Work Incentives?
Ex Post Effects of Unemployment Insurance Sanctions – Evidence from West Germany

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Abstract

Unemployment insurance (UI) sanctions in the form of benefit reductions are intended to set disincentives for UI recipients to stay unemployed. Empirical evidence about the effects of UI sanctions in Germany is sparse. Using administrative data we investigate the effects of sanctions on the reemployment probability in West Germany for individuals who entered UI receipt between April 2000 and March 2001. By applying a matching approach that takes timing of events into account, we identify the ex post effect of UI sanctions. As a robustness check a difference-in-differences matching estimator is applied. The results indicate positive effects on the employment probability in regular employment for both women and men.

JEL Code: J64, J65, J68.

Keywords: unemployment insurance sanctions, dynamic matching.

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1 Introduction

In the last years activation strategies intended to get unemployed individuals back to employment have become increasingly important. Besides active labour market policies (ALMP), activation strategies include regular reporting and confirmation of unemployment status, monitoring of the job-search efforts and/or action plans (Tergeist/Grubb, 2006). Unemployment insurance (UI) benefits usually depend on several eligibility criteria, i.e. UI benefit recipients have to comply with certain rules in order to be eligible for UI benefits[1]. In this context, punitive sanctions have received increasingly more attention. UI benefit sanctions in the form of benefit reductions are intended to set an incentive for UI recipients to reenter work.

Studies on punitive sanctions usually distinguish between an *ex ante* effect of a sanction and an *ex post* effect. If the mere possibility of being sanctioned raises the search efforts of UI recipients *ex ante*, this is called the *ex ante* effect, while the effect arising from the actual imposition of a sanction is called the *ex post* effect. According to job search theory, at the moment of the imposition of a sanction an individual will search for a job more intensely and lower his/her reservation wage, which finally will raise the transition rate into employment. In this paper we focus on the *ex post* effect of UI sanctions in Germany for a random sample of persons who entered UI receipt from April 2000 until March 2001 in West Germany[2]. Our main question is whether the imposition of a UI sanction due to refusing a placement proposition[3] or an ALMP training sets an incentive to reenter work. The key outcome variable is the employment probability after a sanction has been imposed. As we do not have experimental data, where treatment is implemented randomly we have to be aware of a potential selection bias due to endogeneity of treatment. We respond to this problem by using a control group that is built by matching algorithms. We apply a propensity score matching approach that takes timing of events into account by dividing the sample into three different strata of individual unemployment durations. The treatment group consists of those UI recipients who were sanctioned during the stratum considered (and not before), while the controls are the ones who have not been sanctioned during the stratum considered (and neither before) and who are still in UI receipt at the start of the week of the sanction. Using informative data of the federal employment agency (FEA) we rely on the assumption of conditional independence and present the identification and the estimation of the *ex post* effect of UI sanctions. As a robustness check, we apply a difference-in-differences matching estimator.

[1] The term unemployment insurance benefits is used for the German term "Arbeitslosengeld I".

[2] Since during the observation period the sanction rates in East Germany were about half of those in West Germany this analysis is restricted to West Germany.

[3] A placement proposition is a job vacancy that the caseworker proposes to the UI recipient.

2 Literature review

There are several studies about the effects of punitive sanctions (e.g. Fredriksson/Holmlund (2003); Jensen/Rosholm/Svarer (2003); Boone/Sadrieh/van Ours (2004)). In general, results of most of these studies show that punitive sanctions have a positive impact on the transition from unemployment to employment. In a theoretical contribution Fredriksson/Holmlund (2003) analyse time limits of UI payment duration, monitoring in combination with sanctions and workfare as three crucial features of UI policies. Their simulations show that in a system with monitoring and sanctions, search incentives are set most effectively. Jensen/Rosholm/Svarer (2003) analyse the effects of a youth unemployment program (YUP) on the transition rates from unemployment to schooling and employment using quasi-experimental data. They focus on three different effects within this program: an announcement effect, a direct programme effect, and a sanction effect. While they did not find evidence for an effect of mere announcement of the YUP in form of a letter, according to their research results the program itself and also (somewhat weaker) sanctions have a positive effect on the transition rate out of unemployment among young Danish unemployed. Boone/Sadrieh/van Ours (2004) use data of an experiment among 62 students in order to investigate *ex ante* and *ex post* effects of unemployment benefit sanctions and find evidence for both. Their results suggest that the effect of the possibility of being sanctioned (*ex ante* effect) is stronger than the effect of the actual imposition of a sanction (*ex post* effect). These articles investigate the *ex post* effects of sanctions either with experimental data or they do not investigate the *ex post* effect explicitly or only for a subgroup of young unemployed (Jensen/Rosholm/Svarer, 2003). In contrast, the following studies identify the *ex post* effect with non-experimental data using survival analysis: Abbring/van den Berg/van Ours (2005), Lalive/van Ours/Zweimüller (2005), van den Berg/Klaauw/van Ours (2004), Svarer (2007) and Müller/Steiner (2008).

Abbring/van den Berg/van Ours (2005) use administrative data of persons who entered unemployment in 1992 and analyse the *ex post* effects of UI sanctions in the Netherlands. The sanctions they analyse range from a 5% benefit reduction for four weeks up to a 30% benefit reduction for 13 weeks. Their results indicate that punitive sanctions significantly raise individual transition rates into employment of UI recipients. The increase of the transition rates they found ranges from 36% for males in the banking sector to 98% for females in the metal industry sector. By using administrative data of Switzerland Lalive/van Ours/Zweimüller (2005) are able to analyse the effects of sanctions more precisely as they were able to distinguish between the *ex ante* and the *ex post* effect explicitly. The UI sanction they analyse is a 100% benefit reduction ranging from 14 to 60 days. Their results on the *ex post* effect indicate that unemployment duration decreases by about three weeks due to the announcement and the actual imposition of the UI sanction. According to their results, these effects can be separated from each other: the exit rate from unemployment increases by 28% after a warning has been imposed, whereas the actual imposition of a sanction additionally increases the exit rate by 23%.[4] While the analysis of Abbring/van den Berg/van Ours (2005) and Lalive/van Ours/Zweimüller (2005) focus on the group of UI benefit recipients, van den Berg/Klaauw/van Ours (2004) investigate *ex post* effects of punitive sanctions on welfare recipients. Using administrative data from Rotterdam they find an increase of the transition rate from welfare to work after a sanction was imposed. According to their results, the hazard to leave unemployment is about twice as large as before. Svarer (2007) investigates the effects of sanctions on the exit rate from unemployment in a sample of Danish unemployed and finds empirical evidence for *ex post* as well as *ex ante* effects. According to his results the exit rate is increased by more than 50% after the imposition of a sanction. Finally, Müller/Steiner (2008) analyse *ex post* effects of sanctions on UI as well as unemployment assistance (UA) recipients in Germany. They find positive short- and long-term effects of benefit sanctions on the transition from unemployment to employment. In sum, the studies we found on the *ex post* effect of sanctions used survival analysis and found positive incentive effects of sanctions.

Additionally two studies on the determinants of being sanctioned were found: Müller/Oschmiansky (2006) focus on a model of the determinants of regional sanction rates in Germany.[5] Their findings suggest that there are different levels of determinants of a sanction, i.e. a sanction is not only determined by the individual's behaviour itself. According to results of Müller (2007) who analysed the determinants of being sanctioned at the individual level, the age, the level of disability and the qualification, but also the local sanction policy affect the individual sanction risk.

3 Unemployment benefit sanctions in Germany

During our observation period[6], UI benefits were paid if a person had been employed in a job subject to social contribution for at least 12 months within the seven years previous to unemployment. It depended on the duration of the previous employment period and the age for how many months unemployment insurance was paid. The maximum duration of UI benefits receipt was 32 months for people who were older than 56 years old and who had been employed for at least 64 months in the seven year previous to unemployment.[7] Until 2005 a UI benefit recipient received means-tested unemployment assistance (UA) after his claims to UI benefits terminated. Table 1 gives an overview of the entitlement lengths of

[4] According to their results on the *ex ante* effect a one standard deviation increase in the strictness of the sanction policy will reduce individual unemployment duration by one week.

[5] Müller/Oschmiansky (2006) define the sanction rate as the ratio between the sum of effective sanctions imposed in a local employment agency due to refusal of a placement proposition or of an ALMP measure, and the stock of benefit (UI, unemployment assistance (UA), integration aid) recipients of the respective local employment agency.

[6] We use an inflow sample into UI benefits between April 2000 and March 2001.

[7] In 2006 changes of Social Code (SC) III have decreased UI entitlement lengths for various age groups, e.g. possible duration of UI benefits receipt was limited to 18 months for persons older than 54.

UI benefits during our observation period.

