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The Effect of Unemployment Benefit II Sanctions on Reservation Wages

Julia Schneider

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Abstract

In 2005, benefit sanctions in Germany were tightened with the introduction of the new means-tested unemployment benefit II (UB II), codified in Social Code (SC) II. This study analyzes the effect of benefit sanctions on the reservation wage of sanctioned unemployment benefit II recipients. The behavioral effect of a benefit sanction is an empirically open question. According to job search theory, benefit sanctions directly reduce reservation wages. To explore this hypothesis, propensity score matching is adopted. The dataset used is a unique survey of UB II recipients in the first year of SC II. For the identification of the effect, the study relies on the rich individual data and the rather unsystematic sanctioning process in the starting months after the introduction of the SC II. The timing of the sanction is explicitly considered by estimating the effects for the first four quarters of UB II receipt in 2005. The main result is that there was no significant effect of sanctions on the reservation wages of sanctioned unemployment benefit II recipients. A side result is that sanctioned UB II recipients were not more likely to be employed at the time of their interview either. Both results are robust to various matching estimators, estimation specifications and to the timing of the UB II sanction.

JEL classification: J64, J68, C13

Keywords: Job search, unemployment insurance, benefit sanction, reservation wages

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1 Introduction

It is an open question how and to what amount benefit sanctions change the behavior of benefit recipients. I want to contribute to the ongoing debate on the effectiveness of benefit sanctions by estimating the effect of Germany's new unemployment benefit II sanctions on the reservation wages of sanctioned benefit recipients in the year 2005. Several studies on optimal unemployment insurance indicate that a strict benefit sanction policy would be more efficient than lower benefits to enhance compliance with the eligibility requirements of unemployment insurance (see e.g. Boone et al. 2007, Kluge 2006). Enhanced compliance with eligibility requirements, such as greater readiness to search and accept jobs, would lead to a reduction in unemployment.

In fact, many OECD countries, concerned about their unemployment rates, tightened unemployment benefit sanctions (see e.g. Grubb 2000, Boone and Van Ours 2000, Kemmerling and Bruttel 2005, Nickell 2003, OECD 2007). So did Germany, most notably with its recent labor market reform "Hartz IV". The labor market reform was codified in the new Social Code II, which came into force January 2005, and has as its core the basic social security for needy job-seekers called unemployment benefit II (UB II). UB II recipients can receive severe sanctions for various reasons, above all for not showing enough job search effort, for refusing an appropriate job offer or integration measure and for not meeting appointments with case managers in the employment office or medical or psychological appointments (see Statistik der Bundesagentur für Arbeit 2007). The eligibility requirements and sanction intensities for UB II recipients were tightened even further in June 2006 and in January 2007.

There are a couple of empirical studies on the effects of benefit sanctions for European neighbor countries but not for Germany (Van den Berg et al. 2004, Abbring et al. 2005, Lalive et al. 2005, Svarer 2007). These studies mostly focus on the effect of unemployment insurance benefit (UI) sanctions on the exit rates out of benefit receipt. In line with theoretical expectations, results indicate that both warnings before sanctions and imposed sanctions make benefit recipients exit benefit receipt faster. But why sanctions elevate benefit exit rates – this question remains unresolved. Are the observed elevated exit rates out of benefit receipt after a sanction due to a) more job search, b) more effective job search or c) lower reservation wages of the unemployed benefit recipients?

This study wants to contribute to the ongoing discussion about the effectiveness of benefit sanctions by explicitly exploring the hypothesis of job search theory that benefit sanctions directly reduce reservation wages, and by focusing on the German unemployment benefit system. Contrary to most empirical research on sanctions, I regard German unemployment benefit II sanctions, internationally more comparable to social assistance sanctions than to UI sanctions. The data I use is a cross-section survey of unemployment benefit II recipients with rich and unique information on the first fifteen months after the new Social Code II was implemented, called "Life situation and social security 2005". For the identification of the effect of sanctions on res-

ervation wages, I adopt a propensity score matching as selection on observables approach, relying on my informative data and the observation of a rather unsystematic sanctioning process in the first year of the new Social Code II. I explicitly consider the timing of the sanction in my design and estimate the effects for four subsequent quarters of uninterrupted UB II receipt in 2005.

The remainder of the paper is organized as follows. In Section two, I explain the details of the UB II sanctions under evaluation. In Section three, I consider the theoretical relationship between sanctions and reservation wages, and look what previous empirical research can teach us about it. In Section four, I describe the survey and the selection of the sample, with special emphasis on sanctions and reservation wages. In Section five, I explain the methodology I use. In Section six, I present my estimation results. Finally, Section seven summarizes and offers my conclusions.

2 Sanctions in Germany's new means-tested unemployment benefit system

With the establishment of the new Social Code (SC) II, the German government facilitated and intensified the use of sanctions in the unemployment benefit system. The SC II regulates the basic income for needy job-seekers (alias unemployment benefit II or UB II), and was installed on January 1, 2005 through a labor market reform colloquially referred to as "Hartz IV".¹ "Hartz IV" merged former unemployment assistance (UA) and social assistance to means-tested UB II.² Since the level of UB II payments is equivalent to the socio-cultural poverty level, "Hartz IV" reduced average net replacement rates for long-term benefit recipients.³

¹ The law "Hartz IV" ("Viertes Gesetz für moderne Dienstleistungen am Arbeitsmarkt") was passed on December 23, 2003 and is the last of four labor market reforms that rooted in the recommendations of a policy commission in 2002: the "Hartz-Kommission" (named after its chairman Peter Hartz). A comprehensive description of the "Hartz"-reforms offer Ebbinghaus and Eichhorst (2006).

² Previously, the entitled unemployed received UI benefits for up to 32 months at a rate of 60 to 67 percent of prior net salary, followed by means-tested UA benefits of 53 to 57 percent of prior net salary for an unlimited period. From January 1, 2005 on, UI benefits (called unemployment benefit I or UB I now) are followed by UB II benefits (besides, from January 1, 2006 on, UI benefits are only paid for up to 18 months, regulated by "Gesetz zu Reformen am Arbeitsmarkt", passed on December 24, 2004).

³ The OECD measured the effect of the "Hartz IV" reform in Germany by comparing the average net replacement rates for long-term benefit recipients in 2001 and 2005 (OECD 2007: p.173). Net replacement rates fell for all family types the OECD considered in the report: Single, one-earner and two-earner households with no, one or two children. Benefit reductions were generally larger for those who had higher earnings – reflecting the move from a benefit calculated as a percentage of previous earnings to UB II. The reductions were also generally smaller in households with children – families with children received somewhat favourable treatment compared to childless families. A typical long-term unemployed person received around 150 Euro less benefits per month. The simulations of Bloss and Rudolph (2005) that indicated 2/5 of former UA recipients would receive increased benefit levels after the reform were based on 2003 data and could only consider former UA recipients.

The responsible bodies for the implementation of Social Code II are the Federal Employment Agency and the municipalities. According to the “one-stop-shop” principle, UB II recipients have only one contact point called “Job Center”, where the local employment office and the municipality usually work as a team (as “Arbeitsgemeinschaft”, ARGE) and each UB II recipient is attended by her personal case manager. During the implementation until 2008, there are 69 test-districts where, as an alternative, “opting municipalities” run the Job Center by themselves.

In January 2005, around 6.3 million people were immediately affected by the reform (for these and the following numbers see Statistik der Bundesagentur für Arbeit 2005 and Bundesagentur für Arbeit 2007). Around 3.3 million need communities consisting of 6.1 million people received benefits according to SC II; 4.5 million people received UB II and 1.6 million “Sozialgeld”. Approximately 1.7 million UB II recipients were former able-to-work social assistance recipients (around nine of ten social assistance recipients of December 2004), 1.8 million former recipients of unemployment assistance (around nine of ten UA recipients of December 2004), and the final million consisted of relatives of former UA recipients and people who did not receive benefits before. Around 0.2 million former UA recipients ceased to receive benefits between December 2004 and January 2005.⁴

Basically, every person is entitled to UB II, who is able-to-work (defined as being able to work at least three hours a day), who is between 15 and 64 years of age, who generally lives in Germany and who is not fully able to cover her basic needs and the needs of her “need unit” (“Bedarfsgemeinschaft”)⁵. UB II payments involve the base benefit, housing and heating allowances, and social security contributions - as far and as long the measures of active labor market policy have not enabled the unemployment benefit II recipient to cover the basic needs by herself. In the year 2005, the base benefit was 345 Euro in Western Germany and 331 Euro in Eastern Germany.⁶

The benefit is lower if the benefit recipient is member of a need unit: Adult partners receive 90 percent, children between 14 and 25 years 80 percent and children under

⁴ The lack of more exact numbers is due to a change of the labor market statistics evoked by the implementation of the new SC II. Until end of 2004, labor market statistics were solely based on administrative data stemming from the software used in the employment offices (see Statistik der Bundesagentur für Arbeit 2005). In 2005, the data stem from different software products used in the employment offices, in the ARGEs and in the opting municipalities.

⁵ In the year 2005, a need unit consisted in practice of the able-to-work, needy person, her partner and her children (including single, able-to-work children below 18 years; now below 25 years). If a needy person is younger than 15, older than 64 years or not permanently able-to-work and member of a need unit, she is entitled to a benefit called “Sozialgeld” (§ 28 SC II).

⁶ From July 1, 2006 on, the base benefit is the same in East and West Germany. It is adjusted annually on July 1, in line with the current pension value of the statutory pension insurance system (§ 30,4 SC II). At the moment (until July 1, 2008), it is 347 Euro per month.

14 years 60 percent. Since UB II is a means-tested benefit, UB II recipients have to be needy but not necessarily unemployed. In contrast to the former unemployment assistance, eligibility for UB II does not depend on former contributions to unemployment insurance. The base UB II, however, is higher for former UI benefit recipients, because in the two years after moving from UI benefit to UB II a declining bonus is paid.⁷ Additional needs allowances for extra expenses not covered by the standard benefit are paid for expectant mothers from the 13th week of pregnancy, for single parents depending on the age and number of children, for persons with disabilities and for expensive nutrition if demonstrably required for medical reasons.

Apart from receiving passive benefits, UB II recipients can be assigned to measures of active labor market policy (ALMP). ALMP measures aim to improve the chances for their successful reintegration into the regular labor market (and are therefore called integration measures – “Eingliederungsmaßnahmen” – in SC II). The available integration measures in SC II are employment services and so-called psycho-social services.⁸ Employment services are largely identical to those for UI benefit recipients and involve counseling, job placement services, allowances for applications and travel costs and employment and training measures (including employment-creating measures, bridging allowances and allowances for start-ups, subject to § 16 SC II). Psycho-social services are allowances for child care or care for relatives, debt counseling, psycho-social counseling and addiction counseling.

The heart of the integration measures is an individual counseling of the UB II recipient by a personal case manager who concludes an integration contract (“Eingliederungsvereinbarung”) with the benefit recipient. The integration contract has to be signed by both parties, and regulates job search activities, verification of such activities, and all benefit payments and integration measures the benefit recipient is entitled to. This contract is to be renewed every six months. Since UB II recipients have neither right nor duty to receive a specific integration measure, to a large extent, it is the case manager who decides what type of integration measures he regards as suitable for the UB II recipient and offers her. Thus, case managers have a large amount of freedom of choice in the determination of integration contracts and the decision of suitable integration measures compared to UB II recipients.

Besides integration measures, the SC II makes use of strict sanctions as instruments of active labor market policy. If UB II recipients do not comply with certain requirements that should fasten their reintegration in the labor market, they can receive a sanction in form of a cut of benefits. Beforehand, they have to be legally

⁷ The bonus is two thirds of the difference between UB I including housing allowance and UB II with an upper limit of 160 Euro for singles and 360 Euro for couples; each child raises the limit by 60 Euro. After one year, the bonus is cut in half.

⁸ The Federal Employment Agency is responsible for the basic benefit of UB II and the integration measures. The municipality is responsible for housing and heating costs and psycho-social services.

informed about the possibilities of sanctions by the Job Center. Benefit sanctions in SC II can be applied for more reasons than UI benefit sanctions (“Sperrzeiten”) and than former benefit sanctions in social assistance.⁹ The following paragraphs describe the regulation for UB II sanctions during my observation period (and valid until 25 July 2006 when sanctions were further tightened; for details see Bruhn Tripp and Tripp 2007).

UB II recipients can receive sanctions for various reasons. Sanctions can be imposed if a UB II recipient refuses to accept an offered integration measure (including work measures as One-Euro-Jobs) or appropriate job offer. Appropriate is basically every job offer, including every type of job and the form of employment (minor, part- or full-time-employment, self-employment or employment subject to social insurance contributions). Sanctions can also be imposed if the UB II recipient provokes a drop out of such an appropriate job or an integration measure. Sanctions can be imposed if the UB II recipients refuses to sign an integration contract or fails to meet duties of her integration contract (especially do not show sufficient self-effort in job search). Furthermore, sanctions can be imposed if a benefit recipient intentionally reduces his income or assets in order to be entitled to UB II, incorrectly states his earnings or assets to the Job Center, or keeps on spending money in an uneconomical way (for example by spending too much money on telephone bills or repeatedly not paying rent bills). Sanctions can also be imposed if the UB II recipient fails to report to the Job Center (“Meldeversäumnis”) or fails to meet a medical or psychological appointment (“Terminversäumnis”). Finally, if a needy UI recipient (who is entitled to supplementary UB II receipt) temporarily stops to receive UI benefits due to an UI sanction (“Sperrzeit”), he receives an UB II sanction, too.

The following Table 1 lists the possible non-compliances and the respective sanction according to § 31 SC II for the year 2005 for benefit recipients between 25 and 57 years.

⁹ In 2005, a first UI benefit sanction (“Sperrzeit”) according to § 144 SC III cut UI benefits for three, six or 12 weeks, depending on the justification for the sanction. Justifications for UI sanctions were refusal or quit of appropriate job offers or integration measures, lack of sufficient self-effort in the job search or failure to report to the employment office. More details on SC III sanctions offer Müller and Oschmiansky 2006 or WZB and infas 2005; Münder 2006 describes former benefit sanctions in social assistance (regulated in BSHG).

Table 1
Sanction intensities according to type and frequency of non-compliance for UB II recipients between 25 and 57 years in 2005

	Cut of base benefit for first time	Cut of base benefit for repeated time (within prior sanction period)
1. Refusal of appropriate job offer (including vocational training) or integration measure (including work measures)	30 %	+ 30 percentage points
2. (Provoke) Drop out of appropriate job offer (including vocational training) or integration measure (including work measures)		
3. Refusal of signing integration contract		
4. Failure to meet duties of integration contract, especially do not show sufficient self effort in finding a job		
5. Intentional reduction of or misinformation on earnings and assets		
6. UI benefit sanction		
7. Spending money in an uneconomical way		
8. Failure to report to the Job Center ("Meldeversäumnis")	10 %	+ 10 percentage points
9. Failure to meet medical or psychological appointment ("Terminversäumnis")		

Notes: Own Table. Every sanction takes three months. The benefit cut is computed as a percentage of the base benefit. Moreover, the temporary two-year bonus for ex-recipients of unemployment insurance is additionally abolished during a sanction period.

Most non-compliances lead to sanctions that cut the monthly base benefit (without allowances for accommodation and heating) by 30 percent for three months. For the example of a West German single, this reduced the base benefit of 345 Euro to 242 Euro per month. Exceptions are sanctions for the failure to report to the Job Center or to meet appointments ("Melde- und Terminversäumnis"). These failures can reduce the base benefit by ten percent (reduction to 311 Euro per month in the example). Benefits are cut for three months, even if the requirements are met in the meantime. Moreover, each sanction abolishes additionally the temporary two-year bonus for ex-recipients of unemployment insurance for the time of the sanction.

If during the sanction period the sanctioned UB II recipient repeatedly non-complies, the base UB II benefit is supposed to be cut by another 30 percentage points (ten percentage points if the obligation to report is not met), and so on, up to a 100 percent reduction. For the example of a West German single, a second sanction would reduce his base UB II benefit to 138 Euro per month. In the case of a 100 percent reduction, payments for additional needs, accommodation and heating can be affected. If the base benefit is curtailed by more than 30 percent, in-kind transfers can be granted.

The intensity of sanctions is higher for people younger than 25 years and older than 57 years. Young UB II recipients from 16 to 24 years of age can receive a 100 percent cut of the base benefit already for the first non-compliance (with allowances for housing and heating paid directly to the landlords). Older UB II recipients who turned 58 years before January 1, 2008 are allowed to effectively retreat from the labor market, while receiving UB II without the risk of being sanctioned if they commit themselves to apply for a regular pension as soon as possible.

The precondition for sanctions is that the non-compliant benefit recipient herself does not prove to the case manager “important reasons” for her behavior. For the failure to refuse an appropriate job or labor market measure, the jurisdiction accepted reasons leading to personal incapability to do the offered work (a comprehensive overview offers Munder 2005). Accepted reasons for such incapability are temporary non-employability due to illness, having children of one’s own or of one’s partner in the household (parental leave, children under three years, children under 16 years without child care, more than three children in school age), caring for relatives (when there is no other possibility) or special disabilities. Other accepted important reasons are reasons of conscience of belief and lack of compatibility with one’s partnership. Integration measures can be refused if they clearly underchallenge the benefit recipient.

In sum, the new Social Code II thoroughly changed the structure of Germany’s social benefit system, to a considerable extent using benefit sanctions as instruments of active labor market policy. This change affects a large part of the needy population in Germany. The benefit sanctions can be imposed for various non-compliances, including showing not enough effort and accepting not every appropriate job offer or integration measure. It is the non-compliant UB II recipient who has to prove important reasons to avoid a sanction. Sanctions cut the base benefit of UB II for three months by 30 percent for most non-compliances of UB II recipients.

3 Theoretical considerations and previous empirical results

In order to capture the effect of unemployment benefit sanctions on employment, job search literature was enriched with results of the literature of law enforcement and normative optimal insurance theory (a survey is offered by Fredriksson and Holmlund 2006). To facilitate deriving my hypotheses of the theoretical relationship between benefit sanctions and reservation wages, I will shortly introduce the partial job search model with sanctions of Abbring et al. (2005). It allows for a certain degree of arbitrariness when a case manager imposes a sanction. This assumption seems to apply for the sanctioning behavior of the Job Centers 1) in general, given c.p. varying sanction rates between and within employment offices (see Müller 2007 for Germany, and Lalive et al. 2005 for Switzerland) and 2) especially in the first year after the new SC II came into force (see the Section Methodology for details).

