Tightening the use of unemployment benefit sanctions - does it speed up the exit to work?

Evidence from a policy change*

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Unemployment insurance (UI) benefit sanctions are intended to decrease moral hazard in the UI system. Sanctions are assumed to affect behaviour of the unemployed not only after they are imposed (*ex post* effect) but also by the mere threat of being sanctioned, which is referred to as *ex ante* effect. Empirical evidence on the *ex ante* effect is scarce. We estimate duration models of the individual duration from the start of UI benefit receipt until the exit to work and exploit a large set of regional- and time-specific information, as well as a setting provided by a policy change in order to study the *ex ante* effect. According to our preliminary results, the tightened use of sanctions after the policy change speeded up the exit to work.

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1 Introduction

While providing financial backing for unemployed individuals seeking for a new job, unemployment insurance (UI) systems at the same time have to struggle with the problem of moral hazard in form of shirking. Shirking in the UI system typically arises if unemployed individuals intentionally prolong their unemployment duration due to relatively generous benefits. UI systems have introduced benefit sanctions in form of a cut of the UI benefits in case of detection of moral hazard behaviour.

Research on the effectiveness of UI benefit sanctions primarily focused on *ex post* effects by analysing whether the actual imposition of a sanction has an impact on the reemployment rate. Of at least equal policy relevance, is the *ex ante* effect which predicts that the increase of the sanction rate would decrease unemployment by increasing the exit rate to work of those not sanctioned or not sanctioned yet. The *ex ante* effect can be deduced from a job search theory model as presented in Boone and van Ours (2006): in a UI benefit system without sanctions, the search intensity is lower than in a system with the possibility of being sanctioned. A higher search intensity increases the transition rate to employment.

In general, without observing a system without sanctions, the *ex ante* effect is difficult to assess empirically. Empirical strategies in order to mimic different sanction regimes can either use regional differences in the enforcement of sanctions or exploit sanction policy changes. Yet, one has to be aware of potential problems which might affect identification: first, the variation of sanction rates between regions might be endogenous if they are correlated with factors that determine the exit out of unemployment, e.g. the local intensity of the use of training programs. Second, variations in sanction rates over time (meaning that everyone is confronted with the same sanction policy system before as well as after a certain "reform shock") might be parallel to time trends or other institutional changes. Consequently, rather than reflecting a causal relationship, a bivariate correlation between sanction rates and reemployment rates might reflect spurious correlation.

In order to study the *ex ante* effect we exploit first, a large set of regional- and time-specific information, and second, a unique setting that provides us with features of a natural experiment: due to a policy change at the start of 2003, the sanction regime became stricter and local sanction rates on average increased significantly.[1] Yet, some local employment agencies considerably intensified the use of sanctions while others did not or to a lower extent only. Although variation in local sanction rates is mainly driven by local labour market conditions, controlling for indicators of local heterogeneity, we argue that i) remaining variation and ii) the locally different increase in the sanction rates after the policy change are statistically independent of the exit rate to work, as both are driven by differently managed local employment offices.

There is only little empirical evidence on the *ex ante* effect. Beside experimental evidence (Boone, Sadrieh, and van Ours (2009)), Lalive, van Ours, and Zweimüller (2005) and Arni, Lalive, and van Ours (2009) found evidence of an *ex ante* effect of benefit sanctions in the UI systems of Switzerland and Svarer (2007) for Denmark: the higher the local intensity of monitoring and sanctioning, the higher the exit rate to work. Van den Berg and Vikström (2009) on the other hand conclude, that the monitoring system in Sweden does not exert a strong *ex ante* or threat effect of sanctions.

In a recent study among welfare benefit recipients in German, Boockmann, Thomsen, and Walter (2009) use regional specific sanction rates of welfare recipients in order to identify the local average treatment effect (LATE) via instrumental variable (IV) estimation as measure of the effectiveness of an intensified use of sanctions. According to their results, tightening the use of welfare benefit sanctions, would be effective in terms of reducing unemployment duration. Yet, estimating the LATE one not only focusses on a different group[2], but one also has to assume that there is no direct causal effect on of the sanction rates on the unemployment exit and thus, no *ex ante* effect which, is not consistent with the theoretical model in Abbring, van den Berg, and van Ours (2005).

Shedding some more light on the *ex-ante* effectiveness of unemployment benefit sanctions, we pose the following research questions: a) does the local sanction rate affect the

[1] The local sanction rate is defined as number of UI benefit sanctions imposed divided by the stock of UI benefit recipients.

[2] The LATE represents the effectiveness of an increase of sanction rates for those not sanctioned due to a lax local sanction regime (i.e. the lucky ones). Whereas we look at the effectiveness of an increase of the local sanction rates for *all* UI benefit recipients.

individual transition to employment? b) Did the sanction policy change in the German UI system speed up the exit to work? Our study is based on register data of the Federal Employment Agency (FEA) of an inflow sample into UI benefit receipt between May 2001 and April 2003. We use duration analysis and account for local- and time-variations in sanction rates in order to identify the effect of the local sanction rates. By introducing interaction terms of local sanction rates and a controlling for the time from the policy change on we assess whether tightening the sanction regime affected the hazard to employment. This paper is the first to exploit a policy change to study the *ex ante* effect. In the remainder, we describe the institutional setting and its specific features used in our analysis (section 2); and present the method and the data (section 3). In section 4 we report preliminary results and summarize them (section 5).

2 Institutional setting and descriptives

There is large variation between UI systems in different countries regarding not only the rigour of the sanction legislation but also the degree of the use of sanctions by caseworkers. As our empirical analysis is based on German data, we describe the German institutional setting. Generally, an unemployed receiving UI benefits can be punished by either a short-term or a long-term sanction, depending on the type of infringement. Refusing a training program or a job vacancy proposed by the caseworker usually leads to a long-term sanction (twelve, respectively since 2003, three weeks), while missing an appointment with the caseworker causes a short-term sanction (two weeks).[3]

Concerning the imposition of sanctions, Müller and Oschmiansky (2006) discuss four levels of determinants: first, the institutional setting, i.e. the legal framework; second, the local labour market context; third, the individual behaviour; and finally, the sanction policy of the local employment agency (sanction regime). In Germany, there are around 178[4] local employment agencies of the Federal Employment Agency (FEA). Müller and

[3]Note that during our observation period, additional reasons why a sanction can be imposed are a) quitting a job voluntarily or b) dropping out of a training program. Yet, these sanctions are not imposed during an open unemployment spell, i.e. while the individual is actively searching for a job.

[4]The number of local employment offices varies over time; during our observation period, we have data on 180 local employment offices.

