

Work incentives?

Ex-post effects of unemployment insurance sanctions in West Germany ^{*†}

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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.

1 Introduction

In the last couple of years activation strategies intended to get unemployed people 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 and Grubb, 2006). Unemployment insurance (UI) benefits usually depend on several eligibility criteria, i.e. the UI benefits recipient has to comply with certain rules in order to be eligible for UI benefits¹.

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¹The notion unemployment insurance benefits is used for the German notion "Arbeitslosengeld I".

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. The *ex ante* refers to the effect of being unemployed in a system where being sanctioned is possible compared to the situation of being unemployed in a system without sanctions. It is assumed that the mere possibility of being sanctioned raises the search efforts of UI recipients *ex ante*, 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 intensively 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 Germany². The main question we intend to answer is whether the imposition of a UI sanction due to refusing a placement proposal 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 duration. The treatment group consists of those UI recipients who were sanctioned during the stratum considered, while the controls are the ones who have not been sanctioned during the stratum considered 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 introduce a difference-in-differences matching estimator.

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

2 Literature review

There are several studies about the effects of punitive sanctions (e.g. Fredriksson and Holmlund (2003); Jensen, Rosholm, and Svarer (2003); Boone, Sadrieh, and van Ours (2004)). In general, results of most of these studies show that punitive sanctions have an impact on the transition from unemployment to employment. Fredriksson and 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, and Svarer (2003) analyse the effects of a youth unemployment program (YUP) on the transition rates from unemployment to schooling and employment. 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, and 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). While these articles either 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, and Svarer, 2003), identifying the *ex post* effect with non-experimental data is focussed on in the following studies: Abbring, van den Berg, and van Ours (2005), Lalive, van Ours, and Zweimüller (2005), van den Berg, Klaauw, and van Ours (2004), Svarer (2007) and Müller and Steiner (2008).

Abbring, van den Berg, and van Ours (2005) use administrative data of persons who entered unemployment in 1992 in order to 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 range 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, and 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 up to 60 days. Their results 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%. citetvandenBergetal2004 analyse *ex post* effects of punitive sanctions. While the analysis of Abbring, van den Berg, and van Ours (2005) and Lalive, van Ours, and Zweimüller (2005) focus on the group of UI benefit recipients, it has to be stressed, that van den Berg, Klaauw, and 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. 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 and Steiner (2008) analyse *ex post* effects of sanctions on UI as well as unemployment assistance (UA) recipients. They find positive short- and long-term effects of benefit sanctions on the transition from unemployment to employment.

Müller and Oschmiansky (2006) focus on a model of the determinants of regional sanction rates in Germany.³ Their findings suggest that there are different levels of determinants

³Müller and 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 job or of a training and integration measure, and the stock of benefit (UI, unemployment assistance (UA), integration aid) recipients of this local employment agency.

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 on individual level, the age, the level of disability and the qualification, but also the local sanction policy play a role regarding the individual sanction risk.

3 Unemployment benefit sanctions in Germany

During the observation period of our analysis, 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 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.⁴ Until 2005 an UI benefits recipient received means-tested unemployment assistance (UA) if he had lost his claims to unemployment insurance after a certain period of unemployment.⁵ Table 1 in the appendix gives an overview of the entitlement lengths during our observation period:

The monthly amount received was 67% of the previous monthly net wage for unemployed persons with children and 60% for those without child.⁶ 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 a sanction (Karasch, 2005). An unemployment benefit recipient was sanctioned if he did not comply with certain rules. At the time period of our study there were short-term and long-term sanctions. In this case UI or UA benefits stopped *completely* for a certain period (*Sperrzeiten*).⁷ In general there were five sanction reasons: The entitlement time for UI benefits was shortened by 25% or at least twelve

⁴In 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.

⁵From 2005 on this person would receive unemployment benefits (UB) according to SC II.

⁶The replacement ratio for UA was 57% and 53% respectively.

⁷According to the sanction regulations of the Social Code II the UB will be *reduced* in case of noncompliance with certain rules (*Sanktionen*).

weeks, if a person had voluntarily quit his job (1), 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. A person was sanctioned by twelve weeks if he refused a job offer (2) and 3 weeks respectively if the job would have been temporary only. Refusing (3) or dropping out (4) 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. ALMP measures include training measures, professional further education or vocational training measures and integration measures for disabled UI recipients. 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 (*Säumniszeit*). The different types of sanctions according to the SC III valid in 2000/2001 are summarized in Table 2 in the appendix. In the case that the cumulated duration of sanctions adds up to 24 weeks, a UI recipients lost the claim to UI benefits ("sanctions account regulation").

As there is no automatism regarding the implementation of sanctions, the local employment agency and even the caseworkers have the authority to impose a sanction. Sanctions empirically are implemented quite heterogeneously between local employment agencies and even within one local employment agency the probability of one person to be sanctioned is influenced by the assigned caseworker (Müller and Oschmiansky, 2006).

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. Figure 1 in the appendix provides the development of the sanction rates in Germany between 1996 and 2003. Distinguishing the sanction rates by the different types of sanctions, we can see that most sanctions are implemented due to voluntary quits: 75.7% in 2000 and 75% in 2001 (3 in the appendix).

According to the 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 individual implementation, they shall be analysed separately. In the empirical analysis we will investigate the effects of sanctions due to refusal of work (2) or an ALMP measure (3).

4 Job-search-theory with sanctions

The theoretical framework of this article is a job search model with sanctions introduced by Abbring, van den Berg, and van Ours (1996; 2005). Before we derive hypotheses, it is useful to present some general thoughts about an unemployment insurance system with sanctions. A basic job search model with endogenous search intensity is presented e.g. by Mortensen (1986). Abbring, van den Berg, and van Ours (2005) extend this model by introducing sanctions. According to Abbring, van den Berg, and van Ours (2005), referred to as ABO05, we consider a situation where a person has become unemployed and currently is searching for a job. We take different variables into account that are assumed to influence the job search process. First, individuals receive a certain flow of unemployment benefits b . We assume that besides the pecuniary value of the unemployment insurance benefits, there is a non-pecuniary utility of being unemployed which is also included in b . Second, we assume that every individual searches with a particular search intensity s . The level of s is chosen by the unemployed 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 intensively an unemployed person 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 distributions $F(w)$. If a job is offered the individual 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 intensively he searches for a job, the higher the search costs are. As our model is based in a world with rational actors, we assume that every unemployed person 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 an unemployed individual. Now we introduce sanctions in this model. We denote the benefit level an unemployed job seeker receives before a sanction is imposed by $b_1 = bw$, with b being the earnings-

replacement rate and w being the average reference wage of the last job(s). The level of reduction when a sanction is imposed is denoted by r , thus we have $b_2 = (1-r)bw$ being the benefit level an unemployed job seeker receives after a sanction is imposed. As mentioned above sanctions may affect individual behaviour in two different ways. Accordingly, we distinguish between 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 an unemployed individual in a system with sanctions. At the first sight one might assume that every unemployed tries to avoid a sanction and therefore behaves *ex ante* in a certain way in order to prevent a sanction. If this was the case we would not observe sanctions at all. On the other hand we might think that unemployed persons take the possibility of being sanctioned into account to define their job search strategies. ABO05 argue that the results of their study as well as institutional aspects contradict such a view. A major assumption of their model is that individuals cannot foresee *when* exactly a sanction is imposed, i.e. the model is based in a world of incomplete information on the unemployed persons' side regarding the point in time of the imposition of a sanction, 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. On the other hand unemployed people do know the relationship between their behaviour and the probability of being sanctioned, i.e. an unemployed person knows that a certain type of behaviour will raise the probability of being sanctioned while another type will reduce the probability. If the job search intensity exceeds a certain threshold s^* we assume 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 as mentioned above.

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

. According to equation (1), the more intense 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 up to a level beyond s^* ($s \geq s^*$).⁸ 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:

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

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

with $\rho R_1 = \phi_1$ (reservation wage before the imposition of a sanction) and $\rho R_2 = \phi_2$ (reservation wage after the imposition of a sanction). $I(s_1 < s^*)$ denotes the indicator function being one in case that the search intensity is below the threshold level s^* and being zero otherwise, i.e. if the probability of being sanctioned is zero. The right hand side of the equations (2) and (3) can be interpreted as the *flow of what one expects of holding the assets*. 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 negative income change when a sanction is imposed ($I(s_1 < s^*) p_0 (R_2 - R_1)$).

