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# Unemployment compensation and unemployment duration – Evidence from the German Hartz IV reform\*

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**Abstract.** In 2005, the Hartz IV reform reduced the unemployment compensation for long-term unemployed in Germany. Using a large administrative data set on employment and unemployment spells we examine how this reform affected the transition probability to employment. For the short-term unemployed we estimate an increase in the daily transition rate by about 20% in West Germany and by about 70% in East Germany. The effect is increasing over unemployment duration. In contrast, the effect for the long-term unemployed appears to be negative, duration independent and of about the same magnitude in East and West Germany. We do not find robust evidence for gender-specific effects of the Hartz IV reform.

JEL-Classification: J64, J68

Keywords: duration analysis, hazard rate, unemployment benefit, Hartz reforms

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# 1. Introduction

In the early 2000s far-reaching labor market reforms, referred to as “Hartz reforms”, were implemented in Germany. The four reform packages aimed at a reduction of unemployment duration using various measures. A detailed description of the core elements can be found in Jacobi and Kluve (2007). We contribute to the evaluation of the Hartz reforms by examining at the micro level whether the reduction of the unemployment compensation for long-term unemployed in the course of the Hartz IV reform has reduced the duration in unemployment. Search-theoretic models of the labor market predict that a reduction in the level of unemployment compensation induces an increase in the hazard rate and thereby a reduction of unemployment duration (see e. g. Rogerson *et al.*, 2005). If the unemployment compensation scheme differentiates between short-term and long-term unemployed, a decrease in the unemployment compensation for long-term unemployed will increase the hazard rate of the short-term unemployed as well (Mortensen, 1977; van den Berg, 1990).

The literature evaluating the German Hartz reforms is growing rapidly. Several studies examine the effect and effectiveness of active labor market programs which were introduced or changed by the first two Hartz reform packages. For a survey on these evaluations see Jacobi and Kluve (2007) or Eichhorst and Zimmermann (2007). Dlugosz *et al.* (forthcoming) investigate the reforms’ impact on unemployment inflows, while Fahr and Sunde (2009), Klinger and Rothe (2012) and Hertweck and Sigris (2013) assess the reforms’ effect on unemployment outflows. The three latter studies estimate matching functions using aggregated data series. Hertweck and Sigris (2013) inspect the effect of the first two Hartz reform packages in 2003 and find an increase in matching efficiency of more than 20% in West Germany. Using stock flow models, Fahr and Sunde (2009) and Klinger and Rothe (2012) identify positive impacts of the Hartz I, II and III reforms with the effects being larger in East Germany (Fahr and Sunde, 2009) and for long-term unemployed (Klinger and Rothe, 2012). For the Hartz IV reform,

Klinger and Rothe (2012) estimate a slightly negative effect. However, they argue that this finding might be spurious. The result may be due to a change in the definition of unemployment or due to a decline in the positive effect of the Hartz III reform. This view is supported by Krause and Uhlig (2012) who show in a simulation model that the Hartz IV reform has unambiguously shortened the duration in unemployment.

Despite these two studies, the empirical evidence regarding the effect of the Hartz IV reform is scarce. Especially, we are not aware of studies examining the effect of this reform on unemployment duration separately for men and women or East and West Germany. This is remarkable as the Hartz IV reform is probably the most controversial reform within the Hartz legislation.

We fill this gap and check in detail whether the reduction of the unemployment compensation for long-term unemployed has increased the individual hazard rate of all unemployed as predicted by dynamic search theory. As a second contribution we provide the first micro evidence with regard to the effects of the Hartz IV reform. We thus circumvent the difficulties arising from estimating matching functions using aggregate German administrative time series: First, the official definition of “unemployment” or “outflows” has changed over time, causing structural breaks in the aggregated time series (see Fahr and Sunde, 2009, for details). Second, firms are not obliged to register open vacancies at the federal employment agency which distorts the official vacancy series downwards. Instead, we make use of a large administrative micro data set on employment and unemployment spells. We propose a novel data preparation to make this data set usable for our purposes. As a third contribution we present evidence on heterogeneous treatment effects of the Hartz IV reform over regions, gender, the benefit received and unemployment duration. Finally, we are able to determine the effect of the Hartz IV reform in terms of median unemployment duration.

Traditionally, the effect of unemployment benefits on unemployment duration was estimated using proportional hazard models (see e. g. Lancaster, 1979; Moffitt, 1985; Narendranathan *et al.*, 1985; Katz and Meyer, 1990; Meyer, 1990; Winter-Ebmer, 1998;

Roed and Zhang, 2003; Lalive *et al.*, 2006). For an extensive survey see Machin and Manning (1999). Examples for Germany include Hunt (1995), Winterhager (2006), Arntz and Wilke (2009) and Caliendo *et al.* (2013). Recently, quantile regression have been proposed as a more flexible alternative (see e. g. Koenker and Biliias, 2001; Koenker and Geling, 2001; Fitzenberger and Wilke, 2010a). However, quantile regressions cannot deal with covariates which vary over unemployment duration like the GDP growth rate, the type of benefit received or the remaining entitlement period. We therefore study the effect of the Hartz IV reform on an individual's unemployment duration using the traditional proportional hazard model.

Our findings suggest that due to the Hartz IV reform the individual daily transition probability from unemployment to employment has increased by 30% on average. The effect has been lower in West Germany and larger in East Germany. For the short-term unemployed the probability has risen by about 20% in West Germany and by about 70% in East Germany. The effect seems to be increasing over unemployment duration but independent of a job-seeker's gender. For those unemployed whose entitlement for the short-term unemployment compensation has expired we estimate a negative effect of the Hartz IV reform on the hazard rate. The size of the effect is independent of unemployment duration and a job-seekers region of residence. We estimate a larger negative effect for women in our baseline specification, but find this result to be not robust against the specific data preparation.

The remainder of the paper is organized as follows. The next section briefly states the institutional background with regard to unemployment compensation before and after the Hartz IV reform. Section 3 describes our data and provides a descriptive analysis. Section 4 discusses our identification strategy and section 5 presents the results. In section 6 we check the robustness of our results with respect to sample definition and data preparation. Section 7 concludes.

## 2. Unemployment compensation in Germany

Germany, as many other European countries, uses a two component unemployment compensation scheme. The first component is a mandatory unemployment insurance. This type of unemployment compensation is called unemployment benefit I (*Arbeitslosengeld*, UB I) and amounts to a maximum of 67% of a worker's former net wage. Unemployed workers are eligible for UB I if they have worked at least 180 days in the last two years preceding their current unemployment spell. The potential entitlement duration depends on an individual's age and the number of years she worked. For most workers entitlement is restricted to one year. The second component is tax financed. It applies to any unemployed worker who is not eligible for UB I — either because the entitlement has expired or because the worker does not meet the entitlement requirements — and who meets a means-test. Unemployed workers who are eligible for UB I but whose last earnings have been below a certain level receive an additional payment from the tax-based component. Moreover, unemployed job-seekers are allowed to work up to 15 hours a week in order to increase their household income (spare time work, *Nebenbeschäftigung*).

The Hartz IV reform, becoming effective on January 1, 2005, altered the tax-based unemployment compensation scheme. It combined the former long-term unemployment assistance (*Arbeitslosenhilfe*, UA) and the social assistance (*Sozialhilfe*, SA) to a new unemployment compensation scheme for the long-term unemployed, the unemployment benefit II (*Arbeitslosengeld II*, UB II). While the former UA amounted to a maximum of 57% of the last net wage, the new UB II has a fixed value irrespective of one's past labor income. For the vast majority of job-seekers the level of UB II is lower than the level of UA. Moreover, the means test was tightened. The amalgamation of UA and SA to the new UB II had also a statistical side-effect: Former receivers of SA, which were not counted as unemployed, were considered as entering unemployment from out of the labor force on January 1, 2005. Studies on the Hartz reforms which use aggregate

data have to deal with this structural break. The UB I remained almost unaffected by the Hartz legislation. Just the potential benefit duration for older workers has been shortened at the beginning of 2006.

If we apply dynamic search theory (Mortensen, 1977; van den Berg, 1990) to this institutional background, we arrive at the following proposition: The reduction of the tax-based unemployment compensation (“long-term unemployment benefit”) from its UA level to the new UB II level should have increased the hazard rate of all unemployed, irrespective whether they receive the insurance-based or the tax-based unemployment compensation, i. e. whether they are “short-term unemployed” or “long-term unemployed”.

### 3. Data

We base our estimation on the weakly anonymous Sample of Integrated Labour Market Biographies (SIAB), years 1975 – 2008.<sup>1</sup> Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access. The SIAB contains daily spell data of a representative 2% random sample of all individuals who were either unemployed or employed subject to social insurance contribution between 1975 and 2008. The data stems from notifications of employers to institutions of the social security system, and from notifications by the federal employment agency on unemployment registrations and benefit payments. The SIAB thus provides comprising information at the individual level. We observe, among others, an individual’s year of birth, gender, nationality, daily income (while employed and unemployed), occupation (while employed) and benefit type (while unemployed) as well as the number of days until her entitlement for the insurance-based unemployment benefit UB I expires.

