Bounds analysis of competing risks: a nonparametric evaluation of the effect of unemployment benefits on migration in Germany.*

Melanie Arntz†

M. S. Simon Lo‡

Ralf A. Wilke§

Preliminary version: March 2007

Abstract

We consider a model with competing risks failure times and partially identified interval data. The data problems imply that effects on risk-specific failure distributions can only be bounded. We develop a non-parametric bounds analysis of risk-specific cumulative incidence curves (CIC) to bound a difference-in-differences treatment effect on the CIC over different definitions of the latent durations. Our simulations demonstrate the applicability of this approach also in case of dependent competing risks. We then apply our framework to empirically evaluate the effect of unemployment benefits on observed migration probabilities in Germany. Our findings weakly indicate that reducing the maximum receipt of unemployment benefits increases the migration probability, at least among high-skilled individuals.

Keywords: cumulative incidence curve, partially missing data, bounds analysis, difference-in-differences

JEL: C41, C14, J61

---

*We would like to thank Simon Lee and the participants of a seminar at the University of Leicester for helpful comments. Melanie Arntz gratefully acknowledges financial support by the German Research Foundation (DFG) through the research project "Potentials for more flexibility of regional labor markets by means of interregional labor mobility".

†ZEW Mannheim, E–mail: arntz@zew.de.

‡University of Mannheim and ZEW Mannheim, E–mail: losimonms@yahoo.com.hk

§University of Leicester, Department of Economics, University Road, Leicester LE17RH, UK, E–mail: raw27@le.ac.uk

1 Introduction

In this paper, we present an approach how to empirically analyze competing risks models in the case of partially missing information concerning the failure times. As an example, administrative unemployment duration data from Germany provide only incomplete information concerning the duration until leaving unemployment to different failure types because there are unobserved periods in an individual's employment trajectory (Fitzenberger and Wilke, 2004). As a result of such incomplete information, parameters of interest can only be bounded (Manski, 2003). As proposed by Abadie (2005), Lee and Wilke (2005) bound a difference-in-differences treatment effect over different definitions of the latent durations. In the context of incomplete information concerning exits from unemployment to destinations other than employment such as out of labor force or self-employment, Lee and Wilke (2005) assume independence between competing risks and thus independent censoring within a single risk framework. If this assumption holds, estimated bounds for the marginal survivor function are unbiased while they are biased in case of dependent competing risks.

The contribution of this paper is to extend the bounds framework for partially identified data to a competing risks setting and derive bounds for the empirical cumulative incidence function instead of using independent censoring as proposed by Lee and Wilke (2005). Cumulative incidence curves (CIC) refer to the empirical probability of failing from one of the causes until time t in the presence of all competing failure types (Kalbfleisch and Prentice, 1980; Pepe, 1991; Pepe and Mori, 1993). By using the CIC, our analysis yields bounds for estimates of the effects on observed failure time distributions even in case of dependent risks. It is thus a generally applicable tool to describe and analyze observed failure time distributions.

Changes of the CIC, however, convey only limited information concerning the causal effect on the underlying marginal distribution of the latent failure times. This is because the CIC does not tackle the general identification problem of competing risks models and does not recover the underlying risk-specific marginal distribution (Cox, 1962; Tsiatis, 1975). Instead, the CIC gathers information about the observed distribution of failures and thus circumvents the inherent non-identifiability of competing risks data. In light of this fundamental identification problem, non-parametric bounds on the marginal distribution as have been proposed by Peterson (1976) typically are too wide to infer some causal effect. As an alternative, parametric assumptions can be imposed to identify the marginal distribution of a particular failure type. Under rather restrictive assumptions, Heckman and Honoré (1989) and Abbring and van den Berg (2003) show identification of the semiparametric mixed proportional hazard model. Honoré and Lleras-Muney (2006) impose quite mild assumptions to obtain tight bounds for parameters within the accelerated failure time model. Our approach avoids such parametric assumptions which are unlikely to be met. The accelerated failure time model, for example, assumes a constant effect of a regressor on the conditional risk-specific failure distributions. Since we develop our model with the purpose to apply it to a reform of the unemployment compensation system that is unlikely to have a constant effect, we require a more flexible specification. See Fitzenberger and Wilke (2007) for more details on this topic. Although our approach cannot solve the fundamental identification problem of competing risks failure models, it still provides a flexible descriptive tool for the observed distribution of competing failures under a variety of settings such as dependent risks. In particular, our approach is fully nonparametric in the sense that we do not impose assumptions that may be violated in the real world. It tackles an important identification problem that stems from missing failure time information which is present in administrative individual data if interval duration can only be partly observed. Since administrative processes often record only certain labor market states, this should be relevant for many countries. The growing literature using these data is another indicator for the relevance of our work.

