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Imposed benefit sanctions and the unemployment-to-employment transition: the German experience

IZA Discussion Papers, No. 3483

Provided in Cooperation with:
IZA – Institute of Labor Economics

Suggested Citation: Müller, Kai-Uwe; Steiner, Viktor (2008) : Imposed benefit sanctions and the unemployment-to-employment transition: the German experience, IZA Discussion Papers, No. 3483, Institute for the Study of Labor (IZA), Bonn, https://nbn-resolving.de/urn:nbn:de:101:1-2008052738

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DISCUSSION PAPER SERIES

IZA DP No. 3483

Imposed Benefit Sanctions and the Unemployment-to-Employment Transition: The German Experience

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Viktor Steiner

May 2008

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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Discussion Paper No. 3483
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IZA Discussion Paper No. 3483
May 2008

ABSTRACT

Imposed Benefit Sanctions and the Unemployment-to-Employment Transition: The German Experience*

We analyze the effect of imposed benefit sanctions on the unemployment-to-employment transition of unemployed people entitled to unemployment compensation on the basis of register data from the German Federal Employment Agency. We combine propensity score matching with a discrete-time hazard rate model which accounts for the dynamic nature of the treatment. We find positive short- and long-term effects of benefit sanctions which are robust for men and women in East and West Germany. The effects diminish with the elapsed unemployment duration until a sanction is imposed. The limited use of benefit sanctions can thus be an effective activation tool if they take place not too late in an individual's unemployment spell.

JEL Classification: J64, J65, H31

Keywords: benefit sanctions, unemployment transitions, German labor market reform, ex-post evaluation, propensity score matching, hazard rate model, unobserved heterogeneity

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* We would like to thank the Labor Market Policy and Employment unit at the Social Science Center Berlin for making this research possible within the project "Evaluation of the measures taken to implement the Hartz Commission proposals – The reorientation of job placement services". We also thank the Institute for Employment Research (IAB) and the German Public Employment Service (BA) for providing the data and the German Science Foundation (DFG) for partial funding under the project Ste 681/5-2. We are grateful to participants of the Economic Policy seminar at DIW Berlin for helpful comments on a previous version of this paper.

1 Introduction

There is a growing interest in the labor market and welfare effects of unemployment benefit sanctions over the last few years (see, e.g., Van den Berg and Van der Klaauw, 2005; Boone *et al*., 2001). Previous theoretical and empirical research has identified two different impacts of sanctions on the re-employment probabilities of unemployed benefit recipients. First, there is a direct incentive effect for those individuals who have actually received a benefit sanction, which influences the individual's reservation wage and search intensity leading to a higher transition rate into employment. This is called the *ex-post* sanction effect. Second, the incorporation of benefit sanctions into an unemployment insurance (UI) system poses a threat of getting a sanction that affects the reservation wage and search intensity of all unemployed benefit recipients. This so-called *ex-ante* sanction effect increases the transition rate into employment for all unemployed compared to a system without benefit sanctions by inducing a lower reservation wage and/or an increased search intensity leading to higher transition rates from unemployment to employment.

This paper focuses on the *ex-post* sanction effect, as most previous research on benefit-sanction effects has done following the "timing-of-events" approach initiated by Abbring and Van den Berg (2003), Abbring *et al.* (2005), and Van den Berg *et al.* (2004).[1] For example, using administrative data from the Netherlands Abbring *et al*. (2005) find that the transition rate from unemployment to employment roughly doubles upon the imposition of a sanction, which implies an outflow elasticity of 3 with respect to the benefit level. For Germany, there has hitherto been little published research on the effects of benefit sanctions on individual labor market behavior. Using data from the "Employment Sub-sample" (*Beschäftigtenstichprobe*) of the German Federal Employment Agency, Wilke (2004) provides some descriptive evidence showing that most of the benefit sanctions are withdrawn within a short period of time, and that of those who receive a sanction which is not revoked within a week 50% make an unemployment-to-employment transition. However, the data set used does not allow to properly discriminate between benefit sanctions in a narrower sense and minor penalties that are revoked in most cases. Utilizing the German Socioeconomic Panel and relying on a before-after-comparison of unemployment benefit recipients, Pollmann-Schult (2005) finds no significant effects of the 1997 reform which, inter alia, tightened the rules for suitable work on individual unemployment-to-employment transitions. This study does, however, not properly take into account potential selectivity effects and also ignores heterogeneity in the implementation of the legal reforms which is crucial for their labor market effect, as

[1] Using data from the Suisse public employment service (PES) and applying the "timing-of-events" approach, Lalive *et al*. (2005) distinguish between sanction effects related to the mere warning and the actual infliction of a sanc-

described below. Although benefit sanctions have also been part of recent labor market reforms in Germany aimed at strengthening the activation and re-integration of unemployed people (see, e.g., Kemmerling and Bruttel, 2006; Müller and Oschmiansky, 2006), there is currently no empirical analysis of their effects on unemployed people.[2]

This paper fills this gap and contributes to the evolving international literature on the labor market effects of benefit sanctions. In the next section, we provide some theoretical background on potential benefit-sanction effects on individual job search decisions and summarize relevant institutional regulations in Germany. The empirical methodology and the data set are described in Section 3. We combine propensity score matching with a discrete-time hazard rate model to estimate the benefit-sanction effects utilizing the best data on benefit sanctions currently available for Germany. The main advantage of our data set is that, due to its large size and precise information, it allows us to identify the imposition of actual sanctions and to estimate the ex-post effect with some precision despite a very small share of actually sanctioned people. Estimation results are presented and discussed in Section 4. We find strong positive effects on monthly re-employment transition rates of treated unemployed men and women in West and East Germany, especially if a sanction is imposed at a relatively early stage of an individual's unemployment spell. Although the magnitude of this effect on transition rates declines after the sanction has expired, it does not vanish resulting in significant long-term effects of benefit sanctions. Since only a very small share of all unemployed people actually receives a sanction, and of those a large share is imposed on already long-term unemployed people, the overall ex-post effect of sanctions on unemployment is only modest, however. Section 5 summarizes our main results and concludes.

2 Theoretical and Institutional Background

2.1 Benefit sanctions and job search

The potential impact of benefit sanctions on the unemployment-to-employment transition is usually discussed in the framework of the theory of optimal job search which implies that a more generous unemployment compensation system will increase the duration of unemployment (see, e.g., Mortensen, 1977; 1986). The economic rationale for this prediction is simple: Unemployment benefits act as a search subsidy, thus reducing the costs of leisure and increasing the reservation wage.

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tion, where the latter is interpreted as the *ex post* sanction effect.

[2] Schneider (2008) has analyzed sanction effects on the reservation wages of recipients of means-tested unemployment benefits, but could not find significant effects for this group of people. This group of people and the regulations for mean-tested unemployment benefits ("*Arbeitslosengeld II*") and sanctions differ from those relevant for recipients of unemployment insurance whom we analyze here.

This induces the unemployed to search longer for a job. It is relatively easy to incorporate benefit sanctions into the basic job search model assuming their effects are permanent and that the probability of the imposition of a sanction is known to the individual. Under a number of simplifying assumptions it can be shown[3] that

(i) benefit sanctions increase the unemployment-to-employment hazard rate of all unemployment benefit recipients right from the beginning of the unemployment spell (*ex-ante* sanction effect);

(ii) this ex-ante effect grows in the size of the penalty and the probability with which sanctions are imposed (sanction intensity);

(iii) at the moment the benefit sanction is imposed there is a spike in the unemployment-to-employment hazard rate (ex-*post* sanction effect);

(iv) given that a sanction has been imposed effectively, the unemployment-to-employment hazard rate increases with the penalty size but decreases with the intensity of monitoring.

These effects are fairly robust regarding functional form assumptions on search costs, search intensity, and job offer arrival rates as long as these factors do not depend on process time (unemployment duration), i.e. in the absence of duration dependence. Of course, the ex-ante sanction effect depends on its credibility, i.e. the deterrence established by the implementation of the legal regulations. In the model framework this relates to the monitoring intensity that, in turn, determines the sanction probability which has also assumed to be constant throughout an individual's unemployment spell in order to derive the results above. Furthermore, these ex-ante and the ex-post effects are only valid if stricter sanctions and monitoring do not induce substitution between informal and formal job search channels (see, e.g., Van den Berg and Van der Klaauw, 2006).

In addition to their mentioned incentive effects on job search decisions benefit sanctions also have implications for the optimal design of unemployment insurance. As shown by Boone *et al*. (2001) and Fredriksson and Holmlund (2005) incorporating benefit sanctions into UI systems may be more efficient than an across-the-board reduction of UI benefits that reduces the welfare of all benefit recipients. A system with sanctions induces similar employment incentives through the above-mentioned ex-ante effect but only affects the welfare of those unemployed who actually receive a penalty and are faced with reduced benefit payments. Depending on the level of monitoring and enforcement costs of sanctions such a system may improve social welfare.

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[3] Derivations are available from the authors upon request.

2.2 The implementation of benefit sanctions in Germany

Several types of benefit sanctions can be distinguished in the German unemployment insurance (UI) system, all of which suspend or reduce benefit payments for a defined period of time.[4] To impose a sanction, specific legally defined conditions have to be fulfilled. The Federal Employment Agency (FEA) executes those legal rules monitoring the 'willingness to work' and imposing the benefit sanctions (for details see Müller, 2007, or Müller and Oschmiansky, 2006). For the empirical analysis, we distinguish benefit sanctions ("*Sperrzeiten"*) imposed for several weeks from a number of other penalties with a maximum duration of seven days.[5] Until 2004 (when the observation period ends), benefit sanctions could be imposed for the following reasons:

(i) refusal of a "reasonable" job offer from the employment agency;

(ii) lack of willingness to participate in measures of active labor market policy (ALMP);

(iii) job termination initiated by the employee;

(iv) early dropout from programs of ALMP.

We restrict the following analysis to the first two types of penalties which correspond to the theoretically interesting cases of sanctions for already unemployed benefit recipients intended to reduce adverse incentive effects of the UI. These sanctions entail a loss of UI benefits for 12 (later 3) weeks. Sanctioned individuals could claim social assistance benefits for the sanction period and would still receive job search assistance by the FEA.