The specific construction of the SIAB allows a person to be reported by several ad-

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<sup>1</sup>A detailed description of the data is provided by Dorner *et al.* (2010).

ministrative sources simultaneously. This results in some curious observations. For example, a person may be observed to be employed while at the same time she receives some unemployment benefit. We call such a simultaneity “overlap”. Given the German labor market institutions such an overlap may or may not represent an actual event. It may as well reflect some data error arising from delayed notification, e. g. when the individual entered unemployment from employment and her last employer delayed the notification of separation to the institutions of the social security system. Bernhard *et al.* (2006) examine different types of these overlaps. According to them many constellations are legal albeit not very likely to happen. Jaenichen *et al.* (2005) identify some simple inconsistencies in a small sample and check for the true events beyond the different notifications. They propose some procedures to adjust for potential misnotifications but have to admit that a more detailed analysis with a larger sample would be necessary in order to give reliable recommendations. In this regard, Kruppe *et al.* (2007) and Scioch and Oberschachtsiek (2009) demonstrate how different data cleansing procedures of the SIAB (or its predecessors) may affect the outcome of data analyses.

Hence, a careful and elaborate data cleansing procedure is advisable. One procedure has been proposed by Nordmeier (2012). Following a recommendation by Jaenichen *et al.* (2005) she first gives priority to employment spells. She then drops “workers with more than 50 employment notifications in a year” in order to remove freelancers and other unsteady employed persons from her sample. Finally she applies the nonemployment proxy of Fitzenberger and Wilke (2010b) to obtain periods of unemployment. The nonemployment proxy defines all periods as unemployment, which follow “an employment period [and] which contain at least one period with [...] transfer payments by the German federal labor office” (Fitzenberger and Wilke, 2010b, pg. 345). We argue that this procedure may result in a misleading classification of observations into labor market states for our purposes. First, giving always priority to employment spells neglects the fact that a recipient of unemployment benefits is allowed to do some spare-time work.



Thus, according to this rule, a person is considered to be employed while she is actually unemployed. This implies a shorter unemployment duration and a higher hazard rate in our analysis. Second, the nonemployment proxy potentially considers also those periods as unemployment during which the individual is not searching for a job at all, e. g. because the individual is a mother on maternity leave or is self-employed. Moreover, the nonemployment proxy explicitly considers periods of participation in active labor market policy (ALMP) measures as unemployment. Participation in ALMP measures can be identified by the receipt of income maintenance (*Unterhaltsgeld, AlgW*), though the actual type of training remains unknown in the SIAB data. This is problematic as income maintenance can also be paid during in-firm training (*betriebliche Ausbildung*) (see Bernhard *et al.*, 2006, pg. 43). Participants of in-firm training potentially consider themselves not as unemployed and thus do not actively search for a job. Participants in other ALMP measures potentially reduce their search effort as well. After all, they are not as intensively supervised by their case workers at the federal employment agency as non-participants.<sup>2</sup> The lower job search and placement efforts by participants and their case workers potentially result in lock-in effects. Hence, applying the nonemployment proxy of Fitzenberger and Wilke (2010b) in our analysis would probably result in an overestimation of unemployment duration and an underestimation of the hazard rate.

We therefore propose an alternative data preparation.

1. We define three different labor market states: employment (defined as employment subject to social security contributions, vocational training, and marginal employment), unemployment, and participation in ALMP measures (as indicated by the receipt of income maintenance). Within unemployment we distinguish between notifications of registered unemployment<sup>3</sup>, and notifications of receipt of UB I, UB II or UA.

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<sup>2</sup>The official unemployment statistics do not consider participants in ALMP measures as unemployed at all.

<sup>3</sup>In contrast to the official unemployment statistic, we consider short-time illness during unemployment as registered unemployment as well.

2. We drop employment spells with zero income and observations regarding pensioners.
3. We give priority to ALMP periods over employment and unemployment spells and drop the according periods from our sample.
4. We drop periods of repeated rapid changes between unemployment and employment, which indicate freelancing and other forms of non-steady employment. The according unemployment periods have nothing to do with unemployment in the economic sense. We consider as repeated rapid changes more than three changes per month between unemployment and employment or vice versa.
5. We correct short overlaps between employment and unemployment spells if the overlaps last at most 15 days. Jaenichen *et al.* (2005) state that such cases mostly result from delayed notification of the end of the preceding spell. We thus adjust the end of the preceding spell according to the begin of the later notification.
6. If an employment spell is completely embedded in a period of benefit receipt, we consider the period of overlap to be spare-time work during unemployment. Note that this way we consider workers who additionally receive UB II because their labor income is too low as being unemployed instead of being employed. We do so because we are not able to identify similar top-up benefits in the pre-reform period. However, given that we later constrain our sample to job-seekers who enter unemployment with an entitlement to UB I, the potential error should not affect our results significantly. We assess this issue further in section 6.
7. If an employment spell overlaps with a period of registered unemployment while there is no notification on benefit receipt, we consider the unemployment notification to be wrong and classify the time of overlap as employment.
8. For all remaining overlaps, especially those being longer than 15 days, we can not ascertain the actual labor market state beyond doubt. Hence, the respective observations are discarded. Of course, it is for example possible that an unemployed

job-seeker starts some spare-time work (jobs with not more than 15 hours of work a week) and works for more than 15 days. But then it appears unreasonable that the unemployment spell should end one day while the person keeps working for same low number of hours a week and the same (potentially low) wage at the same employer. We prefer to delete those observations from our data instead of assigning potentially wrong labor market states.

9. In order to obtain continuous employment biographies we then fill small gaps of up to eight days between employment and unemployment spells or gaps of up to 35 days between notifications of benefit receipt by letting the following spell starting earlier. This way we overcome the fragmentation of unemployment periods which arises from the way unemployment is recorded by the different administrative data sources.

This data preparation leads to a dataset with a time-consistent, unambiguous classification of observations into labor market states, at the price of being very restrictive. We therefore check the robustness of our estimation results in section 6 using two alternative data preparations, which are simpler but leave more observations for the analysis.

Irrespective of the specific data preparation we need to define a pre- and a post-Hartz subsample in order to identify the effect of the Hartz IV reform. The first subsample covers the period from January 1, 2000, to November 30, 2003, while the second subsample covers the period from January 1, 2007, to November 30, 2008.<sup>4</sup> We do not consider spells before 2000 because of data limitations. The year 2004 is ignored in order to avoid anticipation effects. The years 2005 and 2006 are excluded because in these years the official authorities misreported persons receiving the new UB II (Dorner *et al.*, 2010). In order to reduce right-censoring, we do not take into account individuals who become unemployed in the last five months of either observation period. In order

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<sup>4</sup>We are thus able to observe transitions back to employment after the last day of each sample period. Recall that employment spells are recorded in the SIAB data only up to the end of 2008.

to avoid left-truncation we only take into account individuals for whom we observe the transition from employment into unemployment. Because of these restrictions our data set does not include those persons who entered the official unemployment statistics just due to the redefinition of receivers of the former SA on January 1, 2005. We thus avoid the structural break that is observed in aggregate time series. We finally constrain our sample on job-seekers in the age of 25 to 55 who are not disabled and who are looking for a full-time job. We exclude job-seekers who had no entitlement for UB I when entering unemployment or who were unemployed just over holidays like New Years Eve, Labor Day or the German Unification Day. Table 1 provides a description of our final sample.

Table 1: Descriptive Statistics

	men		women	
	2000–2003	2007–2008	2000–2003	2007–2008
<i>begin of observed unemployment</i>				
East Germany	41.79	30.29	45.95	29.84
foreign citizen	8.71	10.42	5.02	7.06
age	38.34	38.93	39.05	38.29
last wage	74.84	72.11	58.54	a)
initial entitlement in days	341.50	301.40	326.50	302.50
<i>end of observed unemployment</i>				
entitlement expired	16.75	6.21	22.94	4.98
transition to employment	65.69	65.95	58.10	64.27
recall	26.60	24.59	15.64	14.76
median duration in days	137.00	97.00	212.00	120.00
Spells	8999	2899	4520	1545

a) Value was deleted by the FDZ due to laws governing data protection and data security.

Our final sample consists of 17,963 unemployment spells, of which 13,519 occurred prior to the introduction of UB II and 4,444 afterwards. About 34% of the unemployed in our sample are women. The share of East German residents among the unemployed is disproportionately high, though it is decreasing over time. This represents the well-known higher incidence of unemployment in East Germany. Foreign citizens account for less than 10% of the unemployment spells. Their share is slightly higher among male

unemployed and is increasing over time. When entering unemployment, the average job-seeker in our sample is about 38 years old. On average, men have earned more than 70 Euro (in 2010 Euro) in their last job, while a woman’s labor income was less than 60 Euro a day. The average initial entitlement for UB I declined from about 11 months prior to the Hartz IV reform to 10 months afterwards. An average initial entitlement period from less than one year implies that our sample contains workers which have not worked for a sufficiently long period in order to gain a full entitlement, which regularly lasts a whole year. This in turn points to the stylized fact that the incidence of unemployment is higher for workers who have already been unemployed a short time before.

Turning to the end of unemployment we observe that men are more likely than women to return to their last employer (“recall”). The share of spells ending in employment amounts to about two thirds for men and increases from 58% to 64% for women. For both, men and women, the median unemployment duration declined, with the decrease being larger for women. We thus conjecture that at first sight the Hartz IV reform probably has had a positive effect on the hazard rate.