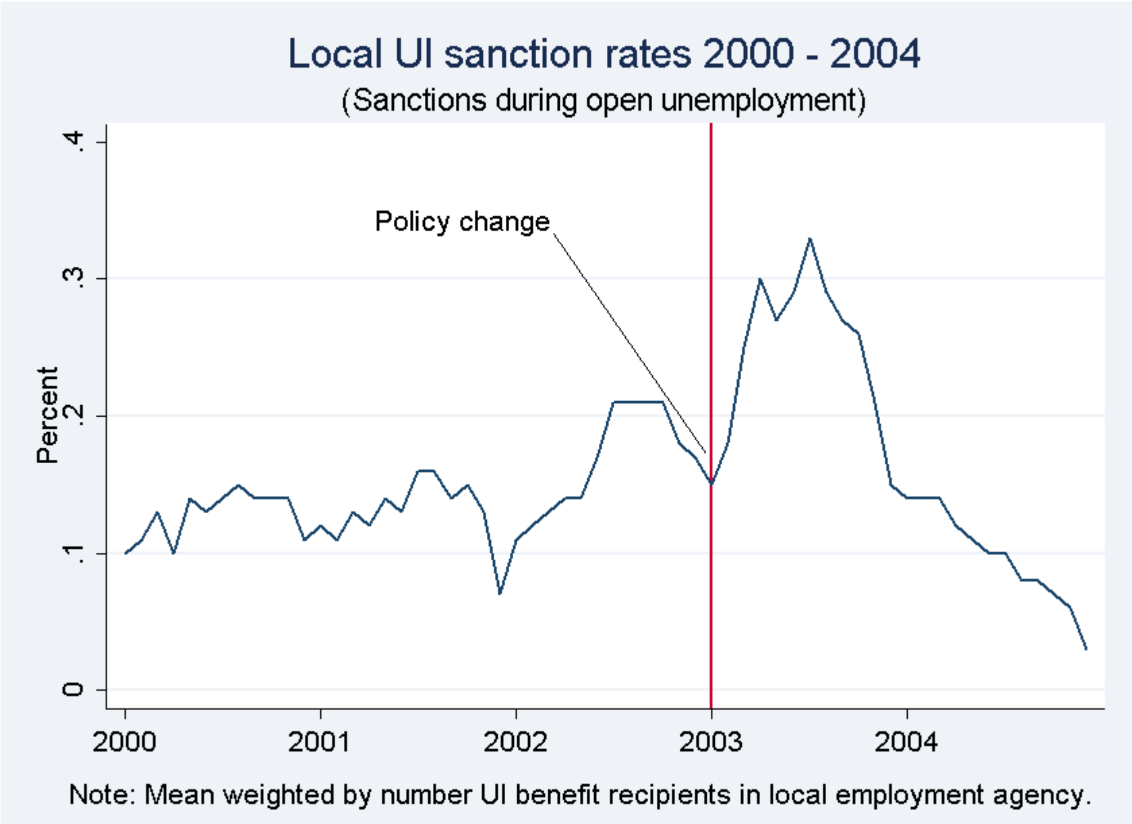
Oschmiansky (2006) collect survey data and conduct interviews among these local employment agencies in order to analyse the sanction regimes. In 2003, the sanction rate increased considerably. According to Müller and Oschmiansky (2006), this increase is mainly due to a policy change effective 1 January 2003 changing two relevant regulations:[5] i) the onus of proof was reversed meaning that individuals (not as before the caseworkers) had to prove that a job vacancy was (not) suitable; ii) suitability criteria were tightened resulting in the fact that UI benefit recipients without family ties had to move for a job (or otherwise risk being punished by a sanction); and iii) the duration of sanctions due to refusing a training program or a job offer was reduced from twelve weeks to three weeks (first refusal). Additionally to these changes, in April 2003 an internal circular of the FEA was launched urging the caseworkers to activate the unemployed more intensely. In the empirical analysis, we use the sanction rate defined as sum of long-term or a short-term sanctions imposed during a respective month divided by the stock of UI benefit recipients during the respective month.[6] In figure 2, the average monthly sanction rates as mean of long-term or a short-term for the years 2000 and 2004 are given.

While before 2002 average monthly sanction rates ranged between 0.1% and 0.15%, in 2002 we already find a slight average increase up to slightly above 0.2% from mid 2002 on until autumn which could result from anticipation of the policy change by the caseworkers. However, already in the first quarter of 2003 we find a sharp increase of the average local sanction rates: in 2003, the sanction rates in most of the months range between 0.25% and 0.3%. The main differences in the sanction rates between 2002 and 2003 are observed between the second and third quarter. Müller and Oschmiansky (2006) find that besides differences in the local sanction policies due to stricter or milder local sanction regimes before 2003, also the increase in the strictness regarding the implementation of sanctions varied significantly between local employment agencies after 2003. They conclude, that

[5] Note that the sanction policy change was part of Hartz I, the first reform of a set of comprehensive labour market reforms in Germany during the years 2003 and 2006. Additional parts of Hartz I and Hartz II, which became effective at the same time, are: introducing start-up subsidies, training vouchers for unemployed individuals, personal service agencies, and wage subsidies for older unemployed as well as a reform of minor employment in April 2003.

[6] Alternatively, statistics on the sanction rates provided by the Federal Employment Agency are available. Results presented in section 4 are robust towards the measure of the sanction rates used.

Figure 1: Local sanction rates



after controlling for differences in local labour market characteristics, significant differences between local employment agencies regarding the implementation of sanctions and especially regarding the increase of the strictness in 2003 remain. They explain these differences by different local sanction policy regimes, i.e. by differently managed local federal employment offices. We use this variation of the local sanction rates as well as the variation in the increase of the local sanction rates in 2003 in order to study the effect of the local sanction regime as well as the effect of tightening the sanction policy.[7]

Increased sanction rates might be driven either by increased use of activation tools, e.g. in form of offering more training programs or proposing more job vacancies. Alternatively, they could result from an increased monitoring of job search activities, e.g. following up job vacancy proposals more frequently. Two reasons indicate that rather increased monitoring than increased use of activation tools induced the increase of the sanction rates: first, on average, the use active labour market policy programmes decreased in 2003

[7]Regressions of the local sanction rates on local employment agency characteristics as unemployment rate or active labour market policy intensity, separately conducted for the years 2002 and 2003 indicate lower R^2 for 2003.

(see e.g. Jacobi and Kluve (2006)), and second, studying the numbers of job vacancies that local agencies proposed to UI benefit recipients, we find that the average intensity of job vacancies proposed by the caseworkers, which we will refer to as the JVC rates - built analogously to the sanction rate - does *not* appear to have changed significantly in 2003 (see figure 5). In sum, first, the sanction policy change in 2003 is expected to have increased the average individual probability of being sanctioned by either a long-term or a short-term sanction considerably, and second, we assume that instead of an increased use of activation tools, it was rather an increased enforcement of sanctions due to an intensified monitoring that induced the increase of the sanction rates in 2003.