The legal rules for what constitutes a "reasonable" job offer and the size and duration of a possible benefit sanction have been repeatedly reformed (in most cases tightened) over the years (see Müller and Oschmiansky, 2006). In particular, the sanction rules were reformed in the wake of the first law of the 'Hartz reforms' which came into effect on January 1, 2003. This reform made the previously uniform length of sanctions (12 weeks) more "flexible". The first sanction carries now a penalty of 3 weeks, the second a penalty of 6 weeks and the third sanction a penalty of 12 weeks (without benefit payments); benefit eligibility is lost altogether after a sanction period of 21 weeks (i.e. following the third "Sperrzeit"). On the other hand, the burden of proof was reversed, i.e. the unemployed person has now to prove that an offered job is not reasonable, and the mobility requirements for singles were tightened. In addition to these changes, the FEA issued an internal

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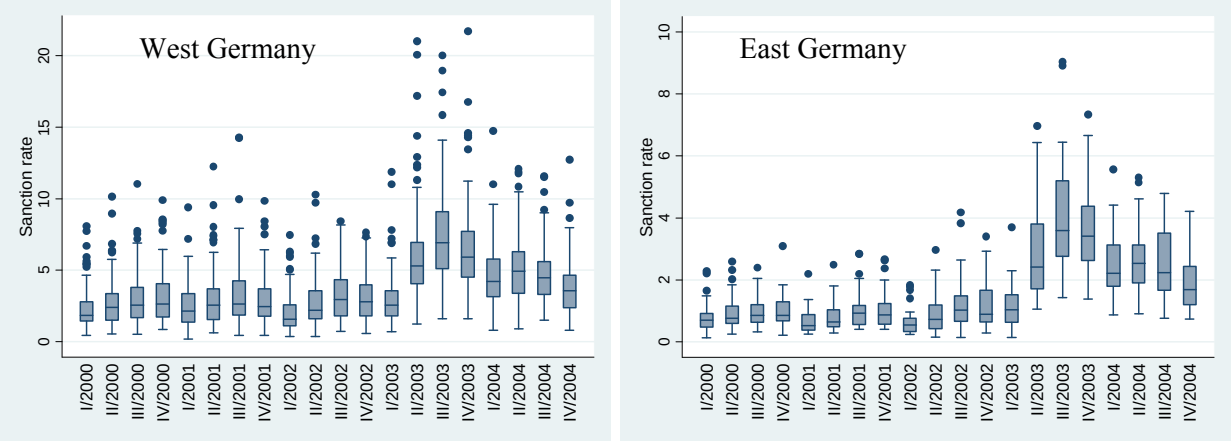
[4] In addition to UI there is a means-tested unemployment benefit (since 2005 called Unemployment Benefit II, previously called Unemployment Assistance; see Schmitz and Steiner, 2007), for which the conditions for impostion a benefit sanction are somewhat different from the ones for recipients of UI (see, e.g., Schneider, 2008).

[5] These include the penalties applied after a missed appointment with an employment officer (*"Säumniszeiten")* and those due to a delayed registration as unemployed (*"Minderungsbeträge").*

decree calling for a stricter activation and monitoring of the unemployed with a more frequent imposition of sanctions starting in the second quarter of 2003 (*cf.* Schütz, 2005).

Figure 1 shows that there is not only longitudinal but also considerable cross-sectional variation in the implementation of sanctions after the refusal of a job offer. In 2003 sanction rates varied between 1% and more than 10% in West German employment agencies. This regional variation can partially be explained by local labor market conditions, such as the level and duration of unemployment and the number of job vacancies, as well as factors within local employment agencies, e.g. their personnel resources (*cf.* Müller and Oschmiansky, 2006). Figure 1 also reveals a sharp increase in sanction rates in the second quarter of 2003 after the legal reforms of the sanction rules and the internal change of the sanction policy in the FEA, which indicates that the sanction policy of the FEA has been clearly tightened by the new labor law mentioned above.

Figure 1 Sanction rates after rejection of job offer or refusal to participate in ALMP program (in %) 2000/I – 2004/IV



Notes: Boxplots, 141 West and 40 East German employment agencies, quarterly sanction rates projected as yearly rates.
Source: Register data from the Federal Employment Agency and the authors' own calculations.

There is empirical evidence for substantial residual heterogeneity after controlling for regional labor market conditions and average reform effects, which suggests a certain degree of regional leeway in the sanction intensity at the level of public employment agencies. Whereas some agencies noticeably tightened their sanction policy following the legal reforms, other agencies reacted very modestly or did not react at all (*cf.* Müller and Oschmiansky, 2006). The individual probability of getting a benefit sanction is thus not only influenced by the individual behavior, centralized laws and regionally variable labor market conditions, but also by regional differences in the implementation of benefit sanctions.

3 Empirical Methodology

3.1 Identification of the ex-post benefit sanction effect

The evaluation literature offers various ways to identify the ex-post benefit sanction effect from data containing information on individual unemployment-to-employment transitions and the actual imposition of sanctions. The traditional *control function* approach to the evaluation of interventions (see, e.g., Heckman and Robb, 1985; Heckman, LaLonde and Smith, 1999) accounts for observed and unobserved characteristics in both the outcome and the selection equation. A possible drawback of the control function approach is that it heavily relies on functional form assumptions and/or exclusion restrictions. Ignoring ”selection on unobservables”, the average treatment effect of benefit sanctions on those affected could also be estimated using an extended matching estimator that accounts for the dynamic nature of treatment assignment and outcomes (see, e.g., Lechner, 2004; Sianesi, 2004; Fitzenberger *et al*., 2006). This method alone seems not appropriate for the analysis of sanction effects because unobservable individual factors are likely to play an important role in the sanction process, especially regarding its ex-post effect. Moreover, both the control function and the matching approach alone are not particularly well suited for duration data due to right-censoring and time-varying covariates.

To account for these factors and especially the dynamic nature of the treatment, i.e. the varying imposition of a benefit sanction over the course of the unemployment spell, Abbring and Van den Berg (2003, 2005) have developed the so-called ”*timing-of-events*” approach which adapts the control function technique to the analysis of the causal effects between duration variables. Assuming a mixed proportional hazards (PH) specification for both the unemployment-to-employment transition rate and the sanction rate, and invoking distributional assumptions on unobservables in both hazards, it can be shown that all parameters of the model are identified under the standard assumptions of a mixed PH model with exogenous time-varying covariates. In particular, the ex-post sanction effect is identified from the information on the ‘timing-of-events’ without exclusion restrictions on the set of observables included in the unemployment-to-employment hazard rate model.

Although this seems an attractive feature of this approach because it circumvents the problem of finding valid instruments for realized benefit sanctions, there are also some problems in practical applications. First, the identification of the causal effect rests on the somewhat restrictive PH assumption. Second, identification is based on the sequence of events only and does not use information about the length of the sanction period. Third, it is assumed that the effect of sanctions on the unemployment-to-employment hazard is constant once they are imposed. Fourth, the specification of the heterogeneity components seems restrictive. Fifth, it is not straightforward to include time-

varying covariates in this framework. And finally, estimation based on this approach is excessively time-consuming in case of very large data sets, as it is the case in our application.

For these reasons, we follow a somewhat different approach to identify the ex-post sanction effect by combining propensity-score matching and hazard-rate modeling (see, e.g., Hujer *et al*., 1998 for a similar approach). There are, in our view, several advantages of this approach: First, although similar to the timing-of-events approach, it is more flexible and requires less restrictive assumptions concerning the specification of the unemployment-to-employment hazard rate. Second, it is more compatible with the transitory and time-varying nature of benefit sanctions actually imposed in Germany. Third, it allows us to compute both short-run and long-term effects of benefit sanctions in a consistent and easier way than in the extended ("dynamic") matching approach. Finally, and in contrast to the matching approach, selection on unobservables can also be controlled for in the estimation. Of course, there are also disadvantages of this approach. In comparison to the matching approach, it requires additional distributional assumptions, and identification of the ex-post treatment effect does depend on functional form or some exclusion restrictions. As in the timing-of-events approach we also have to assume that a sanction can only be effective from the moment it is actually imposed, *i.e.* it has to be assumed that there are no anticipation effects.[6]

We control for selection on unobservables by way of a difference-in-difference estimator adapted to our hazard rate specification of the outcome variable. The short-run sanction effect, measured in the period when the sanction is de facto imposed, is thus computed by comparing the difference in hazards (or survival rates) between the sanctioned and non-sanctioned individuals with the same characteristics before and after the sanction is imposed. We interpret this difference-in-difference estimate as the average treatment effect on the treated. The long-run effect of sanctions can be computed in an analogous way by taking the respective difference-in-difference of periods before the sanction was imposed and some period after the sanction has been discontinued.

Our estimation strategy comprises two main steps: In the first stage, nearest-neighbor-matching on the estimated propensity score is carried out (see Section 3.3) yielding a sample of matched individuals who either received a benefit sanction in course of their unemployment spell, or not. In the second stage, a discrete-time hazard rate model is estimated for this matched sample

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[6] As in all mentioned approaches to program evaluation, we have further to assume that the benefit sanction does not affect the outcome variable – unemployment-to-employment transitions in our case – of people not being sanctioned. This standard assumption, which is known as the "stable unit treatment value" assumption (SUTVA) in the evaluation literature (see, e.g., Heckman *et al*., 1999), precludes the possibility that a sanction may affect the average outcome of the control group, which would result in biased estimates of average treatment effects.

that accounts for observed time-varying factors and unobserved heterogeneity (see Section 3.4). We start with the description of the data and the sample design used in the subsequent analysis.