## 4. Identification Strategy

In order to identify the exact effect of the introduction of UB II on the individual hazard rate we estimate proportional hazard models of the type:<sup>5</sup>

$$h_i(\tau) = h_0(\tau) \cdot \exp \left\{ \delta_1 Hartz + \delta_2 Hartz \times lastwage + \alpha lastwage + \beta_k X_k + \beta_{GDP} \gamma_{GDP_t} + \sum_{i=2}^4 \varphi_i Season_i \right\} \quad (1)$$

The individual hazard rate at duration  $\tau$  is given by the baseline hazard  $h_0(\tau)$  and a shift term depending on observed characteristics. The variable of interest is the Hartz

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<sup>5</sup>For details on the specification of proportional hazard models see van den Berg (2001).

dummy (*Hartz*) which equals one for all observations in the subsample 2007/2008 and zero otherwise. A positive coefficient to this variable indicates a positive effect of the implementation of the Hartz IV reform on the individual hazard rate. Unfortunately, we are not able to base our estimation on a comparison of treatment and control group because all unemployed are potentially affected by the Hartz IV reform. Even unemployed for whom the income difference between UA and UB II appears negligible are potentially affected as they might fail the means-test for UB II. This means-test is related to household income and fortune which we do not observe in our data.

Thus, in order to identify the effect of the Hartz IV reform correctly we have to control for several side-effects. Most importantly, the implementation of the Hartz IV reform coincided with the starting point of economic recovery (Klinger and Rothe, 2012). The period prior to 2005 was characterized by a significant economic downswing, implying quite long unemployment spells. From 2001 (after the burst of the Dotcom bubble) to 2003 the average growth rate of quarterly real GDP was as low as 0.38% in Germany. In contrast, Germany experienced an economic upturn after 2005 which by itself reduced the expected duration in unemployment. The growth rate of quarterly real GDP amounted to 2.19% on average in 2007 and 2008. In order to control for that issue we include the growth rate of real GDP relative to the same quarter of the previous year ( $\gamma_{GDP_t}$ ) in our regressions. Note that the GDP growth rate does not vary over duration  $\tau$ , but with calendar time  $t$ . We do not use the unemployment rate as a measure for economic activity in order to avoid endogeneity problems. Lower unemployment may increase the transition probability to employment, but a higher transition probability also leads to lower unemployment (given a fixed inflow rate to unemployment).

We further control for a set  $X_k$  of  $k$  socio-demographic characteristics. We include the variables age and age squared as well as dummies for gender, foreign citizen, residence in East Germany, recalls and the type of occupation in the last job. Age is centered at the age of 40. Recalls are defined as returning to the last employer prior to the current unemployment spell. Occupations are classified into 83 occupational groups according

to the Statistics of the German Federal Employment Agency (2009). Seasonal variation in the hazard rate is captured by three season dummies, spring being the base category. We also include a measure of worker's productivity. We expect high productive job-seekers to experience a higher hazard rate. Unfortunately, we are not able to use the skill level as a proxy for productivity, as the information on skills is missing in almost every second spell in the post-reform period. We thus rely on a job-seekers last earnings prior to the current unemployment spells. We deflate these earnings using the German CPI and center the deflated earnings at 70 Euro a day (in 2010 Euro) which is about the sample mean.

Finally, we extend the model by an interaction term of the Hartz dummy variable with the deflated and centered earnings in order to allow for productivity related differences in the effect of the Hartz IV reform. With regard to high productive workers, two opposing mechanisms can be imagined. On the one hand, high productive job-seekers probably have a higher hazard rate. They are thus less likely to experience the expiration of their UB I entitlement. In the extreme case, they are sure to leave unemployment during UB I entitlement and thus do not even care about the Hartz IV reform. If that is the case, we should observe the reform's effect to be decreasing with productivity. On the other hand, high productive workers suffer a larger income loss when running out of their UB I entitlement. The Hartz IV reform has exacerbated this income loss for all unemployed, and the effect is more pronounced for more productive job-seekers. Hence, we expect high productive job-seekers to react more sensitive to the Hartz IV reform than low productive job seekers — the estimated effect of the Hartz reform should be increasing with productivity. In order to inspect which of the two transmission channels prevails we include the interaction of our productivity measure with the Hartz dummy variable in our model.

We estimate equation (1) for various subsamples. First, we estimate a Cox proportional hazard model (Cox, 1972) on the whole sample. This gives a robust estimate of the average effect of the Hartz IV reform on the hazard rate. We expect this estimate

to be close to the estimates obtained by aggregate studies. However, we also expect the effect to differ in size between East and West Germany, between men and women, and according to the entitlement to UB I. We thus re-estimate equation (1) for various subsamples. In a third step we then investigate whether the reform's effect itself is even duration dependent. To this aim we model the baseline hazard as a piecewise-constant function whose intervals are 30 days long. In order to allow the Hartz reform effect to vary over duration, we interact these intervals with the Hartz dummy variable. Besides, the parameterized baseline hazard also enables us to estimate the effect of the Hartz IV reform on median unemployment duration, a measure which is much easier to interpret.

## 5. Results

### 5.1. Average Effect on the Hazard Rate

The estimation results obtained with the robust Cox estimator and based on the whole sample are shown in table 2. The first column presents the results from estimating equation (1) for Germany as a whole while the next two columns give the results for estimating the model separately for West and East Germany, respectively.

Consider at first the results for Germany as a whole. Most of the control variables show the expected signs. Job-seekers with residence in East-Germany or foreign citizens have a significantly lower hazard rate. The hazard rate decreases with age at an increasing rate. Higher GDP growth goes along with higher hazard rates. The hazard rate is higher for persons with higher productivity (as indicated by a higher wage during the last employment) and lower for job-seekers whose UB I entitlement has expired. The latter finding may be due to a large loss of human capital during the prolonged unemployment spell, due to long-term unemployment as a bad signal or due to discouragement. Note that the latter finding contradicts search-theoretical



Table 2: Estimation results: Whole sample

	Germany	West Germany	East Germany
East Germany	0.8095***		
women	0.9580	1.0795**	0.8286***
foreign citizen	0.6825***	0.7017***	0.6697***
age (centered at 40)	0.9878***	0.9857***	0.9912***
age squared	1.0003**	1.0001	1.0005**
last wage (centered at 70)	1.0024***	1.0022***	1.0023**
GDP growth rate	1.0544***	1.0844***	1.0176*
summer	0.8696***	0.8055***	0.9634
fall	0.7544***	0.7103***	0.8226***
winter	0.5316***	0.5410***	0.5120***
recall	3.1550***	3.0892***	3.3144***
entitlement expired	0.6427***	0.6359***	0.6656***
hartz	1.3326***	1.1779***	1.5871***
hartz $\times$ last wage	0.9979***	0.9985*	0.9983
Spells	17,963	10,786	7,177

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

predictions which argue that the loss in unemployment income should result in a higher search intensity, a lower reservation wage and thus in a higher hazard rate. We also observe significant seasonal effects, implying that the hazard rate is highest during spring and then decreases to about half of its value during the winter months.

Consider now the variables of interest. Over the whole sample, the Hartz IV reform has increased an individual's hazard rate on average by about 30%. The effect appears to be decreasing with a job-seekers last earnings. Our estimates thus suggest that workers with higher productivity have better job opportunities anyway and thus have reacted less sensitive to the introduction of the new UB II.

## 5.2. Heterogeneous Treatment Effects Across Subsamples

The last two columns of table 2 show the results for estimating equation (1) separately for West and East Germany. Compared to the whole sample we estimate about the same effects for all the covariates except for one: In West Germany, women are more

likely to find a job than men, while in East Germany it is the other way round. This finding might reflect that the male breadwinner model is more present in West Germany (see also Matysiak and Steinmetz, 2008). Future studies may inspect this issue further.

The effect of the Hartz IV reform varies across regions as well. For West Germany, we estimate an average increase in the hazard rate of 18%. This number is quantitatively confirmed by the estimates of Hertweck and Sigrist (2013) who use aggregate time series calculated from the SOEP data set. In East Germany, the Hartz reform has increased the hazard rate on average by almost 60%. The larger effect of the Hartz IV reform in East Germany is in line with the results of Fahr and Sunde (2009) who find a larger impact of the first three Hartz reform packages in East Germany as well.

Given the heterogeneous effects of a job-seeker's gender and the Hartz IV reform on the hazard rate across German regions, we re-estimated equation (1) also separately for West German men and women as well as for East German men and women. The results are shown in table 3.

Table 3: Estimation results: Whole sample

	West Germany		East Germany	
	men	women	men	women
foreign citizen	0.7132***	0.6874***	0.6525***	0.6765
age (centered at 40)	0.9863***	0.9837***	0.9928***	0.9875***
age squared	0.9999	1.0006*	1.0003	1.0009**
last wage (centered at 70)	1.0022***	1.0022**	1.0020	1.0062***
GDP growth rate	1.0834***	1.0875***	1.0265**	0.9973
summer	0.8042***	0.8553***	1.0149	0.9123
fall	0.6936***	0.7888***	0.8297***	0.8587**
winter	0.4813***	0.7429***	0.4428***	0.7325***
recall	3.2052***	2.7657***	3.3223***	3.3738***
entitlement expired	0.6763***	0.5731***	0.6480***	0.7066***
hartz	1.1744***	1.1979***	1.5626***	1.5523***
hartz $\times$ last wage	0.9987	0.9984	0.9979	0.9965
Spells	7,259	3,527	4,639	2,538

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

In each of the four subsamples the control variables keep their sign or become insignifi-

cant. Two findings seem to be noteworthy, though. First, the magnitude of the seasonal effects appears to be lower for women than for men. This very likely represents some self-sorting of genders into different occupations. For example, in Germany construction workers are (1) usually men and (2) usually laid off at begin of the winter season only to be re-employed during spring by their previous employer. Second, while the expiration of the UB I entitlement on a man’s hazard rate has about the same effect in West and East Germany, the drop in the female hazard rate is much larger in West Germany than in East Germany. Again, this might be a result of different female labor force participation patterns in these two regions.