3 Method and data

3.1 Method

In order to estimate the impact of the local sanction rates as well as the change of the effectiveness of the sanction regime, we model the duration of unemployment at the individual level as proportional hazard (PH) model. Taking advantage of daily information on the duration of UI benefit receipt, we use a continuous time model. We are interested in the transition (h) to employment of individual i at time t, given that he or she is still at risk, i.e. in UI benefit receipt in $t-1$:

$$h_i(t|x) = exp(x_i'\beta + \lambda_m), \tag{1}$$

with x being a vector of explanatory variables for individual i, β the corresponding coefficient vector. The individual process time is denoted by m. We model the baseline hazard as piecewise-constant for an interval of 30 days with λ_m being the parameter of duration dependence for month m. Since the model is estimated with a constant, normalization is achieved by setting $\lambda_1 = 0$.

The impact of the local sanction regime on h_i is modeled by the time-varying local sanction rate in the model, SR_{lt}, where index l refers to the local employment agencies and t refers to month $m-1$. By introducing an indicator for the months from January 2003 on, *post*, and an interaction term $after{*}SR_{lt}$, the model captures the variation of the change of the local sanction policy regime due to the policy change:

$$h_i(\cdot) = exp(x_i'\beta + \lambda_m + \delta_1 SR_{lt} + \delta_2 post + \delta_3 post * SR_{lt}), \tag{2}$$

with δ_1 expressing the effect of the strictness of the local sanction policy. The coefficients δ_2 and δ_3 capture the change of the transition rate to employment after the policy change for given local sanction rate. In order to make sure that δ_1- δ_3 can be interpreted causally, we have to control for several regional characteristics given in an extended version of the model:

$$h_i(\cdot) = exp(x_i'\beta + \lambda_m + \delta_1 SR_{lt} + \delta_2 post + \delta_3 post * SR_{lt} + w_{lt}'\gamma_1 + w_l'\gamma_2 + \gamma_3 bc_t), \tag{3}$$

Presuming the usual regularity conditions (see Van den Berg (2001) p.3395) are fulfilled, in the following we discuss the assumptions, which are crucial for identification of δ_1- δ_3. In order to disentangle endogenous variation in SR_{lt}, e.g. by local seasonal variations, from exogenous, we have to control for all local time-varying variables (w_{lt}) that might affect the sanction rate as well as the individual hazard to leave unemployment. Additionally, we control for fixed effects of the local employment agencies (w_l). Again, δ_1 is identified as ceteris paribus effect of the local sanction rate via its local- and time-specific variation only if endogenous variation in SR_{lt} is controlled for, otherwise it partly reflects spurious correlation.

The ceteris paribus change of the transition rate to employment after the reform under the assumption that it does not reflect a time effect is captured by $\delta_2 + \delta_3$ for a given SR_{lt} (before the reform). Note that since in 2003, also other labour market reforms became effective we have to be careful when interpreting the results. However, in order to disentangle time variation from δ_2, we control for business cycle indicators (bc_t). Note that including time-varying local labour market characteristics (w_{lt}) also helps disentangling δ_2 from a pure time effect.

Identification of δ_3 relies on independent variation in the changes of the local sanction rates in 2003, which is driven by different local sanction regimes as the implementation study of Müller and Oschmiansky (2006) suggests. We explicitly assume that the jumps of the local sanction rates are exogenous, meaning they are not driven by any unobserved factors that affect the hazard rate to employment as well. Including local heterogeneity via fixed effects (w_l) and via time-varying variables (w_{lt}), supports identification of δ_3 as it was argued regarding δ_1.

In order to avoid biased estimates due to individual specific time-constant characteristics that affect the hazard but are not recorded in our data we extend the model by introducing a term that captures unobserved heterogeneity (v_i) of such nature, yielding a mixed proportional hazard model (MPH):

$$h_i(\cdot) = exp(x_i'\beta + \lambda_m + \delta_1 SR_{lt} + \delta_2 post + \delta_3 post * SR_{lt} + w_{lt}'\gamma_1 + w_l'\gamma_2 + \gamma_3 bc_t + v_i), \quad (4)$$

with γ_1- γ_3 as corresponding coefficient vectors. The unobserved heterogeneity is assumed to be Gamma distributed.[8]

Finally, we control for the local intensity of job vacancy proposals, $JVCR_{lt}$. Analogously to the sanction rates, we include the job vacancy proposal rate in form of monthly rates on local employment agency level and an interaction term ($post * JVCR_{lt}$).

$$\begin{aligned} h_i(\cdot) = exp(x_i'\beta + \lambda_m + \delta_1 SR_{lt} + \delta_2 post + \delta_3 post * SR_{lt} + \delta_4 JVCR_{lt} + \delta_5 post * JVCR_{lt} \\ + w_{lt}'\gamma_1 + w_l'\gamma_2 + \gamma_3 bc_t + v_i), \quad (5) \end{aligned}$$

3.2 Data

We use a sample consisting of inflows into UI benefit receipt between May 2001 and April 2003. Our dependent variable is the duration from the start of the UI benefit receipt

[8]Assuming Gamma distributed heterogeneity is supported by Abbring and van den Berg (2007): according to their results, in a large class of hazard models, as time proceeds, heterogeneity among survivors converges to a Gamma distribution.

until transition into unsubsidised employment. We right-censor the individual duration after twelve months. Additionally, we right-censor on 31 December 2003 obtaining an observation window reaching from more than one and a half years before the reform until twelve months after the policy change.[9] Since we are interested in the *ex ante* effect, we focus on the duration of UI benefit receipt without a sanction. Thus, in the case of a sanction, the duration is right-censored from the start of the sanction on.[10] On the individual level we control for age and age squared; previous wage; marital status; number of dependent children in the household; maximum eligibility duration for UI benefits; German citizenship; university degree; type of job wanted (blue collar without skills, blue collar with skills or white collar); and a set of variables on the previous employment and unemployment history as e.g. number of days in employment during the previous year. In order to control for local heterogeneity which might be correlated with SR_{lt} and $post{*}SR_{lt}$ (and JVC_{lt} and $post{*}JVC_{lt}$), we control for the following variables on local employment agency level time-varying per month: % of active labour market measure participants of all job seekers[11]; % of job seekers of all dependent employed; % of female job seekers of all dependent employed; % of unemployed of all dependent employed; % of female unemployed of all dependent employed; and % vacancies per job seekers. Additionally, we control for the regional wage level by including yearly average regional wages recorded in June. In order to capture residual seasonal variation, we additionally include a dummy variable on whether the individual enters UI benefit receipt

[9] We chose the end of 2003 as censoring date for two reasons: first, according to Müller and Oschmiansky (2006), the main increase of the local sanction rate was observed in 2003, and second, from 2004 on other elements of the Hartz reforms became effective.