In the model of Abbring et al. (2005), the optimal strategy of an unemployed insurance benefit recipient consists of choosing both her optimal reservation wage r (the lowest remuneration he will accept) and her intensity of the job search s . The more intense the job search is, the higher the search costs c and the more job offers λ arrive. To allow for arbitrariness in the application of a benefit sanction, the following assumption is made: The benefit recipient neither exactly knows the rules that she has to comply with, nor the type of behavior that will generate a sanction, nor when it will be imposed. But she knows that beyond a certain search intensity no sanction will be imposed. Hence, there is a positive probability of receiving a sanction for

those benefit recipients whose optimal search intensity s_1 is ex-ante below a minimum threshold s^* .

The optimal reservation wage r is implicitly defined by (i) the flow of benefits net of search costs and (ii) the job offer arrival rate times the expected gain of finding a job over receiving UI, as usual, and (iii) the rate at which a benefit sanction arrives times the expected loss of a sanction compared to no sanction. In formal terms:

$$r_1 = \rho R_1 = \max_{s_1} [(b_1 - c(s_1)) + \lambda(s_1) \int_{r_1}^{+\infty} (\frac{w}{\rho} - R_1) dF(w) + I(s_1 < s^*) p_0 (R_2 - R_1)] \quad (T1)$$

with ρ being the discount rate, R_1 the present value of future income and $F(w)$ the cumulative distribution function of all possible wages. $I(s_1 < s^*)$ is an indicator function, equal to 1 if the search intensity s_1 is less than the threshold s^* , and associated with p_0 , the non-zero rate that a sanction is imposed. The optimal value of job search intensity s_1 is reached by differentiating relation T1 with respect to s_1 . Benefit recipients for which it is optimal to search more intensively than the minimum threshold (if $s_1 > s^*$) are not affected by a non-zero sanction probability. For others, their reservation wage r_1 should fall and in turn search intensity s_1 rises, but which of the two changes more, is not explicit.

After a sanction, the unemployment benefits of those benefit recipients with a sanction are reduced permanently ($b_1 > b_2$). This lets their reservation wages r_2 fall and raises their search intensity s_2 at a value higher than the threshold level s^* . This is assumed to happen because they want to avoid at all cost additional sanctioning leading to further benefit reduction. In formal terms (R_2 is the present value of future income after a sanction):

$$r_2 = \rho R_2 = \max_{s_2 | s_2 \geq s^*} [(b_2 - c(s_2)) + \lambda(s_2) \int_{r_2}^{+\infty} (\frac{w}{\rho} - R_2) dF(w)] \quad (T2)$$

A more realistic assumption of sanctions that lead to temporary, not permanent benefit reductions do not alter the direction of the effect, but the degree: the reservation wage simply decreases to a smaller degree. Both the decrease in reservation wages as the increased search intensity contribute to higher exit rates to employment, but again it is unclear whether the reservation wage or the search intensity changes more.¹⁰

¹⁰ Note that this is only one way of modeling such sanction effects; e.g. Van den Berg et al. (2004) suggest that benefit recipients could have an imprecise idea of the necessary amount of search intensity to avoid a sanction. After getting a sanction they may adjust their idea of the threshold value in search intensity, and may as a result maintain a higher search effort after the expiration of the sanction. Furthermore, because the additional search effort generates extra search costs, being unemployed is less preferable after a sanction leading reduced reservation wages. Another explanation is that monitoring is typically tightened after a violation of the requirements. This leads to higher search intensity and therefore higher search costs, too.

In sum, based on these theoretical considerations I derive the following hypotheses: Benefit sanctions reduce reservation wages. The probability to receive a sanction depends on the search intensity and the reservation wage of the unemployed benefit recipient. If the search intensity is too low or the reservation wage too high, a sanction will be imposed with a nonzero probability. Both search intensity and reservation wage are influenced by individual characteristics of the benefit recipient (motivation, ability, risk-aversion), search costs, by the frequency and quality of arriving job offers and by the attractiveness of staying unemployed (including the level of UI benefit, the benefit cut by a sanction and the probability to get sanctioned according to one's effort).

Empirical literature on benefit sanctions and reservation wages

In the following subsection, previous empirical results on three research topics are presented: effects of benefit sanctions, reservation wages and selection into benefit sanctions.

There is a small number of recent empirical studies on the effects of UI sanctions on exit rates of benefit recipients in European labor markets, one for the Netherlands (the mentioned study of Abbring et al. 2005 where the model stems from), one for Switzerland (Lalive et al. 2005) and one for Denmark (Svarer 2007). One study analyzes the effects of social assistance sanctions on exit rates of Dutch benefit recipients (Van den Berg et al. 2004).¹¹ All used the timing-of-event approach on non-experimental data and found significant effects of benefit sanctions on exit rates out of benefit receipt, presumably beyond the sanction period, and rather homogenous across the population (though gender effects might exist). The ex-ante effect might be stronger than the ex-post effect in raising exit rates.

We saw that in theory, exit rates from unemployment increase with higher search intensity and lower reservation wages. If one wants to apply the results of the studies for the prediction of the impact of a sanction on the reservation wage, the prob-

¹¹ Abbring et al. (2005), Lalive et al. (2004) and Svarer (2007) study exit rates out of unemployment benefit receipt, Van den Berg et al. (2004) study exit rates to employment. Abbring et al. (2005) found out that exit rates of sanctioned benefit recipients increased by between 36 percent and 98 percent compared to non-sanctioned benefit recipients. This effect was stable across time and population. Lalive et al. (2004) could distinguish in their data the different impacts of warnings and real sanctions on exit rates out of unemployment. Warnings increased the exit rate by 25 percent, sanctions by 20 percent. The effects did not differ over the population, but the warning effect fell to 16 percent after one month; the ex-post effect did not significantly differ over time. Svarer (2007) found heterogeneous effects by gender: For males he noted significant evidence of ex-ante effects because the association between the risk of being sanctioned and the exit rate out of UI receipt was positive. After being sanctioned, exit rates increased by 98 percent for women and by 55 percent for men. Regarding social assistance recipients, Van den Berg et al. (2004) present similar results for the ex-post effect of a sanction. The exit rate to employment increased by more than 140 percent after a two-week reduction in benefits was imposed. Furthermore, they found that the effect persisted beyond the sanction period. A harder sanction did not result in stronger effects. They did not find different impacts across the population.

lem is that the studies could not distinguish the impact of sanctions from the impact of a higher level of job search assistance probably coming along with the imposition of a sanction. Sanctions might elevate exit rates out of UI benefit or social assistance receipt just by raising the job search intensity in giving more attention or more effective search tips to benefit recipients (Gorter and Kalb 1996, Dolton and O'Neill 1996). Ashenfelter et al. (2005) eliminate this obstacle with the help of a social experiment carried out in four states of the US that incorporated only the element of work search verification with sanctions without corresponding job search assistance. They found that work search verification had no significant impact on exit rates out of benefit receipt.¹²

To my knowledge, there is no study on the effects of benefit sanctions on the reservation wage. There are, however, numerous estimations of the elasticity of reservation wages with respect to the income of unemployed persons based on surveys. As from my theoretical considerations to be expected, the elasticity is found to be positive; its magnitude, however, is slight.¹³ As a consequence, the probability of accepting a job offer proves in the majority of cases to be between 0.9 and 1, suggesting that the reservation wage lies very close to the lower bound of the distribution of wages existing in the economy (see Cahuc and Zylberberg 2004: 157p.).

Previous empirical analyses of reservation wages in Germany are mostly based on the German Socio-Economic Panel (GSOEP) which contains self-reported monthly net reservation wages. For the GSOEP one has to keep in mind that it has relatively few cases in the low-income segment. The results suggest that personal characteristics of the unemployed are the most important determinants for reservation wages in the dataset (especially most recent wages, e.g. Prasad 2003, Christensen 2005, Falk et al. 2006).

Christensen does not find an impact of different kinds of unemployment benefits on reservation wages. Prasad finds a small positive impact of the levels of unemployment insurance benefits on reservation wages. Pannenberg (2007) finds that the

¹² Job seekers were randomly separated in three groups, one control group and two treatment groups. The control group was faced with the usual conditions of eligibility for UI benefit. The two treatment groups were at their first visit notified of additional compulsory job search requirements. At their second visit, the two treatment groups were treated differently. For one, job search requirements were monitored, while this was not done for those in the other treatment group. The job-seekers who could not prove that they contacted an employer received a sanction. Ashenfelter et al. (2005) found that the rates of exit from unemployment for the individuals in the two treatment groups were not statistically different.

¹³ Devine and Kiefer (1991) give examples of the scope of estimated elasticities: Lynch's (1983) average estimates lie between 0.08 and 0.11 for youths in the UK, Holzer's (1986) average estimates for youths in the U.S. are smaller with 0.02 to 0.05, like Van den Berg's (1990) estimates for reservation wage elasticity at the onset of a period of unemployment in relation to the future income of 30 to 55 year old unemployed in the Netherlands of 0.04 to 0.09. Other estimations, e.g. Feldstein and Poterba (1984) and Fische (1982), were markedly higher though (0.4 for the 70s in the US).

elasticity of reservation wages regarding unemployment benefits is significantly positive, but only for risk-loving job-seekers: for them, a ten percent cut in the benefit level leads to a four percent decrease in reservation wages.

Bender et al. (2007) are the first to analyze hourly reservation wages of unemployment benefit II recipients, based on the same dataset but another sample used here; they consider unemployment benefit II recipients who explicitly search a job at the time of the interview. They confirm previous findings of the great predictive power of personal characteristics and especially last net wages for reservation wages. They could not estimate the income elasticity of reservation wages; UB II recipients, however, who had received (presumably higher) UI benefit receipt still during 2005 had no significantly higher reservation wages when last net wages were taken into account.

If unemployment income elasticity of reservation wages is similarly small in my sample, sanctions might have no or very limited ex-post effects on reservation wages of risk-averse benefit recipients. From a theoretical perspective, the ex-ante effect of a sanction should be larger for risk-averse than for risk-loving benefit recipients. This means they reduce their reservation wages already before a possible sanction to their lowest possible level that they cannot underbid even in case they receive a sanction.

In sum, previous empirical research indicates that 1) benefit sanctions do raise exit rates out of benefit receipt, maybe ex-ante even more than ex-post, 2) perhaps more via increased job search intensity than reduced reservation wages, because 3) sanctions alone might not raise exit rates and 4) reservation wages seem to be already at the lower bound of the wage distribution. 5) Principally, German reservation wages appear to be influenced by one's labor market position, reflected mainly in last net wages, and only slightly by unemployment income; this elasticity might be influenced by one's degree of risk aversion.

For my methodological approach, I need to understand the selection into treatment and know all variables that influence both sanctions and reservation wages. Above (see Section Theoretical considerations) I derived the hypothesis that both the sanction probability and the reservation wage seem to be influenced by individual characteristics of the benefit recipient (motivation, ability, risk-aversion), search costs, by the frequency and quality of arriving job offers and by the attractiveness of staying unemployed (including the level of UI benefit, the benefit cut by a sanction and the probability to get sanctioned according to one's effort).

Detailed information on the individual's labor market position and household context should be strongly related to unobservable factors related to the individual's motivation, social skills and risk aversion: A more motivated person, for example, will engage more in education and qualification and will try harder to get and keep a good (paid) job, while it is likewise likely that responsibility for other persons increases

one's motivation and risk aversion.¹⁴ To my knowledge, Müller (2007) is the only study that explicitly models determinants of the sanction risk of unemployment insurance recipients in Germany. His results indicate that younger people with lower benefit levels (who might be less risk-averse) and with previous sanctions (proxying maybe less motivation to search or a higher reservation wage) receive more sanctions.

Sociological research on sanctions in US-American TANF ("Temporary Assistance for Needy Families") introduced the concept of employment barriers into the analysis of sanction mechanisms; in the search theoretical framework, these people would face higher search costs (and infinite search costs would impede search). Results indicate that sanctioned benefit recipients suffer from more structural problems on the labor market than non-sanctioned benefit recipients, and that their probability to receive benefits is higher (Meyers et al. 2006). Personal characteristics (analphabetism for example) or statistical discrimination (because of belonging to a certain group) might cause these structural problems of sanctioned benefit recipients on the labor market. As an undesired side effect, benefit sanctions could further deteriorate these lower chances while partly or totally failing to induce the desired behavioral changes.

Taking the duration of TANF receipt into account, lower qualification, no or little work experience, more or younger children, being Afro-American and the duration of benefit receipt prove to robustly increase the probability to receive a TANF sanction.¹⁵ Further structural problems (like lack of physical or mental health, disabilities, care for children or relatives, domestic violence, transportation or technical communication problems or addictions) were not always of significant impact.

Studies on the process of sanctioning in both the US-American TANF as well as in German unemployment insurance found out that beside the UI recipients characteristics the individuals who implement the sanctions and the environment they take their decisions in influence the individual risk of being sanctioned (see Meyers et al. 2006, Müller and Oschmiansky 2006, Müller 2007). First, the regional regular and irregular labor market seem to play a role: the more jobs or ALMP measures the benefit recipient is offered, the more he can possibly reject or quit, thus not complying with eligibility criteria and provoking a sanction.

¹⁴ Among the few studies trying to measure the role of risk attitudes in labor market choices, DeLeire and Levy (2004) suggest family structure as a proxy for risk preferences. They show that primary caregivers tend to work in occupations with lower risk of death.

¹⁵ The majority of the studies described in Meyers et al. (2006) do not consider hazard rates (i.e. sanction rates in specific duration intervals of benefit receipt) but the overall sanction probability of a person receiving TANF. Taking the duration of benefit receipt into account is important because lower chances on the labor market will most probably lead to longer benefit receipt, and longer benefit receipt leads to a higher sanction probability (see also the Section Methodology).

Second, employment offices seem to have milder or stricter sanction ‘philosophies’ leading to different sanction rates. Third, individual case manager’s values, attitudes towards sanctioning, and workloads seem to affect the sanction probability. Front-line workers might impose a sanction only when they feel that “it’s going to pay off”. Thus, benefit recipients with lower labor market chances may be spared: disabled and older persons, parents and especially single parents often face less job search requirements – formally (by law) and informally (by case manager).

In sum, empirical research confirmed theoretical expectations that individual characteristics like motivation, ability and risk-aversion, high search costs (employment barriers) and the frequency and quality of arriving job offers (also determined by local labor market situations) influence the sanction probability. Moreover, previous empirical research stressed the importance of the behavior of the case manager who potentially imposes a sanction.

This paper aims to contribute to the ongoing empirical research on the effects of benefit sanctions by exploring the hypothesis of job search theory that benefit sanctions directly reduce reservation wages. Contrary to most empirical research on sanctions, it regards unemployment benefit II sanctions, internationally more comparable to social assistance sanctions than to UI sanctions.

4 Data

General description of the survey

The data used for this analysis stems from a cross-section survey called “Life situation and social security 2005” that was conducted on behalf of the Institute for Employment Research (IAB) in Winter 2005/2006. The purpose of the survey was to obtain information about the effects of the implementation of the new SC II on persons concerned. Hence, the target population of the survey consists of two groups: first, of course, the unemployment benefit II recipients in January 2005 (with their need communities), and second, registered unemployed receiving unemployment assistance in December 2004 who did not receive unemployment benefit II in January 2005 (“drop-outs”).

The sample population differed from the target population, because at the time when the IAB drew the sample population from the administrative data of the Federal Employment Agency, there was information only on 4.1 of the approximately 6.3 million people affected by the reform (see infas 2006). For 173 of all 439 German districts, there was only information on all former UA recipients in December 2004, independently of their SC II receipt in January 2005, and no full information about those unemployment benefit II recipients who received social assistance or no benefits in December 2004. Since the gross sample was randomly selected from the sample population, the UB II recipients formerly receiving unemployment insurance benefits

should be overrepresented in the gross sample relative to those formerly receiving social assistance or no benefits.¹⁶

Of the gross sample, 27 percent could not be interviewed.¹⁷ Of all people remaining in the gross sample, the interview was successfully realized with 20,832 people (39 percent). The others did refuse to answer (46 percent) or were not accessible (15 percent).¹⁸ A sensitivity analysis regarding selectivity due to design and conductivity of the survey found that better qualified persons, native Germans, East Germans, women and people living in multi-person households participated more often.¹⁹ When interpreting the results of my analysis, one should bear in mind that in the realized sample, there is a slight overrepresentation of persons with relatively high social integration and chances of employment (infas 2006).

The survey is the only dataset available for analyzing effects of UB II sanctions in 2005, directly after the new SC II came into force. Due to the mentioned data problems of the Federal Employment Agency, there is no administrative data on sanctions for the year 2005. An advantage of the dataset in comparison to administrative data from later periods is the extensive information it contains – both about the implementation of UB II during the year 2005 (i.e. detailed information on benefit receipt, exits out of benefit, requirements, monitoring and sanctions) and the private

¹⁶ The samples were drawn from data from the ARGE-software "A2LL" and from data from the software in the employment offices (responsible for UA recipients). Both datasets are linked by a unique identity key for each person. For 266 districts, the full target population is available. For the 69 opting municipalities no data on UB II recipients in January 2005 was available at all when the samples were drawn in June 2005; for further 104 districts only the population of UA recipients in December 2004 is available independently of their UB II receipt in January 2005 due to organizational and software problems.

¹⁷ Mostly their contact information was incorrect (telephone number not available, address wrong: 22 percent). The rest did not understand the interviewer because they spoke another language than German, Russian, Turkish or Croatian (4 percent). A few were ill or deceased.