Table 1: UI entitlement length in 2000/2001

age in years	employment in months during 7 years previous to UI receipt (SC III, §124, §127)	UI entitlement length in months
<45	12	6
<45	16	8
<45	20	10
<45	24	12
$\geq$ 45	30	14
$\geq$ 45	36	18
$\geq$ 47	44	22
$\geq$ 52	52	26
$\geq$ 57	64	32

Source: Social Code (SC) III - Employment Promotion. 4^{th} edition (February 15 2001). Text edition, Nuremberg (Federal Employment Agency).

The monthly benefit amount received was 67% of the previous monthly net wage for unemployed persons with children and 60% for those without a dependent child.[8] The time period of employment relevant for the calculation of the monthly UI benefits amount was 12 months.

In the years 2000 and 2001 there were neither changes in the sanction legislation nor in the labour market policy affecting sanctions (Karasch, 2005). An unemployment benefit recipient was sanctioned if he did not comply with certain rules. In case of both, short-term and long-term sanctions, UI or UA benefits stopped *completely* for a certain period. In general there were five sanction reasons: (1) If a person had voluntarily quit his job, the entitlement time for UI benefits was shortened by 25% or at least twelve weeks, i.e. the person did not receive UI benefits at all at least for the first twelve weeks of unemployment. In case of hardship the sanction could be limited to six weeks and if the job would have ended within four weeks anyway, the person was sanctioned by three weeks only. (2) If he refused work, in the sense that he refused to apply for a reasonable job that was proposed to him (placement proposition) or that he refused a reasonable job that was offered to him, a person was sanctioned by twelve weeks and three weeks respectively if the job would have been temporary only. (3) Refusing or (4) dropping out of an ALMP measure caused a sanction of twelve weeks and six weeks, respectively, if the measure was intended to be less than six weeks. Finally, if an unemployed person failed to report to the local employment agency or to a medical or psychological appointment (5), the UI benefits stopped for two weeks. The different types of sanctions according to the SC III valid in 2000/2001 are summarized in Table 2. If the cumulated duration of sanctions adds up to 24 weeks, a UI recipient lost the claim to UI benefits ("sanctions account regulation").

[8] The replacement ratio for UA was 57% and 53% respectively.

Table 2: Sanction legislation in 2000/2001

Type	Duration	Notes	Reduction
(1) Voluntary quit	At least 1/4 of UI duration ($\geq$ twelve weeks)	6 weeks in case of hard-ship, 3 weeks if the job had ended any-way within 4 weeks	100%
(2) Refusal of work	twelve weeks	3 weeks if a temporary (<6 weeks) job was refused	100%
(3) Refusal of ALMP measure	twelve weeks	3 weeks if integration measure $<$ 6 weeks	100%
(4) Drop out ALMP measure	twelve weeks	3 weeks if integration measure $<$ 6 weeks	100%
(5) Failure to report to job center or to medical / psychological appointment (Säumniszeit)	2 weeks	2. failure: 4 weeks	100%

Source: Social Code (SC) III - Employment Promotion. 4^{th} edition (February 15 2001). Text edition, Nuremberg (Federal Employment Agency).

Sanctions are not implemented automatically, but at the discretion of the local employment agency and even the caseworkers. Empirically, sanctions are implemented quite heterogeneously between local employment agencies (Müller/Oschmiansky, 2006) and even within one local employment agency we assume the probability of one person to be sanctioned to be influenced by the assigned caseworker.

From 1996 until 2003, the yearly sanction rates in West Germany, calculated as total number of sanctions divided by the stock of UI and UA recipients, ranged between 9.7% in 1997 and 13.6% in 2001, while in East Germany in general the sanction rates were lower: they ranged between 4.1% in 1997 and about 6% in 1999 and 2003. Sanction rates differ by the type of sanctions. Most sanctions are implemented due to voluntary quits: 75.7% in 2000 and 75% in 2001. The following table reports the numbers of sanctions by type for West Germany from 1996 until 2003:

Table 3: Numbers of sanctions by type in West Germany 1996-2006*

Year	Absolute numbers total	(1) Voluntary quit	(2) Refusal job	(3) Refusal ALMP measure	(4) Drop out of ALMP measure
1996	205744	88,4	5,8	3	2,8
1997	214021	85,1	8,2	3,6	3,1
1998	241076	80,7	10,8	4,1	4,4
1999	255095	78,6	11,9	4,3	5,2
2000	237228	75,7	15,4	4,3	4,6
2001	244851	75	17,7	3,3	3,9
2002	252592	73,2	18,7	4	4,1
2003	331141	58	34	4,4	3,7

Source: Labour Market 2003; Official Announcements of the Federal Employment Agency 52. Special Edition, July 15 2004, Nuremberg. *Note: Short-term sanctions due to not showing up at the job center are missing as there are no official statistics on this sanction type until 2005.

The abrupt jump of the share of sanctions due to refusal of work in 2003 is most probably caused by an internal circular of the employment agency (Rundbrief 55/03) in which the local employment agencies and the caseworkers were called on to activate unemployed persons more effectively. We will use this observation to support the idea of exogenous variation in the individual sanction probability.

According to job search theory those sanctions are of interest in relation to the *ex post* effectiveness that are imposed during open unemployment. Thus we do not analyse the effects of sanction types (1) and (4). As short-term sanctions due to not showing up at the agency (5) are assumed to be very different to long-term sanctions regarding their implementation, they are analysed separately. In the empirical analysis below we focus on the effects of sanctions due to refusal of placement propositions (2) or an ALMP measure (3).

4 Job-search-theory with sanctions

The theoretical framework is a job search model with sanctions introduced by Abbring/van den Berg/van Ours (1996; 2005), the latter referred to as ABO05. Before we derive a hypothesis on the sanction effect, it is useful to present some general thoughts about a UI system with sanctions. A basic job search model with endogenous search intensity is presented e.g. by Mortensen (1986). ABO05 extend this model by introducing sanctions. Following ABO05, we consider a situation where an individual has become unemployed and currently is searching for a job. We take different parameters into account that are assumed to influence the job search process. First, UI recipients receive a certain flow of unemployment benefits b. We assume that besides the pecuniary value of the UI benefits, there is a non-pecuniary utility of being unemployed which is also included in b. Second, we assume that every UI recipient searches with a particular search intensity s. The level of s is chosen by the individual himself. Third, the rate at which job offers arrive is defined as $\lambda(s)$, where $\lambda(s)$ is increasing in s, i.e. the more intensely a UI recipient will search for a job, the more likely he will be offered a job. The wage that is offered is randomly drawn out of a wage offer distribution $F(w)$. If a job is offered the UI recipient has to decide whether to accept the job given the wage offered or to search further. Fourth, the search costs $c(s)$ increase in s, i.e. the more intensely he searches for a job, the higher the search costs. As our model is based in a world with rational actors, we assume that every UI recipient aims to maximize his expected present value of income over an infinite horizon of time. Finally, it is the reservation wage ϕ together with the search intensity s that defines the optimal strategy of a UI recipient. Following ABO05 we introduce sanctions in this model. We denote the benefit level a UI recipient receives before a sanction is imposed by b_1. The level of reduction when a sanction is imposed is denoted by r, thus we have $b_2 = (1\text{-}r)b_1$ being the benefit level a UI recipient receives after a sanction is imposed. We distinguish two different aspects of sanctions: the institutional aspect meaning the individual acts in a world where he might be sanctioned (*ex ante*) and the aspect of the actual imposition of a sanction (*ex post*). We consider a UI recipient in a system with sanctions. At first sight one might assume that every UI recipient tries to avoid sanctions. If this was the case and it was possible to avoid sanctions we would not observe sanctions at all. At second sight we might think that UI recipients can perfectly anticipate when a sanction is imposed and define their choices accordingly. This is disproved by ABO05 empirically.

A major assumption of their model is that individuals cannot foresee *when* exactly a sanction is imposed, which corresponds to the so called no-anticipation assumption. ABO05 base this assumption on the observation of regional differences in the strictness with which sanctions are applied. Müller (2007) presents very similar findings for Germany: the transition into a sanction is not only influenced by individual characteristics but also by the strictness of the local employment agency.