[10] During the observation period, sanctions imposed during a UI benefit receipt were a quite rare event. The following shares of our sample were affected by at least one imposed sanction: 2.9% (women West), 1.0% (women East), 4.3% (men West) and 1.8% (men East). Note that we have to assume that right-censoring is independent. In our context, this is quite a strong assumption, as naturally, individuals who are sanctioned on average are distinct from other ones. Therefore, despite the facts of including an informative set of observed characteristics as well as modeling unobserved heterogeneity, endogenous right-censoring of the sanctioned individuals might still bias our estimations. Yet, for two reasons, we assume this bias to be negligible at this stage: i) The numbers of imposed sanctions are relatively small themselves and ii) over process time, right-censoring these observations will lead to a pool of unemployed individuals who are on average "better risks" as they are not sanctioned. As the threat of a benefit sanction is expected to be stronger for the "worse risks", the estimates are expected to be biased downwards.

[11] We distinguish different types of active labour market programs: further education; job creation schemes; and a rest category, with e.g. short-time allowance measures.

in January; and two dummies on whether the person enters the sample in 2001 or 2002. Regional fixed effects are included on local employment agency level. Even though we use a relatively small observation window of about two and a half years, on the macro level, there might be a business cycle trend which, staying uncontrolled, could bias our estimation as outlined above. Therefore we use business cycle indicators that are quite established in Germany and provided by the ifo insitute.[12] They are included on monthly basis.

As we allow the baseline hazard to flexibly change each month, we split the data after each month of individual process time. Such data structure simplifies the merge of time-varying regional and macro information with individual data spells. Note that we merge the monthly time-varying variables lagged by one month. For a person who enters UI benefit receipt in April 2002, for example, during his or her first month, we merge the time-varying regional and macro variables of March 2002; during the second month of UI benefit receipt, information of April 2002 is merged and so forth.

4 Results

In this section, we present the results. First, we report the estimates of the full model and compare different specifications. Second, by means of simulations, we quantify the size of the effects of the policy change. In order to test additional hypothesis about the mechanisms of the effectiveness of the policy change, we analyse the effects by subgroups. Finally, two robustness checks are presented.

4.1 Estimates

The models described by equations 4 and 5 is estimated by maximum likelihood separately for East and West Germany. Table A.6 contains the estimates of the parameters of interest:[13]

[12] Two different indicators capturing the business situation (R2) and the business expectations (R3) are used. The values were downloaded at www.cesifo-group.de.

[13] Table A.2 in the appendix contains all estimates.

Table 1: Estimates model A, B

	West:				East:			
	Model A		Model B		Model A		Model B	
SR	0.061	(0.071)	0.020	(0.074)	0.497**	(0.245)	0.209	(0.243)
JVCR	no		0.006***	(0.001)	no		0.012***	(0.004)
post	0.022	(0.026)	-0.038	(0.034)	0.206***	(0.045)	0.213***	(0.068)
post∗SR	0.233***	(0.090)	0.154*	(0.090)	0.562*	(0.301)	0.339	(0.310)
post∗JVCR	no		0.005***	(0.001)	no		0.003	(0.005)
u. H. conv.	no		no		no		no	
BIC	589073.39		589021.93		381052.16		380993.36	
AIC	587671.79		587583.45		380605.21		380546.41	
LL	-293721.90		-293674.73		-190265.60		-190236.21	
N exits	107947		107947		66541		66541	
N	255810		255810		187992		187992	

Notes: Table contains untransformed coefficients. Additional controls not presented in the table: individual and local characteristics, local fixed effects, and business cycle indicators. Standard errors are clustered by local labour market agencies. Significance levels: ***: 1%; **: 5%; *:10%. Source: Own estimations based on microdata of Federal Employment Agency.

According to the coefficients of $post * SR$ of both models, for West Germany, we find a significant increase of the effect of the local sanction rate on the transition into regular employment after the policy change in 2003: a marginal increase of the sanction rate has a larger effect on the hazard rate than before the policy change.[14] Since the total effect of policy change on the transition rate to employment depends on the values of SR_{lt} and JVC_{lt}, in section 4.2, we use simulations in order quantify the overall effect. Regarding the differences between Model A and Model B, we discover that, once controlling for the local intensity of offering job vacancies to the unemployed (Model B), the effect of SR decreases compared to Model A. Thus, without controlling for the local JVC rate, the effect of the local sanction rate is overestimated, before as well as after the policy change. Though according to Wald-tests, the coefficients of $post$ and $post * SR$ do not significantly (5%-level) differ between Model A and Model B, AIC and BIC suggest superiority of Model B. Thus, in the remainder of the analysis, we will use specification of Model B.

In the East German sample, in Model A we find relatively large coefficient estimates

[14]Note, that if the coefficient of *post* is negative and the sanction rates (and the JVC rates) after the policy change are too low, the hazard rate to employment after the policy change can still be lower than before.

of SR and $post * SR$, yet. Compared to Model A, once controlling for the local intensity of offering job vacancies to the unemployed (Model B), we find that before the policy change, SR decreases and even becomes insignificant, while we find a significant positive impact of the local JVC rate on the transition into regular employment. Regarding the effect of tightening the sanction regime, the signs of the estimates of *post* as well as the interaction terms indicate increased hazard rates after the policy change.

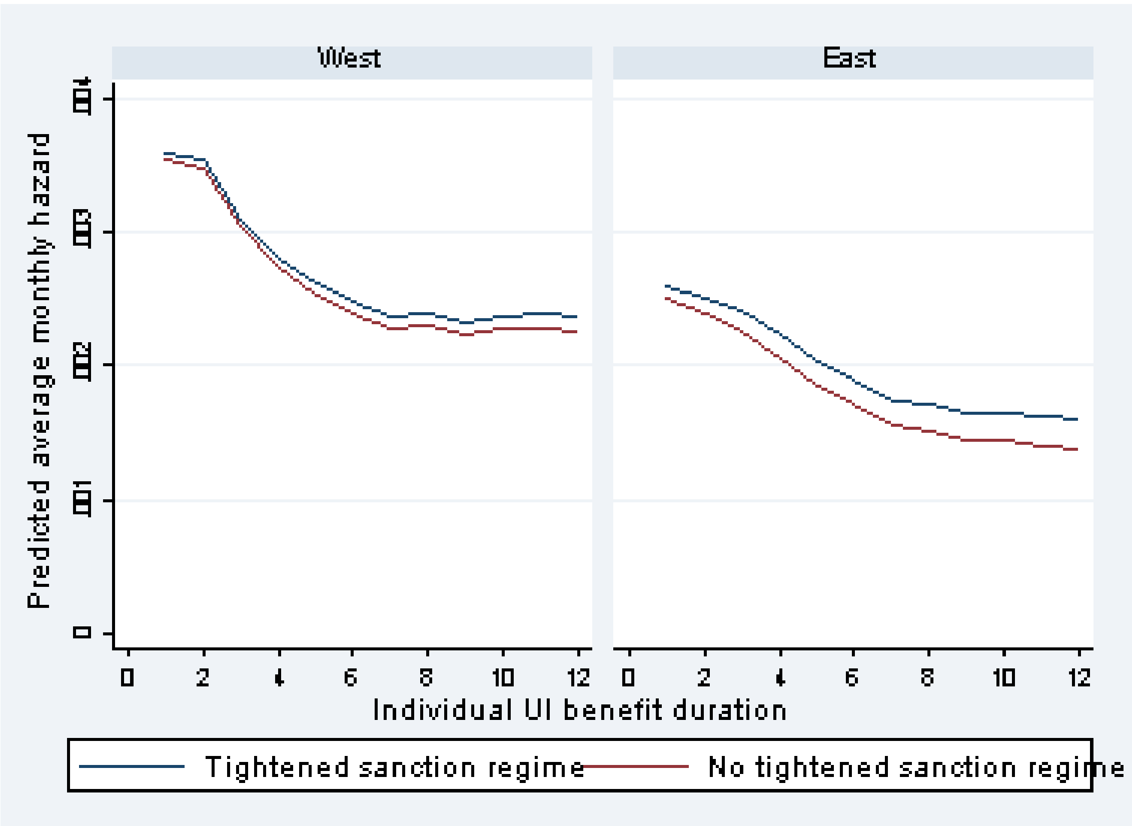
In sum, first, *not* controlling for the local intensity of offering job vacancies to the unemployed via the local JVC rate, yields higher estimates of the local sanction rates and therefore overestimates the *ex ante* effect before as well as after the policy change. Second, before the policy reform, the local JVC rate had a significant positive effect on the transition to work while the local sanction rate did not significantly affect it, indicating no strong *ex ante* effect. Third, the policy change has shifted up the effects of the local sanction rates and JVC rate, yet only for West significantly. Fourth, in order to quantify the overall effect of tightening the sanction regime, we will use simulations.