With regard to the effect of the Hartz IV reform, we again observe significant differences across regions, but we do not observe significant differences across genders within a region. Though, in West Germany the effect appears to be slightly larger for women than for men, while in East Germany it is slightly lower. Contrary to the “aggregated” samples from above we do not observe significant variation in the Hartz effect over a job-seekers last working income.

### **5.3. Heterogeneous Treatment Effects Across Benefit Entitlement**

In order to investigate whether the effect of the Hartz IV reform varies between recipients of the not-altered short-term unemployment benefit and the altered long-term unemployment benefit, we split the observation period for each individual at the date when her entitlement for UB I expires. We call the subsample up to this split date the “UB I subsample” or short-term unemployed and the subsample from the split date on the “UB II subsample” or long-term unemployed. The naming of short- and long-term unemployed follows the distinction made in dynamic search theory though it is somewhat sloppy in the context of the German setting (see the description of the legal setting in section 2). As we separate samples according to the benefit received, we do not need to control for the exhaustion of the UB I entitlement any longer. Instead,

we control for the remaining entitlement period in the UB I subsample. Tables 4 to 7 present the results for the UB I subsamples and UB II subsamples, respectively.

Table 4: Estimation results: UB I subsample

	Germany	West Germany	East Germany
East Germany	0.7956***		
women	0.9566	1.0808**	0.8113***
foreign citizen	0.6741***	0.6925***	0.6027***
age (centered at 40)	0.9855***	0.9853***	0.9867***
age squared	1.0001	1.0001	1.0002
last wage (centered at 70)	1.0026***	1.0026***	1.0022*
GDP growth rate	1.0482***	1.0818***	1.0053
summer	0.8530***	0.7986***	0.9386
fall	0.7297***	0.7018***	0.7715***
winter	0.5194***	0.5365***	0.4861***
recall	3.0862***	2.9905***	3.2660***
remaining entitlement period	1.0002*	1.0000	1.0003**
hartz	1.4064***	1.2255***	1.7080***
hartz $\times$ last wage	0.9971***	0.9977***	0.9978
Spells	17,963	10,786	7,177

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 5: Estimation results: UB I subsample

	West Germany		East Germany	
	men	women	men	women
foreign citizen	0.7050***	0.6753***	0.5869***	0.5733*
age (centered at 40)	0.9845***	0.9855***	0.9882***	0.9838***
age squared	0.9997	1.0007*	1.0001	1.0004
last wage (centered at 70)	1.0026***	1.0023*	1.0021	1.0053***
GDP growth rate	1.0839***	1.0749***	1.0180	0.9728
summer	0.7947***	0.8581**	0.9970	0.8662*
fall	0.6861***	0.7772***	0.7829***	0.7890***
winter	0.4764***	0.7431***	0.4306***	0.6784***
recall	3.1054***	2.6572***	3.3119***	3.2387***
remaining entitlement period	1.0002	0.9995**	1.0004**	1.0000
hartz	1.2374***	1.2281***	1.6879***	1.6893***
hartz $\times$ last wage	0.9978**	0.9980	0.9972	0.9965
Spells	7,259	3,527	4,639	2,538

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 6: Estimation results: UB II subsample

	Germany	West Germany	East Germany
East Germany	0.8551**		
women	0.8787*	0.9549	0.8497
foreign citizen	0.7227***	0.6518***	0.9873
age (centered at 40)	0.9931*	0.9853**	1.0009
age squared	1.0007	1.0004	1.0007
last wage (centered at 70)	1.0000	0.9998	1.0008
GDP growth rate	1.1248***	1.1735***	1.0915**
summer	1.1106	0.9882	1.2428**
fall	0.9921	0.8518	1.1500
winter	0.6664***	0.6508***	0.6910***
recall	4.3028***	5.5444***	4.0108***
hartz	0.1963***	0.1861***	0.2027***
hartz $\times$ last wage	1.0037	1.0021	1.0079
Spells	2801	1254	1547

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 7: Estimation results: UB II subsample

	West Germany		East Germany	
	men	women	men	women
foreign citizen	0.7143**	0.5550**	1.0434	0.8826
age (centered at 40)	0.9766***	0.9944	1.0019	0.9957
age squared	1.0002	1.0002	1.0000	1.0018*
last wage (centered at 70)	0.9990	1.0019	0.9980	1.0109**
GDP growth rate	1.1426***	1.2441***	1.0878*	1.1278**
summer	1.0607	0.9352	1.3587**	1.0636
fall	0.8167	0.9885	1.1808	1.0785
winter	0.6297***	0.6732*	0.5013***	0.9473
recall	6.0920***	6.3968***	3.7823***	4.3225***
hartz	0.1811***	0.1350***	0.2051***	0.1441***
hartz $\times$ last wage	1.0034	0.9939	1.0172**	0.9944
Spells	870	384	817	730

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Compared to the whole sample, in the subsample of those who are still entitled to UB I the control variables exhibit the same effects we have found for the whole sample. Especially, we again find the hazard rate for women compared to men to be higher in

West Germany but lower in East Germany. The remaining entitlement period has only a weak effect on the hazard rate. Surprisingly, the estimated coefficient is positive in most subsamples. This is puzzling, because dynamic search-theory predicts that the hazard rate should increase as the remaining entitlement period diminishes. Probably, the estimated coefficient captures a negative duration dependence in the hazard rate as well. By construction, the remaining entitlement period is highly correlated to the duration in unemployment. The multicollinearity between the two variables is constrained just by the initial entitlement period which varies across job-seekers. If the hazard rate exhibits negative duration dependence — i. e. the probability of finding a job is lower the longer a job-seeker has been unemployed already — and if this effect prevails the effect to increase search efforts as the period of entitlement to UB I diminishes, the coefficient to the remaining entitlement period may turn out positive. In order to disentangle both effects from each other we need to control for duration dependence explicitly. We will do so shortly when we extend our model by a piecewise constant baseline hazard.

In the UB II subsample several estimated coefficients turn out to be statistically insignificant. Neither gender nor a job-seekers last earnings nor the season (except for winter) appear to play a major role in job-search and / or hiring decisions of the long-term unemployed. In contrast, the effect of GDP growth on the hazard rate is still significant and has even increased in value. According to our results, the probability of leaving long-term unemployment to employment is thus less sensitive to a job-seekers own characteristics and more sensitive to general economic conditions.

We also observe very heterogenous treatments effects of the Hartz IV reform. While the estimates point to even larger positive effects for the UB I subsample, we observe significant negative effects for the UB II subsample. For Germany as a whole, the average hazard rate of a UB I recipient increased by 40% due to the Hartz IV reform. The effects amounts to about 22% in West Germany and about 70% in East Germany. Again, we do not observe significant differences in the estimated effects across genders

within a region. The results are reversed in any dimension in the UB II subsample. Here, we observe a drop in the hazard rate by about 80%. The point estimates are stable across regions but differ across genders, implying that the Hartz IV reform harmed female long-term unemployed much more than male long-term unemployed.

The estimated negative effect appears puzzling as job-search theory predicts an unambiguously positive effect of the Hartz IV reform on the hazard rate of the long-term unemployed as well. Moreover, Klinger and Rothe (2012) find that the long-term unemployed benefited more from each of the Hartz reforms than the short-term unemployed. Nevertheless, the observed negative effect is well conceivable. First note that Klinger and Rothe (2012) define long-term unemployment according to the official statistics while our definition applies to the nomenclature used by dynamic search theory. Second, job-seekers who still have an entitlement for UB I may now be willing to accept jobs they would have refused prior to the Hartz IV reform. They now compete with the long-term unemployed, who usually accept these jobs. As most employers prefer short-term to long-term unemployed, the long-term unemployed are left with less job opportunities, which results in a negative effect of the Hartz IV reform on their hazard rate.

The magnitude of the drop, however, seems incredibly large. This may point to a lack of sample size. We will investigate this issue later, when we check the robustness of our results using larger samples stemming from alternative data preparations.

## **5.4. Heterogeneous Treatment Effects Over Unemployment**

### **Duration**

So far, we have discussed the average effect of the Hartz IV reform on the hazard rate. While doing so, we have implicitly assumed that the effect is independent of unemployment duration, at least within the different subsamples. We now relax this assumption. For each region-gender combination of both entitlement-specific subsamples

we re-estimate equation (1) with a piecewise-constant baseline hazard whose intervals are 30 days long. Interacting the intercept for each interval with the Hartz dummy variable enables us to assess the duration dependence of the Hartz effect statistically as well as graphically. Moreover, using a fully parameterized hazard model we are able to estimate the effect of the Hartz IV reform on median unemployment duration.

However, this kind of assessment will be meaningful only if two requirements are met. First, the sample size should be large enough to ensure precise estimation of the interval specific intercepts before and after the Hartz IV reform. This condition is not met by the samples of female long-term unemployed in both, West and East Germany. We thus exclude these two samples from the following analysis. Second, the coefficients of the model should be unbiased. As an approximation we consider an estimate of the full parametric model to be unbiased if it matches its counterpart from the robust Cox estimation qualitatively as well as quantitatively. Results for the full parametric model are given in tables 8 and 9. A comparison of these results to those shown in tables 5 and 7 indicates that in each subsample the estimates for all control variables are unbiased. This makes us confident with regard to the following analysis.