After presenting the theoretical framework for a bounds analysis of risk-specific cumulative incidence curves in the case of partially missing data, a simulation demonstrates the applicability of this approach in case of dependent competing risks in contrast to a bounds analysis based on the Kaplan-Meier estimator of the marginal survivor curve. We then apply our approach to an economic research question that has already raised some research interest, namely the effect of unemployment benefits on the probability of migration. Several studies have already looked at the link between transitions from unemployment and unemployment benefits. These studies generally confirm a disincentive effect of unemployment compensation on the transitions from unemployment to employment (Katz and Meyer, 1990; Card and Levine, 2000; Lalive and Zweimüller, 2004; van Ours and Vodopivec, 2006). These findings are in line with the predictions from search theory that considers unemployment benefits to raise reservation wages (Atkinson and Micklewright, 1991). The effect of unemployment benefits on migration, however, are much less clear. The negative effect of rising reservation wages and smaller geographical search horizons as a reaction to higher benefit levels (Hassler at al., 2005) contrasts a positive resource effect as higher UB levels enable individuals to bear migration cost (Tatsiramos, 2003) and to increase expenditures that enhance the productiveness of job search (Barron and Mellow, 1979; Tannery, 1983). Most studies, however, seem to suggest a mobility-reducing effect of unemployment benefits on migration. Goss and Paul (1990) find that unemployment benefits reduce migration probabilities among involuntary unemployed in the US. Antolin and Bover (1997) present evidence from Spain that registered unemployed with benefit receipt are less mobile than their non-registered counterparts. Consistent with these findings, Arntz (2005) and Arntz and Wilke (2006) provide some evidence that unemployed in Germany who are entitled to receive unemployment benefits (UB) for an extended period of more than 18 months are much less likely to leave unemployment and migrate than individuals with a shorter period of UB receipt. To some extent, however, the findings of these studies may be driven by an unobserved selection of immobile individuals into unemployment benefits or an extensive receipt of UB. In a study with individual fixed effects that should mitigate this selection problem, Tatsiramos (2003) finds a positive effect of unemployment benefits on migration, a result that he assigns to the mobility-enhancing resource effect of unemployment benefits. As a drawback, however, this study does not take account of competing transitions to local employment. Our bounds analysis thus reexamines the effect of shorter unemployment benefit receipt on the transitions to either local or non-local employment via migration. For this purpose, we exploit a natural experiment that generates some exogenous variation of entitlement length, namely the reform of unemployment benefit entitlements in Germany in 1997. This reform reduced the length of entitlements for certain age groups by up to 10 months. As a consequence, it is possible to construct a treatment group with shortened entitlement length and a control group for whom entitlements have been unaffected by the reform. Based on two different definitions of the latent durations until exiting to either local or non-local employment, it is possible to apply our bounds framework to the inference of the effect on the CIC for migration and for other exit states. The findings indicate that the degree of uncertainty in the German administrative data that is due to unobserved periods in an individual's employment trajectory at first precludes any clear inference as the bounds tend to be very wide. When introducing an additional assumption concerning the missing information, bounds are generally much tighter. For high-skilled individuals, for whom the threat of entitlement loss due to the 1997 reform is likely to be largest, the corresponding bounds are indicative for the mobility-reducing effect of extensive UB receipt.

The paper is structured as follows. The following section presents the theory of a competing risks bounds analysis of cumulative incidence curves in the case of partially missing information. We then simulate the applicability of this approach in case of non-random censoring and dependent competing risks. Section four applies the proposed method to analyze the effect of unemployment benefits on the cumulative incidence of migration. Section five concludes.