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 $\bar{F}(\phi_i)$.

⁸The model requires some more assumptions: $\lambda(s) = \lambda_0 s$ and $c(s) = \frac{1}{2} c_0 s^2$. Upon imposition of a sanction, b is permanently reduced from bw (benefits level before a sanction is imposed) to $(1-r)bw$ (benefits level after a sanction is imposed). bw , $(1-r)bw$, F , k_0 , c_0 , p_0 , s^* and the discount rate ρ are constant as a function of unemployment duration. An implication of these assumptions is that within the time interval before a sanction and as well as within the time interval after a sanction, the optimal strategy the individual is going to choose is constant over time.

It is given by:

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

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

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 ($(1-r)bw < bw$) 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 $\bar{F}(\phi_2) > \bar{F}(\phi_1)$. 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 is 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 and van Ours (2000) show that the effect on s_1 (*ex ante* effect) is positive, while the effect on the difference between s_1 and s_2 (*ex post* effect) is negative. Thus if p_0 is increased due to an increased monitoring, the (*ex post* effect) will decrease. $s_2 = s^*$ 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 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.

5 Evaluation approach

As we do not have experimental data, where treatment is implemented randomly and thus can be treated as exogenous, we have no control for non-random assignment to treatment, i.e. for the natural selection process. Factors that influence assignment into treatment are partly those factors, that influence the outcome of interest, i.e. treatment and control group would receive different outcomes anyway, even without treatment. Taking the endogeneity of treatment into account in order to avoid biased estimates based on self selection into treatment⁹, we choose our evaluation approach.

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 $D_i \in \{0,1\}$. 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:

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

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: $E(Y^0|X, D = 0)$.

⁹For an early discussion of what happens when ignoring self selection see Heckman (1979).

5.1 Static matching approach

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

$$Y_0 \parallel D|X.^{10} \tag{A.1}$$

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 and 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

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

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*.¹¹ 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.

¹⁰The stronger version of this assumption is $(Y_0, Y_1) \parallel D|X$ (Heckman, Ichimura, Smith, and Todd, 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 $Y_0 \parallel D|X$ is sufficient.

¹¹Additionally 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 each other individual. As treatment in our case is such an unlikely event as we will see in chapter 7.1 we are convinced that this assumption holds. At the same time we assume the *ex ante* effect to be very low.

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 this subsection we address the question how to deal with the missing start date of treatment for the untreated. There are different approaches to solve the problem of missing start dates.¹² Fredriksson and 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 and 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, 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 three treatment periods: the three strata u .¹³ 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

¹²For a number of different approaches to solve the missing start date problem see Lechner (1999).

¹³There 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 and 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 far from being possible due to the small number of sanctions. Instead we chose the two months period and argue that within these two month 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 decrease immensely after month six (see figure 2 in the appendix), the empirical analysis is restricted to three strata.

sanctioned.¹⁴

In order to obtain the control group, 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 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 $\hat{\Delta}_t^{dynATT}$, i.e. the effect of being sanctioned in stratum u on the outcome at month t and we estimate:

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

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:

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

Effectively 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 ($D_1 = \dots = D_{u-1} = 0$), sanctioned and non-sanctioned individuals are comparable in their outcomes (except for the realisations of D_u) in month t and later.

¹⁴Using 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 and Speckesser, 2007).

5.3 Difference-in-Differences matching estimator

The usual matching estimators introduced so far rely on the data demanding CIA. Though we are very 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-difference matching estimator (DiD) (Heckman, Ichimura, and 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 constant 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, simply taking the differences before and after treatment does not seem to be an reasonable exercise. Therefore we take advantage of the panel-like structure of our data and calculate the sum of the outcome variable over twelve months before and after treatment. Thus, in order to check the robustness of our results, we will estimate the following equation:

$$\begin{aligned} \hat{\Delta}_{DiD}^{dynATT} = & E\left(\sum_{t=1}^{12} Y_t^{1(u)} - \sum_{t'=-1}^{-12} Y_{t'}^{0(u)} \mid X, D^{(u)} = 1, L > u^{split} - 1, D^1 = \dots = D^{u-1} = 0\right) - \\ & E\left(\sum_{t=1}^{12} Y_t^{0(u)} - \sum_{t'=-1}^{-12} Y_{t'}^{0(u)} \mid X, D^{(u)} = 0, L > u^{split} - 1, D^1 = \dots = D^{u-1} = 0\right), \end{aligned} \quad (10)$$

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 due to two major advantages: the remaining sample is expected to be less heterogenous and second doing to we can include two important covariates about placement proposals 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 9). 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 the gap between two UI receipt spells was shorter than seven days¹⁷.

The following matching restrictions were imposed: first a common support restriction¹⁸; second a restriction was imposed by matching only those individuals that have entered UI receipt in the same quarter of calendar time by which seasonal variations are aligned; and third a restriction that excluded those individuals from the pool of potential controls whose UI benefit receipt ended before the sanction starting date of the potentially matched treated.¹⁹ The ATTs and their standard errors result from an OLS regression of the respective outcome on the treatment indicator attaching sampling weights to the matched controls.²⁰ The standard errors obtained by this procedure are robust standard errors.

¹⁵A 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.

¹⁶Nearest 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. Latter one 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. 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.

¹⁷About 20% of sanctions fall in this category of non-effective sanctions (see figure 3).

¹⁸The 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.

¹⁹In order to impose 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.

²⁰As we use five neighbours matching, the usual sampling weight 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.

6 Data

6.1 Sources

Our empirical analysis is based on administrative data of the FEA. The key feature of these data is that they contain daily information on the (un)employment history of every person in Germany.²¹ In order to build our sample we drew 400.000 persons who have entered UI receipt between 1 April 2000 and 31 March 2001²² in West Germany out of the benefit recipient history. These persons had to be between 18 and 55 years old during the above mentioned period and they had to have an employment spell within 12 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 and we expect this group of persons to behave different due to the "sanction accounts regulation" mentioned in section 3. In order to build a set of characteristics that makes the CIA plausible to hold 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, irregular employment and out of labour force. Employment is regarded as regular if it is unsubsidised employment subject to social contribution. Irregular employment is also unsubsidised employment, but implies employment such as minor jobs or short term jobs. The outcomes ($Y_t^{(u)}$) are either 1 if the person is in employment (and respectively out of labour force) or 0 otherwise. We built the regular employment state by drawing all spells of unsubsidised employment out of IEB²⁴

²¹Provided the employment is subject to social insurance contribution or provided the person is registered as unemployed or as job seeker respectively.

²²We chose these numbers based on FEA statistics in order to make sure that our data contain enough sanctioned observations: The ratio between persons who received a sanction due to *refusal of work* or a sanction due to *refusal of training measure* and persons who entered unemployment in 2000 and 2001 in West Germany was 1.03% in 2000 and 1.09% respectively. (In East Germany the ratio was about half that size: 0,48% and 0,46% respectively.)

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

²⁴In IEB we find all administrative spells of a person, i.e. it is possible, that employment exists parallel to benefit receipt spells (subsidised employment). As we want to model the probability of being in *unsubsidised* employment, we first have to clean the IEB in terms of splitting those employment spells that overlap with benefit receipt spells into spells that do not coexist with benefit receipt spells and spells that do, i.e. that indicate a subsidised job.

and match them to the UI benefit receipt spell which contains the start and the end date of the UI receipt. Analogously the irregular employment outcome is built.²⁵ The outcome out of labour force is built by screening all above mentioned administrative data for whether a spell was found in any of labour market state, either employed, unemployed, job seeking or in an ALMP measure. The data used in this study do not allow us distinguishing between a sanction due to refusing a training or a sanction due to refusing a job offer. What we do observe in the data though is the exact date of the imposition of the sanction. Thus we can draw the information we need: 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 A.3, we have to observe all factors that jointly influence the sanction probability and our outcome of interest, the labour market status. The core of our argumentation is the richness of the data available. We argue that the only difference between the group of sanctioned and the group of non-sanctioned after matching is an exogenous incidence, namely the fact that during the stratum considered something happens that causes the caseworker to impose a sanction.²⁶ In the following briefly report on the variables we use, in order to convince the reader of the plausibility of the matching assumption, i.e. in order to convince the reader of the randomness of a sanction in our matched sample. We include information on age as four dummies indicating each one agegroup. We include dummies for German citizenship as well as for non-European citizenship.²⁷ 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 indicator of the self assessment of the qualification. In order to model the UI

²⁵Note that 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 laying 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.