We now turn to the comparison of subsample-specific baseline hazards. All baseline hazards are calculated for a German reference person of age 40 who has earned the sample mean of 70 Euro a day (in 2010 Euro) in her last job, and whose initial entitlement to UB I is 360 days. The person becomes unemployed during spring and the growth rate of quarterly real GDP is set to 1.5%. With the season and the GDP growth rate kept constant throughout unemployment, shifts in the baseline hazard reveal the pattern of duration dependence. Differences between the pre- and post-reform baseline hazards of each subsample then display the effect of the Hartz IV reform.

Figure 1 shows the baseline hazards of the UB I subsample. Note at first the different patterns of duration dependence in the hazard function across regions and genders. Prior to the Hartz IV reform, the hazard function of West German men increases at first and then decreases, while for West German women the hazard function appears rather



Table 8: Full parametric estimation, UB I subsample

	West Germany		East Germany	
	men	women	men	women
foreign citizen	0.7011***	0.6687***	0.5851***	0.5737*
age (centered at 40)	0.9842***	0.9842***	0.9878***	0.9825***
age squared	0.9997	1.0006*	1.0001	1.0003
last wage (centered at 70)	1.0025***	1.0023*	1.0020	1.0052***
GDP growth rate	1.0863***	1.0811***	1.0194	0.9772
summer	0.7897***	0.8439***	0.9808	0.8549**
fall	0.6632***	0.7428***	0.7591***	0.7512***
winter	0.4645***	0.7159***	0.4207***	0.6431***
recall	3.1502***	2.7050***	3.3540***	3.2811***
remaining entitlement period	1.0002	0.9996	1.0005**	1.0002
hartz	1.2031**	1.1809	1.5487***	1.4430**
hartz $\times$ last wage	0.9974**	0.9980	0.9974	0.9966
Spells	7,259	3,527	4,639	2,538

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on a fully parameterized model and are presented as hazard ratios. The model includes a piecewise constant baseline hazard with intervals being 30 days long. Regressions include dummies for 83 occupational groups and interactions of the baseline hazard with the Hartz dummy variable.

Table 9: Full parametric estimation, UB II subsample

	West Germany	East Germany
	men	men
foreign citizen	0.7236**	1.0526
age (centered at 40)	0.9772***	1.0019
age squared	1.0003	1.0000
last wage (centered at 70)	0.9993	0.9979
GDP growth rate	1.1385***	1.0929*
summer	1.0460	1.3651**
fall	0.8215	1.1834
winter	0.6305***	0.4927***
recall	6.0855***	3.7981***
hartz	0.1424***	0.1315***
hartz $\times$ last wage	1.0031	1.0168**
Spells	870	817

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on a fully parameterized model and are presented as hazard ratios. The model includes a piecewise constant baseline hazard with intervals being 30 days long. Regressions include dummies for 83 occupational groups and interactions of the baseline hazard with the Hartz dummy variable.

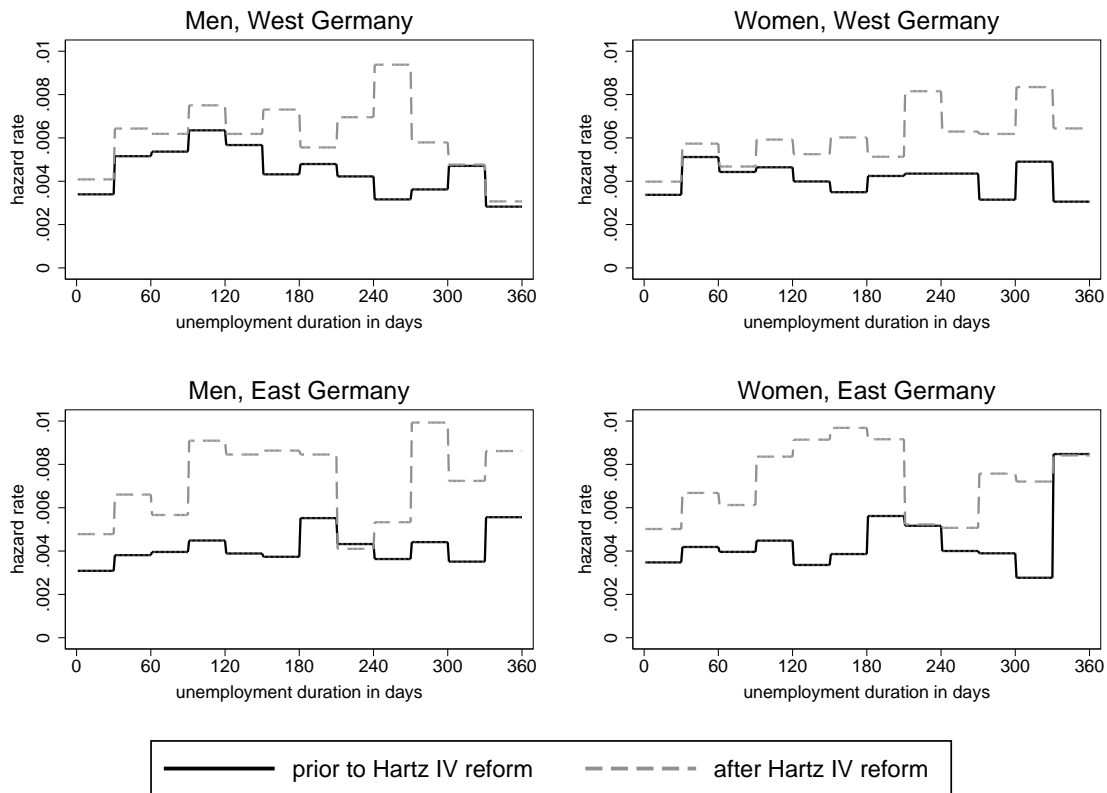


Figure 1: Baseline hazards of the UB I subsample

stable over unemployment duration with only a slight tendency to fall. In contrast, the hazard function of both, East German men and women, is slightly increasing, at least in the first half year of unemployment. East German women then experience a falling hazard rate. With the duration dependence captured now by the baseline hazard, the effect of the remaining entitlement period on the hazard rate becomes insignificant, except for the subsample of East German men. Our earlier presumption regarding this effect is thus confirmed.

The Hartz IV reform had an unambiguously positive effect on the hazard function of each of the four subsamples. This was already suggested by the robust Cox estimates and can now be seen in figure 1. However, the effect of the Hartz IV reform exhibits some form of duration dependence as well. For West German men, the effect is at first duration independent, then increases and afterwards almost vanishes as the entitlement for UB I runs out. In contrast, the effect appears to be positively duration

dependent throughout the UB I entitlement period for West German women. In East Germany the duration dependence of the Hartz effect is less clear cut. At least, figure 1 points to an increasing effect in the first six months of unemployment. Considering that the number of observation diminishes over unemployment duration, we summarize the observed patterns of duration dependence in the Hartz effect as follows: The effect is increasing over unemployment duration in the first ten or eleven months in unemployment, i. e. until shortly before the UB I entitlement expires. Such an outcome is plausible. The Hartz IV reform has increased the loss in unemployment income when the UB I entitlement expires. Thus, unemployed job-seekers will increase their search effort and / or reduce their reservation wage at a higher rate now as the unemployment spell lengthen. This results in a positively duration dependent effect of the Hartz IV reform.

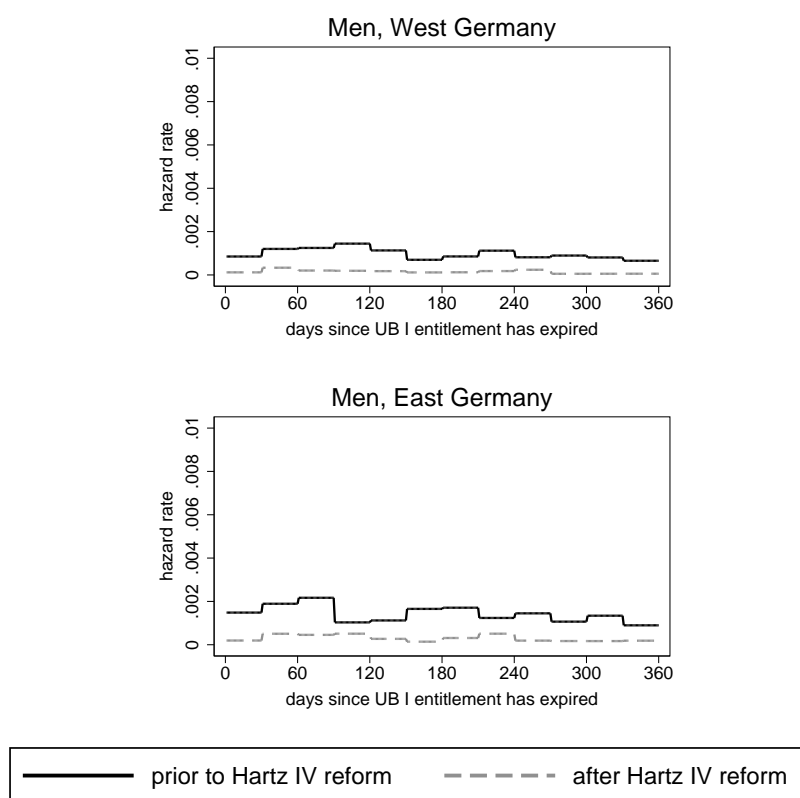


Figure 2: Baseline hazards of the UB II subsample

Figure 2 depicts the baseline hazards of our reference job-seeker after his UB I entitle-

ment has expired. The pre-reform baseline hazards of men in both parts of Germany are comparably stable. They increase slightly at first and then show a tendency to decrease. The Hartz IV reform shifted the baseline hazard downwards, as was already indicated by the robust Cox estimates. This negative effect appears to be duration independent.