2 Model

Let the pair (T, R) be the latent failure times and exit states, respectively, where the failure times are observed on a discrete scale $t_1 < t_2 < \ldots < t_k$ and exit states $R = 1, \ldots, r, \ldots, z$. Latent censoring time is denoted by C. There are $i = 1, \ldots, N$ independent and identically distributed observations. Failure time for risk r of observation i, $(T_i, r) = t_j$ is observed only if

$(T_i, r) = min_R\{(T_i, R), C_i\}$. We assume independent censoring throughout the paper, i.e.

$$\begin{aligned} \lambda_{rj} &= P[(T,r) = t_j | (T,1) \geq t_j \cap \ldots \cap (T,r) \cap \ldots \cap (T,z) \geq t_j \cap C \geq t_j] \\ &= P[(T,r) = t_j | (T,1) \geq t_j \cap \ldots \cap (T,r) \cap \ldots \cap (T,z) \geq t_j], \end{aligned} \quad (1)$$

which means that the censoring in the data does not change the risk-specific distribution of failure times and thus the hazard rate λ_{rj}. We suppose further that failure type r and censoring occurs with multiplicity d_{rj} and c_j at time t_j, respectively. If all competing risks are independent, the hazard rate from (1) becomes

$$\lambda_{rj} = P[(T,r) = t_j | (T,r) \geq t_j], \quad (2)$$

and the likelihood function can be written as

$$L = \prod_{j=1}^{k} \left\{ \prod_{r=1}^{z} P[(T,r) = t_j]^{d_{rj}} P[C = t_j]^{c_j} \right\}. \quad (3)$$

The likelihood L can be maximized by replacing $P[C = t_j]$ by $P[T > t_j] = P[(T,1) > t_j \cap \ldots \cap (T,z) > t_j]$ as the only contribution to the likelihood by the censored data, and after rearranging,

$$L = \prod_{j=1}^{k} \left\{ \prod_{r=1}^{z} \lambda_{rj}^{d_{rj}} (1 - \lambda_j)^{n_j - d_j} \right\}, \quad (4)$$

where $\lambda_j = \sum \lambda_{rj}$, $d_j = \sum d_{rj}$ and $n_j = (d_j + c_j) + \ldots + (d_k + c_k)$ represents the number of observations at risk. Using the first-order-condition, the Kaplan-Meier estimate of λ_{rj} which maximizes (4) and thus the (overall) survivor function and the risk-specific cumulative incidence function (CIC) are given by

$$\begin{aligned} \hat{\lambda}_{rj} &= \frac{d_{rj}}{n_j}, \quad \text{and} && (5) \\ \hat{S}(t_j) = \hat{P}[T > t_j] &= \prod_{l=1}^{j} (1 - \hat{\lambda}_l) = \prod_{l=1}^{j} \frac{n_l - d_l}{n_l}, && (6) \\ \hat{I}_r(t_j) = \hat{P}[(T,r) \leq t_j] &= \sum_{l=1}^{j} \hat{\lambda}_{rl} \hat{S}(t_l) = \sum_{l=1}^{j} \frac{d_{rj}}{n_1}. && (7) \end{aligned}$$

It can be shown that, in the independent competing risks setting, (5) has the same value if we treat the competing risks other than r as censored, i.e. replacing $P[(T, R \neq r) = t_j]$ by $P[T > t_j]$ in (3). In other words, censoring is equivalent to independent competing risks with unknown exit states. For the survivor function, however, the definition of competing risks and censoring are not interchangeable. Since λ_j is a sum of all risk-specific hazard rates, treating some of the competing risks as censored changes the estimated (overall) survivor curve as in (6) to reflect only the probability of survival from the remaining competing risks. In the case of independent competing risks, treating all competing risks besides r as censored still produces consistent results. The Kaplan-Meier survivor curve from (6) becomes a consistent estimator for the marginal survivor curve specific to risk r only, i.e. (4) and (6) becomes

$$\begin{aligned} L^c &= \prod_{j=1}^{k} \left\{ \lambda_{rj}^{d_{rj}} (1-\lambda_{rj})^{n_j - d_{rj}} \right\}, \text{ and} &(8)\\ \hat{S}^c(t_j) &= \prod_{l=1}^{j} (1-\hat{\lambda}_{rl}) = P[(T,r) > t_j]. &(9) \end{aligned}$$

In the case of dependent competing risks, the above Kaplan-Meier type estimator is biased (see Moeschberger and Klein (1995) for a review). As hazard rates are now interdependent, competing risks cannot arbitrarily be considered as censored observations. Nevertheless, as is shown by Cox (1962) and Tsiatis (1975), from any observed distribution $(\tilde{T}, r)$ with dependent joint probability $P[(\tilde{T}, R) > t_j] = P[(T,1) > t_j \cap \ldots \cap (T,z) > t_j]$ an independent joint probability function $(\acute{T}, R)$ can always be fitted which produces the same observed distribution, i.e.