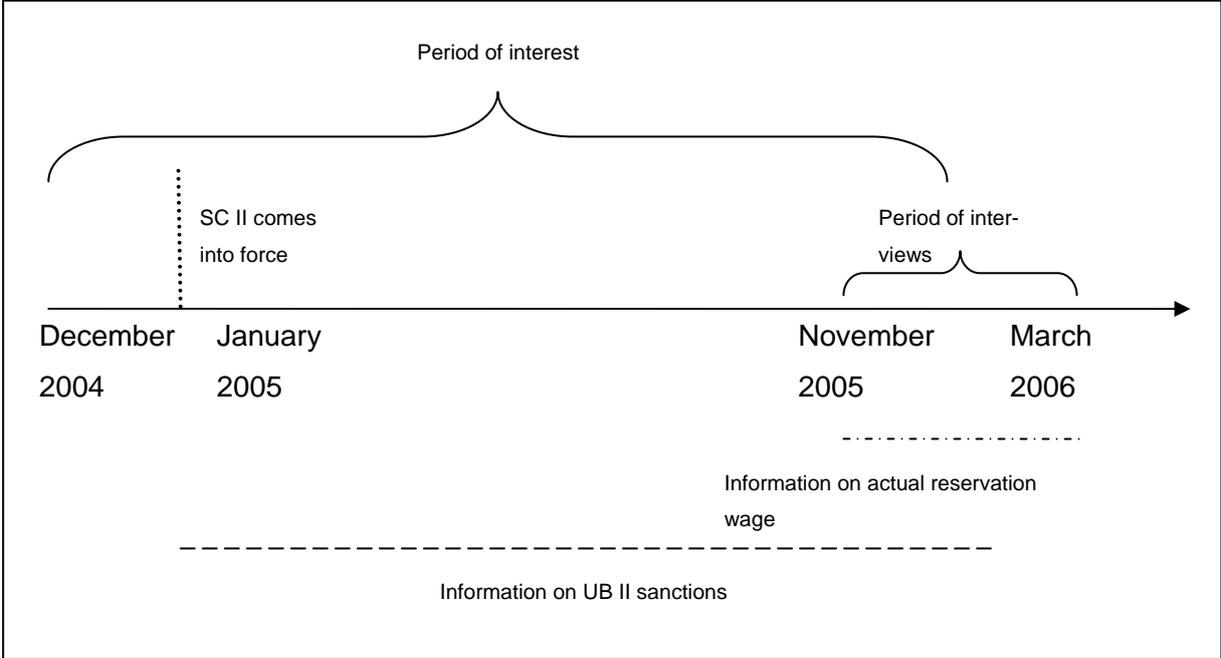
¹⁸ The interviewers explained the comparably low response rate with difficult external conditions and a demanding questionnaire. The external conditions were difficult for the interviewers because the potential respondents were afraid of being controlled through the interviewer, did not trust affirmations of anonymity or were disappointed and filled with bitterness and hostility towards the Federal Employment Agency and the media. During that time, problems with control and moral hazard of UB II recipients were widely and drastically discussed in the media ("parasitic behavior"). In this context, the aim of the survey was difficult to communicate. Furthermore, the interview was very long (on average one hour and 16 minutes), it asked sensitive questions about the personal situation of the respondent, and some questions appeared to be difficult to understand.

¹⁹ In the gross sample, there were 15.2 percent without a school degree, 19.5 percent left school with 15 years with a "Hauptschulabschluss" (secondary general school certificate), 35.2 percent were vocationally trained and 5.3 percent had higher qualification than that. In the final sample 11.2 percent had no degree, 16.4 percent a Hauptschulabschluss, 41.9 percent a vocational training and 7.5 percent a higher qualification. Similarly overrepresented are native Germans (63.6 percent in the gross sample vs. 68.2 percent), persons living in East Germany (39.8 percent vs. 43.6 percent), women (47.7 percent vs. 51.9 percent), and, but only slightly, young respondents under 18 years and respondents living in smaller cities up to 5,000 inhabitants. Singles are underrepresented (39.7 percent vs. 37.3 percent). Older people were easier accessible, but refused to answer more often at the same time, so there is no selectivity bias here.

and professional background of the interviewed persons, including their reservation wages. Individual employment histories are available via spell data the person reported on all kinds of labor market states from the end of school until the date of the interview, on a monthly basis (spells of employment, non-employment, unemployment, active labor market schemes and qualification).

Figure 1 displays how the information for my analysis was collected. The survey's period of interest is the period between December 2004, just before SC II came into force, and the interview date between end of November 2005 and end of March 2006. Interviewers asked for the reservation wage at the actual time of the interview. They did not ask for the reservation wage in December 2004, taking into account that it is highly unlikely that people correctly remember their reservation wages from one year before. Information on sanctions is asked for each month of UB II benefit receipt from January 2005 until the time of the interview.

Figure 1
The survey's period of interest and collection of information on reservation wage and sanctions on the time line



Source: IAB-Survey "Life situation and social security 2005". Own illustration.

Interviewers asked in two steps several questions to find out the net hourly reservation wage, of both employed and unemployed people. The first set of questions asks for the net hourly wage people expect to receive in their next job; the second set of questions asks for a reservation wage that might lie below the expected wage, rep-

representing the lowest limit of their wage demands.²⁰ I assume that this stepwise collection of hourly reservation wages transmits the idea of the reservation wage better to the respondents than a one-step collection of monthly information as typically done in similar surveys (e.g. the GSOEP); therefore I expect less reliability problems otherwise common to measures of reservation wages. An additional advantage of the reservation wage information is that both employed and unemployed persons were asked for their reservation wage; hence, the variable of interest is measured in an identical way for both groups and not proxied by the accepted net wages of the employed persons.

Sample selection

To estimate the effect of a first sanction after entering UB II on the hourly reservation wage, I constructed the sample of analysis as follows (see Table 2 for the specification of selection's magnitudes): 1.) To focus on the target group of my research question, I take only UB II benefit recipients in the sample who entered UB II benefit receipt between January and March 2005 (to allow for a common time lag between the UB II application and its approval); "drop-outs" are excluded. 2.) I exclude persons who were older than 57 years or retired in December 2004 because, as mentioned, they face less or no risk respectively to receive a sanction. 3.) I also exclude people with missing information on hourly reservation wages and sanctions (including the timing of a sanction).

4.) Considering my target group, I assume reservation wages to be plausible if they lie between one Euro and 20 Euro per hour: the equivalent an hourly labor income of 20 Euro per hour is 3,200 Euro (net) labor income per month if one would work full-time. This assumption excludes less than one percent of the observations (32 observations with reported values of less than one Euro and 64 observations with values of more than 20 Euro). 5.) For the estimation of the treatment effect, I consider only the first UB II spell of each sample member. I assume that a person receiving UB II for the first time, e.g. for three months without interruption, is treated differently than a person receiving UB II a second or third time, e.g. for three months for the second time in a year. This assumption excludes 23 people (0.2 percent) who were sanctioned in a later UB II spell.

²⁰ 1.) „What net wage do you expect to earn per month?“ („Was erwarten Sie, monatlich netto zu verdienen?“), accompanied by a question asking how many hours per week the person would expect to work for the reported amount. 2.a) Persons who did answer question one are asked in a second step if they would be willing to work for a monthly net wage lower than the first reported value („Wären Sie auch bereit für einen Netto-Monatslohn zu arbeiten, der geringer ist als der angegebene Lohn?“). If so, they are asked for this reservation wage per month they would be willing to work for („Wie hoch müsste dieser geringere Netto-Monatslohn dann mindestens sein, damit Sie noch bereit wären, dafür zu arbeiten?“). Again, persons are asked for the working hours per week they expect to work for this reported net reservation wage. 2.b) Persons who refuse to answer question one are in a second step asked: „What is the least net wage per month you would be willing to work for?“ („Wie hoch müsste Ihr Netto-Monatslohn mindestens sein, damit Sie noch bereit wären, dafür zu arbeiten?“), together with the working hours they would expect to work.

To construct the key variables net reservation wages and sanctions (including the timing), I need full and plausible information of each final sample member on desired working hours, desired amount of wage per month and (for each month) if a sanction was imposed; hence, the sample gets smaller by dropping persons with missings on any of this information. Nonetheless, the selections do not seem to change the distributions of sanctions and reservation wages in the sample in an unintended way (see, again, Table 2). The median reservation wage of 5.77 Euro/hour does not change at all through the selections while the mean reservation wage is reduced from 6.22 Euro/hour to 6.06 Euro/hour through the exclusion of outliers. The share of sanctioned is reduced from 6.0 percent to 5.5 percent through the exclusion of people getting a sanction in a later UB II spell, and of people with relevant variables missing.

Table 2
Changes in reservation wage and in shares of sanctioned sample members due to sample construction

	Sample size	Share of the target sample	Share of sanctioned persons in sample	Median reservation wage euro/hour	Mean reservation wage euro/hour
Total sample	20,839	-	5.9%	5.77	6.17
1.) Target sample: entering unemployment benefit ii receipt in Jan., Feb. or Mar. 2005	15,232	100.0%	6.0%	5.77	6.22
2.) Younger than 58 and not retired in December 2004	15,036	98.7%	6.0%	5.77	6.22
3.) Information on both net reservation wages and sanctions (incl. timing)	12,951	85.0%	5.7%	5.77	6.22
4.) Reservation wage between one and 20 €/h	12,855	84.4%	5.7%	5.77	6.06
5.) Sanctions only in first spell	12,822	84.2%	5.5%	5.77	6.06

Notes: The median reservation wage of 5.77 Euro per hour is due to typical combinations of desired net wages and working hours, like desired 1,000 Euro per month for 40 hours per week (with one month counting as 4.33 weeks).

Source: IAB-Survey "Life Situation and Social Security 2005". Own calculations.

The following subsection describes additional details on sample members' reservation wages, further characteristics, and the benefit sanctions they received.

Description of the sample

In the final sample remain 12,822 people (84 percent of the target sample). The sample is almost equally divided by gender and region (49 percent were women, 55 percent living in West Germany, see also Table A 1 in the Appendix for details). At the time of the interview, 76 percent of the sample still received UB II.

Table 3
Main labor market state (in percent of all sample members)

	December 2004	Winter 2005/06 (interview)	Change (in percentage points)
Registered unemployed and/or in ALMP measures and training (without One-Euro-Jobs)	76.4	66.1	-10.2
One-euro-jobs (incl. One-euro-job and registered unemployed)	3.2	9.2	6.0
Employed/self-employed	9.1	13.9	4.8
Vocational training or school	0.9	0.6	-0.4
Nonemployment	10.4	10.2	-0.2

Source: IAB-Survey "Life Situation and Social Security 2005". Own calculations.

In Table 3, the main labor market state of the sample members is shown.²¹ At the time of their interview, three quarters of the sample members reported being registered unemployed and/or in active labor market measures (incl. One-Euro-jobs), or in school or vocational training. Five percent have never been employed in their life. The last employment of unemployed respondents who have ever been employed ended four years ago on average. 14 percent of the sample reported being mainly employed or self-employed, i.e. without at the same time being registered unemployed or in an ALMP measure.²² Ten percent reported doing something else, like parental leave, household or care responsibilities, holidays, sickness, etc. Five percent have never been employed in their life. Compared to December 2004, ten percentage points less sample members were registered unemployed and/or in an ALMP measure. Five percentage points more sample members were employed at the time of their interview than one year before, and six percentage points more attended One-Euro-Jobs.

In Table 4, the type of employment is characterized for the 14 percent of the sample who report being mainly employed or self-employed at the time of the interview. Most are regularly employed (this share significantly increased during the observation period); the second largest share is occasional or minor employed. The average net wage of the last employment (for employed the current employment) was 6.02 Euro/hour. 86 percent of the regular or minor employed people were employed in a job subject to social insurance contributions. 36 percent of the employed still received UB II.

²¹ Most respondents reported parallel labor market spells, e.g. at the same time "doing something else", "being in training" and "being employed". Since it is assumed that registered unemployed people and people attending ALMP measures of short and medium duration have to fulfill search requirements and in case of non-compliance can be sanctioned, the labor market status of one person is categorized according to the following priorities: 1. unemployment including ALMP measures without job-creating measures (ABM), 2. (self-)employment, 3. school or vocational training, and 4. nonemployment. Only the first priority state is listed.

²² Of all sample members, not 14 percent but 24 percent reported being employed at the time of the interview; but of these 24 percent, 42 percent reported being unemployed and/or in an integration measure at the same time, so they were categorized as being unemployed or in ALMP measures.

Table 4
Mean characteristics of employment

	December 2004	Winter 2005/06
Type of employment		
Job-creating measure (ABM)	7.33	6.18
Employment in Personal Service Agency	1.52	1.98
Job try-out/internship	3.88	1.76
Occasional or minor employment	35.72	24.49
Subsidized self-employment	1.43	2.15
Regular employment/self-employment	50.13	63.44
Net wage in euro/hour	6.21	6.02
Employed subject to social insurance contributions (in percent of all minor or regular employed) ^a	77.68	85.64

Notes: The type of employment is characterized for those people who report being mainly employed or self-employed and not being registered unemployed or in an ALMP measure at the same time. a. Question was only asked for minor or regular employment.

Source: IAB-Survey "Life Situation and Social Security 2005". Own calculations.

The level of qualification was not particularly high: 28 percent were low qualified, 60 percent have an intermediate qualification, five percent were highly qualified and seven percent very highly qualified.²³ 68 percent had German nationality, parents and interview language; a complete non-German background characterized ten percent of the sample and 22 percent reported mixed features (other nationality, parents or interview language). 22 percent lived together with other earners in the household. More than half of the sample members (56 percent) lived without a partner in the household (24 percent of them as single parents with children). The other 44 percent lived as couple, mostly with an un- or nonemployed partner, less often with an employed partner (of these, 38 percent with an un- or nonemployed partner with children and 30 percent without children, 19 percent with an employed partner with children and 14 percent without children). This description evokes the picture that sample members' chances of finding a job are below average.

On average, people expected to earn net 6.98 Euro per hour, but 78 percent stated to work for a reservation wage lower than that. The average net reservation wage is 6.06 Euro/hour (5.77 Euro/hour in the median), as already shown in Table 2.²⁴ In East Germany, the mean reservation wage was lower (5.41 Euro) than in West

²³ Low qualified means no graduation or graduation from Sonder-/Haupt- and Realschule and no vocational training, middle qualified means (Fach-)Abitur and no vocational training, or graduation Sonder-/Haupt- and Realschule and apprenticeship, highly qualified means (Fach-)Abitur and apprenticeship or master craftsmen and very highly qualified means university degree.

²⁴ Note that I analyze hourly reservation wages and not the reservation wage ratio (rwr), the ratio between reservation wage and last net wage. Information on last net wages (i.e. wages resulting from an employment that started before our observation period 2005), and thus, on rwr, are only available for jobs lasting at least until January 2004; but three quarters of the sample members were already unemployed at this time. Thus, missings in last net wages are not a reason for exclusion. Due to our target group, reported net wages under one Euro per factual working hour (in 91 cases) and over 50 Euro (in two cases) are assumed to be implausible and put to missings.

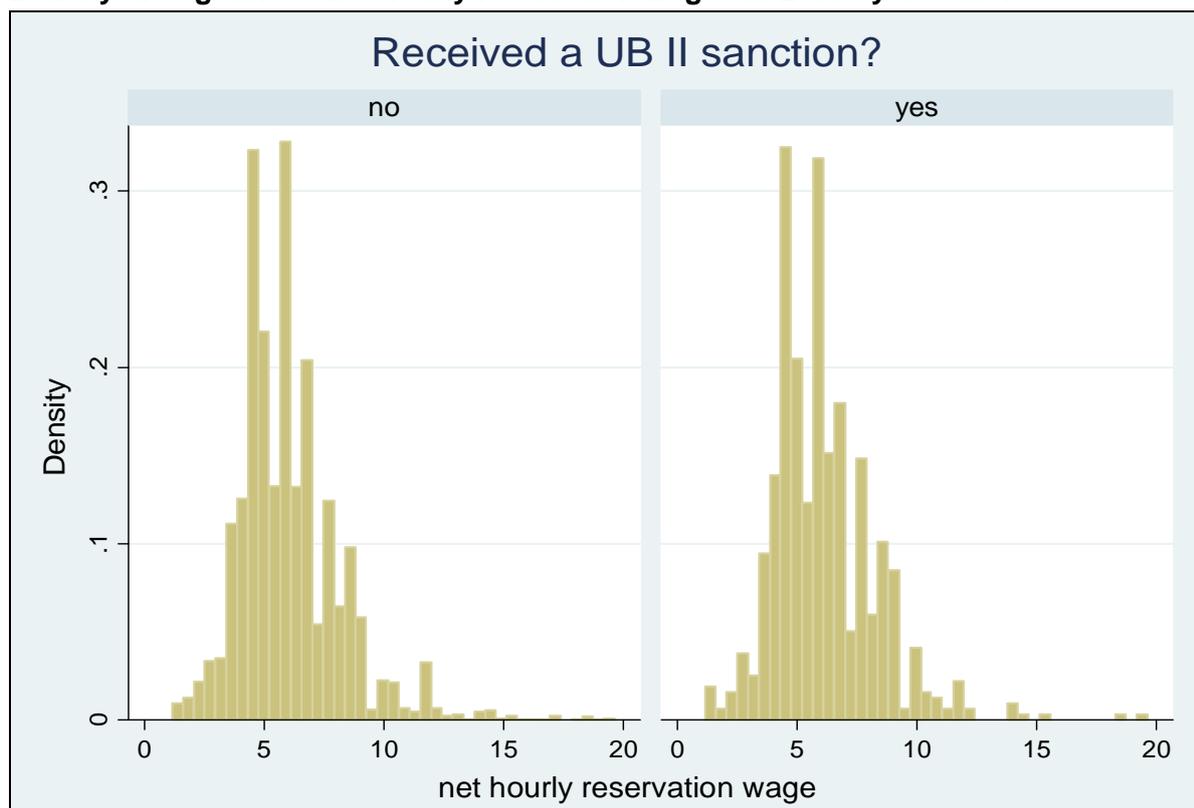
Germany (6.57 Euro). In 2005, net wages at the bottom tenth percentile of the wage distribution reported in the GSOEP were 6.05 Euro/hour in East Germany and 7.97 Euro/hour in West Germany (own calculations based on Gernandt and Pfeiffer 2006). Based on administrative data of the Federal Employment Agency on all workers employed subject to social insurance contributions, Rhein and Stamm (2006) calculated for 2004 the upper threshold for low-wage jobs (defined as two-thirds of the median gross wage). It was 10.20 Euro/hour in West Germany and 7.40 Euro in East Germany.²⁵ Thus, reservation wages of the sample members are positioned at the very bottom of the German wage distribution, within the low-wage sector.

This is important for two reasons. First, low reservation wages probably reflect the sample members' realistic perception of own modest labor market prospects. Second, and very relevant for my analysis, they imply that the sample members would accept almost every wage offer since most paid net wages lie above their reservation wages.

Reservation wages vary not only between sample members living in East and West Germany, but also with other variables suggested in the literature reviewed above. Relatively high reservation wages are reported by men, by older people, by people with financial responsibilities for family members (married, having children in the household, no other earners in household), by better qualified people with higher last net wages, by former UI recipients but also by long-term unemployed with missing net wages (see the results of OLS regressions of the log net hourly reservation wage in the Appendix Table A 3). Reservation wages do not significantly vary with received sanctions on average: the 702 sanctioned sample members reported a mean reservation wage of 6.11 Euro/hour; the 12,120 persons without a sanction reported a wage of 6.05 Euro/hour. The values are not statistically different at a significance level of 0.1 percent. A simple eyeball test of the histograms of the reservation wages by treatment status supports the resemblance of their distributions (see Figure 2).

²⁵ In 2004, 18.4 percent of all full-time workers received a gross wage below this threshold (Rhein and Stamm 2006).

Figure 2
Density histograms of net hourly reservation wages in Euro by treatment status



Source: IAB-Survey "Life situation and social security 2005". Own illustration.