With the full parameterized models at hand we are finally able to compute the effect of the Hartz IV reform on unemployment duration. For our reference job-seeker defined above, we estimate a reduction in median unemployment duration by three to four weeks for West Germany and by about eight to ten weeks for East Germany. The effect has thus been more twice as large in East Germany. As before, we do not detect notable differences between men and women in either of the two regions.

## 6. Robustness Checks

In order to assess the robustness of our results, we proceed along two dimensions. First, we check whether our results are sensitive to the exclusion of observations with specific characteristics. Second, we examine the sensitivity with respect to data preparation.

Table 10 shows some of the estimation results for the first kind of robustness check. The first column repeats the result we have obtained for the whole sample (see table 2). In the second column we exclude all job-seekers whose last occupation is potentially subject to seasonal unemployment (i. e. agricultural workers, bricklayer, roofer, road/track constructors, and other construction workers). In the third column we exclude all unemployment spells which ended with a return to the previous employer (“recalls”). All results presented in table 10 refer to the whole sample, i. e. we do not distinguish between East and West Germany, between men and women or with regard to entitlement to UB I. Detailed results for these various subsamples can be found in the Appendix.

The exclusion of seasonal unemployment or of unemployment spells resulting in recalls

Table 10: Whole sample without seasonal unemployment and recalls

	whole sample	without seasonal unemployment	without recalls
East Germany	0.8095***	0.7863***	0.8026***
women	0.9580	0.9764	0.9850
foreign citizen	0.6825***	0.6709***	0.6226***
age (centered at 40)	0.9878***	0.9863***	0.9864***
age squared	1.0003**	1.0003*	1.0005**
last wage (centered at 70)	1.0024***	1.0022***	1.0015***
GDP growth rate	1.0544***	1.0604***	1.0826***
summer	0.8696***	0.8698***	1.0273
fall	0.7544***	0.7749***	0.9272**
winter	0.5316***	0.6087***	0.7674***
recall	3.1550***	3.0104***	
entitlement expired	0.6427***	0.6721***	0.6101***
hartz	1.3326***	1.3164***	1.3240***
hartz $\times$ last wage	0.9979***	0.9980***	0.9983**
Spells	17,963	15,564	13,921

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

does not alter our results qualitatively and quantitatively. Even the estimated seasonal effects appear to be robust against the exclusion of seasonal unemployment, though they decrease in value if we exclude observations ending in recalls. If anything, the estimated effect of the Hartz IV reform decreases only very slightly in value but always remains in the range of about 30% for the whole sample. Our estimation results are robust as well at the level of the various region-, gender- and benefit-specific subsamples (see tables 13 to 18 in the Appendix).

Yet, the question remains whether our results are driven by our data preparation. We thus re-estimate equation (1) for all the samples and subsamples mentioned in the previous section using two alternative data sets. For the first data set, we again give priority to notifications on ALMP over notifications on unemployment or employment. We then give priority to notifications on benefit receipt over employment notifications, which is why we refer to this kind of data preparation as “benefit first”. As a second

alternative to our baseline data preparation we follow quite the opposite strategy to the “benefit first” preparation: At first we give priority to any employment spell, which is inline with a recommendation by Jaenichen *et al.* (2005). Next, we give priority to ALMP participation over unemployment. We call this data preparation “employment first”. Table 11 provides a descriptive comparison of the data sets obtained by the three different data preparations.

Table 11: Descriptive Statistics: Comparing Data Preparations

	baseline		benefit first		employment first	
	2000–2003	2007–2008	2000–2003	2007–2008	2000–2003	2007–2008
spells	13519	4444	66157	23681	77841	26898
entitlement expired	18.82	5.78	19.25	7.17	16.47	6.29
transition to employment	63.26	65.47	60.76	70.20	73.39	73.98
<i>unemployment duration in days</i>						
lower quartile	67	49	70	47	45	31
median	156	103	172	101	111	80
upper quartile	445	231	502	214	287	174

As already mentioned, the sample based on our baseline data preparation consists of 17,963 unemployment spells. This number is considerably lower than the number of spells obtained by the simpler data preparation procedures. The “benefit first” preparation results in a total of 89,838 unemployment spells, and the “employment first” procedure even in 104,739 unemployment spells. These figures are immediate results from the specific construction of the single data sets. For example, both, the “benefit first” and the “employment first” procedure, solve any overlap irrespective of its length in favor of unemployment or employment, respectively, while our baseline data preparation disregards overlaps which are longer than 15 days because they are ambiguous. The large differences in the respective sample sizes indicate that such severe overlaps appear unexpectedly often in the original SIAB data.

However, the structures of the three data sets are quite similar. In each data set, the post-reform period accounts for about one fourth of all unemployment spells. 16% to

19% of all spells in the pre-reform period and 6% to 7% of all spells in the post-reform period continued after the entitlement for UB I has expired. Note that the reduction in these shares over time does not necessarily indicate a reduction in unemployment duration. Rather, due to the shorter sample period after the Hartz IV reform workers becoming unemployed are less likely to be observed after the entitlement has expired. The different data preparation procedures also lead to similar shares of unemployment spells which ended in employment during the sample period as well as in similar distributions of observed unemployment durations. In this regard, two things are noteworthy. First, unemployment duration is lowest and the probability of observing a transition to employment is highest in the “employment first” data set. This is absolutely plausible, as the “employment first” preparation prefers employment spells over unemployment spells whenever it comes to an overlap. Second, irrespective of the data preparation we observe an increase in the transition probability and a corresponding decrease in unemployment duration across sample periods. This probably points to a robust positive effect of the Hartz IV reform on the hazard rate, at least for the whole sample.

Before we turn to the estimation results, we would like to stress that the results based on the “benefit first” and the “employment first” data sets do not necessarily provide an interval for the results based on our baseline preparation. Both simpler data preparation procedures keep many employment and unemployment spells which are discarded by our baseline preparation. Discarded employment spells reduce the sample size, if they are followed by an unemployment spell, because after the data preparation the unemployment spell appears to be left-truncated and is thus discarded as well. A comparison of the estimates across preparations will show whether the discarded unemployment spells drive our results.

Table 12 presents the results for the whole sample. Results for the various subsamples can be found in the Appendix.

For most coefficients, the estimates from our baseline data preparation method lie

Table 12: Different data preparations, whole sample

	baseline	benefit first	employment first
East Germany	0.8095***	0.7620***	0.8599***
women	0.9580	0.9224***	0.9682***
foreign citizen	0.6825***	0.7208***	0.7771***
age (centered at 40)	0.9878***	0.9895***	0.9901***
age squared	1.0003**	1.0005***	1.0003***
last wage (centered at 70)	1.0024***	1.0008***	0.9996
GDP growth rate	1.0544***	1.0620***	1.0417***
summer	0.8696***	0.8655***	0.8958***
fall	0.7544***	0.7778***	0.8055***
winter	0.5316***	0.5378***	0.5621***
recall	3.1550***	2.7569***	2.7581***
entitlement expired	0.6427***	0.6775***	0.7395***
hartz	1.3326***	1.4548***	1.2952***
hartz $\times$ last wage	0.9979***	0.9989***	0.9984***
Spells	17,963	89,838	104,739

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

within the range of the estimates of the two alternative data sets. Especially, while the “benefit first” preparation indicates an increase in the hazard rate due to the Hartz IV reform by 45%, the “employment first” preparation suggests an increase by about 30%. For the subsample of those who are still entitled to UB I the estimated size of the effect ranges from 30% (“employment first”) to almost 50% (“benefit first”). Again, our preferred data preparation gives an estimate within that range (40%). However, the estimated effect of the Hartz IV reform on the hazard rate of the long-term unemployed differs significantly between our baseline data set (-80%) on the one hand and the “benefit first” (-5%) and the “employment first” (-10%) data sets on the other. Moreover, while long-term unemployed women appear to be more harmed by the Hartz IV reform according to our baseline data set, we do not observe similar gender-specific differences in the Hartz effect when using any of the two alternative data sets. Surprisingly, the differences in all other estimated coefficients are less pronounced, though with our baseline data preparation many of the estimates turn out to be statistically insignificant.



In summary, we see our results to be robust against the exclusion of seasonal unemployment or unemployment spells ending in recalls. They are also not driven by some hidden selection procedure in our elaborate baseline data preparation. Differences in the estimates regarding the UB II subsamples probably result from differences in sample size. As explained in the data section, we prefer to exclude observations for which we are not able to determine the actual labor market state unambiguously. Additionally, recall that we consider participation in ALMP measures not as unemployment periods. Both restrictions result in a loss of observations which is increasing over unemployment duration. Hence, our sample of long-term unemployed is much smaller than the corresponding samples from the other two alternative data preparations. A refinement of the data preparation procedure or the inclusion of further years of observation — when available — may solve this issue.

## 7. Conclusion

Using a large German administrative micro data set we study the effect of the German Hartz IV reform on the transition probability from unemployment to employment. The reform has reduced the unemployment compensation for the long-term unemployed (UB II) while leaving the unemployment compensation for the short-term unemployed (UB I) almost unaffected. According to dynamic search theory (Mortensen, 1977; van den Berg, 1990) such a reform will unambiguously increase the transition rate to employment for both, short-term unemployed and long-term unemployed.