$$\begin{aligned} P[(\tilde{T}, R) > t_j] &= P[(T,1) > t_j \cap \ldots \cap (T,z) > t_j] \\ &= P[(\acute{T},1) > t_j] \ldots P[(\acute{T},z) > t_j]. &(10) \end{aligned}$$

$P[(\acute{T}, r) = t_j]$ is the marginal density function of the hypothetical distribution of value zero if r is not observed at t_j, and thus $P[(\tilde{T}, r) = t_j] = P[(\acute{T}, r) = t_j]$ holds for all r and t_j. Thus, the Kaplan-Meier estimator from (6) still produces unbiased estimates of the overall survivor curve when replacing the true failure time T with the assumed independent $\acute{T}$ in (2) to (6) and treating all dependent risks as competing risks. It is no longer a consistent estimator for the marginal survivor curve specific to risk r. This is the result of the non-identification problem, and thus the underlying marginal probability for each failure cannot be identified without any additional parametric assumptions.

The cumulative incidence curve (CIC) has been suggested as an alternative non-parametric tool which has a meaningful interpretation also in presence of dependent competing risks (Kalbfleisch and Prentice, 1980; Pepe, 1991; Pepe and Mori, 1993). The CIC refers to the observed probability of experiencing a specific failure type prior to time t_j in the presence of all competing failure types. It therefore does not recover the underlying risk-specific marginal distribution. Instead, it refers to observed failure probabilities which are also well defined in the case of dependent competing risks. In other words, the CIC offers a descriptive tool.

In what follows, we present bounds for the CIC in a context of partially identified data. In such a setting, (T_i, z) is not exactly identified and we assume a lower- and upper-bounded latent distribution for z, i.e. $(T^{LB}, z) < (T, z) < (T^{UB}, z)$. The latent distribution for the remaining risks, $(T, R \neq z)$, are fully identified and have no bounds, i.e.

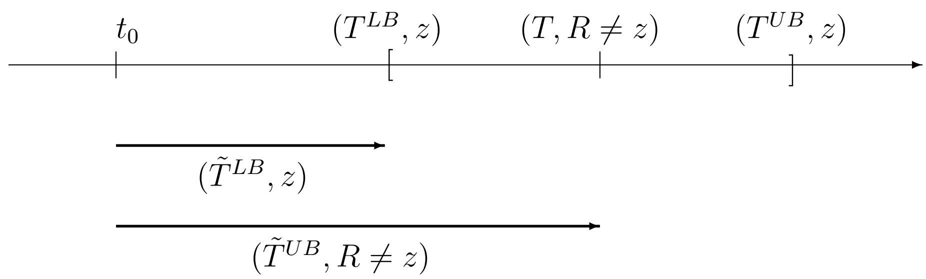
$$\begin{aligned}(T^{LB}, R) &= \{(T,1), \ldots, (T, z-1), (T^{LB}, z)\}, \text{ and} \\ (T^{UB}, R) &= \{(T,1), \ldots, (T, z-1), (T^{UB}, z)\}.\end{aligned}$$

Thus, we also observe a lower bound and upper bound for each risk-specific duration time, $(\tilde{T}_i^{LB}, R)$ and $(\tilde{T}_i^{UB}, R)$. This is because the observed distribution for risks $R \neq z$ depends on the bound for risk z and the bounded failure data on risk z will change the observations on risks other than z. If there was no missing information problem, the observable duration time for risk z is $(\tilde{T}_i, z)$. Instead, given the partial identification problem, if we observe a failure to risk z, then we have for sure observed the lower bound $(\tilde{T}_i^{LB}, z)$. The upper bound for this duration is defined by $min\{(T_i, r), (T_i^{UB}, z), C_i\}$ for $r = 1, \ldots, z-1$. It means the upper bound my differ not also in terms of duration but also in terms of the exit state:

$$\begin{aligned}
&(i) && (\tilde{T}_i^{LB}, z) \leq (\tilde{T}_i, z) \leq (\tilde{T}_i^{UB}, z), \text{ or} \\
&(ii) && (\tilde{T}_i^{LB}, z) \leq (\tilde{T}_i, z) \leq (\tilde{T}_i^{UB}, R \neq z), \text{ or} \\
&(iii) && (\tilde{T}_i^{LB}, z) \leq (\tilde{T}_i, z) \leq C_i^{UB}
\end{aligned}$$

As an illustration, Figure 1 shows the case where the true $(\tilde{T}_i, z)$ falls in region (ii). Under the definition of the lower bound, risk z is observed at $\tilde{T}^{LB}$, whereas risk $R \neq z$ is observed at $\tilde{T}^{UB}$ under the definition of the upper bound.