The share of sanctioned UB II recipients varies with certain characteristics of sample members. In Table 5, the variations in the average share of 5.5 percent sanctioned UB II recipients are described within the categories region, age, partner, other earner, under-age children in household, qualification and migration status. In Table A 1 in the Appendix, you find additional information on mean differences between sanctioned and non-sanctioned sample members for numerous other characteristics. Younger and less qualified persons received more sanctions. Parents, persons with other earners in the household and West Germans in districts with better labor market conditions received more sanctions (significant at the one percent level). Sanctioned sample members lived in districts where people were unemployed for shorter periods and received more job offers.²⁶

²⁶ Unemployment lasted on average 15.5 months in districts of sanctioned sample members instead of 15.9 months; the unemployment rate was 14.5 percent instead of 15.6 percent; and five instead of four jobs were offered to 100 registered unemployed.

Table 5
Share of sanctioned UB II recipients in different groups 2005

	Percentage of persons with sanction
Region	
West Germany	6.2
East Germany	4.6
Age	
15 to 25 years old	10.4
25 to 40 years old	5.9
40 to 50 years old	4.6
50 to 58 years old	3.0
Partner in household	
No partner	5.7
Employed partner	5.8
Non-/unemployed partner	4.9
Other earners in household	
No	5.2
Yes	6.6
Children under 18 years in household	
No	5.2
Yes	5.9
Very high	3.4
Qualification	
Low	6.9
Middle	5.1
High	5.4
Migration status	
Non-German	6.3
Ambiguous	5.6
German	5.3
Total	5.5

Notes: Sanctions received in the first UB II spell are described.

Source: IAB-Survey "Life Situation and Social Security 2005".

As already mentioned, 5.5 percent of the sample received a sanction. Three quarters of the sanctioned sample members received one sanction during the observation period, and one quarter received two or more. A sanctioned UB II recipient received UB II for on average seven months before the sanction. Of the sanctioned UB II recipients, 75 percent never left UB II receipt after the sanction; those who did leave UB II receipt on average four months after the sanction was imposed (spread with a standard deviation of 2.8 months). The rate of UB II recipients receiving a sanction per calendar month increased during 2005 from less than one percent in Spring 2005 to 2.5 percent in Winter 2005 (see Table 6). Hence, sanctions were imposed with increased intensity after the implementation of the SC II. The total number of UB II recipients in the sample logically decreased over time because people stopped to receive UB II during the observation period. The sharp drop in observations from December 2005 on, however, results from the different interview dates, the earliest ones held end of November 2005, as mentioned.

Table 6
Numbers of UB II recipients and mean sanction rates per calendar month
in the observation period

Year	Month	Mean sanction rate	Number of UB II recipients
2005	January	0.5%	12510
	February	0.6%	12576
	March	0.8%	12535
	April	1.0%	12258
	May	1.2%	12019
	June	1.5%	11801
	July	1.7%	11518
	August	1.9%	11334
	September	1.9%	11191
	October	2.4%	11012
	November	2.6%	10841
	December	2.3%	7813
2006	January	1.6%	4077
	February	1.6%	2760
	March	1.3%	780

Notes: Sanctions received in the first UB II spell are described. The mean sanction rate is calculated as the share of sanctioned UB II recipients of the total number of people really receiving UB II in the respective calendar month. The difference between the whole sample size of 12,822 people and the number of people really receiving UB II in the listed calendar months results from the fact that those sample members were selected for the final sample who received UB II the first time in January, February or March 2005; but since some sample members exit UB II receipt already after one or two months, the number of UB I recipients in February and March 2005 is reduced by these people. In later months, the number of UB II recipients continuously decreases because of sample members leaving UB II receipt.

Source: IAB-Survey "Life Situation and Social Security 2005".

As mentioned above in Section two, there are various non-compliances of the UB II recipient that can lead to a sanction. The answers on the question for the justifications for the sanctions are shown in Table 7. Two justifications for a sanction given by law – 1) parallel UI benefit sanction and 2) intentional reduction of earnings and assets in order to be entitled to UB II benefits – are not given as possible justifications in the questionnaire, however.²⁷ The largest part (36 percent) of the sanctioned who are older than 24 years answered that they received a sanction because of refusing or quitting jobs or integration measures, refusing to sign integration contracts or not meeting duties of integration contracts, because of misinformation on income or assets or spending money in an uneconomical way ("justification one"). 46 percent answered they were given "other reasons" for their sanction. This high percentage certainly entails the people who were told the two justifications missing

²⁷ In the interview, sanctioned UB II recipients could choose from the following possibilities the interviewer offered them as justifications received for the sanction: a) refused to sign an integration contract, b) for people with integration contracts: refused to meet duties of integration contract, c) refused or quit offered work, vocational training or work measure ("Arbeitsgelegenheit"), d) refused or quit integration measure ("Maßnahme"), e) did not report to the Job Center ("Meldeaufforderung nicht nachgekommen"), f) did not meet medical or psychological appointment, g) did not correctly report income and/or assets, h) accused of spending money in an uneconomical way, or i) other reasons given (alas, without a further specification). If all questions are answered with "no" or are missing, the sanctioned UB II recipient was asked if j) there were no reasons given.

in the questionnaire; nonetheless, almost all legally possible justifications are given in the questionnaire, hence, the share of sanctioned people who report other justifications than given in law remains surprisingly high. The picture of justifications for the sanctioned respondents who are younger than 25 years is a bit straighter: The majority (54 percent) received justification one.

Table 7
Justification for the sanctions imposed (in percent of all sanctioned)

Justification for the sanction given in the questionnaire	25 to 57 years	15 to 24 years
1: Refused to sign an integration contract, for people with integration contracts: refused to meet duties of integration contract, refused or quit offered work, vocational training or work measure ("Arbeitsgelegenheit"), refused or quit integration measure ("Maßnahme"), did not correctly report income and/or assets, accused of spending money in an uneconomical way	36	54
2: Did not report to the Job Center ("Meldeaufforderung nicht nachgekommen"), did not meet medical or psychological appointment	15	29
3: Other reasons given	46	17
4: No reasons given	3	1

Notes: Described are sanctions for the first UB II spell. Values in grey indicate case numbers under 20.

Source: IAB-Survey "Life Situation and Social Security 2005".

Only 20 percent of the sanctioned sample members fully accepted their sanction as justified. Depending on the type of justification for the sanction, a sanction lasted on average three months for justification one (as regulated by law). However, it lasted shorter (on average two months) for justification two or for "other reasons". Two percent of the 25-year-and-older sanctioned benefit recipients, and five percent of the under-25-year-old ones, received in-kind transfers during the sanction.

In sum, in 2005, the sample members' chances of finding a job are below average, mirrored in their reservation wages positioned at the very bottom within the low-wage sector of the German wage distribution. In the survey, the process of sanctioning was probably not adequately asked and understood by the sanctioned sample members. Alternatively, sanctions were imposed less systematically as intended by the regulations in the SC II. The increasing sanction rates until the observation period indicate, however, that sanctioning became more systematically. Though each sanction is supposed to last three months, the duration of sanctions varied with justifications. Even if imprecise questionnaires are accounted for, the communication and understanding of the justifications for sanctions seem to be improvable, in particular considering the share of sanctions imposed because of "other reasons" apart from the justifications one and two given in SC II.

5 Methodology

This paper wants to estimate the effect of benefit sanctions on the reservation wages of sanctioned unemployment benefit II recipients. The focus here is on the first sanction people may receive within their first spell of UB II in 2005, since the

used data stem from a survey of people who entered UB II for their first time in 2005 and are observed for the limited period of a year. In an ideal scenario, I would have the same unemployment benefit II recipient in two parallel worlds. In world one, she receives a sanction, in world two, she does not - everything else equal. The effect of a sanction on her reservation wage would be the difference in her reservation wage of world one and world two.

In the real world, I observe either the reservation wage when receiving a sanction for each sanctioned individual or the reservation wage when not receiving a sanction for each individual who did not receive a sanction. Taking the mean reservation wage of non-sanctioned individuals as an approximation for the potential mean reservation wage of sanctioned individuals – i.e. their reservation wage for the hypothetical situation that they were not sanctioned – would cause selection bias, since sanctioned and non-sanctioned individuals usually differ even in the absence of treatment (sanctioned individuals should at least partly show a different behavior, otherwise they should not have received a sanction).²⁸ The matching approach is one possible solution to deal with this selection problem.²⁹ Its basic idea is to find non-sanctioned individuals who are similar to the sanctioned individuals in all relevant characteristics before the sanction was imposed. If so, differences in reservation wages between this adequate control group and sanctioned individuals can be attributed to the imposed sanction.

The causal effect of imposed sanctions can be identified with statistical matching only if four assumptions hold: 1.) The data at hand include all relevant variables that affect both treatment assignment (receiving a sanction) and outcome (reservation wage). In formal terms, I must rely on a “Conditional Independence Assumption” (CIA, Lechner 1999).³⁰ Moreover, I rely on the assumptions that 2.), apart from the mentioned ex-ante effect of a sanction, there are no general equilibrium effects: the fact that sample member one is sanctioned does not change the reservation wage of sample member two (SUTVA or consistency condition); that 3.), the relevant conditioning variables should be exogenous in terms of not being influenced by the sanction in a way that is related to the reservation wages ; and that 4.), for a given

²⁸ There are many microeconomic evaluation studies of active labor market policies in Europe dealing with selection bias caused by 1) caseworkers selecting specific types of unemployed into specific programs and 2) specific unemployed self-selecting into specific programs (see e.g. the survey of Heckman et al. 1999).

²⁹ The standard framework in evaluation analysis to formalize this problem is the “Roy-Rubin-model” (Roy 1951, Rubin 1974). The matching approach was originally developed in the statistical literature; see e.g. the benchmark paper of Rosenbaum and Rubin (1983). It is widely applied when evaluating labor market policies (see e.g. Dehejia and Wahba 1999 or Heckman et al. 1997). Caliendo and Kopeinig (2008) offer a comprehensive and practical guideline for the special case of propensity score matching (see below).

³⁰ This assumption is alternatively referred to as “unconfoundedness” (Rosenbaum and Rubin 1983) or “selection on observables” (Heckman and Robb 1985).

value of the relevant conditioning variables, both sanctioned and non-sanctioned sample members could potentially be observed (“common support condition”).³¹

The first assumption, the CIA, is the strongest, and I will argue below why I assume that it holds. The second assumption of no general equilibrium effects of imposed sanctions is plausible since the observed shares of sanctioned of below three percent per calendar month are so small that the actual imposition of these sanctions does not plausibly change reservation wages of non-sanctioned sample members. The third assumption of exogeneity of relevant control variables implies that I use as controls only variables determined prior to the sanction. The fourth assumption implies that statistical matching is performed only on common support.

I assume that the CIA holds because of my unusually informative data and the rather unsystematic sanctioning process in the first year of the new SC II. That is, I assume that, for my observation period and conditional on the rich set of variables in my dataset, the reservation wages associated with receiving a sanction or not receiving a sanction are independent of the real sanction imposed by the caseworker. Though very strong the CIA is plausible for two reasons.

First, the data are very rich and include a great number of variables that are theoretically important for the sanctioning process and reservation wages. This involves demographic variables (age, gender, detailed household context with age and number of own children, existence and employment status of partner, marital state, nationality and nationality of parents, language skills) and variables related to pre-observation period skill levels, employment history and benefit receipt (education and vocational training, ever employed, duration of employment, unemployment, non-employment, school education and vocational training in the past, last earnings, receipt of unemployment insurance, unemployment assistance and social assistance); besides, variables related to details of the local labor market (unemployment rate and duration, share of migrants, women, younger persons and long-term unemployed of all unemployed, share of service jobs, ratio of offered jobs to all unemployed UB II recipients and ratio of One-Euro-Jobs to all UB II recipients).

The detailed information on the individual’s labor market position and household context should be strongly related to important unobservable factors like the individual’s motivation, social skills and risk aversion; all factors that, as I saw, would influence both reservation wage and the risk of receiving a sanction. Therefore, I take them as good proxies for these unobservable characteristics.

Second, in my observation period the new sanction regime of SC II was just being implemented. For this period, the local Job Centers were heavily criticized for uns-

³¹ See Lechner (2007) for a detailed description of the identifying assumptions.

systematically sanctioning UB II recipients.³² Remember that I consider a period where a completely new benefit system was set up and the institutions that deal with it, too. There were major tasks to fulfill like a new means test for all potentially needy households, moreover for all able-to-work members of needy households, a profiling had to be carried out and an integration contract needed to be fixed. That in such a context many people who could have been sanctioned were actually not, is likely. Probably, monitoring was quite unsystematic and even if the reasons for a sanction were properly observed, there was just not sufficient qualified personnel in the Job Centers to deal with all cases. This practice of rather unsystematic sanctioning is already indicated by the unexpected variance in duration and degree of sanctions in my data that I mentioned while describing the data.

But apart from problems due to the implementation period of new SC II, the assumption of a rather unsystematic performance of case managers is echoed in Lechner and Smith (2007). On the basis of Swiss data on the assignment of UI benefit recipients to eight active labor market programs in 1998 they conclude that integration efforts of Swiss employment officers achieve about the same employment rates one year after program initiation as would result from assigning the unemployed randomly to the available treatments in their existing proportions (and that they achieve clearly worse employment rates than assignment according to statistical treatment rules based on observables does).

In formal terms, we face the following situation. Let Z be the vector of all observable and not observable characteristics which influence both whether an UB II recipient receives a sanction ($D=1$, otherwise $D=0$) and the level of her reservation wage r . My task is to estimate the average impact from a sanction for persons characterized by observable characteristics X that are a subset of Z , by simply comparing the reservation wage r_1 of a sanctioned person and r_2 of a not sanctioned person characterized by X . Thus, it is necessary (1) that the sanctioning of sanctioned persons and their behavioral responses are captured by the observable characteristics X , and thus the average effect of receiving a sanction is the same for a sanctioned person and a person who was not sanctioned (CIA).

That is the same as postulating that $r_0, r_1 \perp D | X$, i.e. both potential reservation wages are independent of assignment to treatment D given the observable characteristics X . Similar to randomization in a social experiment, statistical matching balances the distributions of all relevant, pre-treatment characteristics X in the treatment and comparison group. Thus it achieves independence between potential outcomes and the assignment to treatment. In formal terms, $E(r_0 | X, D = 1) = E(r_0 | X, D$

³² The German Federal Court of Audit ("Bundesrechnungshof") inspected in 2006 the Federal Employment Agency, 70 ARGes and 20 opting municipalities (Bundesrechnungshof 2006: 13, own translation): "In six out of ten examined cases, the Job Center did not follow up hints for facts that would lead to a sanction. In most cases, the benefit recipients did not register with the Job Center though they were legally informed of legal consequences or did refuse to accept an appropriate job without proving an important reason."

$= 0) = E(r_0 | X)$ and $E(r_1 | X, D = 1) = E(r_1 | X, D = 0) = E(r_1 | X)$. The missing counterfactual mean reservation wages can be constructed from the outcomes of participants and non-participants: I construct $E(r_0 | X, D = 1)$ from $E(r_0 | X, D = 0)$.³³ The unbiased estimator of the average impact of a sanction on the sanctioned (ATT) can therefore be written $ATT = E(r_1 - r_0 | X, D = 1)$.

Since conditioning on all relevant covariates is limited in case of a high dimensional vector X ('curse of dimensionality'), Rosenbaum and Rubin (1983) suggest the use of balancing scores $b(X)$ that are specific functions of the relevant observed covariates X such that the conditional distribution of X given $b(X)$ is independent of assignment into treatment.

Rosenbaum and Rubin (1983) found that if the treatment is independent of the treatment assignment conditional on X , then it is also independent conditional on balancing scores $b(X)$ that fulfil the so-called "balancing score property". If the balancing score property holds, then after matching on the balancing score, there will be no statistically significant differences in the covariate distributions between the treatment and comparison observations for each distinct value of the estimated balancing score; the covariates should be balanced in both groups. One possible balancing score is the propensity score. The propensity score is the probability of being treated given observed characteristics X . Hence, if the CIA holds for some set of variables, then it also holds when conditioning on the propensity score $p(X) = P(D=1|X=x)$.

Ideally, in the propensity score estimation the duration of benefit receipt until the received sanction would be included for several reasons. First, because I consider a system of ongoing sanctions, taking place continuously over the observed period from January 2005 until the interview date (between November 2005 and March 2006), people could stop receiving UB II during the observation period and therefore stop being at risk of receiving a sanction. All potential controls to a sanctioned person should have at least received unemployment benefit II as long as the sanctioned person when the sanction took place. Second, I mentioned already that because of the implementation problems of the new SC II, the use of sanctions increased over the time. Third, the behavior of a benefit recipient had to be watched by the case manager first, so that the probability of sanctions increased with benefit duration.

Fourth, the elapsed benefit duration is likely to capture to some extent a person's unobserved ability and motivation to exit benefit duration. Ability and motivation, again, will have an impact on reservation wages. But it is not possible to use the whole duration of UB II receipt as regressor in the propensity score because for

³³ Actually, since I am interested in the average treatment effect on the treated (ATT), the CIA can be relaxed to $r_0 \perp D | X$ (Caliendo and Kopeinig 2008). I assume, however, for the reasons given above, that the stronger version of the CIA holds.

sanctioned persons, duration of unemployment benefit II receipt after a sanction might be influenced by the sanction. Therefore, the duration of unemployment benefit II receipt after a sanction is endogenous. The obvious problem is that for the non-sanctioned, the duration of UB II receipt before a sanction which they never received is known purely hypothetical and thus cannot be included in the estimation of the propensity score.

To take care of this problem, I follow Sianesi (2004) and estimate both the propensity score and the average treatment parameter separately for each of the first four quarters of individual uninterrupted UB II receipt (see also Fitzenberger and Völter 2007 for an application of the approach with strata, not single months). The eligibles for each estimation sample are those people who either 1) received a sanction in quarter $u=1,2,3,4$ of their first, uninterrupted UB II spell, or 2) were at risk of receiving a sanction in quarter u because they continued to receive UB II until the end of quarter u but never received a sanction during the observation period. Figure 3 illustrates who of the sample members is eligible for each estimation sample.

Figure 3
Eligibles for the four estimation samples

	Month of individual uninterrupted UB II receipt											
	1	2	3	4	5	6	7	8	9	10	11	12
Sample 1												
Treated	S or UB II	S or UB II	S or UB II	...								
Controls	UB II	UB II	UB II	...								
Sample 2												
Treated	UB II	UB II	UB II	S or UB II	S or UB II	S or UB II	...					
Controls	UB II	UB II	UB II	UB II	UB II	UB II	...					
Sample 3												
Treated	UB II	UB II	UB II	UB II	UB II	UB II	S or UB II	S or UB II	S or UB II	...		
Controls	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II	...		
Sample 4												
Treated	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II	S or UB II	S or UB II	S or UB II
Controls	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II	UB II

Notes: Treated in sample u ($u=1, 2, 3, 4$) are UB II recipients who received a sanction S which started in one of the three months of quarter u of their first UB II spell. Controls in sample u are UB II recipients who received UB II at least until the end of quarter u and received never a sanction during the one year of observation. In case people received more than one sanction, they are eligible only in the quarter where their first sanction starts (as treated).