3.2 Data and sample design

We merge micro data on individual unemployment transitions and the impositions of sanctions at the individual level with administrative data at the level of local Employment Agencies. These data are available to us on a monthly basis for the years 2000-2005 and contain detailed information about employment, unemployment, receipt of unemployment compensation, participation in ALMP, and the incidence of individual benefit sanctions. The individual level data come from the so-called 'Integrated Employment Histories' (*Integrierte Erwerbsbiographien*, IEB), provided by the Institute for Employment Research of the FEA (see Hummel *et al*., 2005). This is a very rich data set designed for program evaluation and derived form various data sources which are merged by means of the social security or the FEA customer number. Since it is a very large data set it is ideally suited for our analysis given the very small share of unemployed people who have received a benefit sanction. Table A1 in the Appendix informs about the structure of this data set. It is a spell data set that consists of information about different employment states and the transitions between those states on a daily basis. These data are available to us for inflow cohorts of the period January 2001 to December 2004 which we can observe up to December 2005. Samples were drawn separately for West and East Germany.

To identify the short-run and long-term effects of benefit sanctions, we restrict the analyses to unemployment inflows in the years 2001 and 2002 for which we observe up to 48 months after the entry date. To account for changes over time due to legal changes in the definition of sanctions (see Section 2.2) and to avoid left-censoring and initial conditions problems, we construct quarterly inflow samples which are randomly drawn from the population of unemployed individuals receiving unemployment insurance or assistance benefits for the years 2001 and 2002. We only consider unemployed people who have been receiving unemployment insurance or unemployment assistance benefits at the beginning of their unemployment spell, since only these individuals are either at risk of a sanction or at some point in time may actually receive a benefit sanction.[7]

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[7] Since the IEB is made up of different data sources, there are parallel, possibly conflicting spells in the raw data. In order to generate a consistent individual event history data set which uniquely defines labor market states at any point in time, parallel spells have to be prioritized and redundant spells deleted. With regard to the analysis of benefit sanctions this means that parallel spells which contradict the assumption of an individual being at risk of a sanction have to be recognized. If an unemployed, who is at first glance identified as being at risk of a sanction (i.e. the individual has an unemployment and a parallel benefit spell), but there is enough (contradicting) information in the data set suggesting that the person is simultaneously employed (with or without any kind of subsidy), the latter state is prioritized. The individual thus is de facto not at risk of receiving a benefit sanction. Consequently in such a case

The outcome variable of interest in the subsequent analysis refers to outflows from unemployment into employment including employment with wage subsidies and subsidized self-employment but excluding job creation schemes, structural adjustment measures and employment in subsidized temporary work agencies.[8] Note that outflows from benefit entitlement result in the right censoring of the spell – even if the unemployment spell continues – as those individuals no longer are at risk of being sanctioned. Very short unemployment spells (< 7 days) are deleted to avoid a too fragmented data set. Several unemployment spells which are split because of a temporary participation in an ALMP are pooled together and consecutive spells with identical labor market states are merged. Moreover, undefined short gaps between spells with identical labor market states are filled. The daily information regarding, respectively, the start and end of spells is recoded into monthly data. This makes it possible to merge the individual and the regionally aggregated data set. Descriptive statistics on unemployment-to-employment and unemployment-to-sanction transitions are presented in Table 1.

Table 1 Unemployment-to-employment and unemployment-to-sanction transitions, inflows 2001-2002

	Unemployment-to-employment transitions				**Unemployment-to-sanction transitions**			
	West Germany		East Germany		West Germany		East Germany	
Number of …	Men	Women	Men	Women	Men	Women	Men	Women
spells	97,857	63,500	96,329	61,203	99,066	64,079	97,752	61,800
individuals	96,311	62,575	94,837	60,483	96,330	62,597	94,857	60,499
spells/individuals	1.02	1.01	1.02	1.01	1.03	1.02	1.03	1.02
transitions	38,536	23,705	36,872	20,036	1,484	555	639	190
trans./spells (%)	39.37	37.33	38.28	32.74	1.50	0.87	0.65	0.31
% trans. within								
1-3 months	46.35	47.91	39.66	35.50	20.15	23.42	13.46	13.16
4-6 months	26.48	22.78	27.62	22.56	20.15	18.2	16.59	21.58
7-9 months	10.22	10.84	11.81	12.58	11.39	13.87	15.02	14.21
10-12 months	6.25	7.18	7.13	9.30	10.38	11.89	10.8	11.58
13-15 months	3.28	3.92	4.30	5.08	7.21	9.19	7.82	10.53
16-18 months	2.24	2.35	2.82	4.00	7.08	6.49	6.57	9.47
> 18 months	5.17	5.02	6.65	10.99	23.65	16.94	29.74	19.48

Source: Authors' calculations based on IEB data.

The inflow samples contain a total of about 100,000 spells for men and 60,000 for women per year. As shown by the spells/individuals ratio in Table 3, there are relatively few multiple unemployment

the employment spell is maintained whereas all spells with lower priority are re-written in the data set. So the extra information is not thrown away but can be used as a time-varying covariate in the analysis.

and sanction spells within the observation period. A transition into employment as defined above and within our evaluation period occurs for about 40 % of all people; only for women in the East this share is markedly smaller with circa 33 %. About two thirds of these transitions take place in the first six months of the unemployment spell with shares monotonically declining with the elapsed unemployment duration for all sub-groups.

A sanction which suspends benefit payments for at least 20 days is imposed in 1.5% (men) and 0.9 % (women) of all inflow spells in West Germany (Table 1). The incidence of a sanction is lower in the East, 0.7 % for men and 0.3 % for women.[9] Table 1 also shows that in West Germany a higher share of sanctions is imposed during the first six months of the unemployment spell. After that the transition rate steadily declines with the elapsed unemployment duration whereas the timing of a sanction is more equally distributed over the unemployment spell in East Germany. Table A2 in the Appendix documents that the incidence of sanctions also vary within regions and gender groups, in particular by level of qualification, age, and the presence of previous sanctions.

Our data contain a large number of control variables measured at the *individual level* including dummy variables for age, level of qualification, the presence of children in the household, disabilities and nationality, gender and region (East and West Germany). The IEB data also provide information on the daily rate of unemployment insurance or assistance benefits the unemployed receives as well as his or her previous cumulated duration of previous unemployment spells dating back to 1996. The latter is standardized by the total time the individual is observed in the data set. Definitions and summary statistics of these variables are given in Tables A1 and A2 in the Appendix.

We combine individual-level data with various indicators measuring the sanction intensity at the *regional level*, such as the number of all actually imposed sanctions in relation to all recipients of unemployment insurance and assistance benefits per quarter and employment agency, or the personnel resources of the agency. This information is obtained from regionally aggregated data at the level of employment agencies. To account for the tightness of regional labor markets, we use information on the unemployment rate, the average duration of unemployment and the vacancy rate, the rate of employment growth and the service sector share, all measured at the level of regional labor agencies. Definitions and summary statistics of these variables are also given in Tables A1 and A2 in the Appendix.

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8 Checks with alternative (more or less restrictive) definitions of employment yielded qualitatively identical results.

9 Abbring *et al. (*2005) report similarly small numbers for the Netherlands, whereas Lalive et al. (2005) report a markedly higher share of sanctions for Switzerland.

3.3 Propensity-score matching

In order to simplify the estimation in the first step we carry out a matching analysis which assigns the sub-group of treated individuals – those who receive a benefit sanction – a comparable control group on the basis of observable independent variables. At this stage, all matching variables either refer to the time the individual entered unemployment in 2001-2002 or, in case of an individual's previous unemployment or sanction history, to periods before this date.

We combine nearest-neighbor and caliper matching on the estimated propensity score (see, e.g., Caliendo and Kopeinig, 2008). The propensity score is estimated by probit models separately for men and women in, respectively, East and West Germany on the pooled sample of inflows in the period 2001-2002. Aggregate time effects are controlled by quarterly dummy variables. The same matching variables are included in probit models. In addition to the usual personal characteristics (see Table A3 in the Appendix), we also include an individual's (un)employment history and information on the individual incidence and cumulated durations of sanctions and other minor penalties prior to the beginning of the current unemployment spell. An individual's previous unemployment experience, which might capture unobserved individual characteristics affecting both program participation and an individual's labor market behavior, has proved an important matching variable in earlier evaluation studies for Germany (see, e.g, Lechner, 2000).

The distribution of propensity scores for both groups is compared in Figure A1 in the Appendix showing that the common support assumption holds for all sub-samples. We only keep observations within the caliper with radius < 0.005 (which is fulfilled for all cases, see Table A3) and allow for the possibility that a control unit is matched to different treated individuals.

Matching results are presented in Table A4 in the Appendix. We have computed the standardized bias (SB) as well as *t*-tests comparing the means of all matching variables for the treated and untreated. The SB between treated and untreated individuals is narrowed considerably for nearly all matching variables, and *t*-tests show no significant differences between the means of the matched samples. Only the regional sanction rate is still significantly higher for sanctioned individuals implying the treated live predominantly in employment districts with rather high sanction rates. This could induce a regional bias in the estimation of the sanction effect as the sanction rate (and individual sanction probability) is higher under favorable labor market conditions (*cf.* Müller, 2007). Therefore we will account for regional labor market conditions in the next stage of the analysis.