We assume that local heterogeneity as well as a time trend in form of the business cycle are the main factors that are correlated as well to the variables *post*, $post * SR$, and $post * JVC$ as well as to the individual transition rate to employment. Consequently, not controlling for them results in biases of our estimates of interest. Table A.2 in the appendix contains results dropping any of the following control variables from Model B: local fixed effects; local time-varying controls; or business cycle indicators. Compared to these models, Model B scores best regarding BIC and AIC. Additionally, we find that each of these specifications results in different estimates of the sanction regime as represented by the coefficients of SR and $post * SR$, as well as different estimates of *post*. In sum, we regard as plausible that Model B yields estimates that are *not* biased due to local heterogeneity nor time trends.

4.2 Simulations

In order to assess the size of the overall impact of the policy change, we use simulations. The simulation results are based on predictions using the estimates of Model B. We simulate the effect by a) predicting average monthly hazards using data as well as estimates as observed and respectively estimated, and b) simulate a world, in which the sanction regime as well as the sanction rates and JVC rates did not change. The latter is simulated by replacing the values of the monthly sanction rates and JVC rates of 2003 by the respective monthly values of 2002 and setting *post* and the interaction terms equal to zero. We interpret the difference of these two worlds as overall policy change effect. Figure 4.2 depicts the results. The %-increase of the monthly hazard rates averaged over twelve months amounts to 3.5% for West Germany and 10.9% for East Germany.

Figure 2: Overall effect of the policy change on monthly hazard rates



4.3 Subgroups

The results might be driven by different subgroups. There are two - not mutually exclusive - reasons why this could be the case: first, tightening the sanction regime might have been targeted to specific subgroups, meaning that only those subgroups actually were affected by the policy change. As described above, one element of the policy change was tightening the suitability regulations in terms of regional flexibility for UI benefit recipients without family ties. Thus, while for individuals with family ties, suitability regulations did not change, from 2003 on, due to a higher pool of suitable job vacancies, we assume unmarried individuals to have been confronted with a higher risk of being sanctioned compared to married ones. Accordingly, we assume, that unmarried individuals have a stronger response to the policy change than married. In order to test this hypothesis, we estimate Model B separately for married and unmarried individuals.

A second reason, why results might differ between subgroups, is that some individuals could respond more sensitively to the tightened sanction regime than others. According to Abbring, van den Berg, and van Ours (2005), those individuals who prove sufficiently high compliance with the regulations and are therefore not at risk of being sanctioned in neither regime, are not affected by a change in the sanction regime. Yet, we do not observe individuals' search intensity and are consequently not able to identify this group in the data. According to findings in Müller and Steiner (2008) and Hofmann (2008), age is negatively related to the risk of being sanctioned. Thus, we assume that the average search intensity among young UI benefit recipients is lower and expect younger UI benefit recipients to respond more to the tightened regime than older. In order to shed empirical light on this question, we divide the sample into younger ($<=$ 30 years) and older ($>$ 30 years) UI benefit recipients. The estimates are listed in table A.3 and table A.4 in the appendix, are based on simulations using the coefficients of the subgroup estimations.

To sum up the results, tightening the sanction regime seems to have affected indeed unmarried individuals more than married ones. Regarding the hypothesis about older individuals on average being less affected by tightening the sanction regime, for West Germany for older UI benefit recipients we find a lower overall effect than for younger ones.

4.4 Robustness

As we saw in the results of different specifications presented above (subsection 4.1), not controlling for local characteristics or excluding the business cycle, yields different estimates of the tightened sanction regime. We argued that the specification used in Model B is superior to these specifications regarding unbiasedness of the estimates of interest. For two more reasons, our estimates of the local sanction rates might be biased.

First, individuals who are sanctioned might bias the estimates of interest since they are assumed to be a selective group of individuals. High sanction rates by definition mean that relatively many individuals are sanctioned. Observing an increased exit rate to work in those regions where we also find more individuals to be punished, might also be due to the *ex post* effect and not (only) due to the *ex ante* effect. However, remember that in order to diminish this problem, we right-censored individuals once a sanction was imposed. Alternatively, we could drop these cases from the analysis. Since excluding all UI benefit recipients who were sanctioned, yields a higher share of transitions to work where sanction rates are higher,[15] we expect higher estimates of the effect of the local sanction rates on the transition to employment. The results listed in table A.7 indicate that this is the case. Yet, since also for sanctioned individuals search intensity might have increased due to the *ex ante* effect, in sum, regarding potential bias due to this group, we assume our estimates to be rather lower bound instead of overestimating the effect of the policy change.

Second, we argue that it is the policy change of 2003 that affected job search behaviour in a way that the hazard rate into regular employment was increased. If this indeed was the case, using different months as month of the policy change, should result in different estimates. In a second robustness check, we estimate Model B, yet pre- and post-poning the policy change by half a year (="pseudo policy change - pre" / "pseudo policy change - post"). Since in the first case, we expect the effectiveness of the pseudo policy change to be weakened by the months in which the actual policy change did not take place, and in the second case, we expect the control group to be strengthened by the months in which the actual policy change did already take place, in both cases, we expect smaller estimates of the pseudo policy change. According to the results in table A.7, indeed, using the "pseudo policy change" results indicate lower effects of the overall policy change.

[15] Note that individuals have to be still in UI benefit receipt in order to be sanctioned, i.e. we can only observe a sanction if we do not observe a transition to work before.