Source: Own illustration.

Sample two, for example, contains all persons receiving at least six months UB II without being sanctioned during the observation period (“controls”) and all persons receiving a sanction in month four, five or six after entering UB II receipt (“treated”). I exclude people as controls from sample two who received a sanction in quarter one, three or four. If I would compare the reservation wages (that are reported after quarter four, remember) between people sanctioned in quarter two and people sanctioned in any of the other three quarters, the reservation wages of the sanctioned

controls would also be influenced by the sanction they received before or after quarter two and the estimation result would be biased. This approach differs from Sianesi (2004) insofar as treated can never be controls because controls can never receive a sanction – but only for the limited observation period of one year.

The starting month of the sanction defines in which quarter the sanction was imposed, i.e. the treatment took place; a sanction imposed, say, in quarter two that still lasts in quarter three, is counted only in quarter two. Implicitly I assume hereby, that the actual beginning of a sanction within one quarter is random conditional on X . In case people received more than one sanction during the observation period, they are eligible for an estimation sample only once: in the quarter where their first sanction starts (as treated).

For the eligibles at u , treatment receipt is denoted by D_u , that is $D_u = 1$ for receiving a sanction that started in quarter u , and $D_u = 0$ for never being sanctioned during all quarters u of the observation period. The estimated treatment parameter for each quarter u is then $ATT_u = E(r_1 - r_0|X, D_u = 1) = E(r_1|X, D_u = 1) - E_X [E(r_0|X, D_u = 1) | D_u = 1] = E(r_1|X, D_u = 1) - E_X [E(r_0|X, D_u = 0) | D_u = 1]$, where the first term can be estimated from the treatment group and the second term from the mean outcomes of the matched controls. The outer expectation is taken over the distribution of X in the treated population.

In the propensity score estimation, I include variables theoretically related to both the probability to receive a sanction and to the level of reservation wages (see Section Results). There are various procedures of propensity score matching to calculate the impact of a treatment. They differ in the decision which controls to use for a treated observation, but for the calculation of the impact they take all the mean difference in outcomes between treated and controls (see Caliendo and Kopeinig 2008 for details).

I preferred to use Radius-Matching with replacement as straightforward matching procedure in order to be able to achieve unbiased estimates without paying for it with enormous variance. This made sense because though my sample is of medium size the estimated propensity scores between treated and controls were similar (see Section Results). Radius-Matching defines as controls for a treated individual all non-treated individuals whose estimated propensity score is within a radius (determined by a chosen maximum absolute propensity score distance) of the propensity score of the treated individual. By matching only within the determined radius I made sure that only the subset of the comparison group that is comparable to the treatment group was used in the analysis. Matching with replacement means that appropriate controls could be used more than once as a match.³⁴

³⁴ For the analytical variances and hence the standard errors of these estimators see Becker and Ichino (2002).

I decided to keep a rich specification of the propensity score model that is essentially based on my theoretical considerations and results of previous empirical literature on benefit sanctions and reservation wages. This helps to account best possible for potential sources of unobserved heterogeneity and maintains the CIA plausible instead of relying mainly on statistical significance of the regressors given that the balancing score property is met. The balancing score property is testable by performing mean comparison tests for both samples of treated and controls to make sure that there are no significant differences in covariate means.³⁵

A common procedure to further assess the matching quality is to check the reduction of standardized bias³⁶ between treated and controls through the matching, for every covariate and all covariates together, as well as likelihood-ratio tests of joint insignificance of all regressors in the propensity score estimation before and after matching. The reduction of the standardized bias clearly shows the bias reduction before and after matching. The t-tests give the statistical significance of the results.

Note that the policy question is not: Does the existence of sanctions lower the reservation wage of those UB II recipients who received a sanction? Instead, it is: Do UB II recipients who received a sanction at a given period of their UB II receipt lower their reservation wage in comparison to those who continue receiving benefits in this period the sanction takes place (and never were sanctioned in the observed year)?

6 Estimation results

To put it briefly, in order to know how the mean reservation wage of sanctioned UB II recipients changes due to the event and timing of the sanction, I took account of the timing of the sanction and estimated the effect separately for the first four quarters of uninterrupted UB II receipt by matching on the propensity score. Each sample contained the population at risk of receiving a first UB II sanction at the given quarter of their first uninterrupted spell of UB II receipt. For my methodological approach, I needed to eliminate potential selection bias between treated and controls by controlling for relevant variables in the propensity score estimation that influence both the probability to receive a UB II sanction and reservation wages.

³⁵ See Rosenbaum and Rubin (1985), Dehejia and Wahba (2002) and Caliendo and Kopeinig (2008) for more details on the assessment of the quality of the matching. The t-tests on the equality of means are performed in a regression setting, assuming homoskedastic errors (Leuven and Sianesi 2003, Stata Corporation 2005: p. 485). Let x and y represent the observations of treated and controls respectively, n the number of observations, s the standard deviation. The formula of the t-statistic is

$$t = \frac{\bar{x} - \bar{y}}{\left\{ \frac{(n_x - 1)s_x^2 + (n_y - 1)s_y^2}{n_x + n_y - 2} \right\}^{1/2} \left(\frac{1}{n_x} + \frac{1}{n_y} \right)^{1/2}}, \text{ distributed as}$$

Student's t with $n_x + n_y - 2$ degrees of freedom.

³⁶ The standardized bias is the difference of the sample means of treated and controls as a percentage of the square root of the average of the sample variances in the treated and non-treated groups.

To account for the behavior of the individual UB II recipient, in the propensity score estimation I included demographic variables (age, gender, detailed household context with age and number of own children, existence and employment status of partner, marital state, nationality and nationality of parents, language skills) and variables related to pre-observation period skill levels, employment history and benefit receipt (education and vocational training, ever employed, employed at 31st December 2004, duration of employment, unemployment and qualification in the five years before December 2004, last earnings, receipt of benefits in December 2004, driver license) as well as if the UB II recipient signed an integration contract (including the timing).

Following previous results in the literature on benefit sanctions and reservation wages (see Section Theoretical considerations and previous empirical results), the chosen individual characteristics are assumed to capture motivation, chances and costs (employment barriers) to reintegrate into the labor market, signaled from the part of the UB II recipient and perceived by the employment office. Highly qualified, skilled people with long employment periods, short unemployment periods and financial responsibilities for relatives are assumed to be better motivated and face less search costs. Better motivation to find a job and less search costs is assumed to lead to a low sanction probability. At the same time, this type of people has higher chances of receiving a job offer, which they then might reject. This might lead to a high sanction probability. In contrast, older people, women, migrants and people with problems of childcare, language and transportation are assumed to face higher search costs and less chances of receiving a job offer. Furthermore, these social groups might be discriminated by the case manager. These factors can lead to either lower or higher sanction probability.

To account for the behavior of case managers and the local labor market situation, I included variables for the level, structure and dynamics of local unemployment in December 2004 (West or East Germany, information by district on unemployment rates, the average unemployment duration and proportions of statistically disadvantaged unemployed like long-term unemployed, foreigner, young people and women, the share of service jobs of all jobs subject to social insurance contributions per district as a proxy for structural change of a region and the ratio of offered jobs to all unemployed per district) and the number of One-Euro-Jobs to all unemployed by district for 2005 (proxying the extent of active labor market policy).

Whether and when UB II recipients received integration contracts is assumed to raise monitoring and the sanction probability while reducing the reservation wage. The regional labor market variables proxy the amount of job offers and offers for labor market measures a UB II recipient can expect: the more offers, the higher the sanction probability. Sanction 'philosophies' by German counties ("Bundesländer") are assumed to capture additional labor market characteristics I cannot observe but the Job Centers can. Both sets of variables therefore are assumed to influence the individual probability to receive a sanction and equally influence reservation wages.

Remember that I took the duration of ongoing unemployment and UB II receipt into account by estimating the effects for four subsequent quarters of uninterrupted UB II receipt. These variables are important as they capture unobserved ability and motivation of the UB II recipients and unobserved increases in monitoring on behalf of the employment officer.

Sample averages of the covariates are displayed in the Appendix Table A 1. Details on the specification of the probit estimation and the estimation results for all four quarters are presented in the Appendix Table A 2. In Table 8, for each quarter propensity scores are shown for treated and controls.

Table 8
Propensity Scores as a function of treatment status

	Propensity scores of sanctioned sample members				Propensity scores of non-sanctioned sample members			
	Minimum	Mean	Maximum	SD	Minimum	Mean	Maximum	SD
Quarter 1	0.001	0.019	0.068	0.013	0.000	0.011	0.085	0.009
Quarter 2	0.002	0.043	0.252	0.042	0.000	0.015	0.261	0.020
Quarter 3	0.004	0.030	0.106	0.020	0.000	0.018	0.167	0.015
Quarter 4	0.009	0.083	0.310	0.060	0.000	0.044	0.346	0.040

Source: IAB-Survey "Life situation and social security 2005".

The mean probability to receive a sanction for treated steadily increases from 0.02 in the first quarter to 0.08 in the fourth quarter; the mean probability to receive a sanction for controls stayed between 0.01 and 0.02 for the first three quarters and rose to 0.04 in the fourth quarter. The estimated average propensity scores for treated are always within the minimum and the maximum propensity score of the controls, while the standard deviation of their propensity scores is higher. In the Appendix Figures A 1 to A 4, the kernel density estimations of the propensity scores are presented for treated and controls respectively. That mean propensity scores of the treated increase with time supports the importance of considering the timing of sanctions when matching on the propensity score.

Not only the propensity score, also the share of treated increases with time: The number of treated rises from 105 in the first quarter to 160 in the fourth quarter because the probability to be sanctioned increases with uninterrupted UB II receipt and calendar months in 2005; the number of potential controls decreases with uninterrupted UB II receipt from 9,036 to 3,336.

I consider the predictive power of the regressors in my propensity score estimations as rather low.³⁷ It might be that sanction probabilities are influenced by individual

³⁷ The pseudo R² varies between 0.05 and 0.12; the probabilities of the likelihood-ratio test of the joint insignificance of all the regressors are zero for all estimations (see Table 10 below and Appendix Table A 2). The area under the ROC curve is on average 77 per cent.

factors the case managers do observe but that are not reflected in benefit duration, nor in my chosen variables for demographic, household and employment history characteristics; recent drug problems for instance. If these factors additionally affect the reservation wage in a systematic way, my methodological approach fails to measure the real effects of sanctions. On the other side, the comparatively low predictive power of the propensity score estimations and the lack of common support problems can, alternatively, be a sign that my assumption of a rather unsystematic process of sanctioning is right, since the regressors prove to predict reservation wage quite well and showed significant impact in previous research on sanctions.³⁸ The wider standard deviation of propensity scores for the treated strengthens this view.

I defined three radiuses with different maximum absolute distances for the propensity score differences between treated and controls (so-called caliper) since mean and variance of the estimated effects might be sensitive to the choice of the radius. The propensity scores of the controls must not be further than (1) 1, (2) 0.1 and (3) 0.01 percentage points from the propensity score of the treated. The smaller the radius, where the propensity scores of the potential controls have to be in, the less controls are found for the treated, of course, thereby increasing the variance of the estimation, but reducing the potential bias resulting from controls being too different to compare.

Since all estimated propensity scores of the treated are within the range of the propensity scores of the controls, all potential controls in each quarter of UB II receipt are on common support. But not for all three radiuses, controls with features sufficiently similar to the treated can be found (see Table 9). By matching within the widest radius (1) (1 percent of the propensity score of the treated), all treated are assigned to almost all potential controls. Within the narrower radiuses (especially the 0.01 percent radius), clearly less controls can be found; hence not all treated can be assigned to a match.

³⁸ Regressions on the log reservation wage with the variables used in the propensity score and dummies for received UB II sanctions explain about 21 percent of the variances in reservation wages (see Appendix Table A 3), a good predictive power, considering previous results (e.g. Bender et al. 2007 with the same dataset).

Table 9
Number of potential and matched treated and controls by quarter and caliper
(outcome variable: hourly net reservation wage)

Number of potential treatment units	Number of potential control units	Number of treatment units that are matched	Number of control units that are matched	Caliper (radius)		Quarter of uninterrupted UB II spell
105	9036	105	9035	(1)	0.01	1
		105	8947	(2)	0.001	
		103	4495	(3)	0.0001	
131	8485	131	8472	(1)	0.01	2
		129	7855	(2)	0.001	
		118	2673	(3)	0.0001	
147	7952	147	7948	(1)	0.01	3
		147	7578	(2)	0.001	
		139	3447	(3)	0.0001	
160	3336	160	3333	(1)	0.01	4
		155	2479	(2)	0.001	
		127	626	(3)	0.0001	

Notes: Matching Algorithm: Radius matching with different calipers defining the maximum absolute propensity score distance between treated and controls. The difference between the number of sample members in the whole sample (12,822) and the sum of treated and controls in the four quarters of uninterrupted UB II spell results from the fact that only those people are selected as potential treatment units who received their first UB II sanction in the respective quarter of uninterrupted UB II receipt; and that only those people are selected as potential control units who continuously received UB II at least including the last month of the respective quarter of uninterrupted UB II receipt, without being sanctioned during the whole observation period.

Source: IAB-Survey "Life situation and social security 2005".

The balancing score property is met in all estimations because the covariate distributions between the treatment and control units are very similar for each value of the estimated propensity scores. In the following Table 10, the overall match quality is displayed: (1) the standardized bias before and after matching, (2) the pseudo R^2 from the propensity score estimation on all the regressors before matching and after matching on the matched samples, and (3) the corresponding p-values of the likelihood-ratio test of the joint insignificance of all the regressors in the propensity score estimation before and after matching. Independently of the radius I used, radius matching markedly reduces differences of the sample means of treated and controls.

There is no best performing radius in all estimations in terms of maximum reduction of standardized bias and of pseudo R^2 's after matching; the wider radiuses, however, seem to perform better than the narrowest one. In the Appendix Tables A 4 to A 6, you find further details on the specific matching quality for each radius and covariate. The p-values of the t-test on no difference in means are over 90 percent in most cases, and the absolute bias is profoundly reduced after the matching. Like the indicators for general matching quality already imply, after matching with the wider radiuses, the covariate distributions are more similar than after matching with the narrowest one. The p-values of a test whether all covariates do not determine the propensity score reject the hypothesis clearly prior to matching but do not reject after matching. This holds for all radiuses.

Table 10**Quality indicators for the matching by sample and caliper (outcome variable: hourly net reservation wage)**

(1a) Standardized bias before matching	(1b) Standardized bias after matching	(2a) Pseudo R ² before matching	(2b) Pseudo R ² after matching	(3a) P-value of test on H0: β if all covariates are zero (before matching)	(3b) P-value of test on H0: β if all covariates are zero (after matching)	Caliper (radius)		Quarter of uninterrupted UB II spell
						(1)	0.01	
8.574	2.328	0.047	0.006	0.210	1.000	(1)	0.01	1
	2.266		0.004			(2)	0.001	
	2.745		0.009			(3)	0.0001	
16.662	1.846	0.120	0.007	0.000	1.000	(1)	0.01	2
	2.285		0.007			(2)	0.001	
	4.834		0.024			(3)	0.0001	
11.080	1.815	0.054	0.003	0.001	1.000	(1)	0.01	3
	1.036		0.002			(2)	0.001	
	3.262		0.014			(3)	0.0001	
12.759	1.225	0.089	0.001	0.000	1.000	(1)	0.01	4
	2.355		0.008			(2)	0.001	
	4.015		0.040			(3)	0.0001	

Notes: Matching for four subsequent quarters of uninterrupted UB II receipt. Matching Algorithm: Radius matching with different calipers defining the maximum absolute propensity score distance between treated and controls.

Source: IAB-Survey "Life situation and social security 2005".

In sum, the radius matching with different radiuses for subsequent quarters of uninterrupted UB II receipt seems appropriate to estimate the average effect of a sanction on the reservation wages of the sanctioned (ATT), given that my identifying assumptions hold. I present the results of the estimations for all estimated effects, their standard errors and 95 percent confidence intervals in Table 11.

Table 11**Estimation results of the effect of sanctions on the reservation wages of sanctioned unemployment benefit II recipients (ATT)**

Average treatment effect on the treated (ATT) in Euro	Standard error of ATT in Euro	95% lower confidence band of ATT in Euro	95% upper confidence band of ATT in Euro	Caliper (radius)		Quarter of uninterrupted UB II spell
0.089	0.241	-0.383	0.561	(1)	0.01	1
0.085	0.243	-0.391	0.561	(2)	0.001	
0.095	0.252	-0.398	0.588	(3)	0.0001	
0.222	0.192	-0.155	0.599	(1)	0.01	2
0.199	0.197	-0.188	0.585	(2)	0.001	
0.290	0.221	-0.143	0.722	(3)	0.0001	
0.194	0.170	-0.139	0.527	(1)	0.01	3
0.222	0.172	-0.115	0.560	(2)	0.001	
0.226	0.181	-0.129	0.582	(3)	0.0001	
-0.032	0.182	-0.389	0.325	(1)	0.01	4
0.045	0.192	-0.333	0.422	(2)	0.001	
-0.088	0.233	-0.544	0.369	(3)	0.0001	

Notes: Matching for four subsequent quarters of uninterrupted UB II receipt: Radius matching with different calipers defining the maximum absolute propensity score distance between treated and controls. The outcome variable is the net hourly reservation wage in Euro at the time of the interview. The treatment is having received a UB II sanction in the respective quarter of uninterrupted UB II receipt vs. having continuously received UB II in the respective quarter and not having received a UB II sanction in the observation period of one year.

Source: IAB-Survey "Life situation and social security 2005".

The main result shown in Table 11 is that I did not find any significant effect of sanctions on the reservation wages of sanctioned unemployment benefit II recipients. This result holds regardless of the specification I chose and the specific quarter of UB II receipt I analyzed. Consider for example the first quarter of uninterrupted UB II receipt in Table 11: sanctioned UB II recipients have on average slightly higher reservation wages (around nine Eurocents, depending on the radius chosen). The 95% confidence interval, however, ranges from around -0.4 Euro to around +0.6 Euro, i.e. the confidence intervals include zero independently of the radius chosen. Hence, the estimated effect of a sanction on the reservation wage of an UB II recipient sanctioned in the first quarter of her UB II spell is not significant.