3.4 Specification and estimation of the hazard rate model

We specify a hazard rate model which is estimated for the matched sample of treated and non-treated individuals to take the dynamic nature of the imposition of sanctions as well as the importance of unobserved variables into account. As time is observed in discrete units and several time-varying explanatory variables are included in the analysis we model the unemployment-to-employment hazard rate in discrete time. To relate our specification to previous literature (e.g., Abbring *et al*., 2005; Lalive *et al.,* 2005), we use the discrete-time proportional hazard model[10]:

$$(1) \quad h_{tij}^{ue,k}\left(t, x_{tij}, x_{tj}, v_{ijl}\right) = 1 - \exp\left[-\exp\left(\kappa_t + v_{ijl} + \delta_{EP}' D_{tij}^{EP} + \beta_1' x_{tij} + \beta_2' x_{tj}\right)\right].$$

where t=1,2,…T_i indexes process time (measured in months) unemployment in individual's i unemployment spell k in j=1,2,…141(40) West (East) German employment districts. $h_t^{ue,k}(.)$ represents the discrete time hazard rate in individual's k-th unemployment spell at the end of the respective month and κ_t is the difference between the integrated baseline hazard rates at the beginning and the end of period t. D^{EP} consists of a set of dummy variables measuring the short-run and long-term ex-post sanction effect as described below. The vector x_{tij} contains observable time-varying individual characteristics, x_{tj} includes observable regional control variables measured at the level of employment districts, with β_1 and β_2 the corresponding vectors of coefficients.[11] v_{ijl} is a time-invariant unobserved individual effect. Following Heckman and Singer (1984), we specify v_{ijl} non-parametrically by a discrete distribution with a small number of mass points, M, as follows:

$$(2) \quad \sum_{l=1}^{M} \Pr\left(v_{ijl}\right) = 1, \ \sum_{l=1}^{M} v_{ijl} \Pr\left(v_{ijl}\right) = 0, \text{ and } E[v_{ijl} x_{ijt}] = E[v_{ijl} x_{jt}] = 0,$$

where $Pr(.)$ stands for probability and $E[.]$ denotes the mathematical expectations operator. Given there are relatively few multiple unemployment spells, we have to assume that this individual effect is constant both within and between multiple individual unemployment spells.

The *baseline* hazard rate which characterizes the form of *duration dependence* is modeled flexibly using a piecewise constant specification with a set of dummy variables (D_{jt}) for grouped months of unemployment duration:

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[10] See, e.g., Cameron and Trivedi (2005, Section 17.10).The discrete-time proportional hazard (PH) model is also known as the *complementary log-log (cloglog)* model in the statistical literature (see, e.g., Kalbfleisch and Prentice, 2002). We check the robustness of estimation results with respect to the underlying PH assumption by using an alternative specification of the hazard rate below

[11] Variables which do not vary over the unemployment spell are not included in x_{tij} and x_{tj} as they are already used as matching variables in the first stage propensity-score matching (see Section 3.3). Initially we included an error component accounting for unobserved regional heterogeneity, in addition to the set of regional control variables men-

$$(3) \quad h_0^{ue,k}(t) = 1 - \exp\left[-\exp\left(\gamma_1 D_{1t} + \gamma_2 D_{2t} + ... + \gamma_J D_{Jt}\right)\right],$$

where we distinguish months 1-4, 5-6, 7-8, 9-10, 11-12, 13-15, 16-18, 19-24 and > 24 months of unemployment duration.

The ex-post-sanction effect is also modeled flexibly by a set of dummy variables. For every sanctioned individual these dummies mark different time periods relative to the imposition of a sanction, and are always zero for all non-sanctioned individuals. We distinguish the following time intervals:

$$(4) \quad \delta'_{EP} D_{tij}^{EP} = \delta_0 D^{EP} \delta_1 D_{t=sanc}^{EP} + \delta_2 D_{t=sanc+1_3}^{EP} + \delta_3 D_{t=sanc+4_6}^{EP} + \delta_4 D_{t=sanc+>6}^{EP},$$

where the sanction dummy D^{EP} is equal to one for the whole observation period if an individual receives a sanction, and zero otherwise. $D_{t=sanc}^{EP}$ is a dummy for the length of an imposed sanction that can vary between one and three months. $D_{t=sanc+1_3}^{EP}$ is equal to one in the first three months after the imposition, $D_{t=sanc+4_6}^{EP}$ for the fourth till the sixth month and $D_{t=sanc+>6}^{EP}$ for more than 6 months after the sanction was imposed.

Defining the censoring indicator c_{ik}, which equals 1 if an indivudal's k-th unemployment spell is completed, and zero if the spell is right-censored, the sample likelihood function can be written as:

$$(5) \quad L = \prod_{i=1}^{n} \left[\sum_{l=1}^{M} \Pr\left(v_{ijl}\right) \prod_{k=1}^{K} \left(\frac{h_{lti}^{ue,k}}{1 - h_{lti}^{ue,k}} \right)^{c_{ik}} \prod_{k=1}^{t} \left(1 - h_{lki}^{ue,k}\right) \right].$$

Plugging the hazard function in equation (1) into equation (5) and using the specification of the unobserved heterogeneity component in equation (2), the (log) likelihood function can be maximized by standard numeric optimization routines.[12]

Estimation is based on the matched sample of unemployed people who received a sanction in the observation period (treatment group) and the control group as derived from the first-stage propensity-score matching described in Section 3.3.[13] The matched estimation sample refers to the pooled unemployment inflows in the period first quarter 2001 to fourth quarter 2002. We include quarterly inflow dummies to account for cohort effects in the estimation. The hazard rate model is estimated separately for men and women in East and West Germany because there are still marked

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tioned above, but the coefficients were not identified (as indicated by non-convergence in estimation).

12 The Gllamm package implemented in STATA (see Rabe-Hesketh *et al.,* 2004) was used for the estimation.

13 As a sensitivity check, we have also estimated the hazard rate model on the basis of a discrete-time random effects logit specification (see, e.g., Steiner, 2001), which yielded very similar results to the discrete-time PH specification.

differences by gender and in the structure of the labor markets between the two regions. Detailed estimation results are reported in Table A4 in the Appendix. Before we discuss sanction effects we briefly comment on the estimation results in general.

Since individual-level variables observed in our data set and used as matching variables in the first stage do not vary over the unemployment spell, they are not included in the hazard rate model. The only time varying individual-level control variable is the daily amount of unemployment benefits. We include this variable here because it may be correlated with the sanction dummies and also affect the unemployment-to-employment transition rate. At the regional level, we include several time-varying control variables which may affect the sanction behavior of regional labor agencies, and thus the incidence of sanctions at the individual level, as well as individual re-employment probabilities. However, most of these variables have either no statistically significant or only rather small effects on the hazard rates. The exception is the average unemployment duration which has a relatively strong negative effect on the re-employment probability of men in West Germany.

Even though we have matched on a large number of individual-level variables in the first stage, unobserved population heterogeneity remains quantitatively important in the estimation for West-German men for whom two heterogeneity groups, i.e. mass points, could be determined.[14] For the other groups estimated unobserved heterogeneity components turned out statistically insignificant or, in case of women in East Germany, were not identified and led to convergence problems.[15] However, estimated coefficients of the sanction dummies, which are of main interest in the following discussion, were robust in all estimations whether or not unobserved heterogeneity was controlled for in the hazard rate models.

4 Ex-Post Sanction Effects

Estimation results for the sanction dummies are summarized in Table A4, but are difficult to interpret on the basis of single coefficients since they interact with the baseline hazard rate (see Section 2.1).[16] Sanction effects can best be illustrated by plotting predicted hazard rates for the treat-

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[14] The number of mass points was chosen on the basis of the Akaike Information Criterion, defined as AIC = lnlik – k, where k is the number of parameters and lnlik is the log likelihood of the model at its maximum. The decision rule is to take the model with the highest AIC.

[15] This may be explained by the relatively small number of sanctioned individuals in East and also of women in West Germany complicating identification of unobserved heterogeneity components for women in West Germany.

[16] To check whether the results are sensitive to the assumption that the baseline hazard may differ between the treatment and the control group, we have also estimated the hazard models separately for the two groups. Estimated coefficients of the sanction dummies, as in fact for most other control variables, are fairly similar to those reported in Table A4 and predicted transition probabilities are nearly identical in the two model specifications. In addition to the overall constants in the two specifications, differences could be detected in some of the coefficients of the baseline hazards, but these differences turned out relatively small.

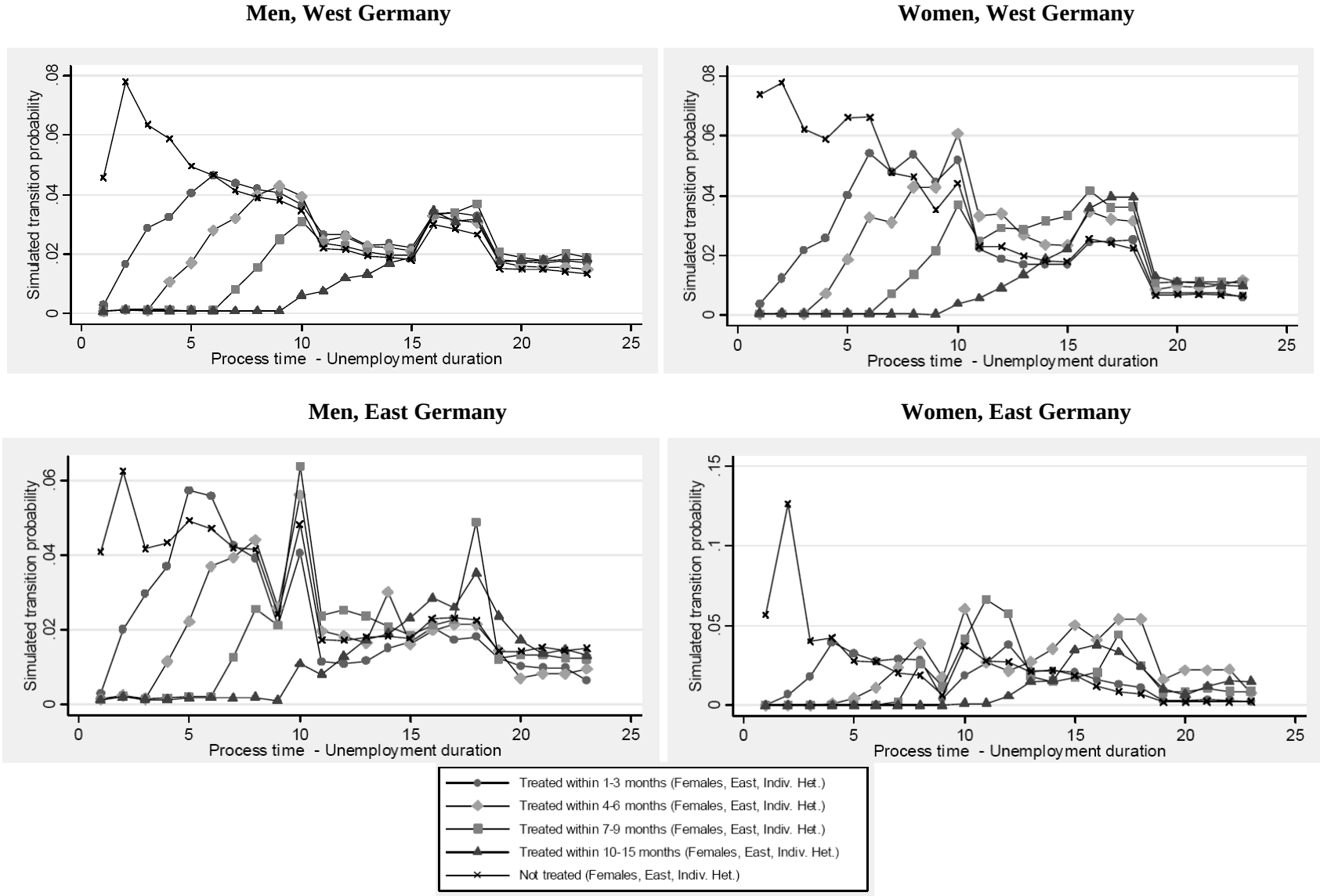
ment and the control group.[17] Figure 2 shows these average simulated unemployment-to-employment hazard rates for the group of sanctioned and non-sanctioned individuals and various sub-samples after they enter the state of unemployment. The timing of the sanction in the unemployment spell is taken into account to show the potential time proximity between the imposition of the sanction and changes in the unemployment-to-employment hazard and to get a clearer picture of the effects. We distinguish between different points in time when the sanction was invoked (within 1-3 months, 4-6 months, 7-9 months, and 10-15 months). In each of these intervals, the actual duration of the imposed sanction may vary between 1-3 months.

