.34	0.52
Immigrant	0.50	0.46	0.52	0.52	0.52	0.52
Married	0.38	0.44	0.38	0.44	0.37	0.44
Unemployed 24 months before	0.31	0.22	0.35	0.28	0.34	0.28
Any program in last 24 months	0.03	0.02	0.03	0.02	0.03	0.02
Duration of last unemployment spell	218	177	242	216	256	215
Any program in last 4 years	0.05	0.04	0.05	0.04	0.05	0.04
Previous income (past 3 years)	1802	1468	1813	1524	1911	1552
Nr. observations	18,301	13,884	5,117	3,351	7,129	5,003
<i>Panel B: AS group</i>						
Age	37.69	37.97	37.61	37.91	37.81	38.01
Education: compulsory	0.21	0.20	0.23	0.20	0.21	0.20
Education: secondary	0.48	0.46	0.47	0.43	0.49	0.42
Education: upper	0.30	0.34	0.30	0.37	0.30	0.38
Any child below 18	0.34	0.53	0.34	0.54	0.34	0.53
Immigrant	0.54	0.51	0.56	0.57	0.55	0.58
Married	0.39	0.46	0.40	0.47	0.39	0.47
Unemployed 24 months before	0.34	0.24	0.39	0.32	0.38	0.32
Any program in last 24 months	0.03	0.03	0.04	0.03	0.04	0.03
Duration of last unemployment spell	243	191	269	229	273	240
Any program in last 4 years	0.05	0.04	0.05	0.05	0.06	0.04
Previous income (past 3 years)	1671	1364	1613	1334	1753	1428
Nr. observations	9,636	6,929	2,998	1,974	4,838	3,161

Notes: Average observables by gender and reform period as defined by the two reform dates (September 2013 and January 2014). All socio-economic characteristics and previous labor market history measured at the inflow into unemployment. Previous income in 100s SEK. *, ** and *** denote significance at the 10, 5 and 1 percent levels.