²⁶Note that we control for the number of placement proposals received during the unemployment spell in order to control for a potential *caseworker* effect.

²⁷The control group thus is non-German Europeans.

benefit recipient’s employment biography, we include of each the cumulated duration of contributory employment, 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 built. The household context will be 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 proposals received during the UI spell: for stratum one we control for the number of placement proposals received during the first week of the UI spell²⁸; for stratum two we additionally control for placement proposals received during stratum one (except those of the first week); for stratum three placement proposals 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 stratum considered 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 a the respective local federal employment agency as the ratio between unemployed and caseworkers as average of the year when the UI spell starts²⁹ and for the sanction rate, built as in Müller and Oschmiansky (2006), one month lagged to the individual UI start.³⁰

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

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

³⁰Note that using the sanction rate as instrumental variable would not identify the *ex post* effect, but a local average treatment effect (LATE; Angrist, Imbens, and 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.

7 Empirical results

7.1 Descriptive evidence of UI sanctions

Table 4 in the appendix contains numbers on the incidences of sanctions by gender and by stratum. Listed are the absolute numbers as well as the share of those having been sanctioned during each stratum conditional on having reached that stratum and not having been sanctioned before (column 4). The third column shows the absolute number of UI recipients being at risk of being sanctioned during each stratum. We can see that though the absolute numbers of sanctions decrease immensely by each stratum, 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: : 0.24% (women) and 0.34% (men).

These results from the micro data support our assessment of the FEA statistics on the incidences of sanctions (see footnote 22), i.e. in sum we can see that without conditioning on any characteristics sanctions due to refusing a job or an ALMP measure are not events that are very likely to happen during an individual UI receipt spell.

7.2 Matching quality

Table 5 and table 6 in the appendix report on the results of the probit estimations. The dependent variable is the probability of being sanctioned during the stratum considered. In table 7 in the appendix some indicators for the matching quality are presented. Out of 2179 treated UI recipients, we lost 74 due to the common support restriction or the caliper. McFadden's R^2 of the fitted probit estimations before and after matching differ (before: ranging from 0.0766 to 0.0991), but there is still some explanatory power in the models after matching (ranging from 0.0071 to 0.0265; column 6). The mean standardised bias³¹

³¹The mean standardised bias (MSB) is calculated as follows: $MSB = \frac{1}{K} \sum_{k=1}^K 100 \cdot \frac{\bar{X}_{k1t} - \bar{X}_{k0t}}{\sqrt{0.5 \cdot (V_{k1t}(X) + V_{k0t}(X))}}$ with K denoting the number of variables and X_1 (V_1) denoting the mean (variance) in the treated group and 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, and Thomsen (2005)).

as indicator of the distances in the covariate distributions between treated and controls (ranging between 10.28% and 14.61% before matching; column 7) is reduced (ranging between 1.83% and 3.32% after matching; column 8) and is for each of these six subsamples below 5% which is regarded as an acceptable level (cf. Caliendo and Kopeinig (2005)) and only for two above 3%. The differences in the means between treated and matched controls per covariate are all insignificant at a 5%-level (table 8 in the appendix).

7.3 Ex-post effects

We will first discuss the results of the monthly Δ_t^{dynATT} estimates and second the Δ_{DiD}^{dynATT} estimates as a robustness check. Additionally to Δ_{DiD}^{dynATT} , in the appendix we provide Δ_{Sum}^{dynATT} estimates, where the outcome is the number of months in employment and out of labour force respectively during a 12-month period after the stratum considered.

Figures 4-9 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 treatment³² and the monthly Δ_t^{dynATT} . The time axis represents the months from 12 month 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. Regarding the outcome irregular employment the monthly Δ_t^{dynATT} estimates suggest an *ex post* effect for stratum one with a time lag of around six months; for stratum two though being not significant, we find an immediate *ex post* effect. For the outcome out of labour force we hardly find empirical evidence for women in terms of monthly effects. Taking results of table 8 into account, we find a quite higher proportion of UI recipients below 25 years to be sanctioned during stratum one and two. This might be a hint that young female UI

³²We present the difference in the outcome before treatment in order to check graphically the quality of the matches.

recipients are more responsive to a sanction than older ones as in general the effects on employment for stratum three are lower.

For men we observe a rise in the probability of being regularly employed immediately after each stratum considered. Interestingly, for stratum one and two the effects are largest in months 13 until 15 after the start of the individual UI receipt. Another interesting finding is that for stratum one and two the monthly effects on the outcome irregular employment are mostly negative. 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 interpreted only after months three after the end of the stratum. Doing so we still find an effect for several months after the sanction for men.

In sum, both, men and women seem to respond to a sanction in terms of being regularly employed after a sanction during stratum one or two. Regarding the outcome irregular employment, we find gender differences: while women in general are more likely irregularly employed after a sanction, the opposite is the case for men (as they are less likely irregularly employed).

The graphical evidence is supported when looking at the Δ_{DiD}^{dynATT} estimates (being the difference in the number of months after the stratum considered and before UI start during a 12 month period) in table 9 in the appendix: reporting 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 (women: 0.82, 0.98, men: 0.63, 0.58). For women being sanctioned during stratum two we additionally find a significant positive effect on the number of months of irregular employment (0.52). Those women having been sanctioned at the very begin of their UI spell (stratum one) are more likely to withdraw from the labour market (0.42).³³ For men being sanctioned during stratum one the negative effect on irregular employment is even significant (-0.14) and those being sanctioned during stratum two are more likely to be out of the labour force.³⁴

³³For the Δ_{DiD}^{dynATT} and the Δ_{Sum}^{dynATT} estimates the twelve months starting from the fourth month after the stratum considered are counted (see comment above).

³⁴Note that though never differing in terms of their sign, the Δ_{Sum}^{dynATT} estimates differ slightly in size (and significance).

As we want to finally check whether the effects we found are driven by a specific sub group, we now focus on a selection of two different characteristics by which we divide the sample: the age and the regional unemployment rate.

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^{dynATT} estimates but only the Δ_{DiD}^{dynATT} 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.³⁵

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 listed in table 10 in the appendix show that the matching quality naturally suffers a bit by dividing the sample as the pools of potential controls are diminished. Out of the 16 subsamples, only of six the MSB after matching is below a value of 3% and of two it is even higher than 5% (5.59 and 5.86). Table 11 in the appendix reports on the Δ_{DiD}^{dynATT} estimate per subgroup, stratum and for men and women.³⁶

Our results suggest that for women the effects on regular employment found for the first stratum are indeed driven by young UI female recipients, while older women seem to be more responsive in terms of irregular employment. Also the effect found for the outcome out of labour force are driven by older women (while for the younger group though not being significant one is 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 irregular employment and out of labour force both male age groups seem to react in a similar way.

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

³⁵We take the median to split the sample.

³⁶In order to save space the probit coefficient tables of the subgroup analysis are not included in this paper.

(1.28, 1.46). For men on the other hand being sanctioned during stratum one in a worse off region is much more effective (1.16) than in better off regions (-0.04). The significant negative effect of a sanction during stratum one on the outcome irregular employment for men seems to be driven by the subgroup living in worse off regions (-0.33). Interestingly, this group is the only male group (among stratum one and two and for the both region types) where we do not find a significant positive effect on the outcome out of labour force (-0.21).