5 Conclusion

This paper analyses the *ex ante* effectiveness of unemployment insurance (UI) benefit sanctions. The *ex ante* effect of sanctions is assumed to raise the exit rate to work of UI benefit recipients by the mere threat of being sanctioned. Yet, empirical evidence on this topic is scarce. The contribution of this paper is to be the first to exploit a policy change in order to study the *ex ante* effect.

We use data from Germany taking advantage of local variation in sanction policy regimes. Moreover, we exploit a policy change at the start of 2003 when regulations regarding job search requirements were tightened leading to a considerable increase of the average sanction rates. Regionally differing increases of the sanction rates, suggest that not in each region, the sanction regime was tightened. Although variation in local sanction rates is mainly driven by local labour market conditions, controlling for a large set of local heterogeneity, we argue that i) remaining variation is independent of unemployment exits and ii) the locally different increase in the sanction rates after the policy change is independent as well as they are driven by differently managed local employment offices.

We use duration analysis and account for local- and time-variations in sanction rates in order to identify the effect of the local sanction rates. Our dependent variable is the duration from the start of the UI benefit receipt until transition into unsubsidised employment. Interaction terms - between the sanction rates and a dummy indicating the time from January 2003 on - are used in order to identify the effect of the policy change. Based on simulations, the overall effect is quantified as percentage increase of the hazard rates. Since we assumed that the some individuals are affected by the policy change more than others, subgroups by marital status and age are analysed. Additionally, several robustness checks were performed.

According to our preliminary results, we find that before the policy change in 2003, the German UI system seems not to have affected job search behaviour *ex ante* by the threat of a sanction. Yet, tightening the sanction regime in 2003 increased the hazard rates to regular employment in East and in West Germany.

In the next steps, we will conduct further robustness checks and investigates whether the increase of the monitoring intensity was also related to an increase of personnel costs of the Federal Employment Agency.

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Figure 3: Local job vacancy offer rates

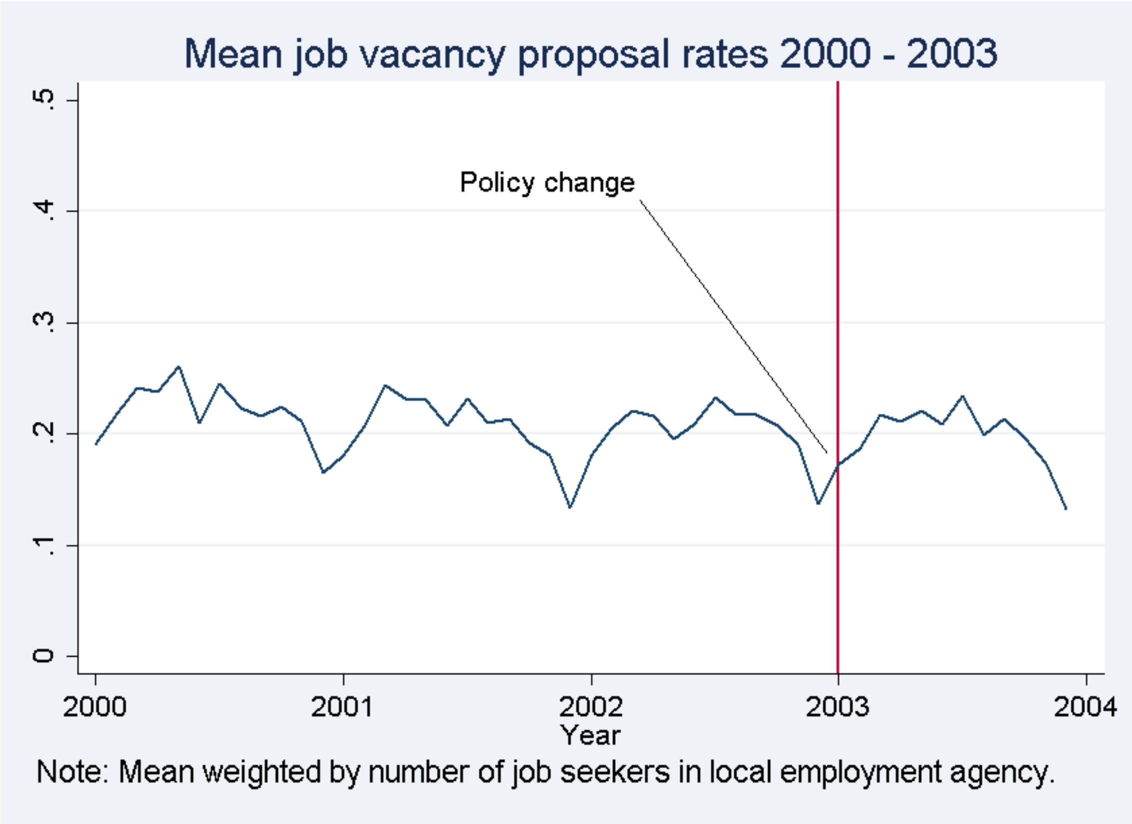

Table A.1: Descriptives

	West		East	
	Mean	S.D.	Mean	S.D.
female	0.45	0.50	0.47	0.50
married	0.20	0.40	0.21	0.41
ifo2	87.36	3.27	87.65	3.41
ifo3	92.20	2.95	92.10	2.95
i2001	0.31	0.46	0.35	0.48
i2002	0.52	0.50	0.50	0.50
januar	0.17	0.37	0.16	0.37
otherempl q	0.08	0.27	0.13	0.33
sanquo mean	0.19	0.10	0.08	0.05
JVC rate	22.63	8.68	14.06	4.75
after	0.17	0.37	0.14	0.35
inter SR	0.04	0.10	0.02	0.04
inter JVC	3.25	7.87	1.80	4.74
almprate	0.17	0.05	0.40	0.10
mbasuquotz	9.53	2.82	23.16	3.59
mbasuquot	10.58	3.01	25.08	3.92
mbasuquotf	9.91	2.55	22.37	9.00

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mbaloquotz	7.75	2.35	18.32	3.18
mbaloquot	8.60	2.51	19.86	3.56
mbaloquotf	7.97	2.09	17.46	7.03
vacrate	0.14	0.08	0.04	0.02
vz	85.54	8.56	65.57	9.14
Female	0.45	0.50	0.47	0.50
Married	0.42	0.49	0.38	0.49
Child	0.41	0.49	0.51	0.50
Maxeli	304.58	87.55	291.73	91.16
German	0.86	0.35	0.98	0.15
Age	32.10	6.86	32.66	7.12
Age sq	1077.37	437.67	1117.19	456.91
Wage	55.72	36.29	44.39	24.80
Uni	0.26	0.44	0.22	0.42
no skill	0.31	0.46	0.21	0.40
blue skill	0.17	0.37	0.31	0.46
white skill	0.34	0.48	0.29	0.45
sector1	55.35	23.32	57.66	25.40
cummn4	7.62	47.80	50.03	118.71
cummn8	2.19	20.91	3.02	24.83
cummn9	1.17	16.03	3.02	28.38
cummn256	1.82	22.51	11.00	57.16
cumemp	322.04	72.71	323.26	74.84
cumalg1	9.19	33.46	13.64	42.37
cumalg2	1.83	15.98	4.74	26.10
dst	466.35	217.72	164.56	287.00