This is also the case in the other three quarters of uninterrupted UB II receipt. If the sanction is received in the second and third quarter, the mean reservation wage of sanctioned UB II recipients is around 0.2 to 0.3 Euro higher, and in the fourth quarter quite the same, as the reservation wage of non-sanctioned UB II recipients. All confidence intervals of the estimated treatment effects on the treated include zero. UB II sanctions do not seem to change the reservation wages of sanctioned UB II recipients in a significant way.

The fact that it is not possible to observe reservation wages in the data before and immediately after a sanction limits my analysis: Sanctioned sample members could have lowered their reservation wage immediately after a sanction. As a result of lower reservation wages due to the sanction, they could have accepted employment and, due to being employed, raised their reservation wage again. They would end up with a reservation wage higher or equal than the non-sanctioned group, thus leading to biased estimation results. In order to confront this limitation, I estimated the effect of sanctions on the probability to be employed at the time of the interview on sanctioned UB II recipients with the same methodological approach as described in the Section Methodology. Thus, the question can be answered if UB II recipients who received a sanction at a given period of their UB II receipt have a higher probability to be employed at the time of their interview in comparison to those who continue receiving benefits in this period the sanction takes place (and never were sanctioned in the observed year).³⁹

³⁹ The outcome variable “employed” is 1 if the person reported to be regular employed, self-employed, employed in a Personal Service Agency or minor employed subject to social insurance contributions at the time of the interview, and not to be unemployed and/or in labor market measures at the same time; the outcome variable “employed” is 0 if the person is at the time of the interview not employed, employed in a job creating measure, a job try-out or internship, in subsidized self-employment or minor employed but not subject to social insurance contributions, or at the same time unemployed and/or in labor market measures.

Table 12 below shows the estimation results of the effect of sanctions on the probability to be employed at the time of the interview on sanctioned UB II recipients.⁴⁰ Sanctioned UB II recipients have on average slightly higher probabilities to be employed at the time of the interview. In the first three quarters of uninterrupted UB II receipt, this probability was between one and four percentage points higher, in the last quarter it was between one and four percentage points lower. But the estimated effects remain insignificant. The 95% confidence intervals include zero independently of the quarter and the radius chosen.

Table 12
Estimation results of the effect of sanctions on the probability to be employed at the time of the interview on sanctioned unemployment benefit II recipients (ATT_{emp})

Average treatment effect on the treated (ATT _{emp}) in percentage points/100	Standard error of ATT _{emp} in percentage points/100	95% lower confidence band of ATT _{emp} in percentage points/100	95% upper confidence band of ATT _{emp} in percentage points/100	Caliper (radius)		Quarter of uninterrupted UB II spell
				(1)	0.01	
0.047	0.035	-0.023	0.116	(1)	0.01	1
0.037	0.036	-0.033	0.107	(2)	0.001	
0.042	0.038	-0.032	0.115	(3)	0.0001	
0.024	0.031	-0.037	0.085	(1)	0.01	2
0.026	0.031	-0.036	0.087	(2)	0.001	
0.015	0.034	-0.052	0.082	(3)	0.0001	
0.024	0.024	-0.023	0.070	(1)	0.01	3
0.025	0.024	-0.022	0.072	(2)	0.001	
0.008	0.025	-0.041	0.057	(3)	0.0001	
-0.018	0.020	-0.058	0.021	(1)	0.01	4
-0.037	0.020	-0.075	0.002	(2)	0.001	
-0.007	0.021	-0.047	0.033	(3)	0.0001	

Notes: Matching for four subsequent quarters of uninterrupted UB II receipt: Radius matching with different calipers defining the maximum absolute propensity score distance between treated and controls. The outcome variable “employed” is 1 if the UB II recipient is employed (regular, self-employed or employed at a Personal Service Agency) or minor employed subject to social insurance contributions at the time of the interview, otherwise 0. The treatment is having received a UB II sanction in the respective quarter of uninterrupted UB II receipt vs. having continuously received UB II in the respective quarter and not having received a UB II sanction in the observation period of one year.

Source: IAB-Survey “Life situation and social security 2005”.

These results confirm that there is no statistical prove that UB II sanctions have an effect on the probability to be employed at the time of their interview. Thus, the main estimation results of the effects of sanctions on reservation wages should not be biased because of higher reservation wages of employed sample members. Sanctioned UB II recipients are not significantly more often employed at the time of the interview. All estimated effects are insignificant.

⁴⁰ In the Appendix Tables A 7 and A 8, the corresponding numbers of potential and matched treated and controls and the quality indicators for the matching are shown for each sample and caliper. The slightly smaller numbers of treated and controls compared to the matching with the outcome variable “net hourly reservation wage” result from the fact that the information on the employment status at the time of the interview is missing in the absent cases.

Further sensitivity checks

Apart from matching with different radiuses, I conducted several further sensitivity analyses. I repeated my estimations using a six and 12-month stratum instead of the estimating the effects for four quarters. For the 12-month-stratum, I additionally conducted the estimations separately for each gender and East and West Germany. The insignificant effect of sanctions on the reservation wages of sanctioned UB II recipients does not change. I also varied the criteria to define the common support, and tried different matching algorithms (various specifications of kernel, nearest neighbor and stratification matching) to check the robustness of results. All estimated effects stay insignificant.

7 Conclusions

I was interested in the effect of sanctions on the reservation wage of sanctioned unemployment benefit II recipients because on the one hand, the exact behavioral effect of a benefit sanction is still undetermined in an ongoing and vivid research debate. On the other hand, benefit sanctions in Germany were markedly tightened with the introduction of the new benefit unemployment benefit II (UB II) in 2005, codified in Social Code (SC) II.

Since 2005, sanctions were further tightened twice. UB II recipients can receive sanctions for various reasons, above all showing not enough effort to end unemployment and accepting not practically every job or integration measure. It is the non-compliant UB II recipient who has to prove important reasons for her non-compliance to avoid a sanction. Sanctions cut the base benefit of UB II by 30 percent for most non-compliances, for at least three months.

According to search theory, benefit sanctions should directly reduce reservation wages, faster than exits out of benefit receipt can be realized. To explore this hypothesis, I adopted propensity score matching as a “selection on observables” approach to estimate the effect of a received benefit sanction on the reservation wages of the sanctioned UB II recipients. The data I used was a survey of unemployment benefit II recipients with rich and unique information on the first fifteen months after the new SC II was implemented.

In the survey, net hourly reservation wages were collected in various steps, thus I expect them to be more reliable than equivalents in other surveys. Even with a slight overrepresentation of UB II recipients with relatively high employment chances in the sample, employment chances of sample members are clearly below average relative to the German population. Their average net reservation wage is with 6.06 Euro/hour positioned in the low wage sector, at the very bottom of the German wage distribution. This implies that the sample members would accept almost every wage offer since most paid net wages lie above their reservation wages.

For the identification of the effect, I relied on my informative data and the observation of a rather unsystematic sanctioning process in the first year of the new SC II

which is also reflected in my data, resulting from setting up a new benefit system including the necessary institutions that deal with it. Benefit sanctions were imposed with increased intensity after the implementation of the SC II, but at a low rate (below three percent per month) and less systematic as intended by the regulations in the SC II in terms of duration, intensity and justification given.

I explicitly considered the timing of the sanction in my design and estimated the effects for four subsequent quarters of uninterrupted UB II receipt in 2005. I defined three radiuses with different maximum absolute distances for the propensity score differences between treated and controls (caliper) since mean and variance of the estimated effects might be sensitive to the choice of the caliper. But as the characteristics of the sanctioned and non-sanctioned UB II recipients were quite similar, matching the two groups caused no major problems, independently of the radius chosen. The balancing score property is met in all estimations.

The main result is that I did not find any significant effect of sanctions on the reservation wages of sanctioned unemployment benefit II recipients. This result is robust regardless of the specification I chose and of the specific quarter of UB II receipt I analyzed. The mean effects were in the first three quarters above zero and in the fourth quarter around zero, but all confidence intervals of the estimated treatment effects on the treated include zero. UB II sanctions do not change the reservation wages of sanctioned UB II recipients in a significant way. Further sensitivity checks did not change the insignificance of effects.

My paper has some shortcomings rooting in the dataset I used. First, I did not estimate the whole effect of a sanction on reservation wages, but the ex-post effect. Previous research on benefit sanctions indicates that ex-ante, sanctions seem to have at least as important behavioral effects as ex-post. Second, since it was not possible to observe reservation wages before and immediately after a sanction, I could not completely rule out the effect of increased reservation wages because people might have taken up employment. Third, though I took heterogeneity between sanctioned and non-sanctioned into account through research design and control variables, I did not model unobserved heterogeneity explicitly: If sanction probabilities and reservation wages are systematically influenced by unobserved individual factors that are not captured in my estimations, my methodological approach fails to measure the real effects of sanctions.

Still, the robustness of results leads me to the conclusion that if benefit sanctions increase exit rates out of benefit receipt they might do it because they stimulate effective job search, and not, because they reduce reservation wages. Since reservation wages are at the utter bottom of the wage distribution, the level of reservation wages seems not to be the main reason that an UB II recipient stays unemployed, but the lack of job offers. That sanctioned UB II recipients therefore might not reduce their already low reservation wages but instead might search with more intensity and maybe more know-how for any job offer seems a plausible conclusion. I did not find

statistical prove, however, that UB II sanctions have an effect on the probability to be employed at the time of the interview. Thus, we do not know if UB II sanctions raise exit rates out of benefit receipt at all.

If the target population has even less employment chances than my sample, I assume that they do not have higher, but rather lower reservation wages and therefore the same result should apply. The result is in line with previous research; it should nevertheless be substantiated by further research on the effects of benefit sanctions. Future research should explore panel data to deal with (fixed) unobserved heterogeneity and time-varying variables. A new panel study of the IAB started in December 2006 with information on benefit sanctions and reservation wages and will be available soon. Additionally, social experiments seem to be a promising source to estimate the (ex-ante and ex-post) effects of benefit sanctions on search behavior and reservation wages and to separate the effect of a UB sanction from increased job search assistance.

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Appendix

Tables

Table A 1
Mean characteristics of all sample members and as a function of treatment status_t

	Non-sanctioned sample members	Sanctioned sample members	All sample members
Demographic and household variables			
Gender=woman	0.49	0.49	0.49
Married man	0.18	0.15	0.18
Married woman	0.17	0.16	0.16
Partner 2004 (reference: no partner)	0.56	0.58	0.56
Employed partner	0.14	0.15	0.14
Non-/unemployed partner	0.30	0.26	0.30
Age 2004 (reference:15 to 25 years old)	0.12	0.23	0.12
25 to 40 years old	0.38	0.41	0.38
40 to 50 years old	0.29	0.24	0.29
50 to 58 years old	0.22	0.12	0.21
More than 2 children in household 2004	0.06	0.07	0.06
Child under 3 in household	0.08	0.10	0.08
OECD equivalent net monthly household income (Euro) 12/2004	635.25	618.60	634.34
Other earners in household	0.21	0.26	0.22
Migrant status (reference: other nationality, parents and interview language)	0.10	0.12	0.10
German/other nationality, parents or interview language	0.22	0.22	0.22
German nationality, parents and interview language	0.68	0.66	0.68
Qualification			
Qualification (reference: low) ^a	0.27	0.35	0.28
Middle	0.60	0.56	0.60
High	0.05	0.05	0.05
Very high	0.07	0.04	0.07
Drivers license?	0.68	0.65	0.68
(very) good German language skills	0.87	0.90	0.87
Employment history ^b			
Employed at 31 December 2004	0.09	0.10	0.09
Cumulated duration of employment	18.98	20.07	19.04
Under 25*Cumulated duration of employment	1.11	2.03	1.16
Cumulated duration of unemployment	30.95	26.38	30.70
Under 25*Cumulated duration of unemployment	1.79	3.83	1.90
Cumulated duration of qualification	6.35	8.04	6.44
Under 25*Cumulated duration of qualification	2.10	4.37	2.23
Hourly net wage(missing are zero)	0.25	0.41	0.25
Indicator for missing net wage	0.96	0.93	0.96
Benefit receipt 12/2004 (reference: no benefits)	0.08	0.10	0.08
Welfare/Sozialhilfe	0.16	0.18	0.16
Unemployment assistance/Arbeitslosenhilfe	0.52	0.45	0.52
Unemployment benefit/Arbeitslosengeld	0.23	0.27	0.24
Integration contract			
Integration contract	0.28	0.33	0.28
Integration contract*month of integration contract	139.00	171.55	140.77

Table A 1 continued
Mean characteristics of all sample members and as a function of treatment status_t

	Non-sanctioned sample members	Sanctioned sample members	Whole sample
Regional labor market			
Share of service jobs 12/2004 per district	0.64	0.62	0.64
Ratio of offered jobs to registered unemployed in 2005 per district	0.04	0.05	0.04
Ratio 1-euro-jobs to stock of registered UB II-unemployed 2005	0.24	0.22	0.24
Unemployment duration 12/2004 in months per district	15.93	15.52	15.91
Share long-term unemployed	0.40	0.39	0.40
Share women unemployed	0.44	0.44	0.44
Share foreigners unemployed	0.12	0.12	0.12
Share under 25 y unemployed	0.11	0.11	0.11
Unemployment rate per district 12/2004	15.58	14.46	15.52
Sanction rate in county group (reference: very high: Rheinland-Pfalz, Baden-Württemberg)	0.08	0.14	0.09
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	0.35	0.26	0.35
Middle: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	0.42	0.42	0.42
High: Thüringen, Bremen, Bayern	0.14	0.18	0.14
Living in West Germany	0.55	0.63	0.55

Notes: t. Treatment is to receive a sanction in the first spell of UB II receipt in 2005 vs. never to receive a sanction during the observation period. a. Low qualified means no graduation or graduation from Sonder-/Haupt- and Realschule and no vocational training, middle qualified means (Fach-)Abitur and no vocational training, or graduation Sonder-/Haupt- and Realschule and apprenticeship, highly qualified means (Fach-)Abitur and apprenticeship or master craftsmen and very highly qualified means university degree. b. in months between 1 Jan 2000 and 31 Dec 2004. c. Average sanction rates were 4.1% for the low group, 5.5% for the middle group, 6.9% for the high group and 8.9% for the very high group.

Source: "Life Situation and Social Security 2005".

Table A 2**Estimation results of the probit specification of the probability to receive a sanction (the propensity score) for all four quarters of permanent UB II receipt**

	Quarter 1: Month 1 to 3 of permanent UB II receipt	Quarter 2: Month 4 to 6 of permanent UB II receipt	Quarter 3: Month 7 to 9 of permanent UB II receipt	Quarter 4: Month 10 to 12 of per- manent UB II receipt
Demographic and household variables				
Gender=woman	-0.042	-0.173*	0.087	0.098
Married man	0.022	0.018	0.107	-0.293*
Married woman	0.133	0.292*	0.058	-0.232
Partner 2004 (reference: no partner)				
Employed partner	-0.023	0.142	0.072	-0.216
Non-/unemployed partner	-0.017	-0.030	-0.027	-0.129
Age 2004 (reference:15 to 25 years old)				
25 to 40 years old	-0.356	-0.705***	-0.057	-0.077
40 to 50 years old	-0.428	-0.781***	-0.150	-0.312
50 to 58 years old	-0.635**	-1.120***	-0.339	-0.589**
More than 2 children in household 2004	0.028	0.204	-0.093	-0.041
Child under 3 in household	-0.328*	-0.061	-0.054	-0.025
OECD equivalent net monthly household income 12/2004	0.000	0.000	0.000	0.000
Other earners in household	0.011	0.220*	0.105	0.237
Migrant status (reference: other nationality, parents and interview language)				
German/other nationality, parents or interview lan- guage	-0.078	0.014	-0.055	0.237
German nationality, parents and interview language	-0.117	0.005	-0.070	0.363**
Qualification				
Qualification (reference: low) _a				
Middle	-0.084	0.161*	-0.151*	-0.309***
High	0.207	0.061	-0.032	-0.276
Very high	-0.330	0.050	-0.097	-0.379*
Driving license?	-0.008	0.084	-0.077	-0.063
(Very) good speaking skills in German	0.342**	-0.113	0.234*	0.020
Employment history				
Employed at 31 December 2004	-0.013	-0.080	-0.053	-0.325
Cumulated duration of employment _b	0.004	0.004	0.000	0.007***
Under 25* Cumulated duration of employment	-0.017	-0.004	-0.004	-0.012
Cumulated duration of unemployment	0.002	-0.002	-0.002	0.000
Under 25* Cumulated duration of unemployment	-0.001	0.002	0.002	0.008
Cumulated duration of qualification	0.006	-0.008	-0.002	0.006
Under 25* Cumulated duration of qualification	-0.003	0.000	0.009	0.010
Hourly net wage(missing are zero)	0.013	-0.051	-0.058	-0.054
Indicator for missing net wage	0.009	-0.549	-0.565	-0.945
Benefit receipt 12/2004 (reference: no benefits)				
Social assistance/Sozialhilfe	-0.003	-0.085	0.150	-0.109
Unemployment assistance/Arbeitslosenhilfe	0.035	0.030	0.078	0.028
Unemployment benefit/Arbeitslosengeld	0.306*	0.030	0.097	-0.045
Integration contract				
Integration contract	-0.114	-1.726	-0.405	-0.037
Integration contract*month of integration contract	0.000	0.003	0.001	0.000

Table A 2 continued
Estimation results of the probit specification of the probability to receive a sanction
(the propensity score) for all four quarters of permanent UB II receipt

	Quarter 1: Month 1 to 3 of permanent UB II receipt	Quarter 2: Month 4 to 6 of permanent UB II receipt	Quarter 3: Month 7 to 9 of permanent UB II receipt	Quarter 4: Month 10 to 12 of per- manent UB II receipt
Regional labor market				
Share of service jobs 12/2004 per district	0.052	-1.210**	-0.755	-1.475**
Ratio of offered jobs to registered unemployed in 2005 per district	0.068	-2.527*	-0.323	0.813
Ratio 1-euro-jobs/stock of UB II-unemployed 2005	0.022	-0.589	-0.202	0.221
Unemployment duration 12/2004 in months per district	0.007	0.158***	-0.031	-0.008
Share long-term unemployed	0.259	-8.124***	2.605	-1.646
Share women unemployed	0.572	-0.716	1.599	-1.487
Share foreigners unemployed	0.012	-1.038	-0.431	0.419
Share under 25 y unemployed	-0.584	-2.495	-0.280	-4.933*
Unemployment rate per district 12/2004	-0.003	-0.026	0.006	0.020
Sanction rate in county group (reference: very high: Rheinland-Pfalz, Baden-Württemberg)				
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	-0.155	-0.490***	-0.347**	-0.085
Middle: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	-0.094	-0.475***	-0.342**	-0.003
High: Thüringen, Bremen, Bayern	-0.025	-0.173	-0.183	0.066
Living in West Germany	-0.059	-0.357*	0.368**	-0.011
Constant	-2.449*	2.366*	-2.289*	1.765
Summary statistics				
N whole estimation sample	9141	8616	8099	3496
N potential controls	9036	8485	7952	3336
N potential treated	105	131	147	160
Log likelihood	-547	-597	-695	-592
Chi2	53	163	80	116
Pseudo R ²	0.05	0.12	0.05	0.09

Notes: Positive coefficients denote that the respective variable contributes positively to the probability to receive a first sanction within the quarter of permanent UB II receipt listed on top of each column. Therefore, the coefficients are estimated for four different samples. (*) significance on the 10 percent, (**) 5 percent, and (***) 1 percent level. a. Low qualified means no graduation or graduation from Sonder-/Haupt- and Realschule and no vocational training, middle qualified means (Fach-)Abitur and no vocational training, or graduation from Sonder-/Haupt- and Realschule and apprenticeship, highly qualified means (Fach-)Abitur and apprenticeship or master craftsmen and very highly qualified means university degree. b. in months between January 1, 2000 and December 31, 2004.

