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) job-seekers 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 males, 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.

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. But 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 misuse of the UI systems through such policies closely resembles attempts to prevent crimes through punishments within criminal prosecution systems. In the economics of crime literature, the terms *deterrence* and *threat effects* refer to the change of behavior due to the fear that a given conduct will be sanctioned; *sanction imposition effect* (or simply *sanction 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 and immediate incentives (see e.g. Weisburd, Einat, and Kowalski, 2008, and Hawken and Kleiman, 2009).[3] In crime studies, most attention has been put on deterrence for two main reasons: first, deterrence can directly modify the behavior of *all* individuals eligible to sanctions (not just of the fraction of those actually sanctioned); second, it has substantial cost-saving potential as compared to the actual sanction imposition, since crime prevention through deterrence does not even require that offenders are identified.

In sharp contrast with the focus on deterrence in the crime literature, almost all studies in the context of UI systems have analyzed the effect of the actual imposition of monetary fines (benefit sanctions) due to lack of job search (the misbehavior). This paper brings together the insights of the crime and UI literatures. Starting from the idea that deterrence is potentially crucial also in UI systems, I study both threat and sanction imposition effects

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[1] UI systems can 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 sanctions size reduce the value of committing a misbehavior (Becker, 1968). A similar setting can be applied in the context of UI systems where individuals' job search activity is monitored, and lack of sufficient job search is sanctioned.

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

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.

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

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, the threat effect is the one that really matters. This is because the main objective is to diminish moral hazard in the entire population of job-seekers exerting low search effort, not just for 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 job-seekers groups before and after the policy change in a difference-in-differences (DID) setting. The job-seekers that I compare are the unemployed individuals receiving UI benefits (UI group) and the longer-term unemployed that 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 nearly non-existing and monitoring intensity was moderate, the stock of UI recipients started being subject to a considerably stricter policy regime. The reform resulted in a considerable increase in the number of UI sanctions issued. Moreover, monitoring got stricter due to the mandatory requirement to submit monthly reports of the job search activity. In January 2014, a second reform introduced the same monthly activity reports tool for the stock of

longer-term unemployed 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 job-seekers as controls), while the second reform allows me to estimate the effect of stricter monitoring on the AS group (using the UI job-seekers as controls). Importantly, the two reforms gives 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 by 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 job-seekers.

I find large and significant reform effects for male job-seekers, and especially for the long-term unemployed affected by the second reform (21% increase in the job exit rate). The fact that job-seekers 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 reforms 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

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[4] If job-seekers dislike being caught breaking the job search rules, then a monitoring-only regime without compelling consequences in the form of monetary fines can still provide job search incentives. Job-seekers may also value the submission of activity reports if they believe that this improves the quality of the job search assistance provided by the caseworker.

flexible bivariate duration model where I jointly model the exit to job rate and the sanction 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 job-seekers, most of the total reform effect is attributable to the threat component, which accounts for a 9.2% increase in the job exit rate out of the total 11% 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, that is, effects on the individuals actually sanctioned. A likely reason for this lack of evidence on deterrence effects is that their identification is challenging. It requires that the researcher can 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 providing direct evidence of the threat effect of benefit sanctions is by Boone, Sadrieh, and van Ours (2009). 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 positive probability of being

sanctioned. They find that the threat of introducing the sanction 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 to the real-world environment and incentives faced by job-seekers.

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 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. They use this in a simulation exercise where 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 job-seekers).

This paper also relates to a large 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 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.

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[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 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. Job-seekers older than 20 years can be eligible either to basic UI compensation or to income-related UI compensation (IAF, 2014b). The entitlement conditions to basic UI benefits are being registered 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 to basic UI benefits gain the right to (higher level) income-related UI benefits under two additional conditions. First, the newly unemployed needs to be voluntary member of a UI fund for at least 12 months (*membership condition*).[9] Second, the person must have worked at least 6 out of the 12 months prior to unemployment, at least 80 hours per month (*work condition*). Full-time unemployed recipients are entitled to a full 300-days UI benefit 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 (≈ €35-75). The lower bound corresponds to the basic UI. Job-seekers eligible to income-related benefits are entitled to 80% of their former salary for the first 200 unemployment days and 70% for the remaining 100 days, capped at 680 SEK 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-days UI period. This allows me to know at which duration time the individuals exhaust their UI benefits. I refer to this first group of job-seekers as the *UI group*.