Yet we assume, unemployed individuals do know the relationship between their behaviour and the probability of being sanctioned. If the job search intensity exceeds a certain threshold s^* we assume that the probability of being sanctioned is zero. The rate at which a sanction might arrive, i.e. the probability of being sanctioned given no sanction has yet been imposed, is given by $p(s)$, with p decreasing in s.

$$p(s) = \begin{cases} p_0 > 0 & \text{if } s < s^* \\ 0 & \text{if } s \geq s^*. \end{cases} \tag{1}$$

According to equation (1), the more intensely a person searches for a job the lower is the probability of being sanctioned. We assume that the punitive effect of being sanctioned is so severe that the person immediately after the imposition of a sanction will raise his search intensity to a level beyond s^* (s $\geq s^*$).[9] In order to identify the optimal strategy of an unemployed individual we assume R_i to be the expected present value of income, ϕ_i to be the reservation wage and s_i the search intensity with $i = 1, 2$ where $i = 1$ relates to the time period before the imposition of a sanction and $i = 2$ relates to the time period after the imposition of a sanction, respectively. Now we use the Bellman equation to express the expected returns to assets:

$$\rho R_1 = \max_{s_1} \left[bw - \frac{1}{2} c_0 s_1^2 + \lambda_0 s_1 \int_{\phi_1}^{\infty} (\frac{w}{\rho} - R_1) dF(w) + I(s_1 < s^*) p_0 (R_2 - R_1) \right] \tag{2}$$

$$\rho R_2 = \max_{s_2 | s_2 \geq s*} \left[(1-r) bw - \frac{1}{2} c_0 s_2^2 + \lambda_0 s_2 \int_{\phi_2}^{\infty} (\frac{w}{\rho} - R_2) dF(w) \right], \tag{3}$$

with $\rho R_1 = \phi_1$ (reservation wage before the imposition of a sanction) and $\rho R_2 = \phi_2$ (reser-

[9] The model requires some more assumptions (ABO05): λ(s)= λ_0s and c(s)=$\frac{1}{2}$ c_0 s^2. Upon imposition of a sanction, b is permanently reduced from b_1 (benefits level before a sanction is imposed) to b_2 (benefits level after a sanction is imposed). b_1, F, λ_0, c_0, p_0, s^* and the discount rate ρ are constant. An implication of these assumptions is that the optimal strategy the individual chooses is constant within the time interval before a sanction and within the time interval after a sanction. p_0, λ_0 and c_0 are exogenous parameters.

vation wage after the imposition of a sanction). $\mathrm{I}(s_1 < s^*)$ denotes the indicator function being one if the search intensity is below the threshold level s^* and being zero otherwise, i.e. if the probability of being sanctioned is zero. We interpret the right hand side of the equations (2) and (3) as the expected flow of income given the search strategy. In equation (2) this expected flow consists of the following parts:

- the utility of unemployment ($bw - \frac{1}{2}\, c_0 s_1^2$),
- expected additional income when a job is found (the job offer arrival rate times the expected gain of finding a job compared to staying unemployed),
- the expected income drop when a sanction is imposed ($\mathrm{I}(s_1 < s^*)p_0\ (R_2\text{-}R_1)$).

The transition rate from unemployment to employment is assumed to depend on the offer arrival rate λ_0, the search intensity s_i, and the distribution of the reservation wage $\bar{F}(\phi_i)$. It is given by:

$$\theta_{u,1} = \lambda_0 s_1 \bar{F}(\phi_1) \tag{4}$$

$$\theta_{u,2} = \lambda_0 s_2 \bar{F}(\phi_2), \tag{5}$$

with $\bar{F} = 1 - F$.

Regarding the transition rate out of unemployment into employment, this model allows to derive the hypothesis that at the moment at which a sanction is imposed the transition rate from unemployment to employment jumps upwards. This hypothesis is based on the following relations: the expected present value of income after a sanction is lower than expected present value of income before the imposition of a sanction ($R_2 < R_1$), because a sanction reduces the flow of benefits ((1-r) $b_1 < b_1$) and the choice of search intensity after a sanction is restricted by $s_2 \geq s^*$. The fact that $R_2 < R_1$ implies that the reservation wage falls at the moment of the imposition of a sanction ($\phi_2 < \phi_1$), so $\bar{F}(\phi_2) > \bar{F}(\phi_1)$. $s_2 = s*$ also holds, while $s_1 < s*$ because otherwise a sanction could not have been imposed. This implies that $s_2 > s_1$. In sum, we expect the transition rate to jump upwards in the moment when a sanction is imposed ($\theta_{u,2} > \theta_{u,1}$). Regarding the probability of being employed after a UI sanction we derive the following hypothesis:

A UI sanction raises the probability of being employed after it has been imposed.

Though it is not focus of the empirical analysis of this paper, the model allows to derive further mechanisms, e.g. by changing r or p_0: The relation between the level of reduction r and the search intensities s_1 and s_2 can be derived from equations (2) and (3): the higher the reduction r the higher will be the expected negative income change, which will raise s_1 *ex ante*. An increase in r will also lead to an increase of s_2 as the gain of finding a job will increase due to the decrease in the utility of staying unemployed. Whether a rise in p_0 has a positive or a negative effect on the search intensity, depends on whether a sanction has been imposed yet or not. Boone/van Ours (2000) show that the (*ex ante*) effect on s_1 is positive, while the (*ex post*) effect on the difference between s_1 and s_2 effect is negative. Thus if p_0 is increased due to an increased monitoring, the *ex post* effect will decrease.

5 Identification strategy

As we do not have experimental data, where treatment is implemented randomly and thus can be treated as exogenous, we have to control for non-random assignment to treatment, i.e. for the natural selection process. Factors that influence assignment to treatment partly influence the outcome of interest. Therefore treatment and control group would receive different outcomes anyway, even without treatment. We choose our evaluation approach taking the endogeneity of treatment into account.[10]

As we want to evaluate the *ex post* effect of UI sanctions on the reemployment probability of a sanctioned person, we have to face the fundamental evaluation problem: we want to compare the outcome of a sanctioned person i (Y_i^1) with the outcome of the same person i in the situation without having been sanctioned (Y_i^0) at the same point in time (the so called counterfactual outcome). Accordingly, the individual causal effect is the difference between these two outcomes: $\Delta_i = Y_i^1 - Y_i^0$. We can either observe one state or the other, i.e. the individual outcome we can observe is: $Y_i = Y_i^1 \cdot D_i + Y_i^0 \cdot (1 - D_i)$ with $D_i \epsilon \{0,1\}$. The evaluation problem refers to the fact that we cannot *observe* the individual causal effect. Our approach to tackle the evaluation problem is to estimate the average treatment effect on the treated (ATT). In our study the ATT is the expected effect of a sanction for sanctioned UI recipients:

$$\Delta^{ATT} = E(Y^1 - Y^0|X, D = 1) = E(Y^1|X, D = 1) - E(Y^0|X, D = 1), \tag{6}$$

where the average outcome of the treated in the state of being untreated, $E(Y^0|X, D = 1)$, is not observable. What we do observe though is the outcome of the untreated: $E(Y^0|X, D = 0)$.

5.1 Static matching approach

The method of matching can be applied to estimate the ATT if the data is sufficiently rich. Since out data meet this requirement, we chose this method. Matching is based on the assumption, that conditional on the observables X that are not affected by treatment and known by the researcher, Y_0 is independent of treatment assignment, i.e.:

$Y_0 \amalg D|X$.[11] (A.1)

[10] For an early discussion of the consequences of self selection see Heckman (1979).

[11] The stronger version of this assumption is $(Y_0, Y_1) \amalg D|X$ (Heckman et al., 1998). As we concentrate on the ATT, i.e. we concentrate on the effect of a sanction on behaviour of the sanctioned persons and not on the effects of a lack of a sanction on the behaviour of the non-sanctioned persons, the use of

If assumption (A.1) holds, then $E(Y^0|X, D = 1) = E(Y^0|X, D = 0)$, which implies that selection bias does not occur as we have found an appropriate substitute for our unobservable outcome ($E(Y^0|X, D = 1)$). In other words we have to find a "statistical twin" regarding all variables of X. This intention is quite data demanding as the more dimensions X has the more individuals would be needed to satisfy this assumption.

Rosenbaum/Rubin (1983) propose a matching method where individuals are not matched conditional on X but on their conditional probability, to be assigned to treatment given X, which they call the propensity score: $P(X) = Pr(D = 1|X)$. They show that if (A.1) is satisfied, then

$$Y_0 \amalg D|P(X), \tag{A.2}$$

provided the probability of the non-treated to receive treatment is positive ($0 < P(X) < 1$). An implication of (A.2) is that

$$E(Y^0|P(X), D = 1) = E(Y^0|P(X), D = 0), \tag{7}$$

so that our results are not biased even when conditioning on the propensity score. Thus when (A.2), also known as conditional independence assumption (CIA) holds, we can identify the ATT.[12] In order to fulfill (A.2), we need to control for all factors that affect both, the probability of a sanction and the probability to get back into employment.