8 Conclusion and outlook

In this paper we used administrative data in order to evaluate the *ex post* effect of sanctions due to refusing a job 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 identified the *ex post* effect using a matching approach that takes timing of the treatment explicitly into account: we modeled 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 introduced a difference-in-differences matching estimator. A potential influence of unobserved time invariant characteristics is eliminated herewith. Is it possible though that time varying characteristics might undermine the identifying assumption (CIA)? We therefore include time varying potentially confounding factors, namely variables on whether a person took up an irregular job and the number of placement proposals he received during the pre-treatment UI period and are confident that the assumption holds. In order to give some insights into different subgroups we finally distinguished the sample by the age and by the 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 was quite a "tough" instrument meaning 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. For women these effects are mainly driven by younger UI recipients and for those in better off regions. Regarding the outcome irregular employment the results are ambiguous: for women they are positive, but negative for men. Taking the subgroups into account, we find the positive effect (on irregular employment) results from the older subgroup while the negative effect (for stratum one) is found largest in the subsample of male UI recipients in worse off regions. In 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 withdraw from the labour market.

The results are in line with the empirical literature on *ex post* effects of unemployment benefit sanctions summarized in section 2. 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 the employment (e.g. analysing the job duration of the first job after a sanction).

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A Appendix

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
≥ 45	30	14
≥ 45	36	18
≥ 47	44	22
≥ 52	52	26
≥ 57	64	32

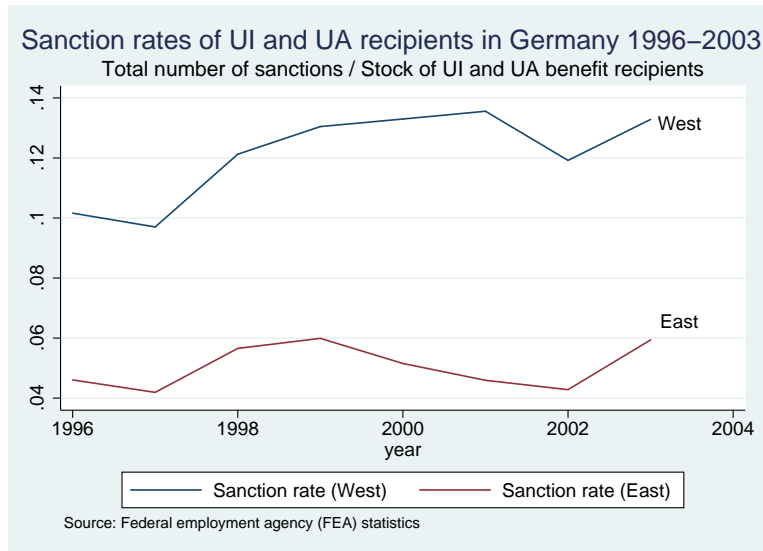
Source: Social Code (SC) III - Employment Promotion, 4th edition (February 15 2001). Text edition, Nuremberg (Federal Employment Agency).

Table 2: Sanctions legislation in 2000/2001

Type	Duration	Notes	Reduction
(1) Voluntary quit	→ At least 1/4 of UI duration (≥ 12 weeks)	6 weeks in case of hard-ship, 3 weeks if the job had ended anyway within 4 weeks	100%
(2) Refusal of work	→ 12 weeks	3 weeks if a temporary (<6 weeks) job was refused	100%
(3) Refusal of ALMP measure	→ 12 weeks	3 weeks if integration measure < 6 weeks	100%
(4) Drop out ALMP measure	→ 12 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, 4th edition (February 15 2001). Text edition, Nuremberg (Federal Employment Agency).

Figure 1: Sanction rates in Germany 1996-2003



Note: Short-terms sanctions due to not showing up at the job center are missing as there are no official statistics on this type of sanctions until 2005.

Table 3: Sanction rates by sanction types 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.

Figure 2: Distribution of sanction starts

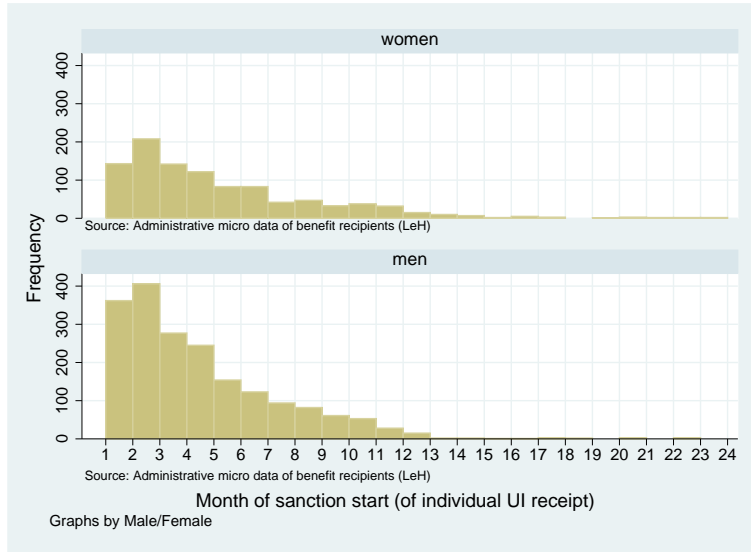


Figure 3: Distribution of starts of non-effective sanctions (i.e. < 7 days)

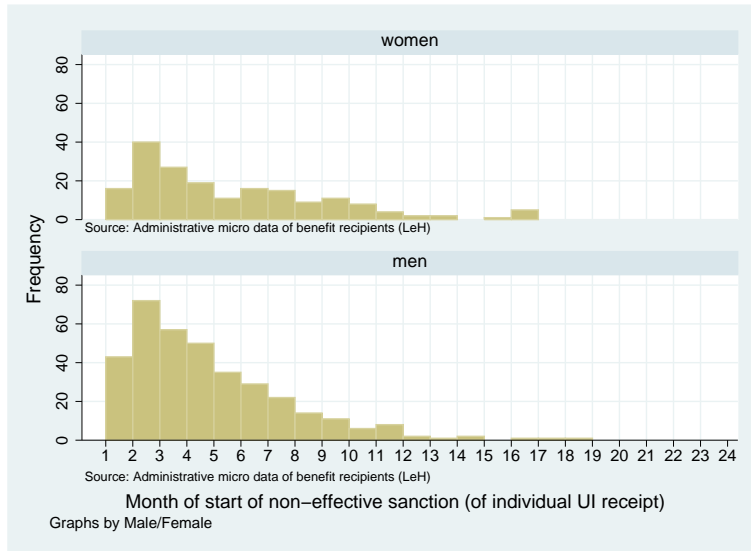


Table 4: Sanctions: number of incidences, number of persons at risk and sanction probabilities

Stratum (month of UI receipt) (1)	Treated of (2)	Potential controls (3)	Sanctions conditional on being at risk (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 on the number of sanctions due to refusal of a job or refusal of an ALMP measure. Source: Administrative micro data of benefit recipients (LeH).