After exhausting their UI benefits, job-seekers become eligible to receive activity support (AS) upon enrolling into the so-called *Job and development program*. The daily transfers are equal to 65% of the previous earnings, with the same minimum and maximum levels as

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[8] People not satisfying these conditions can 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% 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.

for the UI benefits.[11] Since I restrict my attention to job-seekers with full UI replacement period at the inflow, in my sample people that reach 420 unemployment duration days are eligible to transition to the Job and development program and to start collecting activity support benefits. I refer to this second group of job-seekers 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 job-seeker's activities are monitored by a caseworker at the PES office. Caseworkers inform job-seekers about the general conditions for UI entitlement, the requirement of seeking 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 which they propose ALMPs, refer appropriate vacancies, and provide counseling. During these meetings caseworkers also monitor the job-seeker's 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 of a violation of the rules, the caseworker sends a notification to the UI fund, which carries out an investigation and takes the decision to impose a sanction.[13]

The Swedish sanctions system is characterized by a staircase model, with increasing

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[11] The program provides long-term unemployed with targeted activities corresponding to 75% of the 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 job-seekers ineligible to UI benefits become eligible to receive activity support if they are registered as unemployed or enrolled in a labor market policy program for 14 months. This group of unemployed 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 unemployed older than 24 years of age.

[13] The proportion of notifications leading to a sanction for the 2013-2014 period is close to 80% (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.

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 rules, and job quits with no valid cause. In the pre-reform period, the refusal of suitable job offers without acceptable reason is punished with a 25% benefits reduction for 40 days at the first offense, with a 50% 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 up the action plan, and failing to apply to assigned job. 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 sanctions rate (see also Gray, 2003). As discussed by van den Berg and Vikström (2014), who study the effect of sanction imposition in the Swedish pre-reform 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. The only from of monitoring occurred through meetings with the caseworker, which took place less than once a month on average (0.8 job-seeker meetings per month; Liljeberg and Söderström, 2017). Thus, the pre-reforms period is characterized by moderate monitoring and very few sanctions.

During the time frame of the analyses, AS recipients are not subject to any sanctions. In principle, the right to activity support 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 provide better job search incentives for the unemployed through enhanced monitoring technology and increased sanction rate (see 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. The reports are typically submitted electronically, and caseworkers should use them to monitor the UI recipients' job search effort and to provide job search assistance.[14] The activity reports gave caseworkers a new and improved way to detect violations of the rules.

Recall that in the pre-reform period caseworkers' monitoring activity was exclusively carried out during meetings with the job-seekers. 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 policy change (Liljeberg and Söderström, 2017). Thus, the new activity reports led to tighter monitoring of the job-seekers.

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 making caseworkers more willing to impose sanctions in case of infringements.[15] Second, failing to submit a monthly activity report on time was included among the reasons to receive a sanction. Third, some notifications started being automatically sent to the UI funds (failure of showing up at meetings or to submit the activity report), and caseworkers were no longer able to withdraw notifications sent to UI funds. Overall, the aim was to make sanction process more efficient and less arbitrary.

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

As mentioned, the reform also introduced a less harsh sanction schedule. Figure 2 shows that before the reform, the average sanction size was around 20 suspension days. 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 UI suspension days within the first 12 months of unemployment. This provides a measure of the expected sanction cost, which