Source: "Life Situation and Social Security 2005".

Table A 3
Estimation results of the OLS regression of the log reservation wage for all four quarters of permanent UB II receipt

	Quarter 1: Month 1 to 3 of permanent UB II receipt	Quarter 2: Month 4 to 6 of permanent UB II receipt	Quarter 3: Month 7 to 9 of permanent UB II receipt	Quarter 4: Month 10 to 12 of per- manent UB II receipt
Demographic and household variables				
Gender=woman	-0.008	-0.005	-0.006	-0.009
Married man	0.124***	0.129***	0.126***	0.120***
Married woman	-0.019	-0.014	-0.017	0.002
Partner 2004 (reference: no partner)				
Employed partner	-0.014	-0.010	-0.015	-0.017
Non-/unemployed partner	0.009	0.011	0.011	-0.012
Age 2004 (reference: 15 to 25 years old)				
25 to 40 years old	0.264***	0.276***	0.237***	0.277***
40 to 50 years old	0.275***	0.289***	0.246***	0.283***
50 to 58 years old	0.255***	0.268***	0.225***	0.290***
More than 2 children in household 2004	0.060***	0.060***	0.059***	0.065***
Child under 3 in household	0.073***	0.074***	0.078***	0.091***
OECD equivalent net monthly household income 12/2004	0.000*	0.000	0.000	0.000
Other earners in household	-0.057***	-0.055***	-0.052***	-0.039
Migrant status (reference: other nationality, parents and interview language)				
German/other nationality, parents or interview language	-0.052***	-0.050***	-0.044***	-0.042**
German nationality, parents and interview language	-0.081***	-0.077***	-0.072***	-0.085***
Qualification				
Qualification (reference: low) _a				
Middle	0.033***	0.037***	0.039***	0.019
High	0.081***	0.086***	0.095***	0.078***
Very high	0.186***	0.192***	0.197***	0.162***
Driving license?	0.052***	0.053***	0.054***	0.054***
(very) good language skills in German	0.012	0.012	0.003	0.010
Employment history				
Employment status (reference: unemployed 2004 and 2005)				
Employed at 31 dec~2004	0.079**	0.072**	0.076**	0.014
Became employed 2004 to 2005	0.017	0.012	0.017	0.040
Cumulated duration of employment _b	0.000	0.000	0.000	-0.001
Under 25* Cumulated duration of employment	0.003***	0.003***	0.003***	0.004**
Cumulated duration of unemployment	0.000	0.000	0.000	0.000
Under 25* Cumulated duration of unemployment	0.004***	0.004***	0.003***	0.003***
Cumulated duration of qualification	0.000	-0.001**	-0.001**	-0.001
Under 25* Cumulated duration of qualification	0.004***	0.005***	0.004***	0.006***
Hourly net wage(missing are zero)	0.020***	0.020***	0.020***	0.011**
Indicator for missing net wage	0.156***	0.152***	0.162***	0.031
Benefit receipt 12/2004 (reference: no benefits)				
Social assistance/Sozialhilfe	0.000	0.005	0.009	0.031
Unemployment assistance/Arbeitslosenhilfe	-0.025*	-0.021	-0.011	0.021
Unemployment benefit/Arbeitslosengeld	-0.021	-0.017	-0.009	0.007
Integration contract				
Integration contract	-0.022	-0.019	-0.019	-0.052**
Integration contract*month of integration contract	0.000	0.000	0.000	0.000

Table A 3 continued
Estimation results of the OLS regression of the log reservation wage for all four quarters of permanent UB II receipt

	Quarter 1: Month 1 to 3 of permanent UB II receipt	Quarter 2: Month 4 to 6 of permanent UB II receipt	Quarter 3: Month 7 to 9 of permanent UB II receipt	Quarter 4: Month 10 to 12 of per- manent UB II receipt
Regional labor market				
Share of service jobs 12/2004 per district	0.039	0.030	0.044	0.020
Ratio of offered jobs to registered unemployed in 2005 per district	0.091	0.134	0.164	0.263
Ratio 1-euro-jobs/stock of UB II-unemployed 2005	0.012	0.005	0.032	-0.009
Unemployment duration 12/2004 in months per district	0.000	0.001	0.000	0.000
Share long-term unemployed	0.082	0.014	0.037	-0.046
Share women unemployed	-0.420***	-0.451***	-0.432***	-0.335
Share foreigners unemployed	0.383***	0.373***	0.387***	0.338***
Share under 25 y unemployed	-0.195	-0.288	-0.266	-0.605*
Unemployment rate per district 12/2004	-0.006***	-0.006***	-0.005***	-0.005*
Sanction rate in county group (reference: very high: Rheinland-Pfalz, Baden-Württemberg)				
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	0.035**	0.039**	0.042**	0.031
Middle: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	-0.001	0.003	0.003	-0.008
High: Thüringen, Bremen, Bayern	-0.001	0.002	0.007	-0.007
Living in West Germany	0.054***	0.051***	0.064***	0.060**
Received sanction in quarter 1	-0.004			
Received sanction in quarter 2		0.048*		
Received sanction in quarter 3			0.036	
Received sanction in quarter 4				-0.008
Constant	1.471***	1.491***	1.463***	1.625***
<hr/>				
N whole estimation sample	9141	8616	8099	3496
N potential controls	9036	8485	7952	3336
N potential treated	105	131	147	160
Log likelihood	-2000	-1885	-1733	-737
R ²	0.208	0.208	0.210	0.218

Notes: Interpretation of the estimated coefficients: An additional unit of the respective variables is estimated to raise the actual reservation wage by a factor of $\exp(\text{coefficient})$ (rule of thumb: $\text{coefficient} \cdot 100 = \text{change of actual reservation wage in \%}$). (*) significance on the 10 percent, (**) 5 percent, and (***) 1 percent level. a. Low qualified means no graduation or graduation from Sonder-/Haupt- and Realschule and no vocational training, middle qualified means (Fach-)Abitur and no vocational training, or graduation from Sonder-/Haupt- and Realschule and apprenticeship, highly qualified means (Fach-)Abitur and apprenticeship or master craftsmen and very highly qualified means university degree. b. in months between January 1, 2000 and December 31, 2004.

Source: "Life Situation and Social Security 2005".

Table A 4
Balancing for each regressor in the propensity score estimation for caliper 0.01

	Quarter 1		Quarter 2		Quarter 3		Quarter 4	
	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)
Demographic and household variables								
Gender=woman	0.937	66.6	0.929	76.4	0.885	87.3	0.907	91.4
Married man	0.986	91.4	0.818	61.4	0.85	79.1	0.983	99.1
Married woman	0.918	46.9	0.878	76.1	0.957	11	0.852	68.6
Partner 2004 (reference: no partner)								
Employed partner	0.961	-446.3	0.976	87.5	0.833	73.1	0.945	89.9
Non-/unemployed partner	0.976	68	0.967	94.4	0.885	-257.7	0.94	81
Age 2004 (reference:15 to 25 years old)								
25 to 40 years old	0.82	60.8	0.83	47.4	0.913	86.7	0.96	96.9
40 to 50 years old	0.905	43.6	0.984	97.7	0.932	87.2	0.94	94.6
50 to 58 years old	0.508	71.8	0.624	88.9	0.659	85.7	0.91	97.7
More than 2 children in household 2004	0.996	94.1	0.986	98.3	0.998	81.4	0.912	48.9
Child under 3 in household	0.713	70.2	0.792	61.7	0.868	59.1	0.982	96
OECD equivalent net monthly household income 12/2004	0.798	26	0.917	83.5	0.852	66.7	0.975	91.8
Other earners in household	0.936	83.2	0.969	98.2	0.957	92.1	0.946	94.6
Migrant status (reference: other nationality, parents and interview language)								
German/other nationality, parents or interview language	0.867	-23.7	0.889	64.2	0.963	63.6	0.881	79.2
German nationality, parents and interview language	0.871	-1511.5	0.873	67.7	0.999	96.8	0.911	94.1
Qualification								
Qualification (reference: low) ^a								
Middle	0.981	94.6	0.983	97.6	0.801	82.8	0.931	94.7
High	0.885	86.8	0.871	70.3	0.982	82.4	0.873	61.2
Very high	0.614	69	0.945	94.6	0.886	81.6	0.976	98.3
Driver's license?	0.696	84.3	0.934	91.4	0.933	82.2	0.998	99.7
(Very) good speaking skills in German	0.51	66.3	0.822	48.3	0.726	75.6	0.89	92.6
Employment history								
Employed at 31 December 2004	0.902	49.5	0.936	92.2	0.958	92.2	0.962	96.5
Cumulated duration of employment ^b	0.773	42.5	0.981	97.8	0.98	86.5	0.985	98.4
Under 25*Cumulated duration of employment	0.983	83.3	0.742	85.2	0.986	97.8	0.876	87.2
Cumulated duration of unemployment	0.784	81	0.603	84.6	0.75	83.4	0.864	93.6
Under 25*Cumulated duration of unemployment	0.897	87.4	0.996	99.8	0.797	84.9	0.888	93.5
Cumulated duration of qualification	0.837	87.2	0.991	98.7	0.939	93.7	0.998	99.9
Under 25*Cumulated duration of qualification	0.86	88.3	0.863	92.6	0.82	88.2	0.854	92.8
Hourly net wage(missing are zero)	0.933	73.1	0.992	98.8	0.996	98.9	0.945	94.1
Indicator for missing net wage	0.923	62.8	0.933	92.5	0.959	93.6	0.958	96.6
Benefit receipt 12/2004 (reference: no benefits)								
Social assistance/Sozialhilfe	0.852	79.8	0.999	99.1	0.778	76.1	0.927	-70.2
Unemployment assistance/ Arbeitslosenhilfe	0.796	85.2	0.837	87.1	0.787	80.9	0.829	80.1
Unemployment benefit/ Arbeitslosengeld	0.696	84.3	0.934	91.4	0.933	82.2	0.998	99.7

Table A 4 continued
Balancing for each regressor in the propensity score estimation for caliper 0.01

	Quarter 1		Quarter 2		Quarter 3		Quarter 4	
	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)
Integration contract								
Integration contract	0.815	67.4	0.883	-98.9	0.787	83.1	0.855	74.2
Integration contract*month of integration contract	0.843	70.2	0.81	50.6	0.745	82.9	0.863	79.3
Regional labor market								
Share of service jobs 12/2004 per district	0.936	75.4	0.745	91.2	0.683	83.6	0.939	94.7
Ratio of offered jobs to registered unemployed in 2005 per district	0.965	-25.4	0.887	-124.9	0.895	82	0.974	95.5
Ratio 1-euro-jobs/stock of UB II-unemployed 2005	0.845	37.3	0.86	80.5	0.913	87.3	0.774	-157.7
Unemployment duration 12/2004 in months per district	0.893	60.6	0.937	96.4	0.931	79.7	0.892	85
Share long-term unemployed	0.906	62.2	0.854	93.2	0.961	89.8	0.864	82.9
Share women unemployed	0.862	64.7	0.861	85.8	0.893	87.4	0.908	44.1
Share foreigners unemployed	0.909	32.4	0.959	96.3	0.969	6.9	0.911	-63
Share under 25 y unemployed	0.989	48.4	0.771	90.1	0.877	80.1	0.9	-2316
Unemployment rate per district 12/2004	0.93	45.9	0.791	87.7	0.929	90.7	0.874	76.8
Sanction rate in county group (reference: very high: Rheinland-Pfalz, Baden-Württemberg)								
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	0.905	80.2	0.773	88.5	0.704	79.9	0.936	91.9
Middle: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	0.953	-7876.5	0.996	99.6	0.99	96.6	0.933	48.5
High: Thüringen, Bremen, Bayern	0.911	76.4	0.933	95.9	0.848	81.7	0.797	75.1
Living in West Germany	0.859	30.1	0.81	74.7	0.814	84.2	0.882	75.5

Source: "Life Situation and Social Security 2005". Note: Radius Matching with Caliper 0.01

Table A 5
Balancing for each regressor in the propensity score estimation for caliper 0.001

	Quarter 1		Quarter 2		Quarter 3		Quarter 4	
	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)
Demographic and household variables								
Gender=woman	0.952	74.6	0.855	51	0.838	82.2	0.817	82.7
Married man	0.916	47.6	0.86	70.1	0.991	98.8	0.726	85.7
Married woman	0.907	38.6	0.925	85.4	0.898	-109.7	0.806	57.3
Partner 2004 (reference: no partner)								
Employed partner	0.961	-446.3	0.976	87.5	0.833	73.1	0.945	89.9
Non-/unemployed partner	0.976	68	0.967	94.4	0.885	-257.7	0.94	81
Age 2004 (reference:15 to 25 years old)								
25 to 40 years old	0.896	77.5	0.781	31.2	0.879	81.6	0.922	93.7
40 to 50 years old	0.773	-35.1	0.927	89.2	0.921	85.2	0.932	93.7
50 to 58 years old	0.879	93.8	0.889	97	0.914	96.6	0.954	98.8
More than 2 children in household 2004	0.92	-4.6	0.906	88.5	0.988	-6	0.871	21.6
Child under 3 in household	0.876	87.8	0.884	79	0.924	76.5	0.929	84.4
OECD equivalent net monthly household income 12/2004	0.776	17	0.897	79.6	0.886	74.7	0.912	69.8
Other earners in household	0.942	84.8	0.768	86.5	0.849	72.5	0.705	68.7
Migrant status (reference: other nationality, parents and interview language)								
German/other nationality, parents or interview language	0.727	-160	0.987	95.8	0.95	50.1	0.689	42.3
German nationality, parents and interview language	0.784	-2626.3	0.924	80.4	0.943	-22.6	0.745	82.3
Qualification								
Qualification (reference: low) _a								
Middle	0.793	39.6	0.911	87.2	0.928	93.8	0.934	94.9
High	0.89	87.1	0.981	95.7	0.927	29.7	0.757	24
Very high	0.842	86.3	0.849	85.4	0.946	91.4	0.876	91.2
Driver's license?	0.887	71.7	0.697	58.5	0.975	93.3	0.972	93.8
(Very) good speaking skills in German	0.557	63.9	0.869	62.3	0.936	94.6	0.885	92.1
Employment history								
Employed at 31 December 2004	0.796	82.4	0.798	75.2	0.87	75.5	0.777	80.2
Cumulated duration of employment _b	0.501	37.9	0.926	91.4	0.999	99.5	0.94	93.6
Under 25*Cumulated duration of employment	0.244	-2.2	0.439	66.2	0.857	77.8	0.883	87.2
Cumulated duration of unemployment	0.549	74.6	0.997	99.9	0.991	99.4	0.891	94.8
Under 25*Cumulated duration of unemployment	0.959	97.7	0.655	82.8	0.953	96.5	0.982	99
Cumulated duration of qualification	0.925	95.3	0.872	79.9	0.825	81.6	0.79	87
Under 25*Cumulated duration of qualification	0.585	83.1	0.874	92.8	0.92	94.6	0.512	75.8
Hourly net wage(missing are zero)	0.974	97.3	0.802	70.5	0.913	76.5	0.898	89
Indicator for missing net wage	0.836	87.6	0.765	73.7	0.855	76.7	0.771	81.8

Table A 5 continued

Balancing for each regressor in the propensity score estimation for caliper 0.001

	Quarter 1		Quarter 2		Quarter 3		Quarter 4	
	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)
Benefit receipt 12/2004 (reference: no benefits)								
Social assistance/Sozialhilfe	0.455	-1421.9	0.861	-18.8	0.992	99.1	0.8	-379.2
Unemployment assistance/Arbeitslosenhilfe	0.502	30.1	0.889	91.2	0.989	99	0.762	71.6
Unemployment benefit/Arbeitslosengeld	0.887	71.7	0.697	58.5	0.975	93.3	0.972	93.8
Integration contract								
Integration contract	0.877	75.5	0.864	-134.9	0.987	99	0.917	85.1
Integration contract*month of integration contract	0.868	77.6	0.867	65	0.975	98.4	0.949	92.3
Regional labor market								
Share of service jobs 12/2004 per district	0.934	93.7	0.794	92.8	0.991	99.6	0.924	93.3
Ratio of offered jobs to registered unemployed in 2005 per district	0.887	81.6	0.953	7	0.971	94.9	0.604	24.9
Ratio 1-euro-jobs/stock of UB II-unemployed 2005	0.903	-15	0.853	79.7	0.998	99.7	0.925	17.2
Unemployment duration 12/2004 in months per district	0.794	68.3	0.889	93.7	0.948	84.5	0.85	79.1
Share long-term unemployed	0.845	78.1	0.851	93.1	0.988	96.9	0.81	76.1
Share women unemployed	0.942	61.3	0.864	85.8	0.993	99.1	0.846	4.4
Share foreigners unemployed	0.331	-1475.4	0.852	86.2	0.926	-122.6	0.777	-327.3
Share under 25 y unemployed	0.788	-5997.1	0.81	91.8	0.849	75.5	0.718	-7032.2
Unemployment rate per district 12/2004	0.563	6.4	0.845	90.9	0.995	99.4	0.958	92.2
Sanction rate in county group (reference: very high: Rheinland-Pfalz, Baden-Württemberg)								
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	0.94	91.4	0.999	100	0.976	98.4	0.862	82.1
Middle: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	0.923	33.2	0.907	90.2	0.875	56.9	0.897	18.6
High: Thüringen, Bremen, Bayern	0.822	76.5	0.807	88	0.98	97.7	0.992	99.1
Living in West Germany	0.773	47.4	0.834	77.7	0.957	96.4	0.929	85.1