5.2 Dynamic matching approach

As we are interested in the *ex post* effect of a sanction after the sanction has been imposed, we are confronted with a missing data problem not only for the term $E(Y^0|X, D = 1)$ but also for the point in time *when* treatment is not implemented for untreated people. In our case treatment may start at any time the person receives UI benefits. In order to account for a potential selectivity bias due to time-constant unobserved heterogeneity Abbring/Berg (2003) suggest a mixed proportional hazard model, the *timing of events* model, where the duration until treatment and the duration of unemployment are modelled jointly (for an application see e.g. Abbring/van den Berg/van Ours (2005)). Applying the *timing of events* approach one has to be careful at specifying the model: according to the findings in Gaure/Roed/Zhang (2007), imposing unjustified restrictions on the heterogeneity distribution can cause substantial bias. Moreover, the proportional hazard specification already imposes functional form restrictions in the outcome equation, that could bias parameter estimates. Such functional form restrictions are not imposed by propensity score matching.

$Y_0 \amalg D|X$ is sufficient.

[12] Additionally the stable unit treatment value assumption ($SUTVA$) has to hold: potential outcomes and potential treatment status of each individual are independent of potential outcomes and potential treatment status of all other individuals. As treatment in our case is a seldom event as we will see in chapter 7.1 this assumption is plausible to hold. At the same time we assume the *ex ante* effect to be very low.

We do not apply the *timing of events* approach here, but regard it as an alternative to the matching approach.

In this subsection we address the question how we deal with the missing start date of treatment for the untreated. There are different approaches to solve the problem of missing start dates applying matching estimators.[13] Fredriksson/Johansson (2004) point out the importance of the dynamic process of treatment assignment. According to their results using a time window defined by the treatment information observed in the data at hand in order to define who is treated and who is not treated is problematic: an estimator with a binary treatment indicator that is based on such a time window is always biased as it conditions on the future.

In this article we follow Sianesi (2004) and Fitzenberger/Speckesser (2007) and use an evaluation approach that takes timing of treatment into account. We estimate the effect of being sanctioned in stratum u, defined as a short time interval during the UI spell, on the outcome variable, the labour market status in different months t after the stratum u, Y_t (with $t > u$). Each stratum u consists of a two month period counting from the individual start of UI receipt. The treatment indicator of stratum u is denoted by $D^{(u)} = 1$ for individuals being sanctioned in stratum u and $D^{(u)} = 0$ for those neither having been sanctioned before stratum u nor being sanctioned within stratum u. Thus we distinguish between different treatment periods: the three strata u. By applying this approach, treatment and outcome decisions of the past are taken into account, i.e. the approach controls for the dynamic sorting process of treated and controls into the group of being at risk of being sanctioned.[14] Using the matching approach in such a stratified manner one allows for an interaction of the treatment effects with the dynamic sorting process and for heterogenous treatment among the different strata considered (Fitzenberger/Speckesser, 2007).

In order to avoid that individuals who are sanctioned during a certain week of a stratum are matched to individuals who have already left UI receipt before this week, we divide each stratum into eight weekly treatment intervals u^{split} and for each u^{split} we exclude those from the analysis who are not at risk of being sanctioned anymore during the respective week as their UI receipt ended before. Our estimator of interest is the difference in the labour market status over time between those who were sanctioned in stratum u and those not having been sanctioned up to the end of u but still being in UI receipt at u^{split} ($L > u^{split} - 1$, with L being the total weeks of UI receipt and $u^{split} - 1$ denoting the end of the

[13] For a number of different approaches to solve the missing start date problem see Lechner (1999a).

[14] There are two reasons for defining a two months period as one stratum: first, a relatively short period as observation window, reduces the potential bias due to conditioning on future outcomes described in Fredriksson/Johansson (2004). Second, the shorter the strata are defined the more precisely this approach is able to control for the dynamic sorting process. Ideally, one would estimate daily probit models. This is not possible due to the small number of sanctions. Instead we chose the two months period and argue that within these two months treatment is exogenous, i.e. the exact start date of a sanction within a stratum is not influenced by the elapsed duration of UI receipt. As the absolute numbers of sanctions per month in our sample after month six is relatively small, the empirical analysis is restricted to three strata.

week before u^{split}), i.e. being at the risk of being sanctioned during u^{split}. The outcomes we focus on are $Y_t^{1(u)}$ and $Y_t^{0(u)}$ as labour market status in month t if having been sanctioned during stratum u and if *not* having been sanctioned during stratum u or before, respectively. For each u we thus focus on a dynamic version of the average treatment effect on the treated Δ_t^{ATT}, i.e. the effect of being sanctioned in stratum u on the outcome at month t and we estimate:

$$\begin{aligned}\hat{\Delta}_t^{ATT} &= E(Y_t^{1(u)}|D^{(u)} = 1, L > u^{split} - 1, D^1 = ... = D^{u-1} = 0) \\ &-E(Y_t^{0(u)}|D^{(u)} = 0, L > u^{split} - 1, D^1 = ... = D^{u-1} = 0)\end{aligned} \quad (8)$$

In order to use the dynamic matching approach above to create ex-post a setting that comes closest to an experimental setting, the CIA has to be expanded by the dynamic aspect. Accordingly we assume the dynamic version of the CIA (DCIA) to hold:

$$\begin{aligned}&E(Y_t^{0(u)}|D_u = 1, L > u^{split} - 1, D_1 = ... = D_{u-1} = 0; P(X)) \\ &= E(Y_t^{0(u)}|D_u = 0, L > u^{split} - 1, D_1 = ... = D_{u-1} = 0; P(X))\end{aligned} \quad (A.3)$$

Thus we assume that conditional on the propensity score $P(X)$, conditional on being at risk of being sanctioned ($L > u^{split} - 1$) and conditional on not having been sanctioned up to the beginning of the stratum considered ($D_1 = ... = D_{u-1} = 0$), sanctioned and non-sanctioned individuals are comparable in their outcomes (except for the realisations of D_u) during stratum u and in the months after.

5.3 Difference-in-Differences matching estimator

The usual matching estimators introduced so far rely on the data demanding $(D)CIA$. Though we are confident that our data contain the relevant information so that it is highly plausible that this assumption is satisfied, as a robustness check we introduce an estimator which is able to tackle the problem of individual specific, time-invariant unobserved differences in the expected outcomes: the difference-in-differences matching estimator (DiD) (Heckman/Ichimura/Todd, 1998):

$$\Delta_{DiD}^{ATT} = E(Y_{after}^1 - Y_{before}^0|X, D = 1) - E(Y_{after}^0 - Y_{before}^0|X, D = 1), \quad (9)$$

Using this estimator, time-invariant individual specific factors are eliminated, i.e. a bias due to unobservables of this nature does not occur. As in our application we deal with a binary outcome variable, the employment status, simply taking the differences before and after treatment does not seem to be a reasonable exercise. Therefore we take advantage of the panel-like structure of our data and calculate the individual-specific sum of the monthly outcome variable over twelve months before and after treatment. Thus, we will estimate the following equation:

$$\hat{\Delta}^{ATT}_{DiD} = E(\sum_{t=1}^{12} Y_t^{1(u)} - \sum_{t'=-1}^{-12} Y_{t'}^{0(u)} | X, D^{(u)} = 1, L > u^{split} - 1, D^1 = ... = D^{u-1} = 0) -$$
$$E(\sum_{t=1}^{12} Y_t^{0(u)} - \sum_{t'=-1}^{-12} Y_{t'}^{0(u)} | X, D^{(u)} = 0, L > u^{split} - 1, D^1 = ... = D^{u-1} = 0), \quad (10)$$

where t indicates the months after treatment as introduced above and t' refers to the months before the start of the UI spell. Basically, we compare the difference in the sum of the outcomes during the twelve months after treatment and before UI start of the treated to the very difference of the untreated.

5.4 Details of the matching approach

The probit models for the estimation of the propensity scores are estimated by stratum u for men and women. Those cases who left UI receipt before less than eight days are excluded from the analysis for two reasons: the remaining sample is expected to be less heterogenous and second doing so we can include two important covariates about placement propositions received during the first week. We use the linear prediction of a probit model of the probability of being treated given observed characteristics as propensity score.[15]

The results presented are based on nearest neighbourhood matching with five neighbours with replacement with a caliper of 0.005 in order to avoid extremely bad matches.[16] For the analysis of stratum two and three, we exclude those UI recipients who have been sanctioned during one and one or two respectively from the probit estimation (see equation 6). In order to make sure that UI recipients sanctioned by a short-term sanction were not used as controls, we excluded those persons who have been sanctioned by a two-week sanction during the respective stratum. Additionally we dropped those cases, where a sanction was obviously taken back as it was shorter than seven days[17].

The following matching restrictions were imposed: first a common support restriction[18]; second we matched only those individuals who entered UI receipt in the same quarter of calender time in order to align seasonal variations; third we excluded those individuals

[15] A linear prediction as balancing score has a higher discriminative power than the predicted probabilities as the variances of the latter is much lower and may thus create more duplicates in terms of the propensity score.