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[14] According to survey evidence, in about 80% of 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 the first 3 times, and loss of entitlement until new work requirement (capped at 112 days) for the fourth time. UI eligibility sanctions (including the failure to submit the activity report) correspond to a first time warning, 1 day, 5 days, 10 days, and loss of entitlement for the subsequent infringements.
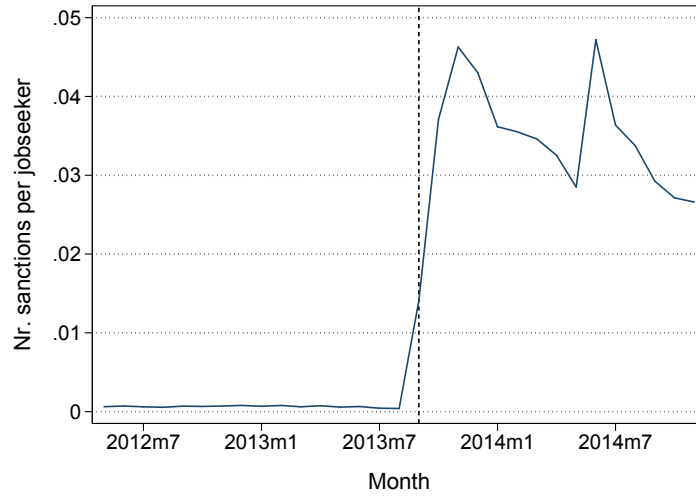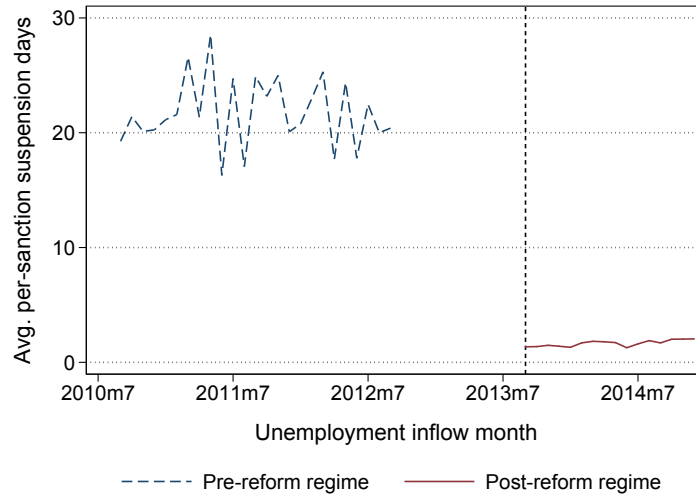
Figure 1 – Number of sanctions per job-seeker

Figure 2 – Average sanctions length in days

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 was virtually zero, so that the increase in the number of sanctions more than outweighs the decrease in the size of the sanction. Thus, unless job-seekers are extremely risk averse, the new sanction sys-

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

tem provides considerably enhanced job search incentives than 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

Figure 3 – Expected sanction cost per-newly unemployed
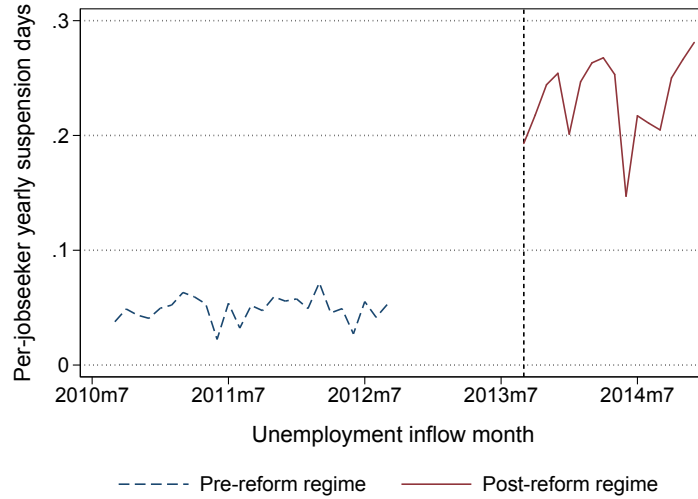
new monthly activity reports and the sharp increase in the number of sanctions implied a substantially stricter monitoring and sanctions system. No other changes of the UI system were made.

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 job-seekers who exhausted their UI benefits and start receiving activity support benefits. The goal of the reform was to make the overall monitoring system homogeneous across UI and AS job-seekers. Before January 2014, the AS group was only subject to monitoring through caseworker meetings, and not to sanctions.[17] Th reform enhanced monitoring by extending the system of monthly activity reports already in place for the UI job-seekers to all AS recipients. To illustrate this, Figure 4 shows the aggregate number of monthly activity reports per job-seeker. The figure shows a first increase in September

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[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
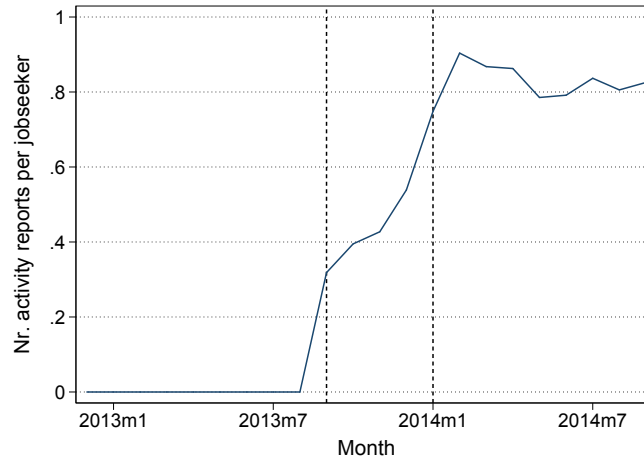
2013, due to the activity reports introduction for the UI recipients, and a second increase in January 2014, relative to the second reform affecting the AS group. The second reform did not change the sanction rules, so that the sanction rate remained practically zero for the AS group also after the reform. 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 compelling 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 roll-out 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 job-seekers that remain unemployed after 420 days are eligible to transition to the AS group (the individuals receiving activity support upon enrolling into the Job and development program). Thus, these two groups are