As shown by Figure 2 for the various groups, average hazard rates at the beginning of the unemployment spell are much higher for the control group of unemployed people who never received a sanction than for people in the treatment group, irrespective of the process time when they were eventually sanctioned. It is not surprising that re-employment hazard rates of treated people are virtually zero at the beginning of the spell and generally low before they actually receive a sanction, because otherwise they would obviously not have been sanctioned. The relatively large hazard rates observed for the control group at the beginning of the unemployment spell may seem surprising, however, given that we have matched on a large number of observable characteristics which are known to strongly affect individual re-employment probabilities, in particular age, level of qualification, and an individual's previous unemployment history. In our view, the difference in predicted hazard rates at the beginning of the unemployment spell between the treatment and control group, which is similar in magnitude for both men and women and in both regions, reflects the importance of unobserved differences between the two groups which our simple first-stage matching approach could not detect. As discussed in Section 3.1, this suggests to interpret the difference-in-difference in hazard rates between the two groups before and after a sanction is imposed as the average treatment effect on the treated.

Looking at the difference of the predicted hazards between treated and controls before and after the imposition, Figure 2 shows a fairly consistent pattern. For each group, average re-employment hazard rates jump up at the time the sanction is actually imposed or shortly thereafter, rising above the average hazard of the control group. There is a clear time relationship between the timing of the sanction and the change of the hazards as the pattern arises for all sub-groups irrespec-

[17] In case the hazard rate contains unobserved heterogeneity we calculate "average" hazards obtained by averaging the hazards for the heterogeneity groups with the respective probabilities to fall into one of the groups used as weights.

Figure 2 Average transition probabilities for sanctioned and non-sanctioned unemployed people, unemployment inflows 2001-2002



Source: Authors' calculations based on estimation results in Table A4.

tive when the sanction is invoked. Interestingly, the effect seems to diminish the later the sanction is imposed during the unemployment spell. After the sanction has expired the hazard rate tends to decline but stays at a substantially higher level than at the beginning of the unemployment spell and remains above the average hazard of the non-sanctioned for most of the analyzed sub-groups.

In contrast, the average hazard rate for the control group declines sharply with process time which holds for both men and women and for both regions.[18] Thus, irrespective when the sanction is actually imposed, the hazard rates of the treatment and the control group seem to converge with increasing duration of unemployment. Given the very large differences in hazard rates at the beginning of the unemployment spell, this pattern implies quite large ex-post sanction effects for all groups considered in Figure 2, given the difference-in-difference interpretation of these effects.

To measure longer-term sanction effects we calculate the difference-in-difference of conditional survival rates in unemployment which can be derived from the parameter estimates of the discrete-time hazard rate model.[19] The first difference refers to average survival rates at different months of unemployment duration (process time) and the first month of the spell for each of the two groups. The second difference is taken from the differences between the two groups. Table A6 in the Appendix shows difference-in-difference estimates of the ex-post sanction effect by the timing of the sanction. The same information is presented in graphical form in Figure 3 below.

The difference-in-difference estimates of survival rates in unemployment are positive and quite large at the beginning of the unemployment spell for all groups. For example, for men in West Germany the difference of survival rates between the treatment and the control group is about 8 percentage points after three months, if the sanction was received within the first three months. For longer durations, and depending on the timing of the sanction, differences in survival rates become substantially smaller and may even turn negative. For instance, for men in West Germany differences in survival rates between the treatment and the control group disappear after 12 months, if the sanction was imposed early in the unemployment spell. Given the difference of the survival rate after three months compared to the first month for the treatment group was, on average, substantially higher than for the control group, this implies that the early sanction has substantially reduced the survival rate in unemployment after 12 months, i.e. had a positive effect on the treatment group's average re-employment probability. In the long-run, an early imposed sanction even leads

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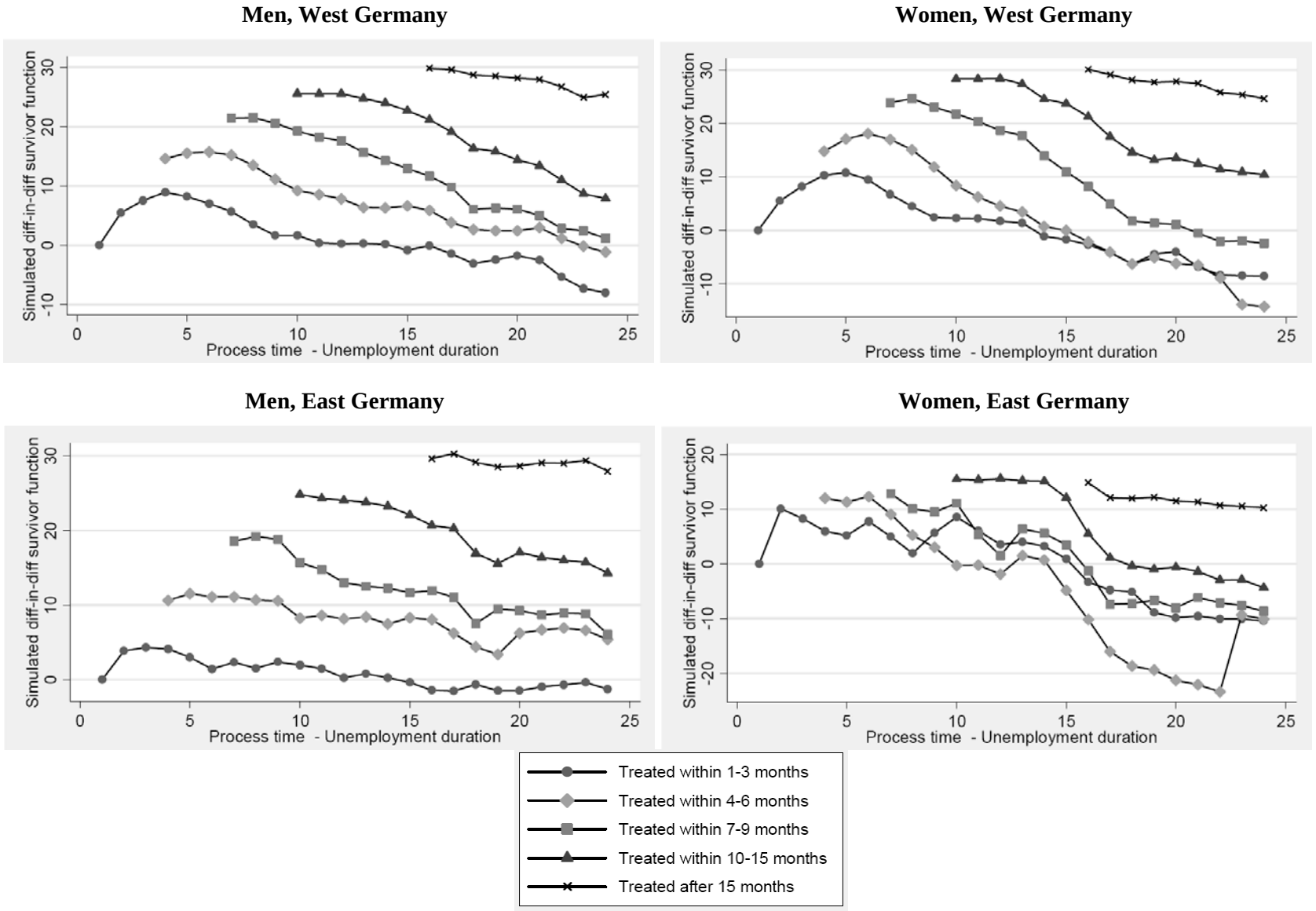
[18] Since these hazard rates are "averaged" over heterogeneity groups, this pattern need not reflect negative duration dependence but could also be the result of a "sorting" process (see, e.g., Steiner, 2001). The spikes at certain unemployment durations, in particular in months 9-10 for men in East Germany and for women in both regions, observed for both the treatment and the control groups are consistent with previous research on the "benefit-entitlement effect" induced by the eligibility rules inherent in the German UI system; see, e.g., Schmitz and Steiner (2007).

[19] The survivor function and the hazard rate are related by $S(t) = \prod_{k=1}^{t}(1-h_k)$.

Figure 3 Difference-in-difference estimates of average survival rates, relative to first unemployment month (in percentage points)



Source: Authors' calculations based on estimation results in Table A4.

to a smaller 24-months' survival rate for the treatment than for the control group, implying a strongly positive long-term effect on re-employment probabilities after the sanction is actually imposed. For those cases the *overall* employment probability is higher compared to the counterfactual situation of them not being sanctioned.