We first develop a novel data preparation procedure to solve a major problem of the original data, namely simultaneous observations on the same individual in different labor market states. We then inspect the effect of the reform on the transition probability to employment as well as on median unemployment duration. We also check for heterogeneous treatment effects in various subsamples.

We find that the reform has increased the daily hazard rate by about 30% on average.

For recipients of UB I the effect even amounts to 40%. The effect is larger in East Germany and for lower productive workers, but independent of a job-seekers gender. It is also increasing over unemployment duration for almost the whole first year in unemployment. In contrast, the Hartz IV reform has apparently decreased the transition probability of UB II recipients. This probably reflects an increased competition about jobs between the short-term and the long-term unemployed. The negative effect does not diminish over unemployment duration. It is the same for job-seekers in East and West Germany as well as for men and women. Given our estimates we conclude that the Hartz IV reform has reduced the duration in unemployment by three to four weeks in West Germany and by eight to ten weeks in East Germany. All our results are robust to the exclusion of seasonal unemployment or unemployment resulting in a return to the previous employer. They are also robust against alternative data preparation procedures.

Nevertheless, several questions regarding unemployment duration in Germany require a deeper investigation: Why does gender affect the hazard rate differently in East and West Germany? Why have the Hartz reforms affected East Germany more than West Germany? Does the remaining entitlement period to the short-term unemployment benefit affect a job-seekers search effort as dynamic search theory predicts? We leave it to future research to investigate these issues further.

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## A. Appendix

### A.1. Estimates without seasonal unemployment and recalls

Table 13: Without seasonal unemployment, whole sample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.7863***						0.8554***		
women	0.9764			1.082**			0.6842***	0.6584***	0.6866
foreign citizen	0.6709***			0.6899***	0.6979***	0.683***	0.9899***	0.9917***	0.9868***
age (centered at 40)	0.9863***			0.9842***	0.984***	0.9836***	1.0005*	1.0002	1.0009**
age squared	1.0003*			1.0001	0.9997	1.0006*	1.0015	1.0008	1.0061***
last wage (centered at 70)	1.0022***			1.0022***	1.0022***	1.0022*	1.0182	1.0364**	0.9898
GDP growth rate	1.0604***			1.0924***	1.0953***	1.0874***	0.923*	0.9588	0.8949
summer	0.8698***			0.837***	0.8338***	0.8637**	0.8083***	0.8091***	0.8407**
fall	0.7749***			0.7558***	0.7454***	0.7969***	0.5708***	0.4998***	0.7274***
winter	0.6087***			0.626***	0.5778***	0.7455***	3.1414***	3.1092***	3.2875***
recall	3.0104***			2.9546***	3.0331***	2.7485***	0.6931***	0.6814***	0.7129***
entitlement expired	0.6721***			0.6721***	0.7464***	0.5674***	1.631***	1.6044***	1.56***
hartz	1.3164***			1.1623***	1.155***	1.1949***	0.9994	0.9992	0.9967
hartz × last wage	0.998***			0.9984*	0.9986	0.9984			

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 14: Without seasonal unemployment, UB I subsample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.7746***						0.8349***		
women	0.9753		1.0857**				0.5724***		0.575*
foreign citizen	0.6538***		0.6768***		0.6847***	0.6707***	0.9865***		0.9836***
age (centered at 40)	0.9845***		0.9845***		0.9832***	0.9853***	1.0001	1	1.0005
age squared	1.0001		1.0001		0.9997	1.0007*	1.0013	1.0008	1.0052***
last wage (centered at 70)	1.0024***		1.0026***		1.0027***	1.0023*	1.0064	1.0283*	0.9672
GDP growth rate	1.0548***		1.0903***		1.0969***	1.0753***	0.8997**	0.9388	0.8605*
summer	0.8542***		0.8284***		0.8192***	0.8673**	0.7661***	0.7706***	0.7865***
fall	0.753***		0.7443***		0.7327***	0.7853***	0.5446***	0.486***	0.6829***
winter	0.5996***		0.6253***		0.5756***	0.7491***	3.0703***	3.0784***	3.1445***
recall	2.9362***		2.8616***		2.9456***	2.6352***	1.0003*	1.0005**	1
remaining entitlement period	1.0001		0.9999		1	0.9995*	1.7619***	1.755***	1.6942***
hartz	1.3868***		1.2054***		1.2136***	1.2243***	0.9988	0.9984	0.9967
hartz × last wage	0.9973***		0.9976***		0.9976**	0.998			

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.



Table 15: Without seasonal unemployment, UB Ii subsample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.8131***								
women	0.8904			0.9417			0.8691		
foreign citizen	0.7519***			0.6688***			1.1304		
age (centered at 40)	0.9917**			0.9822***		0.556**	1.0013		0.9316
age squared	1.0009*			1.0002		0.9941	1.0014**		0.9958
last wage (centered at 70)	0.9999			1		1.0003	1		1.0018*
GDP growth rate	1.1269***			1.1715***		1.0023	1		1.0103**
summer	1.0879			1.0038		1.2423***	1.0999**		1.1083*
fall	0.9729			0.8977		0.9341	1.1964		1.0205
winter	0.6962***			0.6584***		0.9974	1.0681		1.0105
recall	4.2459***			5.4055***		0.6349*	0.7338**		0.9248
hartz	0.1858***			0.1811***		6.4537***	3.9414***		4.2122***
hartz × last wage	1.0042			1.0027		0.1364***	0.1738***		0.1345***

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 16: Without recalls, whole sample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.8026***								
women	0.985	1.0916**	0.8299***				0.8299***		
foreign citizen	0.6226***	0.643***	0.7162***	0.6197***	0.6141***	0.6067***	0.6067***	0.6141***	0.589*
age (centered at 40)	0.9864***	0.9834***	0.9821***	0.9836***	0.9821***	0.9821***	0.9907***	0.9928**	0.9862***
age squared	1.0005**	1	1.0004	0.9998	1.0004	1.0004	1.0008***	1.0007*	1.0011**
last wage (centered at 70)	1.0015***	1.0013**	1.0013	1.0014*	1.0013	1.0013	1.0008	1.0001	1.0053***
GDP growth rate	1.0826***	1.1155***	1.1031***	1.1211***	1.1031***	1.1031***	1.0407***	1.0498***	1.0167
summer	1.0273	0.9821	0.9808	0.9882	0.9808	0.9808	1.1034**	1.1695***	0.9974
fall	0.9272**	0.904**	0.8885*	0.9185	0.8885*	0.8885*	0.9738	0.9773	1.0011
winter	0.7674***	0.8226***	0.9467	0.756***	0.756***	0.9467	0.6788***	0.5703***	0.9368
entitlement expired	0.6101***	0.5744***	0.5003***	0.6206***	0.6206***	0.5003***	0.6592***	0.6779***	0.6234***
hartz	1.324***	1.1624***	1.187***	1.163***	1.163***	1.187***	1.7***	1.7195***	1.5712***
hartz × last wage	0.9983**	0.9987	0.999	0.9985	0.9985	0.999	0.9995	0.9991	0.9968

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 17: Without recalls, UB I subsample

	Germany				West Germany			East Germany		
	all	men	women	all	men	women	all	men	women	
East Germany	0.7788***						0.8173***			
women	0.9893			1.0978**			0.537***	0.5626**	0.4874*	
foreign citizen	0.604***			0.6227***	0.5894***	0.721***	0.9866***	0.9907**	0.9791***	
age (centered at 40)	0.9857***			0.9851***	0.985***	0.9842***	1.0005	1.0007*	1.0003	
age squared	1.0003*			1.0001	0.9998	1.0006	1.0009	1.0007	1.0042**	
last wage (centered at 70)	1.0019***			1.0019***	1.0021**	1.0015	1.0245	1.0428**	0.9821	
GDP growth rate	1.0768***			1.1144***	1.1265***	1.0898***	1.0722	1.1722**	0.8956	
summer	1.0078			0.9705	0.9757	0.9716	0.9404	0.9573	0.9278	
fall	0.9068***			0.8886***	0.9043*	0.8637**	0.6476***	0.5698***	0.8429*	
winter	0.7602***			0.8226***	0.7572***	0.9408	1.0002	1.0001	1.0004	
remaining entitlement period	0.9999			0.9996**	0.9997	0.9995*	1.8714***	1.8928***	1.7475***	
hartz	1.401***			1.2042***	1.2151***	1.2146***	0.9987	0.9975	0.9969	
hartz $\times$ last wage	0.9973***			0.9977**	0.9973**	0.9987				

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 18: Without recalls, UB II subsample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.8795*						0.7697**		
women	0.8618*		0.968				0.8665	0.8512	0.772
foreign citizen	0.685***		0.6266***	0.6819**		0.4971**		1.0014	0.9977
age (centered at 40)	0.9927*		0.9807***	0.9733***		0.9956	1.0013	1.0009	1.0025**
age squared	1.0012**		1.0002	1.0005		0.9999	1.0016**	0.9949*	1.0091**
last wage (centered at 70)	0.9994		0.9995	0.9992		1.0014	0.9985	1.1374**	1.1645**
GDP growth rate	1.1591***		1.1941***	1.1538***		1.2726***	1.1401***	1.2737*	1.2954
summer	1.2417**		1.187	1.2005		1.2218	1.2874**	1.0499	1.178
fall	1.0883		1.0813	1.0264		1.2191	1.0918	0.5174***	1.2701
winter	0.7967**		0.8014	0.7493		0.8775	0.8009*	0.1519***	0.1036***
hartz	0.1545***		0.1512***	0.1374***		0.1364***	0.1491***	1.0205**	0.9907
hartz × last wage	1.0046		1.0026	1.0049		0.9958	1.0074		