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[18] Under the new monitoring rules, job-seekers can have increased incentives to search even in the absence of benefit sanctions. This is the case if they dislike being caught breaking the rules or if they value the regular submission of the activity reports (e.g. because they believe that this may enhance the quality of the job search assistance provided by the caseworker).

exposed to similar business cycle conditions since they compete on the same labor market, and are composed by very similar individuals (in fact, all AS recipients originally *were* UI recipients). The main difference between the two groups is that the AS job-seekers are longer-term unemployed.

For each reform, I compare 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 policy shift, averaged across the treated individuals. Since the final goal is to quantify threat effects, these total reforms estimates still need to be decomposed into a threat effect and a 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 monthly activity reports introduction 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 into 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 since 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. 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 days threshold and the reforms dates contribute to the identification of the parameters of interest. Figure 5 shows the unemployment duration margin used for identification of the first reform effect. The left- and right-hand side 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 null, 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 unemployment duration, and plunges after the 420-days threshold. Note that in Figure 5 the hazard does note exactly reach 0 for the AS group, since the ITT setting introduces some noise.

Figure 5 – Sanction hazard in the two regimes
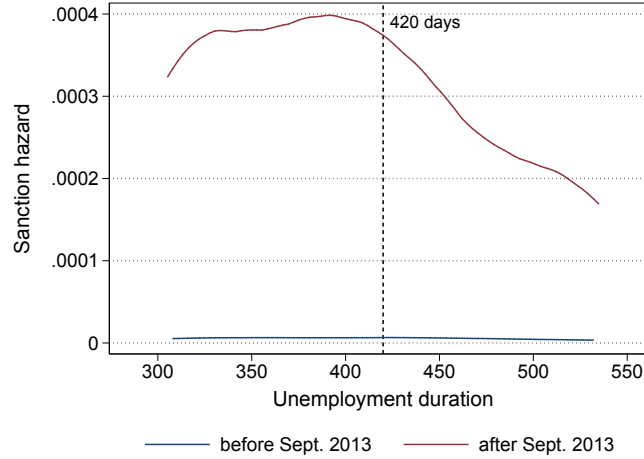
Identification of the reforms effects in the DID setting requires absence of differential time trends in the two groups and no anticipatory effects of the reform. If this is the case, the observed pre- and post-reform average outcomes of the comparison group can be used to retrieve the counterfactual 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 job-seeker group. Define $D^{(1)} \equiv D_d^{UI} \cdot D^{\text{Sept2013}}$ 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^{Sept2013} is a time-varying indicator for being in the post-September 2013 period. Moreover, $D^{(2)} \equiv (1 - D_d^{UI}) \cdot D^{\text{Jan2014}}$ is the second reform indicator equal to one for the AS group after January 1st 2014.[19] I estimate the following

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

DID-duration model for the hazard of exiting unemployment:[20]

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

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 $d = 420$ the job-seekers 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 based on the group of belonging 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 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

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[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 the duration dependence component.

[22] The main effects D^{Sept2013} and D^{Jan2014} 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.

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 job-seekers are treated by 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.

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 youths are subject to special eligibility rules for the Job and development program participation, and older workers may be eligible to early retirement schemes and other targeted policies. The analyses also exclude all disabled individuals. 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 with duration between 280 an 560 days, i.e. close to and centered around the 420-days 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 in September 2013, and I include spells up until March 2015, where any ongoing spells are right-censored. This makes sure that enough pre-reforms observation is 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.