As shown by Figure 3, the long-term sanction effect strongly depends on the timing of the sanction. Benefit sanctions which are imposed 15 months or later after the beginning of an individual's unemployment spell do not reduce survival in unemployment. This is partially related to the relatively short-time period we observe after the imposition of a sanction at this stage. Still, an intervention after 6 months seems to yield much larger effects on the re-employment probability than imposed after, e.g., 15 months. This relationship varies also across treatment groups. Generally the effects are stronger for women than for men. In case the sanction is imposed between months 7- 9, for instance, there is still a large positive treatment effect after 24 months for women in both regions, whereas this effect is rather small for men, especially in East Germany.[20] A possible explanation for these differences, which would be consistent with the theoretical predictions derived in Section 2.1, is that the ex-post sanction effect is decreasing in the monitoring intensity. As sanction rates are, on average, markedly higher for men than for women, and are generally higher in West Germany compared to the East (see Table 1), one would expect the effects to be relatively small for West-German men actually receiving a benefit sanction.

[20] For women in East Germany, these effects should be interpreted with some caution given the small number of treated person. This also explains the spike in the difference-in-difference survival rate for East-German women who received a sanction between months 4-6 or after a spell duration of 23 months when only very few persons of the treatment group were still unemployed.

5 Summary and Conclusion

We have analyzed the effects of imposed benefit sanctions on the transition rate from unemployment to employment taking advantage of a rich data set for Germany. These data combine individual spell data containing precise information about the timing and duration of both unemployment and sanctions with data at the level of local labor markets. We combine propensity-score matching with a discrete-time hazard rate model of unemployment-to-employment transitions that takes into account the dynamic nature of the treatment, short-term and long-run effects of sanctions are derived on the basis of difference-in-difference estimates of predicted average transition and survival rates.

We find consistent and strongly positive short-term effects on the re-employment probabilities of all analyzed sub-groups at the time or shortly after a sanction is imposed. The effects remain positive after the benefit sanction has expired suggesting lasting and substantial cumulative sanction effects. Sanctions seem particularly effective if imposed at an early stage of an individual's unemployment spell. The later a sanction is imposed, the weaker are the sanction effects. The measured effects vary by gender and between East and West Germany, which seems consistent with theoretical predictions of job search models incorporating benefit-sanction effects. These effects depend on the validity of the assumptions underlying our difference-in-difference identification of the average treatment effect. In particular, these imply that the large differences in the outcome variable that remain after matching on a large number of observable individual characteristics and which may result in the future imposition of a benefit sanction do not change during the course of the unemployment spell, or are effectively controlled for in the hazard rate model.

From a policy perspective, our empirical results do suggest positive effects on the re-employment of unemployed people actually receiving a sanction in the early stage of the unemployment spell. However, the share of actually sanctioned people in Germany is rather small, and sanctions are, perhaps for good reasons of efficiency and equity, not concentrated on the first few months of unemployment when they would be relatively effective. Thus, actually imposed sanctions seem to have had only a minor overall effect of unemployment in Germany. The overall effect of benefit sanctions on unemployment could well be substantial, though, due to the "anticipation effect" which may induce more intensive job search of unemployed people in anticipation of a sanction. Given all unemployed are potentially affected by the type of sanctions we have been analyzing here, we could not determine the importance of this effect and, thus, the overall sanction effect.

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Appendix

Table A1 Definition of individual and regional control variables

Variable	Definition
Individual level covariates	
Previous sanctions	Dummy for incidence of sanctions before individual enters the sample
Other penalties	Dummy for incidence of minor penalties (others than the sanctions analyzed) before individual enters the sample
Previous unemployment	Share of time spent in registered unemployment in relation to total time the individual is observed before entering the sample
University qualification	Dummy for high education
No qualification	Dummy for low education
< 25 years of age	Dummy for age group of younger people
> 50 years of age	Dummy for age group of older people
Immigrants	Dummy for immigrants
Disabled	Dummy for disabled
Children <3 years	Dummy for child younger than three years of age in household
>2 Childs in household	Dummy for more than two children in household
Daily benefit payment	Daily benefit payment measured in Euros
Regional labor market conditions and local labor market policy (measured at level of regional employment agencies)	
Unemployment rate	Number of registered unemployed divided by the civilian labor force (in percent)
Average duration of unemployment	Average duration of unemployment in weeks. Approximation: $unemployment\,duration = \frac{average\ stock\ of\ unemployed}{0{,}5*(entries + exits\ from\ unemployment)} * 52$
Share of benefit recipients	Average stock of unemployment insurance recipients divided by average stock of all unemployed (in percent)
Vacancy rate	Ratio of newly registered unemployed and newly registered vacancies (in percent)
Employment growth	Percentage change in number of employed with mandatory social security coverage in December of current year in relation to number in December of preceding year (in percent)
Service sector share	Number of employed service-sector persons with mandatory social security coverage divided by total number of employed persons with mandatory social security coverage (in percent)
Population density	Population density of the district served by the public employment agency in the year 2000
Size of the secondary labor market	Inflow of persons in job creation schemes (ABM) and structural adjustment measures (SAM) divided by newly unemployed persons (in percent)
Personnel resources	Average stock of unemployed divided by the number of job counselors ('Arbeitsvermittler' and 'Arbeitsberater') in employment agencies
Sanction rate	Absolute number of imposed sanctions in regional employment office in relation to all unemployment insurance and assistance benefit recipients

Table A2 Descriptive statistics: means (standard deviations), inflows 2001 and 2002

Variable	West Germany				East Germany			
	Men		Women		Men		Women	
	Treated	Control	Treated	Control	Treated	Control	Treated	Control
Inflow quarters								
2001/I	0.1299	0.1354	0.1161	0.1144	0.1117	0.1384	0.1560	0.1070
2001/II	0.1147	0.1204	0.1636	0.1388	0.1197	0.1155	0.1489	0.1431
2001/III	0.1212	0.1116	0.1530	0.1348	0.1383	0.1155	0.0709	0.1375
2001/IV	0.1234	0.1267	0.0923	0.1157	0.1117	0.1246	0.1135	0.1232
2002/I	0.1245	0.1389	0.1293	0.1144	0.1622	0.1409	0.0851	0.1094
2002/II	0.1429	0.1256	0.1135	0.1388	0.0957	0.1181	0.1135	0.1395
2002/III	0.1331	0.1122	0.1319	0.1355	0.1649	0.1163	0.1773	0.1310
2002/IV	0.1104	0.1292	0.1003	0.1076	0.0957	0.1307	0.1348	0.1093
Individual- level covariates (time invariant)								
Previous sanctions	0.0271	0.0086	0.0185	0.0040	0.0160	0.0050	0.0142	0.0027
Other penalties	0.0227	0.0063	0.0132	0.0023	0.0186	0.0051	0.0071	0.0020
Prev. unemployment	0.1838	0.1532	0.1304	0.1178	0.2272	0.1878	0.2136	0.2500
University qualification	0.0065	0.0304	0.0079	0.0371	0.0053	0.0331	0.0142	0.0362
No qualification	0.4188	0.3420	0.3852	0.3082	0.2846	0.1548	0.1986	0.1520
< 25 years of age	0.3820	0.2064	0.3799	0.1911	0.4282	0.2158	0.5390	0.1817
> 50 years of age	0.0249	0.1547	0.0475	0.1604	0.0532	0.1936	0.0922	0.2134
Immigrants	0.1742	0.1320	0.1504	0.0923	0.0372	0.0305	0.0142	0.0234
Disabled	0.0087	0.0316	0.0185	0.0282	0.0133	0.0214	0.0000	0.0254
Children <3 years	0.0595	0.0709	0.1029	0.0752	0.0399	0.0453	0.0567	0.0658
>2 Childs in household	0.0433	0.0442	0.0237	0.0308	0.0160	0.0220	0.0000	0.0353
Daily benefit payment	21.01	26.99	15.46	18.86	17.52	22.07	12.72	17.57
	(7.02)	(10.49)	(6.32)	(8.42)	(5.84)	(7.60)	(5.35)	(7.19)
Regional-level covariates (measured at time of inflow)								
Sanction rate	0.8761	0.7018	0.8956	0.7206	0.2975	0.2314	0.2827	0.2360
	(0.4587)	(0.4057)	(0.4845)	(0.4151)	(0.1870)	(0.1524)	(0.1866)	(0.1534)
Personnel resources	416.78	424.27	411.97	422.99	466.74	489.03	476.28	489.09
	(66.00)	(71.16)	(71.14)	(72.06)	(80.20)	(80.78)	(75.69)	(81.38)
Secondary labor market	1.25	1.25	1.29	1.22	7.27	7.22	7.63	7.47
	(1.03)	(0.96)	(1.01)	(0.96)	(3.60)	(3.40)	(4.05)	(3.53)
Unemployment rate	8.38	8.67	8.35	8.50	19.32	19.78	19.85	19.65
	(2.36)	(2.44)	(2.55)	(2.46)	(3.88)	(3.79)	(4.31)	(3.93)
Avg. unemployment duration	3.28	3.29	3.27	3.29	3.48	3.51	3.49	3.50
	(0.16)	(0.18)	(0.18)	(0.18)	(0.12)	(0.12)	(0.12)	(0.12)
Vacancy rate	63.44	61.66	67.87	63.00	38.99	38.52	38.77	39.34
	(32.26)	(30.34)	(36.79)	(31.37)	(12.73)	(12.60)	(13.01)	(12.84)
Service sector share	68.70	68.26	67.77	68.19	78.06	78.29	78.30	78.40
	(10.00)	(9.77)	(11.94)	(10.34)	(6.48)	(6.93)	(6.38)	(7.04)
Employment growth	-0.87	-0.86	-1.71	-1.06	-0.91	-1.74	-1.45	-1.70
	(7.19)	(6.55)	(11.28)	(7.90)	(11.35)	(8.57)	(8.88)	(8.55)

Source: Authors' calculations based on register data from the Federal Employment Agency.