A.1 Probit estimates

Table 5: Probit estimates - women

Stratum	1		2		3	
	Coefficients	S.E.	Coefficients	S.E.	Coefficients	S.E.
age 18-24	0.405***	(0.080)	0.638***	(0.113)	0.507***	(0.142)
age 25-29	0.222***	(0.084)	0.471***	(0.112)	0.400***	(0.135)
age 30-34	0.158*	(0.084)	0.356***	(0.113)	0.166	(0.144)
age 35-40	0.170**	(0.081)	0.324***	(0.109)	0.200	(0.138)
age 41-45	-0.075	(0.094)	0.278**	(0.112)	0.206	(0.144)
daily wage in last job in €	-0.003***	(0.001)	-0.003***	(0.001)	-0.004***	(0.001)
child 0-3 in household	-0.186*	(0.098)	-0.155*	(0.092)	-0.005	(0.109)
child 4-11 in household	-0.287***	(0.079)	-0.150*	(0.081)	-0.086	(0.090)
child 12-17 in household	-0.083	(0.083)	-0.043	(0.093)	-0.020	(0.106)
sanction history (no. in last 1 year)	0.207	(0.196)	0.077	(0.334)		
German citizen	-0.022	(0.092)	0.048	(0.117)	-0.035	(0.138)
non European citizenship	0.161	(0.105)	0.150	(0.131)	0.048	(0.159)
no vocational degree	0.029	(0.056)	0.060	(0.066)	0.061	(0.077)
school degree: low	0.411***	(0.091)	0.135*	(0.081)	0.067	(0.095)
school degree: middle	0.275***	(0.093)	0.057	(0.083)	-0.021	(0.099)
desired job: low or no qualification	-0.035	(0.068)	0.018	(0.088)	0.051	(0.086)
desired job: middle qualification	-0.038	(0.073)	-0.027	(0.093)	-0.070	(0.082)
desired job: fulltime	-0.032	(0.058)	0.058	(0.065)	-0.051	(0.071)
marital status: married	-0.021	(0.045)	0.037	(0.050)	0.018	(0.060)
health problems (caseworker's assessment)	-0.513***	(0.130)	-0.379***	(0.131)	-0.172	(0.125)
local unemployment rate	0.018	(0.015)	0.020	(0.017)	-0.032	(0.022)
local vacancy rate	0.004*	(0.002)	-0.002	(0.003)	-0.010***	(0.004)
caseload in local employment agency	-0.001	(0.004)	-0.015***	(0.005)	-0.006	(0.006)
sanction rate in local employment agency	0.352***	(0.077)	0.416***	(0.083)	0.197	(0.123)
commuting distance to last job in 10 km	-0.004	(0.003)	-0.007*	(0.004)	-0.001	(0.005)
UI eligibility duration in months	-0.004	(0.006)	0.025***	(0.008)	0.009	(0.011)
irregular job in month 2 after UI start			0.217***	(0.077)		
irregular job in month 4 after UI start					0.170**	(0.085)
no. placement proposals received in week 1	0.051	(0.046)	-0.107	(0.078)	0.109**	(0.044)
any placement proposal received in week 1	0.096	(0.140)	0.359**	(0.170)	-0.043	(0.175)
no. placement proposals received in stratum 1			0.044***	(0.007)	0.014	(0.018)
any placement proposal received in stratum 1			0.312***	(0.048)	0.194***	(0.065)
no. placement proposals received in stratum 2					0.036***	(0.012)
any placement proposals received in stratum 2					0.243***	(0.061)
Constant	-3.706***	(0.324)	-3.289***	(0.398)	-2.928***	(0.496)

Variables controlled for in each estimation:

- dummies for industrial sector
- cumulated duration of contributory jobs for each of three years
- average durations of contributory jobs (half a year - three years history)
- number of firms of contributory jobs (half a year - three years history)
- cumulated UI benefit receipt (half a year - three years history)
- outcome in month -1 until -12 previous to UI start
- dummies for job position in last job
- cumulated UA benefit receipt (half a year - three years history)
- dummies for firm size of previous employer
- cumulated duration of minor jobs for each of three years

McFadden's R^2	0.078	0.124	0.114
N	144711.000	100136.000	67618.000

Note: This table reports on the coefficient estimates for the treatment probits per stratum considered. For stratum one, cases where the duration of UI receipt was less than eight days as well as those cases where a sanction was imposed during the first seven days are excluded. Robust standard errors in parentheses. Significance levels: ***: 1%; **: 5%; *: 10%

Table 6: Probit estimates - men

Stratum	1		2		3	
	Coefficients	S.E.	Coefficients	S.E.	Coefficients	S.E.
age 18-24	0.359***	(0.067)	0.453***	(0.082)	0.558***	(0.118)
age 25-29	0.248***	(0.067)	0.426***	(0.081)	0.489***	(0.113)
age 30-34	0.127*	(0.069)	0.252***	(0.080)	0.384***	(0.113)
age 35-40	0.028	(0.070)	0.262***	(0.080)	0.236**	(0.117)
age 41-45	0.065	(0.074)	0.072	(0.092)	0.167	(0.122)
daily wage in last job in €	-0.002***	(0.001)	-0.003***	(0.001)	-0.000	(0.001)
child 0-3 in household	-0.177***	(0.063)	-0.112	(0.069)	-0.037	(0.089)
child 4-11 in household	-0.063	(0.058)	-0.018	(0.065)	0.079	(0.084)
child 12-17 in household	-0.098	(0.091)	-0.204*	(0.119)	0.099	(0.115)
sanction history (no. in last 1 year)	0.280***	(0.086)	0.290**	(0.121)	0.217	(0.173)
German citizen	0.020	(0.067)	0.015	(0.073)	-0.070	(0.089)
non European citizenship	0.197***	(0.072)	0.063	(0.080)	-0.076	(0.101)
no vocational degree	-0.019	(0.042)	0.081*	(0.049)	0.129**	(0.062)
school degree: low	0.205***	(0.066)	0.160**	(0.076)	0.095	(0.102)
school degree: middle	0.131*	(0.071)	0.182**	(0.080)	0.045	(0.112)
desired job: low or no qualification	0.002	(0.049)	0.066	(0.074)	0.060	(0.086)
desired job: middle qualification	0.071	(0.053)	0.066	(0.077)	0.076	(0.086)
desired job: fulltime	-0.364***	(0.106)	0.123	(0.200)	0.158	(0.225)
marital status: married	-0.087**	(0.039)	-0.053	(0.048)	-0.112*	(0.063)
health problems (caseworker's assessment)	-0.306***	(0.076)	-0.337***	(0.091)	-0.159*	(0.092)
local unemployment rate	-0.003	(0.010)	-0.013	(0.013)	0.018	(0.016)
local vacancy rate	-0.002	(0.002)	-0.001	(0.002)	-0.001	(0.003)
caseload in local employment agency	-0.004	(0.003)	-0.005	(0.004)	-0.002	(0.005)
sanction rate in local employment agency	0.520***	(0.054)	0.526***	(0.063)	0.435***	(0.090)
commuting distance to last job in 10 km	-0.002	(0.002)	-0.005	(0.003)	-0.004	(0.005)
irregular job in month 2 after UI start			0.023	(0.074)		
irregular job in month 4 after UI start					0.081	(0.086)
UI eligibility duration in months	-0.016***	(0.005)	0.007	(0.006)	0.021**	(0.008)
no. placement proposals received in week 1	0.006	(0.025)	0.018	(0.037)	0.014	(0.034)
any placement proposal received in week 1	0.261***	(0.078)	0.167	(0.114)	0.143	(0.139)
no. placement proposals received in stratum 1			0.058***	(0.006)	-0.023	(0.016)
any placement proposal received in stratum 1			0.382***	(0.039)	0.125**	(0.054)
no. placement proposals received in stratum 2					0.048***	(0.011)
any placement proposals received in stratum 2					0.321***	(0.051)
Constant	-2.498***	(0.263)	-3.362***	(0.366)	-4.284***	(0.470)

Variables controlled for in each estimation:

- dummies for industrial sector
- cumulated duration of contributory jobs for each of three years
- average durations of contributory jobs (half a year - three years history)
- number of firms of contributory jobs (half a year - three years history)
- cumulated UI benefit receipt (half a year - three years history)
- outcome in month -1 until -12 previous to UI start
- dummies for job position in last job
- cumulated UA benefit receipt (half a year - three years history)
- dummies for firm size of previous employer
- cumulated duration of minor jobs for each of three years

McFadden's R^2	0.081	0.148	0.112
N	213521.000	140497.000	79506.000

Note: This table reports on the coefficient estimates for the treatment probits per stratum considered. For stratum one, cases where the duration of UI receipt was less than eight days as well as those cases where a sanction was imposed during the first seven days are excluded. Robust standard errors in parentheses. Significance levels: ***: 1%; **: 5%; *: 10%

A.2 Indicators of covariates balancing, before and after matching, by stratum considered

Table 7: Balacing quality indicators

Stratum	Treated lost	Controls used	McFadden's R^2	McFadden's R^2	MSB	MSB
(1)	(2)	(3)	Before (4)	After (5)	Before (6)	After (7)
Women:						
1	8	1477	.0781	.0142	13.7	2.84
2	14	1161	.1242	.0274	15.77	3.74
3	8	743	.1135	.0278	13.07	3.16
Men:						
1	11	3089	.0813	.0091	14.36	2.49
2	22	2320	.1479	.0108	16.09	1.96
3	11	1217	.1118	.018	12.17	2.45

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 31.