Table A.2: Estimates full model - all estimates⋆

	Coefficients	S.E.	Coefficients	S.E.
SR	0.020	(0.074)	0.209	(0.243)
JVC	0.006***	(0.001)	0.012***	(0.004)
After	-0.038	(0.034)	0.213***	(0.068)
After X SR	0.154*	(0.090)	0.339	(0.310)
After X JVC	0.005***	(0.001)	0.003	(0.005)
Fbw rate	-1.802***	(0.661)	-0.831	(1.018)
Abm rate	1.378	(0.874)	0.846	(0.943)
Sam rate	3.798***	(1.389)	1.166	(0.781)
Rest rate	-0.128	(0.134)	0.039	(0.142)
Mbalo	-0.000	(0.000)	-0.000	(0.000)
Mbasuquotz	-1.608**	(0.700)	-2.391***	(0.844)
Mbasuquot	1.276**	(0.622)	1.900**	(0.759)
Mbasuquotf	0.195***	(0.072)	0.292***	(0.041)
Mbaloquotz	2.416***	(0.847)	3.630***	(0.972)

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Mbaloquot	-1.691**	(0.748)	-2.952***	(0.882)
Mbaloquotf	-0.332***	(0.070)	-0.253***	(0.041)
Vacrate	0.481***	(0.100)	0.328	(1.422)
Vz	0.001	(0.001)	0.006**	(0.003)
Ifo2	0.032***	(0.002)	0.025***	(0.003)
Ifo3	0.032***	(0.002)	0.042***	(0.004)
Female	0.130***	(0.010)	-0.049**	(0.019)
Married female	-0.506***	(0.015)	-0.235***	(0.020)
λ_2	-0.049***	(0.012)	-0.094***	(0.017)
λ_3	-0.270***	(0.014)	-0.185***	(0.016)
λ_4	-0.463***	(0.016)	-0.320***	(0.026)
λ_5	-0.609***	(0.014)	-0.472***	(0.027)
λ_6	-0.768***	(0.016)	-0.597***	(0.024)
λ_7	-0.999***	(0.021)	-0.884***	(0.032)
λ_8	-1.070***	(0.019)	-1.012***	(0.025)
λ_9	-1.298***	(0.023)	-1.201***	(0.022)
λ_{10}	-1.268***	(0.026)	-1.228***	(0.036)
λ_{11}	-1.366***	(0.030)	-1.300***	(0.040)
λ_{12}	-1.593***	(0.034)	-1.303***	(0.049)
Inflow 2001	0.587***	(0.026)	0.426***	(0.050)
Inflow 2002	0.331***	(0.016)	0.273***	(0.026)
Inflow januar	0.255***	(0.012)	0.303***	(0.019)
Mblealg1	-0.000	(0.000)	0.000***	(0.000)
Married	0.218***	(0.010)	0.213***	(0.012)
Child	-0.186***	(0.009)	-0.108***	(0.009)
Maxeli	0.002***	(0.000)	0.003***	(0.000)
German	0.278***	(0.011)	0.418***	(0.055)
Age 2630	-0.042***	(0.011)	-0.004	(0.014)
Age 3135	-0.119***	(0.013)	-0.066***	(0.019)
Age 3640	-0.150***	(0.014)	-0.111***	(0.019)
Age 4145	-0.193***	(0.016)	-0.158***	(0.018)
Wage	0.001***	(0.000)	0.003***	(0.000)
Uni	-0.013	(0.008)	0.010	(0.012)
Worker blue no skill	0.035***	(0.013)	0.080***	(0.020)
Worker blue skill	0.262***	(0.014)	0.218***	(0.018)
Worker white	0.121***	(0.011)	0.014	(0.015)
Cummn4	-0.002***	(0.000)	-0.003***	(0.000)
Cummn8	0.001***	(0.000)	0.002***	(0.000)
Cummn9	-0.001***	(0.000)	-0.001***	(0.000)
Cummn256	-0.002***	(0.000)	-0.002***	(0.000)
Cumemp	0.002***	(0.000)	0.002***	(0.000)
Cumalg1	0.004***	(0.000)	0.005***	(0.000)
Cumalg2	0.000	(0.000)	0.001***	(0.000)
Cons	-14.517***	(0.333)	-19.034***	(0.682)
Cons	-64.970		-213.161	(0.000)
BIC	589021.934		380993.361	

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AIC	587583.453	380546.412
ll	- 293674.726	- 190236.206
N exits	107947	66541
N	255810	187992

$\star$: All estimates except for local employment agency fixed effects (w_l) and effects of individual's profession. Standard errors are clustered by local labour market agencies. Notes: Table contains untransformed coefficients. Significance levels: ***: 1%; **: 5%; *:10%. Source: Own estimations based on microdata of Federal Employment Agency.

Table A.3: Estimates full model - by marital status

	West:				**East:**			
	Not married		Married		Not married		Married	
SR	0.021	(0.079)	0.022	(0.118)	0.169	(0.277)	0.292	(0.281)
JVCR	0.006***	(0.001)	0.007***	(0.002)	0.010**	(0.005)	0.015***	(0.005)
post	-0.007	(0.039)	-0.071	(0.051)	0.172***	(0.065)	0.277***	(0.089)
post X SR	0.154	(0.098)	0.132	(0.143)	0.500	(0.320)	0.061	(0.368)
post X JVCR	0.004***	(0.001)	0.006**	(0.002)	0.006	(0.005)	-0.001	(0.006)
individual X	yes		yes		yes		yes	
calendar month	yes		yes		yes		yes	
w_{lt}	yes		yes		yes		yes	
bc_t	yes		yes		yes		yes	
w_l	yes		yes		yes		yes	
u. H. converged	yes		yes		yes		yes	
bic	351665.755		238020.026		235577.568		145264.224	
aic	350098.097		236698.252		235149.143		144851.749	
ll	-174915.049		-118234.126		-117537.572		- 72388.874	
N exits	65216		42731		40628		25913	
N	149254		106556		116467		71525	

Notes: Table contains untransformed coefficients. Standard errors are clustered by local labour market agencies. Significance levels: ***: 1%; **: 5%; *:10%. Source: Own estimations based on microdata of Federal Employment Agency.