Table A3 Estimation of the propensity score, inflows 2001 and 2002

Variable	West Germany				East Germany			
	Men		Women		Men		Women	
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.
Inflow quarters (base: 2001/I)								
2001/II	-0.0568	0.0520	0.0472	0.0732	0.0851	0.0847	-0.1927	0.1273
2001/III	-0.0220	0.0529	0.0210	0.0769	0.0712	0.0818	-0.3612	0.1419
2001/IV	0.0070	0.0539	-0.0357	0.0859	0.0359	0.0999	0.0349	0.1517
2002/I	-0.0242	0.0527	0.0882	0.0795	0.0801	0.0974	-0.1013	0.1623
2002/II	0.0287	0.0524	-0.0223	0.0808	-0.1406	0.1025	-0.1578	0.1487
2002/III	0.0271	0.0562	0.0271	0.0836	0.0551	0.1053	0.0009	0.1615
2002/IV	-0.0371	0.0605	0.0369	0.0922	-0.1459	0.1239	0.0779	0.1829
Individual level covariates (time invariant)								
Previous sanctions	0.2687	0.0910	0.4042	0.1709	0.2264	0.1821	0.5515	0.2929
Other penalties	0.3083	0.1000	0.4786	0.2065	0.1736	0.1775	0.4881	0.3736
Previous unemployment	0.2075	0.0701	0.2357	0.1072	0.3743	0.1060	0.1617	0.1547
University qualification	-0.2526	0.1359	-0.3096	0.1814	-0.2852	0.3024	0.2293	0.2339
No qualification	0.0324	0.0280	0.0944	0.0406	0.1211	0.0495	-0.1081	0.0935
< 25 years of age	0.1595	0.0322	0.2585	0.0442	0.2214	0.0500	0.4561	0.0822
> 50 years of age	-0.4895	0.0685	-0.3620	0.0798	-0.3403	0.0770	-0.0979	0.1012
Immigrants	0.0479	0.0358	0.1259	0.0556	0.0887	0.1284	-0.0738	0.3017
Disabled	-0.4035	0.1203	-0.0790	0.1365	-0.0902	0.1520	[1]	
Children <3 years	0.0279	0.0547	0.1295	0.0652	-0.0596	0.1111	-0.0401	0.1232
>2 Childs in household	0.0924	0.0654	-0.1450	0.1196	-0.1001	0.1652	[1]	
Unemployment benefits	0.0161	0.0077	0.0017	0.0113	0.0044	0.0146	-0.0295	0.0211
Unempl. benefits squared	-0.0008	0.0002	-0.0004	0.0003	-0.0007	0.0004	0.0000	0.0007
Regional level covariates (measured at inflow time)								
Personnel resources	-0.0005	0.0003	-0.0004	0.0005	-0.0013	0.0004	-0.0001	0.0006
Sec. labor market	0.0219	0.0173	-0.0010	0.0005	-0.0010	0.0092	-0.0025	0.0142
Unemployment rate	-0.0911	0.0460	0.0142	0.0243	-0.1199	0.0950	0.0816	0.1576
Unempl. rate squared	0.0031	0.0023	0.0058	0.0646	0.0030	0.0024	-0.0013	0.0040
Unemployment duration	7.4539	2.3701	0.0004	0.0033	2.2756	9.3384	1.4024	14.4011
Unempl. duration squared	-1.1114	0.3660	-0.4863	3.2043	-0.3614	1.3561	-0.3283	2.0871
Vacancy rate	-0.0001	0.0006	0.0669	0.4970	-0.0004	0.0036	0.0046	0.0055
Service sector share	0.0044	0.0018	0.0017	0.0008	0.0050	0.0045	0.0006	0.0070
Employment growth	-0.0032	0.0024	0.0012	0.0026	-0.0201	0.0199	-0.0193	0.0316
Constant	-14.3365	3.7656	-0.0033	0.0028	-4.7307	15.7193	-4.6714	24.2693
Log likelihood	-4,934.4817		-2,213.1757		-2,031.4263		-802.4433	
Observations	99,971		64,632		85,666		53,399	
Treated	924		379		376		141	
Treated on support	923		378		331		123	
Absolute difference propensity score-propensity score of nearest neighbor								
Maximum	0.0025		0.0003		0.0002		2.85e-05	
Mean	3.52e-06		2.89e-06		1.71e-06		7.72e-07	

Notes: [1] Variable predicted outcome perfectly – cases omitted from matching procedure.

Source: Authors'calculations based on register data from the Federal Employment Agency.

Table A4a Standardized Bias (SB) and t-Test, West Germany, inflows 2001-2002

Variable	Men				Women			
	SB		t-Test		SB		t-Test	
	Before	After	Before	After	Before	After	Before	After
Previous sanctions	13.4	-3.3	5.69	-0.56	13.8	-5.0	4.45	-0.54
Other penalties	13.8	-0.9	6.27	-0.15	12.4	6.0	4.32	0.82
Previous unemployment	15.4	-4.6	4.81	-0.93	7.2	6.2	1.41	0.85
University qualification	-17.9	0.0	-4.24	0.00	-19.8	1.8	-3.00	0.45
No qualification	15.8	-3.1	4.88	-0.66	16.5	2.2	3.28	0.30
< 25 years of age	39.2	4.1	13.09	0.82	42.4	-7.8	9.23	-0.97
> 50 years of age	-46.7	0.0	-10.91	0.00	-37.6	5.3	-5.97	1.12
Immigrants	11.8	0.3	3.80	0.06	18.0	5.7	3.92	0.73
Disabled	-16.4	-0.8	-3.98	-0.24	-6.4	5.3	-1.14	0.91
Children <3 years	-4.6	-5.7	-1.35	-1.21	9.8	0.9	2.04	0.12
>2 Childs in household	-0.4	2.1	-0.13	0.47	-4.3	0.0	-0.79	0.00
Daily benefit payment	-67.0	-1.3	-17.28	-0.35	-45.6	1.4	-7.82	0.22
Daily benefit payment squared	-66.3	-0.6	-15.83	-0.22	-45.2	2.1	-7.19	0.42
Regional sanction rate	40.2	28.6	12.98	6.12	38.5	32.5	8.11	4.29
Personnel resources	-10.9	-1.5	-3.19	-0.34	-15.2	-9.5	-2.93	-1.27
Secondary labor market	0.3	-3.9	0.09	-0.86	7.6	3.7	1.50	0.50
Unemployment rate	-12.0	-1.8	-3.56	-0.41	-5.6	-5.5	-1.10	-0.73
Unemployment rate squared	-12.1	-1.1	-3.56	-0.24	-4.2	-5.6	-0.83	-0.75
Avg. unemployment duration	-5.6	0.5	-1.60	0.12	-11.7	-4.7	-2.30	-0.62
Avg. unemployment duration squared	-6.2	0.6	-1.78	0.14	-11.7	-5.0	-2.29	-0.67
Vacancy rate	5.7	2.3	1.78	0.49	14.1	8.6	2.98	1.15
Service sector share	4.3	-2.9	1.31	-0.62	-3.8	-2.1	-0.79	-0.28
Employment growth	-0.2	-0.8	-0.07	-0.17	-6.7	-2.2	-1.59	-0.28

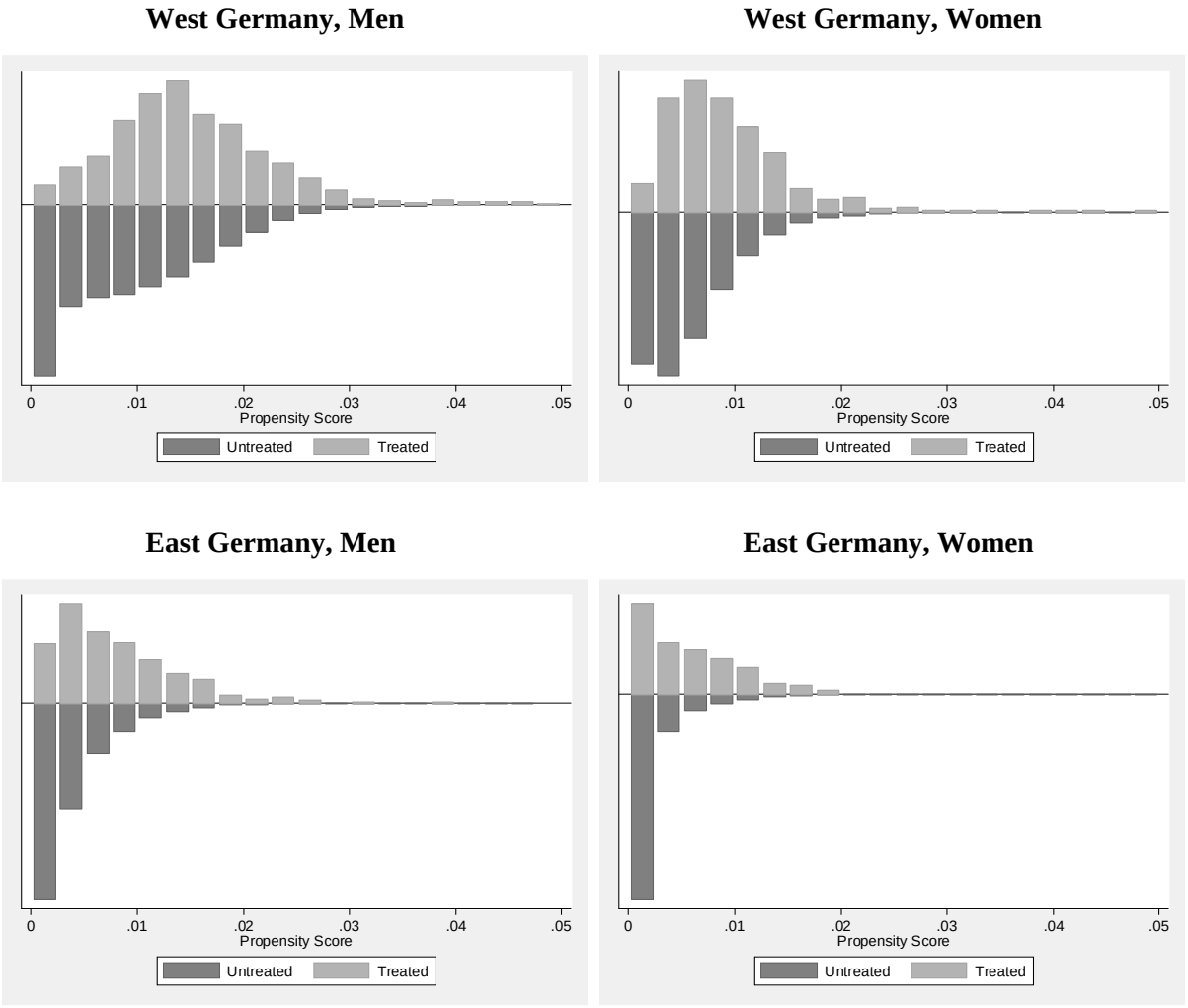
Source: Authors' calculations based on register data from the Federal Employment Agency.