Table 8: Matching quality indicators per covariate

Covariate	Mean Treated	Mean Con-trols	%bias	bias re-duction	t-test: p-value
Women: stratum one					
UI eligibility duration in months	316.39	321.84	-3.7	84.7	0.621
age 18-24	.34539	.32916	3.9	92.6	0.674
age 25-29	.15789	.16065	-0.8	91.1	0.926
age 30-34	.13487	.13722	-0.7	94.8	0.933
age 35-40	.16447	.16364	0.2	97.9	0.978
age 41-45	.0625	.06114	0.5	98.2	0.945
daily wage in last job in €	39.734	37.462	8.5	56.8	0.210
no. placement proposals received in week 1	.11513	.07841	6.8	51.6	0.455
any placement proposal received in week 1	.04605	.04054	3.0	78.8	0.740
child 0-3 in household	.06908	.08439	-5.8	11.8	0.480
child 4-11 in household	.08553	.08838	-0.9	96.5	0.901
child 12-17 in household	.06579	.06595	-0.1	99.4	0.994
local unemployment rate	8.0028	8.0001	0.1	99.5	0.989
local vacancy rate	19.637	19.816	-1.6	93.6	0.861
desired job: fulltime	.74342	.73668	1.5	93.1	0.851
caseload in local employment agency	51.629	51.665	-0.4	98.3	0.956
sanction rate in local employment agency	.78095	.7817	-0.3	99.2	0.975
sanction history (no. in last 1 year)	.00658	.01063	-3.9	60.1	0.628
German citizen	.82566	.82418	0.4	98.3	0.962
non European citizenship	.13158	.12587	2.0	92.2	0.834
commuting distance to last job in 10 km	1.9019	2.0068	-1.6	82.5	0.821
no vocational degree	.38487	.39445	-2.0	88.7	0.810
school degree: low	.69737	.7096	-2.5	92.2	0.743
school degree: middle	.25329	.23325	4.5	48.8	0.567
desired job: low or no qualification	.49013	.50199	-2.4	85.1	0.771
desired job: middle qualification	.52961	.50665	4.6	25.0	0.573
marital status: married	.39803	.41001	-2.4	89.6	0.765
health problems (caseworker's assessment)	.01645	.02658	-4.9	83.0	0.391
Women: stratum two					
UI eligibility duration in months	336.36	346.34	-7.1	53.3	0.373
age 18-24	.31727	.29216	6.3	88.8	0.548
age 25-29	.16867	.17861	-2.8	79.0	0.773
age 30-34	.15663	.18161	-6.7	-12.1	0.463
age 35-40	.16064	.14462	4.1	71.1	0.624
age 41-45	.09237	.08596	2.0	88.2	0.804
daily wage in last job in €	38.49	37.46	3.8	76.6	0.634
irregular job in month 2 after UI start	.14859	.15068	-0.6	95.9	0.949
no. placement proposals received in week 1	.06827	.05099	5.6	51.4	0.568
any placement proposal received in week 1	.04418	.03595	4.9	66.6	0.644

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no. placement proposals received in stratum 1	2.0803	1.6555	16.3	73.9	0.085
any placement proposal received in stratum 1	.63052	.61379	3.6	95.0	0.704
child 0-3 in household	.11647	.14873	-10.4	-549.6	0.294
child 4-11 in household	.11647	.11202	1.3	93.4	0.878
child 12-17 in household	.06426	.05852	2.1	83.1	0.792
local unemployment rate	8.0037	7.8938	4.6	83.2	0.604
local vacancy rate	19.078	19.009	0.6	97.3	0.944
desired job: fulltime	.70683	.69606	2.3	91.2	0.796
caseload in local employment agency	51.206	50.968	3.0	92.4	0.720
sanction rate in local employment agency	.78341	.78595	-1.0	98.1	0.918
sanction history (no. in last 1 year)	.00402	.00334	1.2	54.0	0.903
German citizen	.85944	.82173	11.7	27.0	0.256
non European citizenship	.10442	.14399	-14.3	23.0	0.185
commuting distance to last job in 10 km	1.6148	1.6537	-0.7	94.4	0.926
no vocational degree	.36546	.38015	-3.1	62.9	0.738
school degree: low	.61446	.62626	-2.4	78.2	0.789
school degree: middle	.28112	.25754	5.3	-606.3	0.558
desired job: low or no qualification	.4739	.48004	-1.2	84.8	0.892
desired job: middle qualification	.5502	.52762	4.5	-43.1	0.618
marital status: married	.42972	.46137	-6.4	75.6	0.483
health problems (caseworker's assessment)	.0241	.01839	2.5	91.2	0.663
Women: stratum three					
UI eligibility duration in months	349.28	350.22	-0.7	96.9	0.948
age 18-24	.19745	.16905	8.1	79.2	0.519
age 25-29	.19745	.19955	-0.6	97.5	0.963
age 30-34	.15924	.17121	-3.2	54.5	0.777
age 35-40	.19108	.20495	-3.4	52.1	0.760
age 41-45	.12102	.1207	0.1	98.8	0.993
daily wage in last job in €	34.58	34.332	0.9	96.0	0.927
irregular job in month 2 after UI start	.17834	.20971	-8.6	54.6	0.486
no. placement proposals received in week 1	.07006	.06511	1.1	93.1	0.919
any placement proposal received in week 1	.03185	.03526	-2.1	83.7	0.868
no. placement proposals received in week 1	1.2611	1.269	-0.5	98.9	0.972
any placement proposal received in week 1	.51592	.51904	-0.7	98.7	0.956
no. placement proposals received in stratum 1	1.5159	1.4159	5.3	90.2	0.690
any placement proposal received in stratum 1	.57962	.59128	-2.5	95.9	0.835
child 0-3 in household	.17197	.18376	-3.3	70.9	0.787
child 4-11 in household	.15287	.17294	-5.4	53.3	0.633
child 12-17 in household	.07643	.07982	-1.2	82.9	0.912
local unemployment rate	8.0152	8.0523	-1.5	93.4	0.889
local vacancy rate	17.135	17.309	-1.8	73.3	0.866
desired job: fulltime	.56051	.52985	6.2	-28.9	0.589
caseload in local employment agency	52.341	52.065	3.4	87.0	0.749
sanction rate in local employment agency	.7393	.72763	4.2	83.1	0.717
sanction history (no. in last 1 year)	0	0	0.0	100.0	.
German citizen	.8535	.86967	-4.9	72.5	0.681
non European citizenship	.10191	.08923	4.6	74.1	0.705
commuting distance to last job in 10 km	1.9579	1.8936	1.0	78.5	0.927
no vocational degree	.3949	.38146	2.8	79.1	0.809
school degree: low	.64331	.61551	5.7	58.4	0.613
school degree: middle	.24204	.26119	-4.4	39.2	0.698
desired job: low or no qualification	.50318	.50757	-0.9	93.0	0.939
desired job: middle qualification	.49045	.47491	3.1	53.6	0.785
marital status: married	.52229	.55191	-6.0	62.4	0.602
health problems (caseworker's assessment)	.04459	.05581	-4.3	78.1	0.652
Men: stratum one					
UI eligibility duration in months	291.78	290.29	1.1	97.0	0.819
age 18-24	.36809	.35176	3.8	92.5	0.546
age 25-29	.21959	.22557	-1.5	91.4	0.798
age 30-34	.15324	.16351	-2.8	66.7	0.617
age 35-40	.11532	.10806	2.0	91.8	0.682
age 41-45	.07583	.08191	-2.0	87.9	0.688
daily wage in last job in €	54.728	53.155	5.7	85.6	0.220
no. placement proposals received in week 1	.11374	.09681	3.7	76.3	0.576
any placement proposal received in week 1	.06003	.05361	3.3	82.5	0.622
child 0-3 in household	.05687	.05908	-0.8	93.9	0.866
child 4-11 in household	.07267	.06698	1.9	90.2	0.692
child 12-17 in household	.02054	.01991	0.3	98.3	0.936
local unemployment rate	8.1374	8.2047	-2.9	85.3	0.593
local vacancy rate	17.511	17.336	1.8	88.9	0.750
desired job: fulltime	.97946	.98049	-0.8	90.3	0.896
caseload in local employment agency	52.024	52.182	-2.0	90.9	0.696