[16] Nearest neighbour matching with one, three and five neighbours without caliper and with calipers 0.010 and 0.005 and a "95th-percentile caliper" was applied. The latter was the 95th percentile of the distribution of the difference in the propensity score between treated and matched controls after a one to one matching with replacement. Note the we use a linear prediction instead of the predicted probability. The decision for the specification presented in this paper is based on the matching quality indicator MSB (see section 7.2) and the number of treated lost. The results reported are sensitive to the choice of specification.

[17] About 20% of sanctions fall in this category of non-effective sanctions.

[18] The common support restriction causes observations to be dropped if their propensity score is higher than the maximum or lower than the minimum propensity score of the controls.

from the pool of potential controls whose UI benefit receipt ended before the sanction starting date of the potentially matched treated (u^{split}).[19] The standard errors are estimated according to the following formula proposed by Lechner (1999b):

$$Var = \frac{V(Y(1)|D=1)}{N} + \frac{\sum weight^2 \cdot V(Y(0)|D=0)}{N^2}, \tag{11}$$

where the sampling weights are obtained by the matching procedure and N is the number of matched treated.[20]

6 Data

6.1 Sources

Our empirical analysis is based on administrative data of the federal employment agency (FEA).[21] The key feature of these data is that they contain daily information on the (un)employment history of every person in Germany.[22] In order to build our sample we drew 400.000 persons who entered UI receipt between 1 April 2000 and 31 March 2001 in West Germany out of the benefit recipient history. These persons had to be between 18 and 55 years old when they entered UI receipt and they had to have an employment spell within twelve weeks before they got unemployed. By the latter restriction we tried to avoid including persons who already were sanctioned due to a voluntary quit before they entered unemployment as we were interested in effects of the first sanction. In order to build a set of characteristics that yields the $(D)CIA$ plausible we had to create an analysis dataset containing a broad amount of information: of these randomly drawn persons we merged all unemployment, employment, job seeking and ALMP program participation spells that were found in the administrative data.[23]

We use three different outcomes: "regular employment", "other employment" and out of labour force. Employment is regarded as regular if it is unsubsidised employment subject to social contributions. "Other employment" might be subidised employment and implies employment such as minor jobs or short term jobs. The outcomes ($Y_t^{(u)}$) are either 1

[19] In order to impose the latter restriction each stratum was divided into eight weeks and eight dummies were build indicating in which week of the stratum the treated individuals were sanctioned. In the next step for each week only those treated were kept that were sanctioned during the respective week and only those controls that were still in UI receipt at the beginning of the respective week were kept.

[20] As we use five neighbours matching, the usual sampling weight of the matched untreated is 0.2. In those cases were only four neighbours were found, it is 0.25 etc.. If one control is used twice, the sampling weight was e.g. 0.4.

[21] The micro data were drawn from the benefit recipient history (LeH), the integrated employment biographies (IEB) and an additional data base called ISAAK.

[22] Provided the employment is subject to social insurance contribution or provided the person is registerd as unemployed or as job seeker respectively.

[23] For further information about the data sets used see Dundler (2006).

if the person is in employment (or out of labour force) or 0 otherwise.[24] The outcome out of labour force is built by screening all administrative data for whether a spell was found in a labour market state, either employed, unemployed, job seeking or in an ALMP measure. The data used in this study do not allow us to distinguish between a sanction due to refusing training or a sanction due to refusing work. What we do observe in the data though is the exact date of the imposition of the sanction. Thus we can draw the information about the month when (if at all) the UI recipient was sanctioned relative to the start of UI receipt ($D^{(u)}$).

6.2 Plausibility of the matching assumption

In order to justify the $(D)CIA$, we have to observe all factors that jointly influence treatment and our outcome of interest. The core of our argument is the richness of the available data. According to Wilke (2004) the selection into an effective sanction follows a two-step process: first, a sanction will be mainly imposed if the UI recipients is not able to prevent a sanction e.g. by applying for the job offered in the placement proposition in such a way that any potential employer is not interested in him. Second, as it is possible to file an objection against a sanction meaning that it might become ineffective, those finally will be sanctioned effectively who did neither file an objection or who were not successful in doing so. We argue that the only difference between the groups of sanctioned and the non-sanctioned after matching is an exogenous incidence, e.g. the caseworker "randomly" proposes a job vacancy where the UI recipient refuses to apply. Accordingly the matched individuals would have reacted the same way; simply they were lucky as to not having been offered such a job vacancy.[25] We base this argument on the observation of the abrupt jump of the sanction rate in 2003 supporting the idea of exogenous variation in the individual sanction probability (see section 3).

As the aim of the matching procedure is for each stratum u to create a sample wherein a sanction is randomly imposed we have to control for a number of variables, namely all variables that jointly influence the probability of being sanctioned during stratum u and the labour market status during stratum u and later. We include information on age, on German citizenship as well as on non-European citizenship.[26] In order to control for heterogeneity regarding the qualification between treated and controls we include the following variables: the wage earned in the last job, dummies for the school education and for training qualification, as well as for qualification level of the desired job as an

[24] In order to build $Y_t^{(u)}$, first t*30.5 days are added to the individual UI spell start and stored as t-day. Second all employment spells found were screened whether this t-day was within the spell - if yes $Y_t^{(u)}$ was set to 1 if no, it was set to 0. In other words, only if the employment spell included the individual reference day (t-day), it was counted as employment.

[25] Note that we control for the number of placement propositions received during the unemployment spell in order to control for a potential *caseworker* effect.

[26] The reference group thus is non-German Europeans.

indicator of the assessment of qualification. In order to model the UI benefit recipient's employment biography, we include the cumulated duration of contributory employment[27], of minor employment, of UI benefit receipt, and of UA benefit receipt within half a year, one, two and three years previous to UI receipt start. We control for the average duration of contributory jobs and the number of different firms that the person had a contributory job at, both also in sets of variables covering half a year, one year, two and three years previous to UI start. Additionally, dummies for the industrial sector and the firmsize of the last employer as well as for the job position held in the last job are considered. The household context is controlled for by including marital status and the age of the youngest child as dummies for three different age groups. We control for the caseworker's appraisal of potential health restrictions. Using time-varying covariates we control for placement propositions received during the UI spell: for stratum one we control for the number of placement propositions received during the first week of the UI spell[28]; for stratum two we additionally control for placement propositions received during stratum one (except those of the first week); for stratum three placement propositions received during stratum two are added to the probit model. In order to model the UI benefit recipients regional flexibility, we include the expected commuting distance to the previous job. A dummy variable indicating whether the person has been sanctioned during the 12 months before UI start is included in order to capture heterogeneity among UI recipients in terms of financial punishment experience. Finally, for stratum two and three we include an indicator for whether the UI recipient holds an irregular job during the month before the considered stratum starts.

On the regional level we control for unemployment rate and vacancy rate, each one month before the individual UI spell starts, and we control for the caseload in the respective local employment agency as the ratio between unemployed and caseworkers as average of the year when the UI spell starts[29] and for the sanction rate, defined as in Müller/Oschmiansky (2006), one month lagged to the individual UI start.[30]

7 Empirical results

7.1 Descriptive evidence of UI sanctions

Table 4 describes the incidences of sanctions by gender and by stratum:

We can see that though the absolute numbers of sanctions decrease by each stratum,

[27]I.e. employment subject to social contribution.

[28]Note as mentioned above, we exclude those who left UI receipt before less than eight days.

[29]The FEA human resource department provided us with this information.

[30]Note that using the sanction rate as instrumental variable would not identify the *ex post* effect, but a local average treatment effect (LATE; Angrist/Imbens/Rubin (1996)), which would include the *ex ante* effect. Therefore we balance the differences of local sanction rates between treated and controls by including the sanction rate in the matching procedure.

Table 4: Sanctions: number of incidences, number of persons at risk and sanction probabilities

Stratum (month of UI receipt)	Treated	Potential controls	Sanctions conditional on being at risk
(1)	(2)	(3)	(4)
Women:			
1 (1-2)	312	147199	0.21%
2 (3-4)	263	101710	0.26%
3 (5-6)	165	68935	0.24%
Men:			
1 (1-2)	644	217472	0.3%
2 (3-4)	518	142945	0.36%
3 (5-6)	277	81272	0.34%

The table reports the number of sanctions due to refusal of a job or refusal of an ALMP measure. Source: Administrative micro data of benefit recipients (LeH).

the probability of getting sanctioned conditional on being at risk even increases slightly between stratum one and two for both, men and women from 0.30% to 0.36% (men) and 0.21% to 0.26% (women). In stratum three it slightly decreases: 0.34% (men) and 0.24% (women).

These results from the micro data support our earlier assessment: without conditioning on any characteristics sanctions due to refusing work or an ALMP measure are rare events during an individual UI receipt spell.