Table A4b Standardized Bias (SB) and t-Test, East Germany, inflows 2001-2002

Variable	Men				Women			
	SB		t-Test		SB		t-Test	
	Before	After	Before	After	Before	After	Before	After
Previous sanctions	10.6	0.0	2.78	0.00	14.3	0.0	3.04	0.00
Other penalties	10.3	6.1	2.66	0.71	9.0	11.6	1.65	1.00
Previous unemployment	16.0	2.7	3.00	0.33	-20.5	10.6	-2.30	0.88
University qualification	-19.2	-2.5	-2.62	-0.58	-6.6	5.7	-0.67	0.58
No qualification	31.1	-2.3	6.51	-0.27	-0.3	9.7	-0.03	0.80
< 25 years of age	50.1	-7.9	10.04	-0.93	81.0	5.5	10.37	0.38
> 50 years of age	-43.9	-7.5	-6.50	-1.25	-33.3	-9.1	-3.22	-0.80
Immigrants	4.9	0.0	0.98	0.00	-4.0	0.0	-0.40	0.00
Disabled	-3.9	0.0	-0.66	0.00	-22.3	-22.6	-1.75	-1.75
Children <3 years	-7.5	9.6	-1.26	1.62	-0.6	-6.6	-0.07	-0.49
>2 Childs in household	-7.5	4.7	-1.21	0.82	-27.7	-24.6	-2.17	-2.03
Daily benefit payment	-67.3	2.2	-11.21	0.32	-74.8	0.5	-7.43	0.04
Daily benefit payment squared	-64.0	1.7	-9.50	0.30	-68.7	1.0	-6.13	0.12
Regional sanction rate	43.9	21.9	8.79	2.68	29.8	22.2	3.66	1.63
Personnel resources	-27.7	2.1	-5.01	0.28	-16.3	-11.4	-1.75	-0.89
Secondary labor market	0.0	-0.5	0.00	-0.06	6.5	-8.4	0.77	-0.61
Unemployment rate	-15.5	-2.0	-2.92	-0.26	4.2	-18.2	0.47	-1.48
Unemployment rate squared	-14.6	-1.9	-2.72	-0.24	4.2	-17.6	0.47	-1.41
Avg. unemployment duration	-25.5	-1.7	-4.75	-0.22	-13.5	-12.5	-1.51	-0.96
Avg. unemployment duration squared	-25.5	-1.8	-4.75	-0.23	-13.7	-12.7	-1.52	-0.97
Vacancy rate	9.3	-1.7	1.67	-0.21	-2.8	21.6	-0.31	1.75
Service sector share	-3.5	5.8	-0.65	0.74	-0.9	12.4	-0.10	1.01
Employment growth	1.3	-2.3	0.23	-0.30	-7.6	19.2	-0.81	1.51

Source: Authors' calculations based on register data from the Federal Employment Agency.

Figure A1 Distribution of Propensity Scores for treatment and control group



Note: Only propensity scores <= 0.5 reported in graph.

Source: Authors' calculations based on register data from the Federal Employment Agency.

Table A5 Discrete-time proportional hazards model; transitions from unemployment to regular employment, inflows 2001- 2002

	West Germany				East Germany			
	Men		Women		Men		Women	
Variable	**coeff.**	**s. e.[1]**	**coeff.**	**s. e.[1]**	**coeff.**	**s. e.[1]**	**coeff.**	**s. e.[1]**
Quarter dummies								
II/2001	-0.363	0.215	-0.425	0.300	-0.562	0.312	0.824	0.567
III/2001	-0.298	0.210	0.019	0.297	-0.782*	0.373	0.929	0.756
IV/2001	0.048	0.199	-0.792*	0.374	-0.615	0.372	0.286	0.589
I/2002	-0.217	0.213	-0.279	0.316	-0.217	0.320	0.390	0.714
II/2002	-0.362	0.217	-0.523	0.326	-0.450	0.403	0.337	0.707
III/2002	-0.365	0.224	-0.529	0.339	-0.394	0.366	1.062	0.592
IV/2002	-0.208	0.237	-0.794*	0.379	-0.651	0.437	0.848	0.678
Baseline hazard								
1 month	0.180	0.406	1.153	0.613	1.279	0.771	2.022	1.253
2 months	0.793*	0.391	1.259*	0.609	1.786*	0.724	2.883*	1.215
3 months	0.643	0.393	1.066	0.615	1.385	0.725	1.711	1.279
4 months	0.609	0.394	1.027	0.618	1.458*	0.709	1.760	1.273
5-6 months	0.526	0.379	1.166*	0.584	1.647*	0.656	1.314	1.246
7-8 months	0.448	0.380	0.869	0.588	1.528*	0.652	1.263	1.243
9-10 months	0.452	0.296	0.658	0.442	0.999*	0.457	0.293	0.699
11-12 months	-0.072	0.295	0.255	0.452	0.745	0.499	1.629	1.102
13-15 months	-0.130	0.400	0.157	0.625	0.784	0.685	1.105	1.230
16-18 months	0.361	0.395	0.510	0.628	1.003	0.697	0.410	1.303
19-24 months	-0.185	0.414	-0.706	0.729	0.615	0.709	-0.226	1.357
> 24 months	-0.183	0.442	-0.133	0.691	0.970	0.715	-1.224	1.545
Sanction dummies								
Sanction imposed yes/no	-4.209**	0.510	-5.059**	1.035	-3.403**	0.579	-3.509**	0.737
During impos. of sanction	3.132**	0.527	3.790**	1.060	2.768**	0.502	1.477	1.237
Months after imposition								
1-3 months	4.207**	0.530	4.951**	1.053	3.522**	0.477	4.211**	0.762
4-6 months	3.986**	0.549	5.259**	1.076	3.200**	0.563	1.929*	0.910
> 6 months	4.114**	0.561	4.908**	1.101	2.803**	0.549	4.090**	0.858
Benefit payments	0.028**	0.008	0.022	0.013	0.058**	0.018	0.021	0.028
Regional level covariates								
Sanction rate	0.128	0.087	-0.109	0.145	0.482	0.296	-0.987	0.633
Personnel resources	0.002	0.001	-0.002	0.002	-0.004*	0.002	-0.004	0.003
Sec. labor market	0.040	0.056	-0.061	0.092	0.005	0.023	0.027	0.035
Unemployment rate	0.025**	0.037	0.061	0.057	0.059	0.045	0.001	0.077
Avg. unempl. duration	-1.408*	0.482	-0.594	0.717	2.195	1.169	-1.840	1.965
Vacancy rate	0.005*	0.002	-0.005	0.004	0.025**	0.009	-0.013	0.017
Service sector share	-0.023**	0.008	0.001	0.011	-0.014	0.018	-0.003	0.028
Employment growth	0.017	0.011	0.004	0.013	-0.038	0.073	-0.029	0.109
Constant	0.086	1.434	-4.598	101.791	-12.609**	3.499	3.101	5.813
Unobserved heterogeneity[1]								
ε_1	-2.740**	0.887	-11.418	315.625	-0.449	0.752		
ε_2	1.331		3.681		0.920			
"Probability coefficient"	-0.722**	0.249	-1.132*	0.439	0.717	2.188		
Prob(ε_1)	0.327**		0.244*		0.672			
Prob(ε_2)	0.673		0.756		0.328			
Log likelihood	-2,119.83		-873.61		-773.18		-239.24	
Akaike Info. Criterion								
Number of observations	20,451		8,011		8,376		3,384	

Notes: [1] White/Huber robust standard errors. [2] Convergence was not achieved in model with unobserved heterogeneity; results reported for model without heterogeneity terms. * Significance at 5% level. ** Significance at 1% level.

Source: Authors' calculations based on register data from the Federal Employment Agency .

Table A6 Difference-in-difference estimates of average survival rates, relative to first unemployment month (in percentage points)

	Men, West Germany				Women, West Germany			
Process time (months)	3	6	12	24	3	6	12	24
Timing of sanction								
1-3	7.59	7.07	0.28	-7.96	8.24	9.52	1.73	-8.57
4-6	11.71	15.71	7.86	-1.16	11.35	18.12	4.57	-14.29
7-9	11.71	20.31	17.57	1.21	11.35	23.36	18.75	-2.46
10-15	11.73	20.35	25.57	7.90	11.34	23.35	28.45	10.37
> 15 months	11.73	20.35	27.72	25.41	11.34	23.34	30.09	24.70
	Men, East Germany				**Women, East Germany**			
Process time (months)	3	6	12	24	3	6	12	24
Timing of sanction								
1-3	4.35	1.42	0.27	-1.25	8.26	7.74	3.49	-10.46
4-6	8.79	11.13	8.16	5.38	10.71	12.27	-1.93	-10.16
7-9	8.82	16.89	13.02	6.11	10.71	13.83	1.52	-8.68
10-15	8.85	17.01	24.05	14.31	10.71	13.83	15.59	-4.34
> 15 months	8.72	16.67	26.16	27.93	10.71	13.83	16.35	10.25

Source: Authors' calculations based on estimation results in Table A4.