Source: "Life Situation and Social Security 2005". Note: Radius Matching with Caliper 0.001

Table A 6
Balancing for each regressor in the propensity score estimation for caliper 0.0001

	Quarter 1		Quarter 2		Quarter 3		Quarter 4	
	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)
Demographic and household variables								
Gender=woman	0.972	84.9	0.532	-75	0.967	96.3	0.934	93.1
Married man	0.975	84.5	0.822	58.6	0.992	98.8	0.586	74.1
Married woman	0.898	32.9	0.906	80.5	0.96	13.3	0.488	-31.9
Partner 2004 (reference: no partner)								
Employed partner	0.979	-196.1	0.757	-37.2	0.925	87.7	0.856	70.6
Non-/unemployed partner	0.904	-27.8	0.645	32.3	0.978	30.4	0.983	94.1
Age 2004 (reference:15 to 25 years old)								
25 to 40 years old	0.807	57.5	0.596	-39.3	0.565	27.9	0.991	99.2
40 to 50 years old	0.646	-117.6	0.749	60.1	0.893	79.1	0.531	47.6
50 to 58 years old	0.82	90.5	0.845	95.4	0.771	90.6	0.979	99.3
More than 2 children in household 2004	0.923	3.1	0.862	82.4	0.988	-6.1	0.873	9.9
Child under 3 in household	0.817	81.4	0.264	-69.7	0.721	9.6	0.532	-15.5
OECD equivalent net monthly household income 12/2004	0.885	56.3	0.662	29.6	0.989	97.6	0.702	-18.6
Other earners in household	0.86	63.6	0.896	93.9	0.871	75.7	0.703	67.1
Migrant status (reference: other nationality, parents and interview language)								
German/other nationality, parents or interview language	0.807	-83.9	0.678	-6.4	0.749	-163	0.682	31.5
German nationality, parents and interview language	0.7	-3774.7	0.695	19	0.907	-105.9	0.894	91.8
Qualification								
Qualification (reference: low) ^a								
Middle	0.867	61.2	0.807	71.1	0.891	90.4	0.73	76.3
High	0.962	95.7	0.947	87	0.956	55.9	0.727	-5.6
Very high	0.595	66.6	0.707	69.4	0.809	69	0.842	86.3
Driver's license?	0.916	95.7	0.994	99.1	0.97	92	0.887	71.7
(Very) good speaking skills in German	0.671	78.5	0.716	15.3	0.883	89.4	0.557	63.9
Employment history								
Employed at 31 December 2004	0.903	49.5	0.681	59.3	0.76	55.8	0.796	82.4
Cumulated duration of employment _b	0.648	8	0.642	56	0.5	-272.2	0.501	37.9
Under 25*Cumulated duration of employment	0.994	94	0.408	66.4	0.807	68.5	0.244	-2.2
Cumulated duration of unemployment	0.743	77.3	0.799	92.2	0.992	99.5	0.549	74.6
Under 25*Cumulated duration of unemployment	0.638	50.4	0.951	97.7	0.636	75.4	0.959	97.7
Cumulated duration of qualification	0.611	67.9	0.53	17.8	0.641	63.1	0.925	95.3
Under 25*Cumulated duration of qualification	0.541	57.7	0.606	75.9	0.352	56.5	0.585	83.1
Hourly net wage(missing are zero)	0.852	20.5	0.663	47.1	0.843	58.4	0.974	97.3
Indicator for missing net wage	0.929	65.1	0.634	57.4	0.699	52.4	0.836	87.6
Benefit receipt 12/2004 (reference: no benefits)								
Social assistance/Sozialhilfe	0.72	61.1	0.876	-10.2	0.605	55.2	0.455	-1421.9
Unemployment assistance/Arbeitslosenhilfe	0.69	77.2	0.937	94.8	0.611	63.1	0.502	30.1
Unemployment benefit/Arbeitslosengeld	0.916	95.7	0.994	99.1	0.97	92	0.887	71.7

Table A 6 continued

Balancing for each regressor in the propensity score estimation for caliper 0.0001

	Quarter 1		Quarter 2		Quarter 3		Quarter 4	
	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)	P(t-Test with H0: no difference in means)	Reduction of absolute mean bias (in %)
Integration contract								
Integration contract	0.937	88.9	0.708	-440.1	0.723	77.1	0.877	75.5
Integration contract*month of integration contract	0.942	89.1	0.708	17.5	0.704	79.4	0.868	77.6
Regional labor market								
Share of service jobs 12/2004 per district	0.908	64.8	0.703	89.2	0.846	92.1	0.934	93.7
Ratio of offered jobs to registered unemployed in 2005 per district	0.939	-115.6	0.9	-107	0.562	4.3	0.887	81.6
Ratio 1-euro-jobs/stock of UB II-unemployed 2005	0.967	86.8	0.934	90.4	0.621	41.7	0.903	-15
Unemployment duration 12/2004 in months per district	0.996	98.4	0.915	94.9	0.758	25.4	0.794	68.3
Share long-term unemployed	0.977	90.9	0.974	98.8	0.695	15.3	0.845	78.1
Share women unemployed	0.882	69.5	0.813	79	0.927	91.3	0.942	61.3
Share foreigners unemployed	0.784	-63.4	0.42	37.4	0.94	-85.6	0.331	-1475.4
Share under 25 y unemployed	0.905	-357	0.759	89.3	0.66	40.8	0.788	-5997.1
Unemployment rate per district 12/2004	0.95	61	0.666	78.7	0.648	51.6	0.563	6.4
Sanction rate in county group (reference: very high: Rheinland-Pfalz, Baden-Württemberg)								
Low: Brandenburg, Berlin, Sachsen-Anhalt, Schleswig-Holstein, Mecklenburg, Hamburg	0.89	77	0.513	71.5	0.992	99.5	0.94	91.4
Middle: Sachsen, Saarland, Nordrhein-Westfalen, Hessen, Niedersachsen	0.978	-3734.1	0.509	41.5	0.93	75	0.923	33.2
High: Thüringen, Bremen, Bayern	0.926	80.6	0.878	92.5	0.873	84.3	0.822	76.5
Living in West Germany	0.846	22.6	0.6	41.2	0.783	81.2	0.773	47.4

Source: "Life Situation and Social Security 2005". Note: Radius Matching with Caliper 0.0001

Table A 7**Quality indicators for the matching by sample and caliper (outcome variable: probability to be employed at the time of the interview)**

(1a) Standardized bias before matching	(1b) Standardized bias after matching	(2a) Pseudo R ² before matching	(2b) Pseudo R ² after matching	(3a) P-value of test on H0: β if all covariates are zero (before matching)	(3b) P-value of test on H0: β if all covariates are zero (after matching)	Caliper (radius)		Quarter of uninterrupted UB II spell
						(1)	(2)	
8.731	2.307	0.047	0.006	0.175	1.000	(1)	0.01	1
	2.167		0.004			(2)	0.001	
	3.257		0.012			(3)	0.0001	
16.764	1.954	0.120	0.008	0.000	1.000	(1)	0.01	2
	2.879		0.009			(2)	0.001	
	3.395		0.021			(3)	0.0001	
10.750	1.797	0.055	0.003	0.001	1.000	(1)	0.01	3
	1.350		0.003			(2)	0.001	
	2.461		0.011			(3)	0.0001	
12.652	1.251	0.089	0.0012	0.000	1.000	(1)	0.01	4
	3.226		0.0077			(2)	0.001	
	5.375		0.0449			(3)	0.0001	

Notes: Matching for four subsequent quarters of uninterrupted UB II receipt. Matching Algorithm: Radius matching with different calipers defining the maximum absolute propensity score distance between treated and controls.

Source: IAB-Survey "Life situation and social security 2005".

Table A 8**Number of potential and matched treated and controls by quarter and caliper (outcome variable: probability to be employed at the time of the interview)**

Number of potential treatment units	Number of potential control units	Number of treatment units that are matched	Number of control units that are matched	Caliper (radius)		Quarter of uninterrupted UB II spell
				(1)	(2)	
105	8976	105	8975	(1)	0.01	1
		105	8892	(2)	0.001	
		103	4377	(3)	0.0001	
129	8433	129	8420	(1)	0.01	2
		124	7800	(2)	0.001	
		109	2512	(3)	0.0001	
147	7913	147	7911	(1)	0.01	3
		147	7383	(2)	0.001	
		139	3514	(3)	0.0001	
159	3324	159	3320	(1)	0.01	4
		153	2526	(2)	0.001	
		129	617	(3)	0.0001	

Notes: Matching Algorithm: Radius matching with different calipers defining the maximum absolute propensity score distance between treated and controls. The differences in the numbers of treated and controls compared to the matching with the outcome variable "net hourly reservation wage" result from the fact that the information on the employment status at the time of the interview is missing in the absent cases.

Source: IAB-Survey "Life situation and social security 2005".

Figures

Figure A 1
Distribution of the propensity score in quarter 1

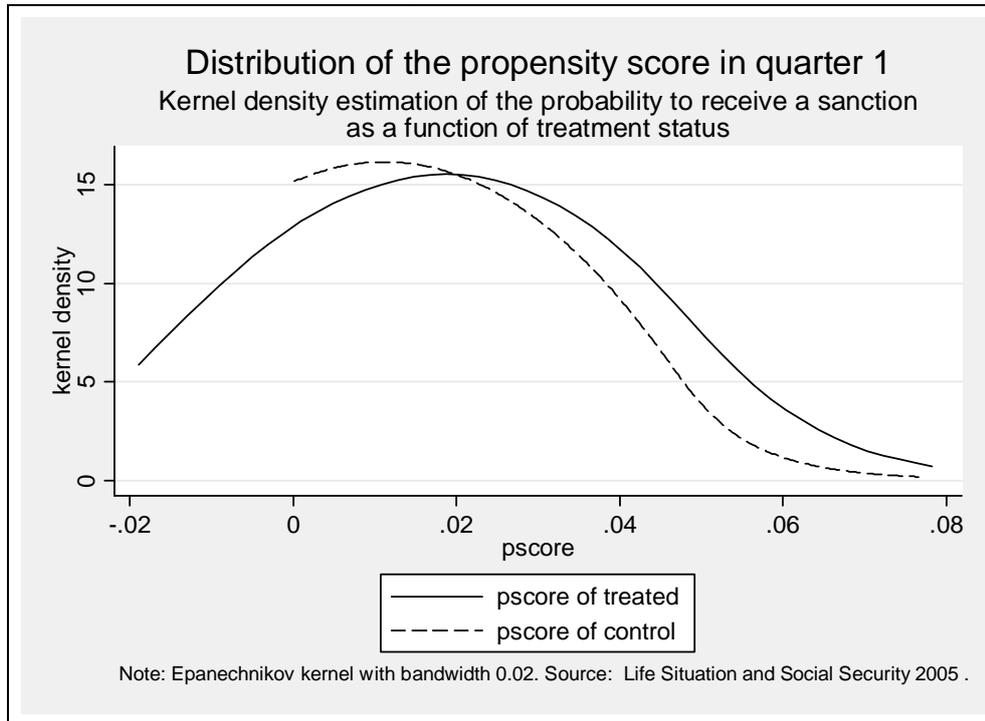


Figure A 2
Distribution of the propensity score in quarter 2

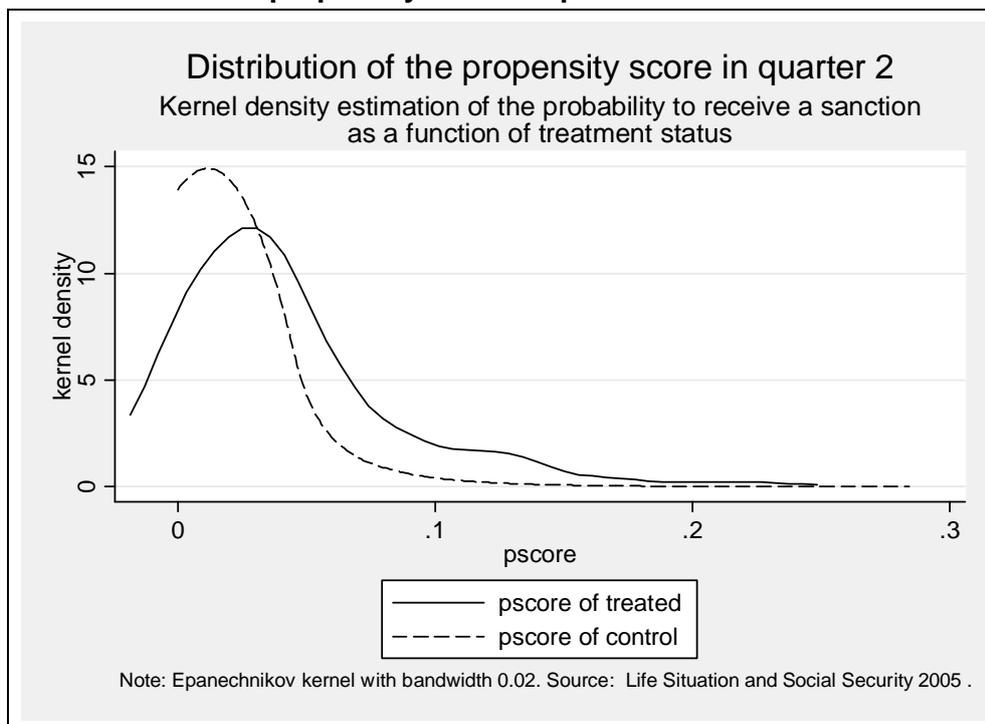


Figure A 3
Distribution of the propensity score in quarter 3

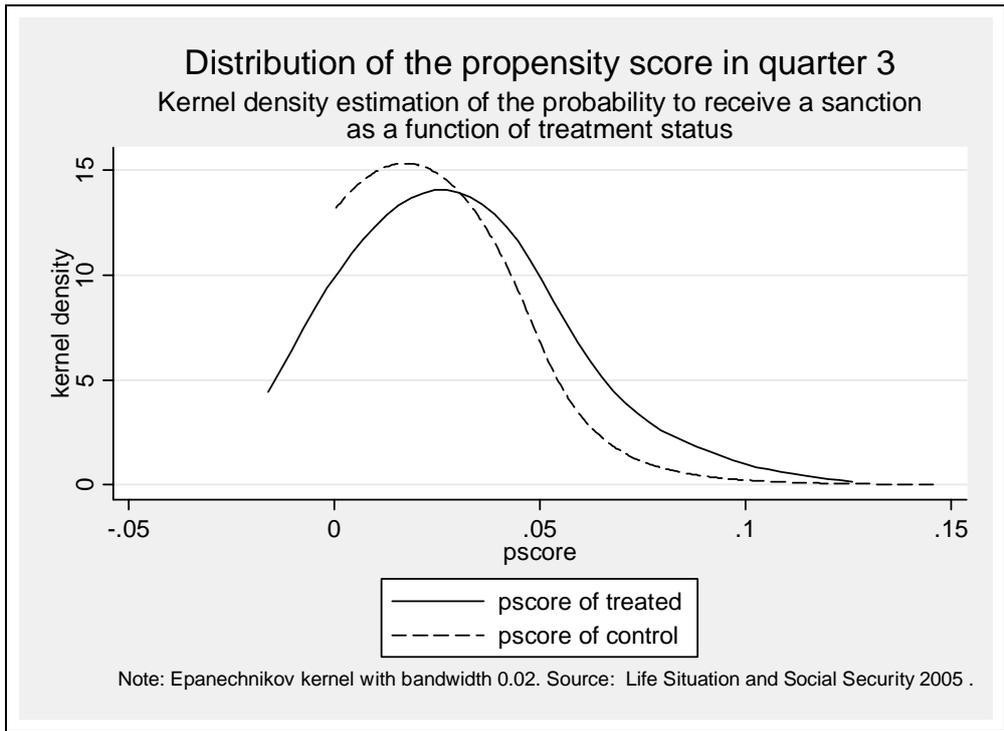
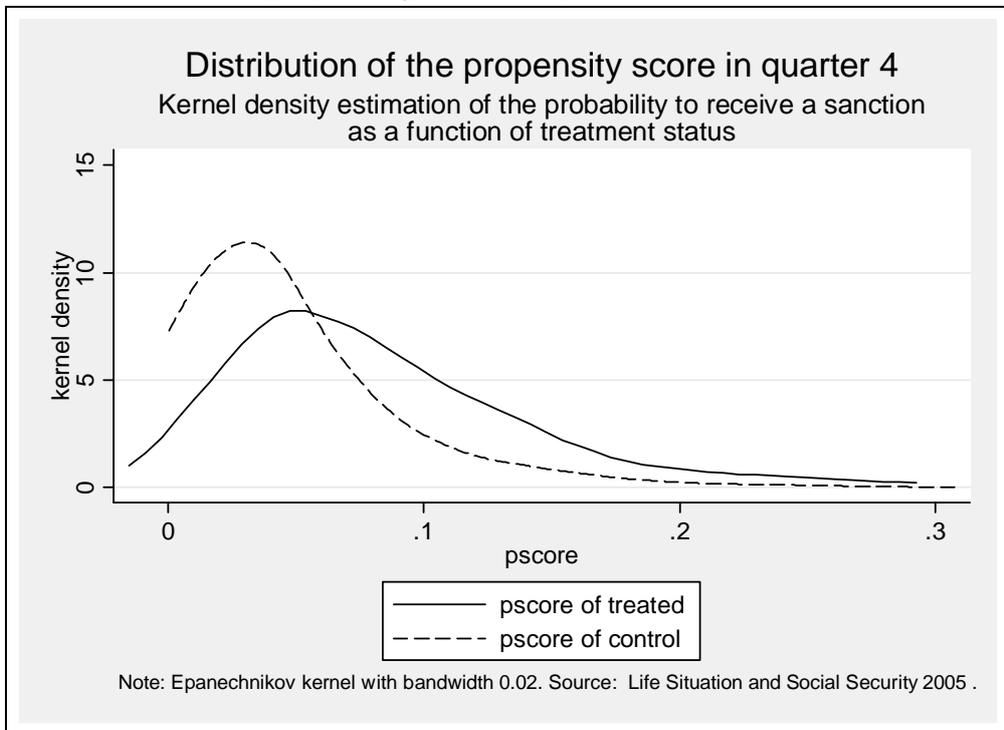


Figure A 4
Distribution of the propensity score in quarter 4



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Institute for Employment Research
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D-90478 Nuremberg

Editorial staff

Regina Stoll, Jutta Palm-Nowak

Technical completion

Jutta Sebald

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For further inquiries contact the author:

Julia Schneider

Phone +49.176.64 39 96 05

E-mail julia.schneider@iab.de