7.2 Matching quality

Table 5 presents indicators for the matching quality:[31]

Table 5: Balacing quality indicators

Stratum	Treated lost	Controls used	McFadden's R^2 Before	McFadden's R^2 After	MSB Before	MSB After
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Women:						
1	11	1459	.0825	.0162	12.96	2.46
2	11	1192	.1239	.0203	14.8	3.06
3	8	738	.114	.0278	12.8	3.2
Men:						
1	9	3076	.0806	.0067	14.69	1.96
2	23	2288	.1456	.0126	15.46	2.34
3	11	1241	.1097	.0189	11.55	2.26

Propensity score matching with five neighbours and replacement, common support and a caliper of 0.005. For the formula of the meas standardised bias (MSB) see footnote 32.

Out of 2179 treated UI recipients, we lost 73 due to the common support restriction or the caliper. McFadden's R^2 of the fitted probit estimations before and after matching

[31] In order to save space we do not include the probit estimates tables in this version of the paper. They are available on request or in the IAB discussion paper version (http://doku.iab.de/discussionpapers/2008/dp4308.pdf).

differ (before: ranging from 0.0806 to 0.1456), but there is still some explanatory power in the models after matching (ranging from 0.0067 to 0.0278; column 6). The mean standardised bias[32] as indicator of the distances in the covariate distributions between treated and controls (ranging between 11.55% and 15.46% before matching; column 7) is reduced (ranging between 1.96% and 3.20% after matching; column 8) and is for each of these six subsamples below 5% which is regarded as an acceptable level (cf. Caliendo/Kopeinig (2005)). The differences in the means between treated and matched controls per covariate are all insignificant at a 5%-level.[33]

7.3 Ex-post effects

We will first discuss the results of the monthly Δ_t^{ATT} estimates and second the Δ_{DiD}^{ATT} estimates as a robustness check. Additionally to Δ_{DiD}^{ATT}, in the appendix we provide Δ_{Sum}^{ATT} estimates, where the outcome is the number of months in employment and out of labour force respectively during a twelve-month period after the stratum considered.

Figures 1-6 in the appendix report graphically on the effect of a UI sanction for the months after the sanction has been imposed:

For men and women separately and for each outcome used, a group of three graphics is presented: per stratum a graph of the monthly differences in the outcome before treatment[34] and the monthly Δ_t^{ATT}. The time axis represents the months from twelve months before UI receipt start until 18 months after the end of the stratum considered, the time axis is presented relative to the start of the UI spell (=0). Two vertical lines shall help to distinguish between the months before (left hand side line) and after the stratum considered (right hand side line).

For women being sanctioned in stratum one or two (significantly) raises the probability of being regularly employed in the months after the stratum considered. The significant effects range between 5 and 10 %-points. Regarding the outcome "other employment" the monthly Δ_t^{ATT} estimates suggest an *ex post* effect for stratum one and two with a time lag. The significant effects on "other employment" are smaller (around 5 %-points). For the outcome out of labour force we hardly find empirical evidence for women in terms of

[32] The mean standardised bias (MSB) is calculated as follows: $MSB = \frac{1}{K}\sum_{k=1}^{K} 100 \cdot \frac{\mid \bar{X}_{k1t} - \bar{X}_{k0t} \mid}{\sqrt{0.5 \cdot (V_{k1t}(X) + V_{k0t}(X))}}$ with K denoting the number of variables and $\bar{X}_1$ (V_1) denoting the mean (variance) in the treated group and $\bar{X}_0$ (V_0) the mean (variance) in the comparison group before matching if $t = 0$, and the corresponding moments after matching if $t = 1$ (cf. Caliendo/Hujer/Thomsen (2005)).

[33] In order to save space we do not include the matching indicators per covariate in this version of the paper. They are available on request or in the IAB discussion paper version (http://doku.iab.de/discussionpapers/2008/dp4308.pdf).

[34] We present the difference in the outcome before treatment in order to check graphically the quality of the matches.

monthly effects.[35]

For men we observe a rise in the probability of being regularly employed immediately after each stratum considered. For stratum one and two the monthly effects on the outcome "other employment" are mostly negative. For men we find positive effects on the outcome out of labour force for several months after the sanction.

In sum, both, men and women seem to respond to a sanction in terms of being regularily employed after a sanction during stratum one or two. Regarding the outcome "other employment", we find gender differences: while women in general are more likely to be in "other employment" due to the sanction, the opposite is the case for men.

The graphical evidence is supported when looking at the Δ_{DiD}^{ATT} estimates (being the difference in the number of months after the stratum considered and before UI start during a 12 month period) in table 6:

Table 6: $\Delta_{D}^{ATT} iD$ and Δ_{sum}^{ATT}estimates for three different outcomes

Estimand	Stratum (month of UI receipt)	Outcome: "regular employment" (1)	Outcome: "other employment" (2)	Outcome: out of labour force (3)	Outcome: "regular employment" (4)	Outcome: "other employment" (5)	Outcome: out of labour force (6)
		Women:			Men:		
Δ_{sum}^{ATT}:	1 (1-2)	.67**	.28*	.26	.58***	-.13*	.21
	2 (3-4)	.72**	.51**	.16	.78***	-.15*	.53***
	3 (5-6)	.33	.26	.16	.39	.07	.48**
Δ_{DiD}^{ATT}:	1 (1-2)	.66*	.27	.35	.6**	-.11*	.23
	2 (3-4)	.85**	.6**	.31	.8***	-.15*	.64***
	3 (5-6)	.21	.28	.17	.42	.05	.47*

Note: Results of regression in matched sample with only treatment indicator as regressor and weights attached to controls. Robust standard errors. Significance levels: *: 10% ; **: 5% ; ***: 1% ; Dependent variables: Δ_{sum}^{ATT}: Number of months in employment during 12 months after stratum, and respectively months 4-15 after (out of labour market); Δ_{DiD}^{ATT} Dependent variable of Δ_{sum}^{ATT} minus number of months in employment during 12 months before UI start.

Focussing on the significant effects only, we find that for both, women and men, a sanction during stratum one or two raises the number of months of "regular employment" during the twelve month period after the stratum considered (women: 0.66, 0.85, men: 0.60, 0.80). For women being sanctioned during stratum two we additionally find a significant positive effect on the number of months of "other employment" (0.6).[36] For men being sanctioned during stratum one the negative effect on "other employment" is even significant at a 10-%-level (-0.11, -0.15) and those men being sanctioned during stratum two or three are more likely to be out of the labour force.[37]

[35] As being sanctioned will systematically lead to a disappearance from the administrative data for the duration of the sanction, regarding the outcome out of labour force the monthly effects should be interpret only after months three after the end of the stratum.

[36] For the Δ_{DiD}^{ATT} and the Δ_{Sum}^{ATT} estimates the twelve months starting from the fourth month after the stratum considered are counted (see comment above).

[37] Note that never differing in terms of their sign, the Δ_{Sum}^{ATT} estimates only differ slightly in size (and

7.4 Ex-post effects - Evidence from subgroups

We used the same estimation procedure described above, starting from separate probit models for each subgroup considered. We do not report the monthly Δ_t^{ATT} estimates but only the Δ_{DiD}^{ATT} estimates. We divide our sample into UI recipients below 30 years and above 29 years. Second, we analyse the subgroups of those being unemployed in a region with lower and respectively higher unemployment rates.[38]

As the number of treated within the subgroups are quite small for stratum three, we only report on the results of the first two strata. The matching quality indicators of the subgroup estimates indicate that the matching quality naturally suffers a bit by dividing the sample as the pools of potential controls are diminished.[39] Out of the 16 subsamples, only of two the MSB after matching is below a value of 3% and of two it is even higher than 5% (5.01 and 5.53). Table 7 in the appendix reports on the Δ_{DiD}^{ATT} estimate per subgroup, stratum and for men and women:[40]

Table 7: Δ_{DiD}^{ToTATT} estimates for subgroups and for three different outcomes

Subgroup	Estimand	Stratum	Outcome: "regular employment" (1)	Outcome: "other employment" (2)	Outcome: out of labour force (3)	Outcome: "regular employment" (4)	Outcome: "other employment" (5)	Outcome: out of labour force (6)
			Women:			Men:		
age 18-29	Δ_{sum}^{ATT}:	1	.79*	.09	-.21	.54**	-.09	.12
		2	1.38***	.21	-.21	.95***	-.11	.41*
age 30-55		1	.3	.55**	.35	.31	-.28**	.35*
		2	.11	.59	.49	.58*	-.12	.42*
Local unemployment rate low		1	.85**	-.02	.32	-.12	.06	.74***
		2	.69	.57	.16	.97***	-.15	.16
Local unemployment rate high		1	.06	.59**	-.15	.83***	-.34***	-.07
		2	.51	.06	-.01	.44	-.08	.73***
age 18-29	Δ_{DiD}^{ATT}:	1	.73	.15	-.1	.58*	-.07	.17
		2	1.32**	.37	-.37	.98**	-.12	.51*
age 30-55		1	.48	.55*	.3	.32	-.29**	.38*
		2	.19	.56	.59*	.55	-.08	.56**
Local unemployment rate low		1	1.03**	.07	.32	.04	.06	.75***
		2	.72	.96**	.28	1.08***	-.17	.2
Local unemployment rate high		1	-.24	.78**	-.16	.82**	-.33***	-.04
		2	.67	.08	.02	.61 *	-.09	.81***

Note: Results of regression in matched sample with only treatment indicator as regressor and weights attached to controls. Robust standard errors. Significance levels: *: 10% ; **: 5% ; ***: 1% ; Dependent variables: Δ_{sum}^{ATT}: Number of months in employment during 12 months after stratum, and respectively months 4-15 after (out of labour market); Δ_{DiD}^{ATT}: Dependent variable of Δ_{sum}^{ATT} minus number of months in employment during 12 months before UI start.