Table 1 – Group averages in the three reform periods

	Period 1		Period 2		Period 3	
	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	201	221	233	257	236	259
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 characteristics of the UI and AS job-seekers, by time period (as defined by the September 2013 and January 2014 reforms). All socio-economic characteristics and previous labor market history are measured at the unemployment start. Previous income in 100s SEK. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

The table shows the AS group is composed by job-seekers that are lower-educated and more likely to be immigrants and married. Compared to the UI group, they are also more weakly attached to the labor market and have lower income in the three years preceding the spell start. All this shows that the longer-term unemployed (AS group) have less favorable characteristics than their shorter-term unemployed counterparts (UI group). Note that this is not a problem for the identification of the reforms effects, since the DID model adjusts for all time-fixed differences between the two groups. What would be problematic is *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 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 along which ALMPs effects vary (see e.g. Card, Kluve, 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 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.

Next, Column 2 of Panel A reports the estimates for the actual reform period. The table shows that the re-employment rate for male job-seekers is significantly affected by the first monitoring reform (11% increase).[23] The effect of the second reform is even larger, with a 21% 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, recall that the two reforms affect different groups of job-seekers: the second reform affects long-term unemployed (AS group), while the first one affects more short-term unemployed (UI group). If the long-term unemployed react differently to monitoring incentives, this can explain the different effects of the two reforms. In fact, a common finding in the literature is that 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-reform strictness of 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.

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[23] Estimates are interpreted as percentage changes in the re-employment rate when the corresponding covariates are increased by one unit. This is because the model coefficients measure changes in log re-employment rates.

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

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

	Placebo period	Reform period	
	(1)	(2)	(3)
Panel A: Men			
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
Panel B: Women			
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.

Interestingly, Panel B shows no significant effects of any of the two reforms for women. One way to interpret the very different effects for men and women is that gender differences reflect differences in labor market attachment. In fact, the existing evidence suggests that ALMPs are more effective for more disadvantaged groups, such as long-term unemployed (Card, Kluve, and Weber, 2017). This is confirmed by the fact that in Sweden and other Nordic countries (where the gender gap is low by international comparisons), the heterogeneous effects of training and other ALMPs by gender are less pronounced; accordingly, women tend to benefit relatively more than men from ALMPs participation in countries where the gender gap is large (Bergemann and Van Den Berg, 2008).

To better understand 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 – Total effects of the monitoring and sanction reforms

	Placebo	Reforms period	
	-1 Year	Main	With covariates
	(1)	(2)	(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.

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% increase in the exit to job rate of AS job-seekers due the monitoring reform. The point estimate for 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 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.

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

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 job-seekers 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 to study 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 in the same DID regression framework as previously described.[26] The cross-reforms groups differences in the predicted unemployment are also insignificant. All this suggests that dynamic selection is not an issue, giving further support to my main results and identification strategy.

4.2.2 Robustness analysis

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 calender time indicators. Here, Column 2 of Table 5 reports model estimates where I add

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[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

| | 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 |
Outcome	(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	20.171	24.504	23.321	4.334	0.462	-1.183	0.859
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
Joint significance p-value				0.409		0.880	
Predicted unemployment duration	1.549	1.421	1.470	-.129	0.554	.05	0.841

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 reforms 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-days 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.

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 job-seekers 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 transition to the AS group already before this threshold, and others do so after the threshold. This is not problematic for identification, since the exogenous 420-days threshold is used for *all* job-seekers, but it may reduce the precision of the estimates. Therefore, in Column 3 I explore if 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 420-days 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 as in the main analyses.

The UI recipients that stay 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 of the two reforms. To check for this, Column 4 reports estimates when the period before the AS transition are "dummied out" in similar ways as above (with the 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-days threshold. In both cases, the results are very similar to those in the main analyses.

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

Table 6 – Robustness analyses for the total reforms 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)
Panel A: Males						
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
Panel B: Females						
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-days 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 reforms 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.

4.2.3   Extended placebo analyses

Identification of the reforms effects is based on variation across calendar time and spell duration. Table 7 shows 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, in Columns 1 and 2 I show results when moving the entire sampling window back in time one and two years, respectively. 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 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   Relationship between threat and sanction effects

To obtain the threat effects of the monitoring and sanction regime, it is necessary to decompose the total reform effects into a threat effect component and a sanction imposition effect. I focus on the sanctions imposed during the new monitoring and sanctions regime, where the large increase in the sanctions rate took place.

Since the interest is in estimating sanction effects under the new, stricter rules, I sample unemployment spells starting after September 2013. I merge UI benefit sanctions to the spells and right-censor durations at the end of 2015. I proceed as in the main analyses, and select only spells of full-time and non-disabled unemployed between 25 and 50 years of age at the inflow. I sample only spells of UI recipients.[28] With respect to the UI benefit sanctions, I do not distinguish between the different types of sanctions, and I focus on the

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

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

first sanction during unemployment spells. To avoid misclassification, I restrict the spells to be at 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).