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

## A.2. Estimates using alternative data preparations

Table 19: “benefit first”, whole sample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.762***						0.807***		
women	0.9224***	1.0318**					0.5643***	0.5725***	0.5638***
foreign citizen	0.7208***	0.7481***	0.7171***	0.7612***	0.988***	0.9855***	0.9931***	0.9951***	0.989***
age (centered at 40)	0.9895***	0.9872***	0.9855***	0.988***	0.988***	0.9855***	0.9931***	0.9951***	0.989***
age squared	1.0005***	1.0003***	1.0006***	1.0002*	1.0002*	1.0006***	1.0007***	1.0005***	1.0012***
last wage (centered at 70)	1.0008***	1.0008***	1.0791***	1.086***	1.086***	1.0791***	1.0034***	1.04***	1.0208**
GDP growth rate	1.062***	1.0833***	0.9002***	0.8179***	0.8179***	0.9002***	1.0345***	0.9745	0.8892***
summer	0.8655***	0.8304***	0.9073***	0.6994***	0.6994***	0.9073***	0.9254***	0.8227***	0.8714***
fall	0.7778***	0.7555***	0.7729***	0.4819***	0.4819***	0.7729***	0.8242***	0.4378***	0.717***
winter	0.5378***	0.5537***	2.4579***	2.7273***	2.7273***	2.4579***	0.5098***	2.9978***	2.8979***
recall	2.7569***	2.6378***	0.63	0.7715***	0.7715***	0.63	2.9517***	0.6623***	0.6575***
entitlement expired	0.6775***	0.7215***	1.2831***	1.337***	1.337***	1.2831***	0.6521***	1.6267***	1.804***
hartz	1.4548***	1.3117***	1.0016***	1.0008***	1.0008***	1.0016***	1.6786***	1.0032***	1.0044***
hartz × last wage	0.9989***	0.9992***	0.9995	0.9989***	0.9989***	0.9995	0.9975***	0.9979**	0.9976**

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 20: “benefit first”, UB I subsample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.7543***						0.7961***		
women	0.9307***		1.0421**				0.5236***	0.5247***	0.5278***
foreign citizen	0.7007***		0.727***		0.7379***	0.7069***	0.9877***	0.9895***	0.9849***
age (centered at 40)	0.9862***		0.986***		0.9849***	0.9873***	1.0002**	1	1.0008***
age squared	1.0002***		1.0002**		0.9999	1.0008***	1.0031***	1.0028***	1.0044***
last wage (centered at 70)	1.0008***		1.0008***		1.0008***	1.0018***	1.0241***	1.0313***	1.0068
GDP growth rate	1.0572***		1.082***		1.084***	1.0791***	0.9107***	0.9539**	0.889***
summer	0.849***		0.8161***		0.8045***	0.8912***	0.7956***	0.7966***	0.8475***
fall	0.7625***		0.7492***		0.696***	0.8985***	0.4891***	0.4217***	0.7113***
winter	0.5258***		0.5461***		0.4711***	0.7775***	2.9486***	3.03***	2.794***
recall	2.7349***		2.6061***		2.6985***	2.3826***	1.0005***	1.0006***	1.0001
remaining entitlement period	1.0003***		1.0001*		1.0005***	0.9994***	1.7796***	1.7312***	1.8867***
hartz	1.4911***		1.3199***		1.3679***	1.2637***	0.9975***	0.9981**	0.9972**
hartz × last wage	0.9988***		0.9992***		0.9988***	0.9993			

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 21: “benefit first”, UB II subsample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.871***								
women	0.9034***			0.9707			0.8523***		
foreign citizen	0.8561***			0.883***	0.9116**	0.7747***	0.6839***	0.7075***	0.6872**
age (centered at 40)	0.9924***			0.9854***	0.9839***	0.9882***	0.9994	0.9997	0.9972
age squared	1.0014***			1.0008***	1.001***	1.0005	1.0017***	1.0015***	1.0021***
last wage (centered at 70)	1.0012***			1.0006	1.0007	1.0013	1.0024**	1.0023**	1.0035**
GDP growth rate	1.0712***			1.0759***	1.0733***	1.0819***	1.0668***	1.0597***	1.0879***
summer	1.0722***			1.0347	1.0353	1.0338	1.1151***	1.2754***	0.8945*
fall	0.9655			0.9116**	0.8836***	0.9819	1.0315	1.0919*	0.9601
winter	0.6397***			0.6496***	0.6184***	0.7259***	0.6292***	0.5332***	0.7646***
recall	3.2845***			3.2745***	3.2241***	3.4983***	3.3084***	3.1***	3.6512***
hartz	0.946*			0.927*	0.9219*	0.9615	0.9451	1.0186	0.7681**
hartz × last wage	1.0021***			1.0024***	1.0016	1.0043**	1.0021	1.0022	1.0009

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 22: “employment first”, whole sample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.8599***						0.8735***		
women	0.9682***			1.0497***			0.6695***	0.6996***	0.612***
foreign citizen	0.7771***			0.7972***		0.6877***	0.9935***	0.9937***	0.9934***
age (centered at 40)	0.9901***			0.9879***		0.9885***	1.0002**	1.0001	1.0004***
age squared	1.0003***			1.0002***	1.0001	1.0004***	0.9985***	1.0174***	1.0114
last wage (centered at 70)	0.9996			0.9999	1.0694***	1.0529***	1.0155***	1.0022	0.9147***
GDP growth rate	1.0417***			1.064***	0.8378***	0.9373***	0.9545***	0.8281***	0.9123***
summer	0.8958***			0.8554***	0.7253***	0.9449**	0.8411***	0.4734***	0.7769***
fall	0.8055***			0.7826***	0.5001***	0.7975***	0.5494***	2.9666***	2.6741***
winter	0.5621***			0.5696***	2.6842***	2.5688***	2.8596***	0.6825***	0.7847***
recall	2.7581***			2.6455***	0.7885***	0.7501***	0.7123***	1.4494***	1.6084***
entitlement expired	0.7395***			0.7757***	1.2068***	1.1728***	1.4693***	0.9993	0.9966***
hartz	1.2952***			1.1866***	1.0003	0.999	0.9989	0.9983**	1.0015
hartz × last wage	0.9984***			0.9989***	0.9982***	1.0002			

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.



Table 23: “employment first”, UB I subsample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.8571***								
women	0.9689**			1.0553***			0.8589***		
foreign citizen	0.7547***			0.776***	0.8187***	0.6625***	0.6243***	0.6396***	0.6002***
age (centered at 40)	0.9898***			0.9891***	0.987***	0.9921***	0.9913***	0.9909***	0.9922***
age squared	1.0003***			1.0004***	1.0001	1.0009***	1.0001	0.9999	1.0005**
last wage (centered at 70)	0.9995			1	1.0002	0.9998	0.9978***	0.9985**	0.996***
GDP growth rate	1.0387***			1.0639***	1.0688***	1.0541***	1.0083*	1.0115**	1.0012
summer	0.8829***			0.8453***	0.8272***	0.9361***	0.9413***	0.9829	0.9162***
fall	0.7943***			0.7743***	0.7214***	0.9331***	0.8266***	0.8075***	0.9203***
winter	0.5534***			0.5642***	0.4917***	0.8032***	0.5361***	0.463***	0.7802***
recall	2.773***			2.6279***	2.6662***	2.5127***	2.9162***	3.0168***	2.7103***
remaining entitlement period	1			0.9998***	1.0001	0.9991***	1.0002**	1.0003***	1
hartz	1.3078***			1.182***	1.2165***	1.1471***	1.5143***	1.4973***	1.6633***
hartz × last wage	0.9982***			0.9987***	0.9981***	0.9997	0.9987*	0.9983**	1.0013

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are based on the robust Cox estimator and are presented as hazard ratios. Regressions include dummies for 83 occupational groups.

Table 24: “employment first”, UB I subsample

	Germany			West Germany			East Germany		
	all	men	women	all	men	women	all	men	women
East Germany	0.8841***								
women	0.942***			0.9729			0.9291**		
foreign citizen	0.9329**			0.956	1.0053	0.8192***	0.8249***	0.9375	0.5778***
age (centered at 40)	0.9928***			0.9894***	0.9856***	0.9979	0.9968**	0.996*	0.9981
age squared	1.0004**			1.0002	1.0004	0.9998	1.0004**	1.0005**	1.0001
last wage (centered at 70)	0.9992*			0.9991	1.0001	0.9965***	0.9996	1.0007	0.9984
GDP growth rate	1.0792***			1.0779***	1.0854***	1.0615***	1.0801***	1.0875***	1.0719***
summer	1.0457**			1.0069	1.008	1.0094	1.0942***	1.2262***	0.9232
fall	0.9078***			0.8758***	0.8295***	1.0102	0.9493	1.0032	0.8837**
winter	0.633***			0.6493***	0.6202***	0.7285***	0.6177***	0.5561***	0.7038***
recall	2.7808***			2.874***	2.8547***	3.0108***	2.7179***	2.6839***	2.7596***
hartz	0.9122***			0.904***	0.8945***	0.98	0.9216*	0.9258	0.9014
hartz × last wage	1.0013**			1.0012	0.9992	1.0063***	1.002*	1.0009	1.003

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01; Standard errors are clustered at the individual level. Regression results are presented as hazard ratios. Regressions include dummies for 83 occupational groups.