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sanction rate in local employment agency	.77594	.77502	0.4	99.3	0.953
sanction history (no. in last 1 year)	.0237	.02541	-1.3	91.4	0.859
German citizen	.75671	.73512	5.5	76.7	0.378
non European citizenship	.19747	.21685	-5.4	77.9	0.395
commuting distance to last job in 10 km	2.072	2.0336	0.6	90.3	0.905
no vocational degree	.38073	.41501	-7.2	30.9	0.213
school degree: low	.78989	.80845	-4.3	68.1	0.410
school degree: middle	.15798	.14415	3.8	-307.9	0.493
desired job: low or no qualification	.48973	.52227	-6.5	29.7	0.247
desired job: middle qualification	.5466	.51382	6.6	-131.7	0.243
marital status: married	.2812	.26561	3.3	91.9	0.534
health problems (caseworker's assessment)	.03002	.03254	-1.1	94.8	0.796
Men: stratum two					
UI eligibility duration in months	314.06	312.17	1.4	94.3	0.784
age 18-24	.28831	.2833	1.3	96.8	0.862
age 25-29	.24798	.25622	-2.1	92.5	0.766
age 30-34	.16734	.17977	-3.3	11.3	0.606
age 35-40	.16734	.15531	3.1	67.5	0.607
age 41-45	.05645	.05766	-0.4	98.3	0.935
daily wage in last job in €	54.921	53.531	4.8	85.8	0.344
irregular job in month 2 after UI start	.05847	.05833	0.1	68.8	0.993
no. placement proposals received in week 1	.09879	.11048	-2.5	84.8	0.799
any placement proposal received in week 1	.04637	.05208	-3.2	81.8	0.678
no. placement proposals received in stratum 1	2.3609	2.282	3.0	96.0	0.682
any placement proposal received in stratum 1	.70363	.70847	-1.1	98.9	0.867
child 0-3 in household	.08065	.0753	1.9	62.7	0.754
child 4-11 in household	.09476	.10897	-4.5	62.3	0.460
child 12-17 in household	.01613	.01734	-0.6	97.2	0.882
local unemployment rate	7.8602	7.9383	-3.3	92.3	0.591
local vacancy rate	19.266	18.986	2.6	92.3	0.697
desired job: fulltime	.99395	.98911	5.1	22.6	0.406
caseload in local employment agency	51.363	51.631	-3.4	90.4	0.575
sanction rate in local employment agency	.79698	.78557	4.2	93.4	0.552
sanction history (no. in last 1 year)	.02016	.01613	3.3	69.8	0.668
German citizen	.7621	.76058	0.4	98.2	0.956
non European citizenship	.18145	.18468	-0.9	95.2	0.896
commuting distance to last job in 10 km	1.7587	1.7603	-0.0	99.7	0.996
no vocational degree	.41734	.43888	-4.4	62.0	0.494
school degree: low	.76815	.77366	-1.3	57.9	0.837
school degree: middle	.17742	.17544	0.5	95.1	0.935
desired job: low or no qualification	.5121	.52443	-2.5	65.1	0.698
desired job: middle qualification	.51613	.50444	2.3	18.9	0.713
marital status: married	.32258	.32046	0.4	98.7	0.943
health problems (caseworker's assessment)	.02621	.02873	-1.1	96.2	0.808
Men: stratum three					
UI eligibility duration in months	332.67	334.48	-1.3	92.8	0.861
age 18-24	.26316	.27024	-1.8	95.5	0.854
age 25-29	.22932	.20808	5.5	77.9	0.554
age 30-34	.19549	.19298	0.7	83.4	0.942
age 35-40	.1391	.14561	-1.7	90.2	0.830
age 41-45	.07519	.07462	0.2	99.1	0.980
daily wage in last job in €	57.47	57.471	-0.0	100.0	0.999
irregular job in month 2 after UI start	.08271	.08816	-2.0	65.0	0.822
no. placement proposals received in week 1	.07519	.07406	0.3	97.5	0.976
any placement proposal received in week 1	.03759	.04305	-3.4	72.8	0.750
no. placement proposals received in week 1	1.0714	1.0203	3.4	89.6	0.726
any placement proposal received in week 1	.45489	.44875	1.3	96.6	0.887
no. placement proposals received in stratum 1	1.8571	1.6808	9.3	86.0	0.391
any placement proposal received in stratum 1	.63158	.62707	1.0	98.7	0.914
child 0-3 in household	.08647	.07838	2.9	4.9	0.735
child 4-11 in household	.10526	.12306	-5.6	27.5	0.520
child 12-17 in household	.03759	.04229	-2.2	78.4	0.782
local unemployment rate	8.4383	8.4219	0.7	97.0	0.936
local vacancy rate	17.279	17.008	2.6	83.8	0.766
desired job: fulltime	.99248	.99549	-2.8	65.9	0.654
caseload in local employment agency	53.218	53.046	2.1	91.0	0.802
sanction rate in local employment agency	.75992	.76874	-3.3	91.8	0.731
sanction history (no. in last 1 year)	.01504	.01554	-0.5	94.7	0.964
German citizen	.77068	.74129	7.3	48.3	0.431
non European citizenship	.16165	.18477	-6.5	29.7	0.482
commuting distance to last job in 10 km	1.7332	1.9384	-3.3	64.4	0.687
no vocational degree	.45865	.49311	-7.0	51.6	0.427

Table continued next page

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school degree: low	.79699	.78102	3.7	71.5	0.652
school degree: middle	.1391	.14455	-1.6	18.6	0.857
desired job: low or no qualification	.53008	.56021	-6.0	5.4	0.486
desired job: middle qualification	.48496	.46159	4.7	-59.8	0.590
marital status: married	.33835	.35194	-2.8	90.9	0.742
health problems (caseworker's assessment)	.05263	.04361	3.3	86.3	0.628

Note:

A.3 Ex-post effects no subgroups

Figure 4: Women - Outcome: regular employment

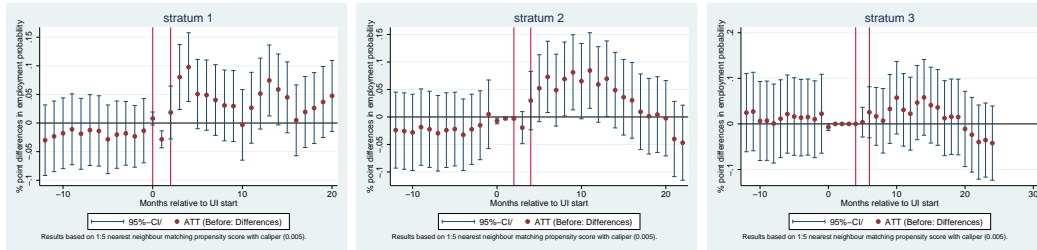


Figure 5: Women - Outcome: other employment

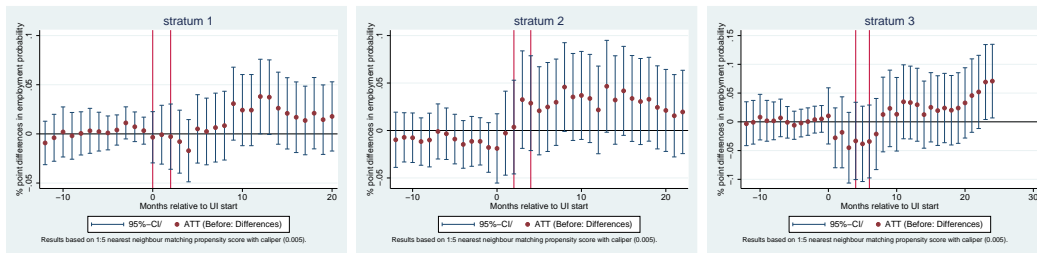


Figure 6: Women - Outcome: out of labour market

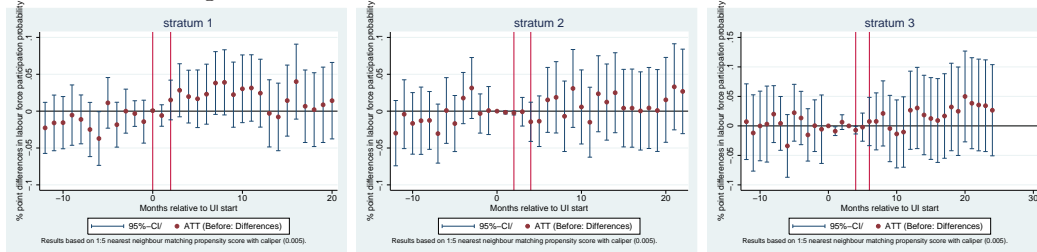


Figure 7: Men - Outcome: regular employment

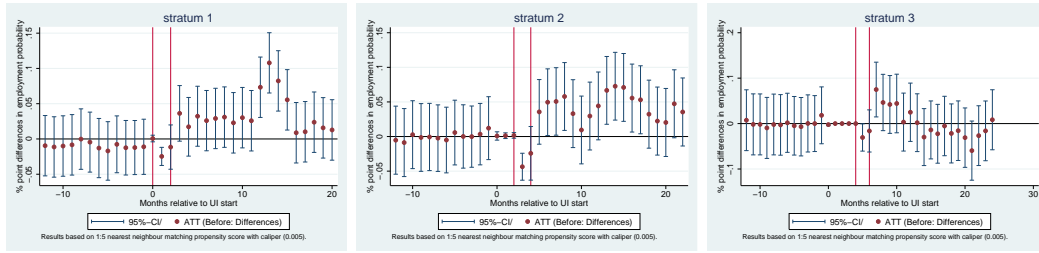


Figure 8: Men - Outcome: other employment

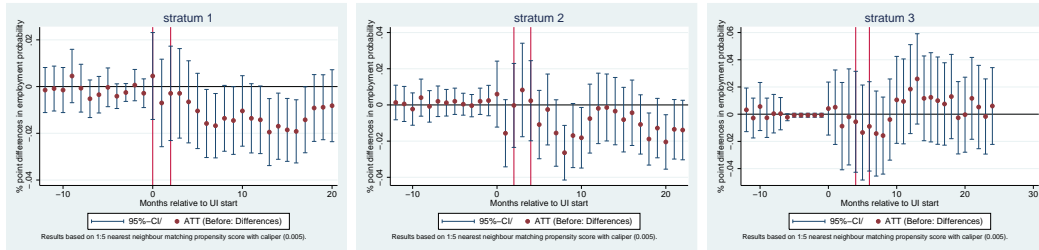


Figure 9: Men - Outcome: out of labour market

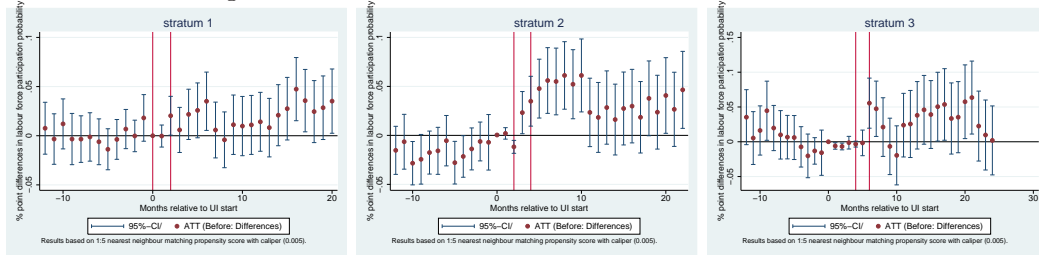


Table 9: $\Delta_D^{dynATT} iD$ and Δ_{sum}^{dynATT} estimates for three different outcomes

Estimand	Stratum (month of UI receipt)	Outcome: regular employment	Outcome: other employment	Outcome: out of labour force	Outcome: regular employment	Outcome: other employment	Outcome: out of labour force
		(1)	(2)	(3)	(4)	(5)	(6)
		Women:			Men:		
Δ_{sum}^{dynATT} :	1 (1-2)	.59*	.18	.26	.51**	-.16**	.22*
	2 (3-4)	.72**	.4*	.09	.58**	-.12	.4**
	3 (5-6)	.38	.23	.24	.14	.08	.45*
Δ_{DiD}^{dynATT} :	1 (1-2)	.82**	.16	.42*	.63**	-.14**	.21
	2 (3-4)	.98**	.52**	.16	.58*	-.13	.59***
	3 (5-6)	.2	.21	.23	.15	.09	.36

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}^{dynATT} ; Number of months in employment during 12 months after stratum, and respectively months 4-15 after (out of labour market); Δ_{DiD}^{dynATT} Dependent variable of Δ_{sum}^{dynATT} minus number of months in employment during 12 months before UI start.

A.4 Subgroup analyses

Table 10: Balacing quality indicators for subgroup analysis

Stratum	Subgroup	Treated	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)	(8)	(9)
Women:								
age 18-29	1	160	24	651	.0885	.0363	12.01	3.33
	2	130	14	508	.1204	.0437	13.22	3.49
age 30-55	1	152	18	637	.0763	.0394	11.41	4.06
	2	133	19	550	.1216	.041	12.99	3.82
Local unemployment rate low	1	180	24	737	.1014	.039	14.93	3.76
	2	144	17	566	.1603	.0621	16.67	5.59
Local unemployment rate high	1	132	18	549	.0964	.045	12.01	4.17
	2	119	14	469	.1432	.0968	14.86	5.86
Men:								
age 18-29	1	379	26	1684	.0553	.0143	10.05	3.1
	2	280	31	1162	.1242	.0204	11.16	2.68
age 30-55	1	265	30	1162	.0819	.0208	13.59	2.95
	2	238	21	999	.1593	.023	16.64	2.85
Local unemployment rate low	1	327	26	1417	.1013	.017	15.59	2.24
	2	302	29	1220	.1729	.0175	18.2	2.51
Local unemployment rate high	1	317	32	1378	.0907	.0238	14.11	3.5
	2	216	20	925	.1402	.0336	14.16	2.82

Local unemployment rate low: below the median; local unemployment rate high: above or equal the median

Table 11: Δ_{DiD}^{ToTATT} estimates for subgroups and for three different outcomes

Subgroup	Estimand	Stratum	Outcome: regular employ- ment (1)	Outcome: other em- ployment (2)	Outcome: out of labour force (3)	Outcome: regular employ- ment (4)	Outcome: other em- ployment (5)	Outcome: out of labour force (6)
			Women:			Men:		
age 18-29	Δ_{sum}^{dynATT} :	1	1.19***	.1	-.11	.59**	-.17**	.21
		2	1.03**	.34	-.14	.87***	-.23**	.62***
age 30-55		1	.49	.45*	.53*	.23	-.23**	.26
		2	.12	.67*	.53	.28	-.14	.37
Local unemployment rate low		1	1.28***	.17	-.01	-.05	-.03	.46**
		2	1.06**	.64*	.1	.72**	-.12	.54**
Local unemployment rate high		1	.17	.55*	-.02	1.11***	-.32***	-.17
		2	.92*	.33	.18	.64*	-.12	.51**
age 18-29	Δ_{DiD}^{dynATT} :	1	1.24**	.13	.05	.47	-.13	.2
		2	.82	.46	-.23	.84*	-.24**	.72***
age 30-55		1	.74	.48	.41	.2	-.23**	.22
		2	.15	.73*	.71**	.43	-.16	.48**
Local unemployment rate low		1	1.28**	.27	.18	-.04	-.01	.45**
		2	1.46**	.51	.28	.66	-.1	.63**
Local unemployment rate high		1	.39	.52	-.02	1.16***	-.33***	-.21
		2	.84	.27	.27	.56	-.12	.59**

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}^{dynATT} : Number of months in employment during 12 months after stratum, and respectively months 4-15 after (out of labour market); Δ_{DiD}^{dynATT} : Dependent variable of Δ_{sum}^{dynATT} minus number of months in employment during 12 months before UI start.