Our results suggest that for women the effects on "regular employment" found for the first stratum are indeed driven by *young* women, while older women seem to be more responsive in terms of "other employment". Also the positive (though insignificant) effect found for the

significance). This finding supports the matching quality indicators which suggest good matching quality.

[38] We take the median to split the sample.

[39] In order to save space we do not include the matching quality indicators for the subgroups in this version of the paper. They are available on request or in the IAB discussion paper version (http://doku.iab.de/discussionpapers/2008/dp4308.pdf).

[40] In order to save space the probit coefficient tables of the subgroup analysis are not included in this paper.

outcome out of labour force is driven by older women (while for the younger group though not being significant they are even negative). For men it is also the subsample of younger UI recipients that is more responsive to the financial incentive of a sanction regarding the number of months in "regular employment" afterwards. Regarding the outcomes "other employment" and out of labour force both male age groups seem to react in a similar way. The latter result might be a hint that a sanction causes an increase in the probability to work in the shadow economy.

Dividing the sample by the regional unemployment rate we find that for the outcome "regular employment" especially for women in better off regions sanctions have an effect (1.03, .72). For men on the other hand being sanctioned during stratum one in a worse off region is much more effective (0.82) than in better off regions (0.04). This seems to be an interesting finding as to one might ask whether the effectiveness of sanctions does not only depend on the individual reaction but also depend on the labour market conditions. Women in worse off regions being sanctioned in stratum one on the other hand respond to a sanction by taking up "other employment". The significant negative effect of a sanction during stratum one on the outcome "other employment" for men seems to be driven by the subgroup living in worse off regions (-0.33).

In sum, for men and women we find that the effect of a sanction on the months in "regular employment" is driven by the younger UI recipients. The results regarding the subgroups defined by the local unemployment rate appear somewhat erratic.

8 Conclusion and outlook

In this paper we use administrative data in order to evaluate the *ex post* effect of sanctions due to refusing work or an active labour market policy (ALMP) measure for a sample of individuals who entered unemployment insurance (UI) benefit receipt in West Germany during April 2000 and March 2001. We identify the *ex post* effect using a matching approach that takes timing of the treatment explicitly into account: we model the effects of a sanction imposed during either of three strata consisting each of two months on the employment probability in each out of twelve months after the end of the stratum considered. As a robustness check we introduce a difference-in-differences matching estimator. A potential influence of unobserved time-invariant characteristics is eliminated thereby. In order to avoid biases due to time-varying characteristics we include potentially confounding factors, namely variables on whether a person took up an irregular job and the number of placement propositions he or she received during the pre-treatment UI period and are confident that the identifying assumption holds. In order to give some insights into different subgroups we finally distinguish the sample by age and by local labour market conditions. The outcome states we consider are holding a regular job, holding an irregular job and being out of the labour force.

This study is based on a sample which was faced with a sanction regulation framework different to the one existing today. Compared to the currently effective regulation, where the first sanction imposed due to refusing a training or an active labour market policy (ALMP) measure is a 100% benefit reduction lasting for three weeks only, in 2000/2001 a UI benefit sanction implied a 100% benefit reduction for twelve weeks. We suppose that this is part of the reason why the numbers of incidences and the sanction probability respectively were extremely low during our observation period.

For both, men and women, we find evidence of an average *ex post* effect of a UI sanction during stratum one or two on the "regular employment" probability. These effects are mainly driven by young UI recipients. Regarding the outcome "other employment" the results are ambigous: for women they are positive, but negative for men. Taking the subgroups into account, we find that the positive effect on "other employment" for women results from the older subgroup while the negative effect for men (for stratum one) is found largest in regions with relatively higher unemployment rates. With respect to the outcome out of labour force, especially older women seem to respond to sanctions, while among men especially those having been sanctioned during stratum two or three withdraw from the labour market.

The differences in the effects between the three different strata considered might partly be traced back to a dynamic sorting process, i.e. it is different types of persons a) who are at risk of being sanctioned and b) who are sanctioned during stratum three compared to the first two strata.

The results are in line with the empirical literature on *ex post* effects of unemployment benefit sanctions summarized in section 2: on average a sanction has a positive effect on the employment outcome. As we saw in section 4, job search theory suggests the causal mechanism to work via a decrease in the reservation wage and an increase in the search intensity. Both might affect the quality of post-unemployment jobs. Though the estimation framework used in this paper gives some hints about the stability of the employment taken up after a sanction, future research should investigate the effects of unemployment benefit sanctions on the quality (e.g. in terms of wages and qualificational level) and the sustainability of post-unemployment jobs (e.g. analysing the job duration of the first job after a sanction).

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Figure 1: Women - Outcome: "regular employment"

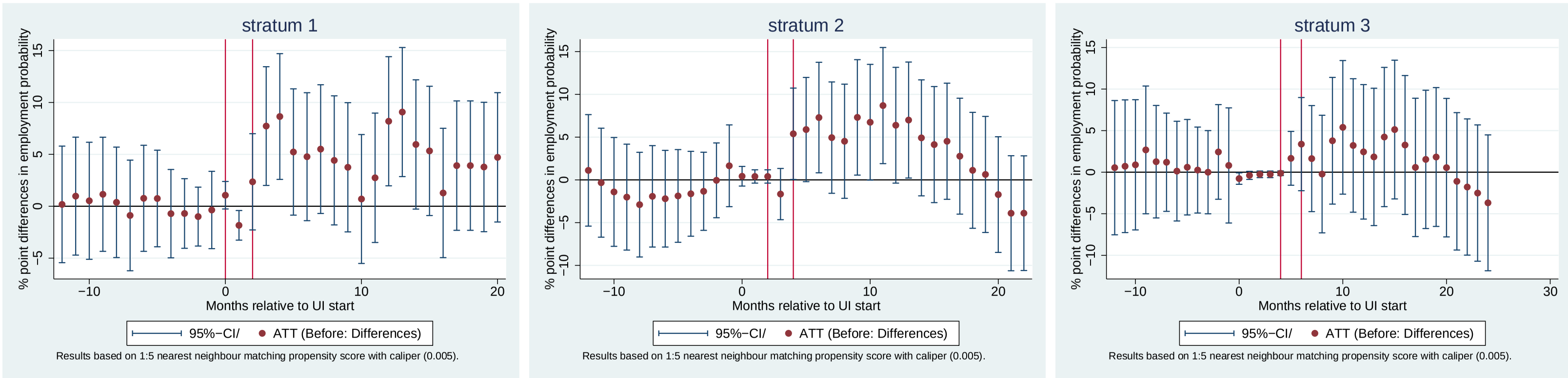


Figure 2: Women - Outcome: "other employment"