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 can 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 also 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:

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

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

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).

Identification of this ToE model relies on the following assumptions (Abbring and van den Berg, 2003). First, individuals must not anticipate the *exact* timing of the sanction (*no anticipation*). In this setting several aspects of the sanction assignment process are unknown to the job-seeker, for instance because the actual decision is taken by the UI fund. Moreover, even if some workers anticipate the timing of a sanction, the time between a notification is sent to the UI and the decision by the UI fund is typically short, so that the sanction imposition estimates are unlikely to be due to 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, implying that the observed characteristics are uncorrelated with the unobserved characteristics (*random effects assumption*). These 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 quick successions 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   Sanction effects model specification

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 van den Berg, Lombardi, 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 van den Berg, Lombardi, 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.

Here, I use three different criteria. They are a function of the number of support points (W), the overall number of model parameters ($k \equiv k(W)$), and the number of spell parts used in the estimation ($\mathbf{N}$). 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.

5.1.3 Sanction effects estimates

In accordance with what was done in the reforms analyses, I estimate sanctions 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 job-seekers exit to job 24.3% 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 in the pre-reform period. Overall, the estimated sanction effect is substantial, but smaller than the effect of sanction in other countries. 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 increase the re-employment rate by around 50% (Lalive, van Ours, and Zweimüller, 2005).

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% faster after a sanction, while the same increase is 36.7% for women. The fact that the sanction effect is very similar for men and women is different from what was found in the analyses of the total reform effects, where we saw large effects for men but small and insignificant effects for women (for both reforms). From these estimates

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

[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 the percent changes in the re-employment rate. This because the sanction effect coefficient δ corresponds to the change in the log re-employment hazard when the sanction indicator switches from 0 to 1, *ceteris paribus*.

of the sanction effect, it is clear that the heterogeneity in the total effects of the reforms are not explained by difference in the effects of imposed sanctions. Instead, it must be due to differences in threat effects. This motivates the decomposition analyses and the calculation of the threat effect in the next section.

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

	Baseline (1)	Males (2)	Females (3)
Sanction effect	0.340***	0.336***	0.367***
	(0.015)	(0.019)	(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.

5.2   Relationship between threat and sanction effects

In this section I decompose the total effect of the first reform into its 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 job-seekers were subject to sanctions already before September 2013 (although as mentioned the sanction rate was very close to zero). Lastly, the magnitude of threat effects may in principle change over the time spent in unemployment. In the decomposition exercise, I simplify the analyses 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:

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

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 $p \cdot coverage$ 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 sanction for the subset of sanctioned individuals.

Table 9 – Threat and sanction imposition effects comparison

Group	Total reform effect	Proportion sanctioned	Spell part covered by sanction	Sanction effect	Weighted sanction effect	Threat effect
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.05	0.092	49.7%	0.339	0.015	0.035
Males	0.11	0.107	50.3%	0.336	0.018	0.092
Females	-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 job-seekers sanctioned during the post-reform period (Column 2) multiplied by the average spell part covered by the sanction (Column 3).

Table 9 shows that most of the total reform effects estimated with the DID model is 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 reforms 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% 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% job exit increase out of a total 11% 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, 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.

6 Conclusions

In this paper I explore threat effects in the context of UI systems, where the job search behavior of job-seekers 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 unemployed, threat effects have received very limited attention in the UI literature. In fact, most studies have focused on changes in the job exit rates of individuals actually sanctioned. In contrast with this, here I study both sanction imposition and threat effects in the same policy setting. In doing so, I provide the first quasi-experimental estimates of the threat of benefit sanctions.

In the first part of the paper, I exploit two reforms of the Swedish monitoring and sanctions systems in a DID setting. I show that male job-seekers significantly and robustly increase their job finding rates. In line with existing evidence, I find even larger effects for the long-term unemployed, and no effects for women. These overall reforms 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 the two effects. This is the first time that this type of exercise is proposed in the context of UI monitoring and sanctions, where so far no evidence has been produced on the relative importance of threat effects and sanction imposition effects. I find that threat effects largely dominate sanction imposition effects.

Overall, this study shows that the threat of sanction imposition can enhance job search effort of the eligible job-seekers, 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, the total impact of sanctions may be severely underestimated when focusing solely on sanction effects.

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