Table A.4: Estimates full model - by age

	West:				East:			
	Age < 25		Age 25 - 44		Age < 25		Age 25 - 44	
SR	-0.090	(0.086)	0.081	(0.099)	0.313	(0.266)	0.151	(0.278)
JVCR	0.006***	(0.002)	0.005***	(0.001)	0.009*	(0.005)	0.014***	(0.005)
post	-0.024	(0.044)	-0.050	(0.040)	0.192***	(0.071)	0.231***	(0.081)
post X SR	0.202*	(0.112)	0.127	(0.111)	0.093	(0.403)	0.477	(0.341)
post X JVCR	0.006***	(0.002)	0.004**	(0.002)	0.006	(0.006)	0.001	(0.005)
individual X	yes		yes		yes		yes	
calendar month	yes		yes		yes		yes	
w_{lt}	yes		yes		yes		yes	
bc_t	yes		yes		yes		yes	
w_l	yes		yes		yes		yes	
u. H. converged	no				no		no	
bic	232899.924		356353.077		142162.822		238152.223	
aic	231629.324		354929.364		141756.063		237732.175	
ll	-115701.662		-177344.682		-70841.031		-118830.088	
N exits	43130				24334		42207	
N	99776				68753		119239	

Notes: Table contains untransformed coefficients. Standard errors are clustered by local labour market agencies. Significance levels: ***: 1%; **: 5%; *:10%. Source: Own estimations based on microdata of Federal Employment Agency.

Table A.5: Estimates full model - different specifications West

	West:							
	Model w/o local fixed effects		Model w/o local variables		Model w/o business indicators		Model B	
SR	-0.033	(0.086)	-0.525***	(0.096)	-0.013	(0.083)	0.020	(0.074)
JVCR	0.003***	(0.001)	0.009***	(0.001)	0.012***	(0.001)	0.006***	(0.001)
post	0.169***	(0.035)	0.214***	(0.029)	-0.102***	(0.036)	-0.038	(0.034)
post X SR	0.110	(0.090)	0.445***	(0.095)	0.156	(0.099)	0.154*	(0.090)
post X JVCR	0.003**	(0.001)	0.002	(0.001)	0.005***	(0.002)	0.005***	(0.001)
individual X	yes		yes		yes		yes	
w_l	no		yes		yes		yes	
w_{lt}	yes		no		yes		yes	
bc_t	yes		yes		no		yes	
u. H. converged	no		no		no		no	
bic	590582.847		590500.113		589897.608		588993.345	
aic	589156.661		589258.347		588471.421		587579.453	
ll	-294462.330		-294528.174		-294119.711		-293674.726	
N exits	107947		107947		107947		107947	
N	255810		255810		255810		255810	

Notes: Table contains untransformed coefficients. Standard errors are clustered by local labour market agencies. Significance levels: ***: 1%; **: 5%; *:10%. Source: Own estimations based on microdata of Federal Employment Agency.

Table A.6: Estimates full model -different specifications East

	East:							
	Model w/o local fixed effects		Model w/o local variables		Model w/o business indicators		Model B	
SR	0.227	(0.300)	-1.286***	(0.332)	-0.167	(0.371)	0.209	(0.243)
JVCR	0.013***	(0.005)	0.027***	(0.005)	0.026***	(0.005)	0.012***	(0.004)
post	0.310***	(0.064)	0.368***	(0.054)	0.218***	(0.067)	0.213***	(0.068)
post X SR	0.140	(0.287)	0.617	(0.439)	0.770*	(0.403)	0.339	(0.310)
post X JVCR	-0.003	(0.004)	0.003	(0.005)	-0.008	(0.005)	0.003	(0.005)
individual X	yes		yes		yes		yes	
w_l	no		yes		yes		yes	
w_{lt}	yes		no		yes		yes	
bc_t	yes		yes		no		yes	
u. H. converged	yes		no		yes		no	
bic	381746.615		382619.162		381645.464		380993.361	
aic	381287.586		382172.213		381198.515		380546.412	
ll	-190605.793		-191049.107		-190562.257		-190236.206	
N exits	66541		66541		66541		66541	
N	187992		187992		187992		187992	

Notes: Table contains untransformed coefficients. Standard errors are clustered by local labour market agencies. Significance levels: ***: 1%; **: 5%; *:10%. Source: Own estimations based on microdata of Federal Employment Agency.

Table A.7: Estimates full model - Pre- and postponing reform by 6 months

	West:				East:			
	Preponing		Postponing		Preponing		Postponing	
SR	0.027	(0.090)	0.115**	(0.057)	0.751**	(0.364)	0.563***	(0.210)
JVCR	0.005***	(0.001)	0.006***	(0.001)	0.008*	(0.005)	0.010**	(0.004)
post	-0.095***	(0.028)	0.026	(0.044)	-0.052	(0.091)	0.199**	(0.087)
post X SR	0.169*	(0.100)	-0.050	(0.098)	-0.247	(0.439)	-0.239	(0.434)
post X JVCR	0.001	(0.001)	0.006***	(0.002)	0.003	(0.005)	-0.004	(0.005)
individual X	yes		yes		yes		yes	
calendar month	yes		yes		yes		yes	
w_{lt}	yes		yes		yes		yes	
bc_t	yes		yes		yes		yes	
w_l	yes		yes		yes		yes	
u. H. converged	no		no		no		no	
bic	589052.595		589133.418		381185.597		381172.840	
aic	587626.408		587596.579		380738.647		380725.891	
ll	-293697.204		-293673.289		-190332.324		-190325.945	
N exits	107947		107947		66541		66541	
N	255810		255810		187992		187992	

Notes: Table contains untransformed coefficients. Standard errors are clustered by local labour market agencies. Significance levels: ***: 1%; **: 5%; *:10%. Source: Own estimations based on microdata of Federal Employment Agency.

Table A.7: Estimates full model - W/o sanctioned

	West:		East:	
SR	0.030	(0.074)	0.239	(0.243)
JVCR	0.006***	(0.001)	0.012***	(0.004)
post	-0.032	(0.034)	0.219***	(0.068)
post X SR	0.180**	(0.089)	0.301	(0.314)
post X JVCR	0.005***	(0.001)	0.003	(0.005)
u. H. converged	no		no	
bic	582319.177		378979.644	
aic	580835.431		378533.243	
ll	-290296.715		-189229.622	
N exits	107947		66541	
N	244809		184203	

Notes: Table contains untransformed coefficients. Standard errors are clustered by local labour market agencies. Significance levels: ***: 1%; **: 5%; *:10%. Source: Own estimations based on microdata of Federal Employment Agency.

Table A.7: Simulation results: percent increase of predicted average monthly hazard rates

	West:	**East:**
Full sample	3.5	10.9
Married	2.7	9.2
Not married	4.0	12.0
$<=$ 30 years old	4.9	10.9
$>$ 30 years old	2.5	10.9
Sanctioned dropped	3.9	11.0
Pseudo policy change "pre"	-1.9	-.7
Pseudo policy change "post"	2.1	2.9

Notes: Table contains average over twelve months of the simulated percent increase based on predicted average monthly hazard rates for the respective sample. For prediction of "without policy change" *post*, $post * SR$ and $post * JVCR$ are set to zero and for SR and $JVCR$ monthly values of year 2002 are used for the respective months of the year 2003. Prediction of "with policy change" is based on estimated coefficients and observed data.