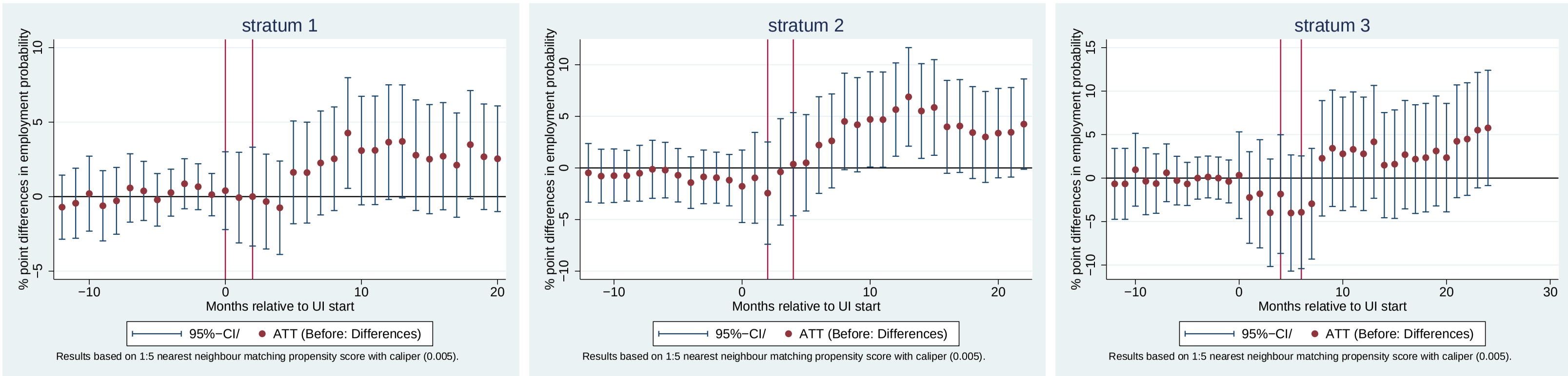


Figure 3: Women - Outcome: out of labour market

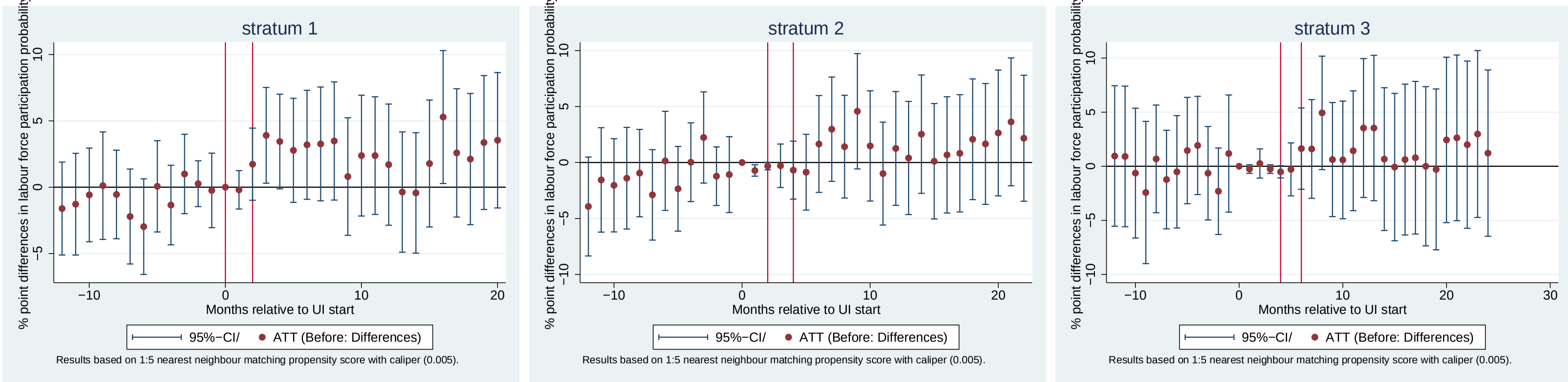


Figure 4: Men - Outcome: "regular employment"

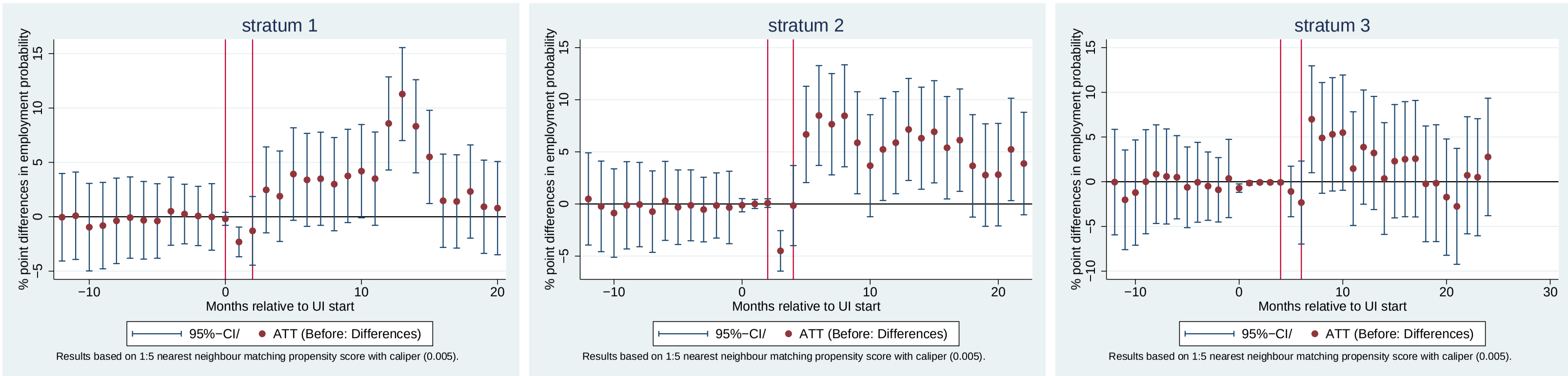


Figure 5: Men - Outcome: "other employment"

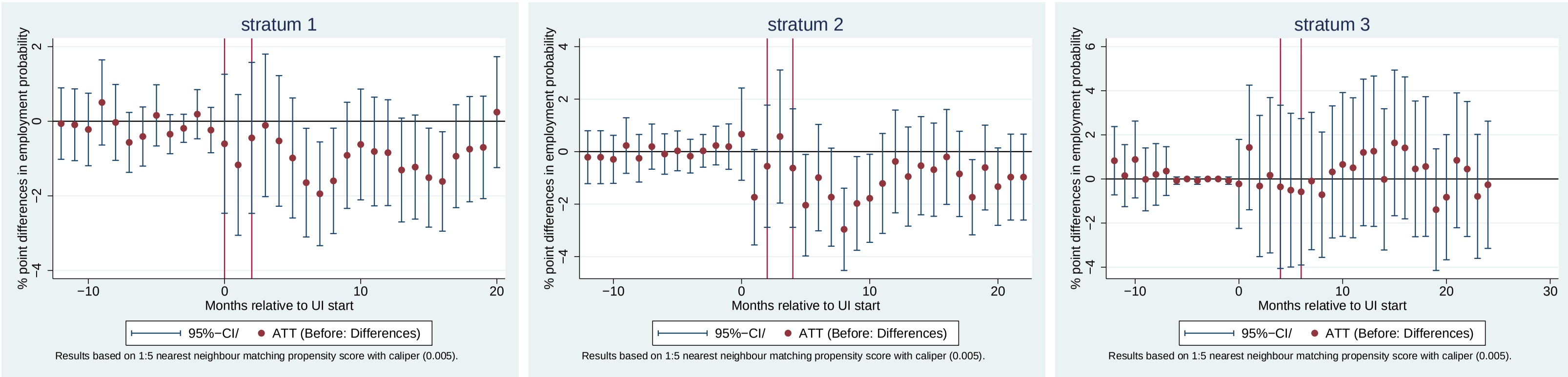
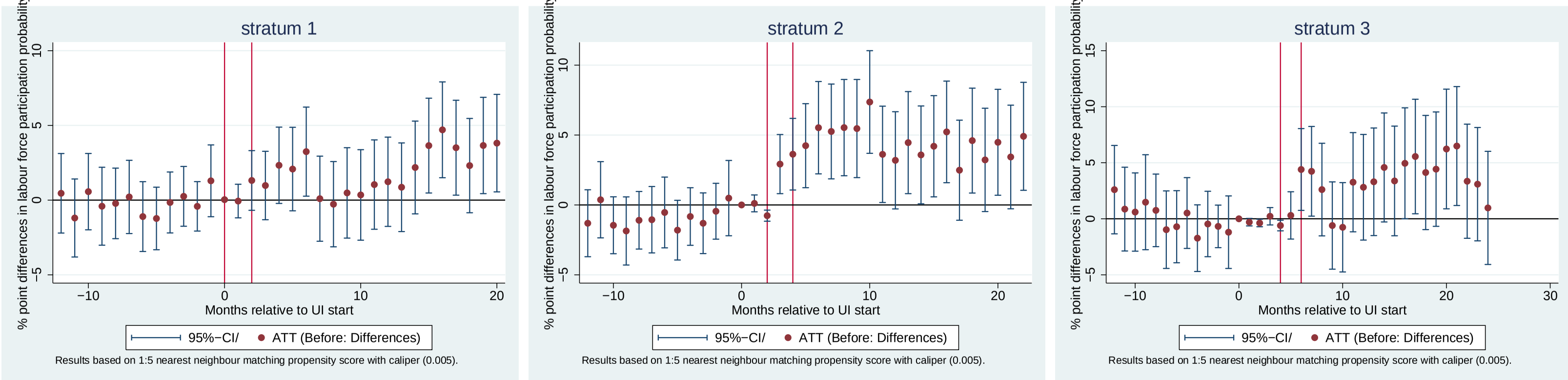


Figure 6: Men - Outcome: out of labour market



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