Figure 1: Upper and lower bound of the observed risk specific duration



Regarding the empirical section, the particular data problem is that only a lower bound for exit state z is observed, and thus only regions (ii) and (iii) are valid. Formally speaking, the model studies the latent distributions of $(T,1), \ldots, (T, z-1)$ and $min\{T_{R\neq z}, T_z\}$, as (T, z) does not necessarily fall into the bounds indicated by the second and third region if it is not the minimum of (T, R) and thus could never be observed.

The hazard rate, survivor curve and the CIC are estimated nonparametrically using the above equations by replacing $\tilde{T}$ with $\tilde{T}^{LB}$ and $\tilde{T}^{UB}$ respectively. In analogy to Lee and Wilke (2005), we use the monotone relations of the survival function and the CIC to formulate the bounds analysis and to study the treatment effect of some policy reforms. Using the relations $\tilde{T}^{LB} \leq \tilde{T} \leq \tilde{T}^{UB}$, it is straightforward to see the following relations, which hold for $r = 1, \ldots, z-1$:

$$\begin{aligned} d_{rj}^{LB} &\leq d_{rj}^{UB}, \\ \sum_{l=1}^{j} d_{zl}^{LB} &\geq \sum_{l=1}^{j} d_{zl}^{UB}, \\ n_j^{LB} &\leq n_j^{UB}, \text{ and thus} \end{aligned}$$

$$\hat{S}^{LB}(t_j) \leq \hat{S}(t_j) \leq \hat{S}^{UB}(t_j) \tag{11}$$

$$\hat{I}_r^{LB}(t_j) \leq \hat{I}_r(t_j) \leq \hat{I}_r^{UB}(t_j) \tag{12}$$

Bounds for other functions such as the cause-specific hazard rate or the cause-specific cumulative hazard rate cannot be derived. Thus, the identification problem concerning the latent duration (T, z) implies that we observe only bounds for all risk-specific survivor curves and CIC. The implications of dependent competing risks for these bounds call for some remarks.

Remark 1 If the exit state z is independent to the remaining competing risks and we treat it as censored, the distributions of the fully observed failure times are then estimated in a way without making use of the additional information provided by this risk, and thus the bounds analysis is obsolete[1], i.e.

$$\hat{S}^{c,\,LB}(t_j) = \hat{S}^{c,\,UB}(t_j) = P[(T,1) > t_j \cap \ldots \cap (T, z-1) > t_j], \text{ and} \tag{13}$$

$$\hat{I}_r^{c,\,LB}(t_j) = \hat{I}_r^{c,\,UB}(t_j) = \sum_{l=1}^{j} P[(T, r) = t_l]. \tag{14}$$

(13) is the unbiased estimator for the survivor curve of the remaining competing risks and - since risk z is independent - of the overall survivor curve. (14) is the latent marginal density of exit r.

Remark 2 If risk z is dependent, the previously discussed properties of the survivor curve and the CIC under competing risks carry over to their respective bounds. Moreover, the estimated bounds depend on the chosen upper and lower bound for the unobserved latent durations. If we treat risk z as censored, the bounds of the survivor curve bound the survivor curve

[1] This follows from the definition of independent censoring. Kaplan-Meier type estimators are consistent estimators for the survivor curve and the CIC of the latent marginal distributions. Since this property has an asymptotic nature, there may be some deviations in an application. We may observe some slightly discrepancy of the lower and upper bound for which a monotone relation does not necessarily hold.

for the hypothetical independent distribution and not the actual overall survival function. The bounds for the CIC exist even if risk z is dependent and we treat it as a competing risk because $\acute{I}_r^{LB}(t_j) = \sum_{l=1}^{j} P[(\tilde{T}^{LB}, r) = t_l]$ and $\acute{I}_r^{UB}(t_j) = \sum_{l=1}^{j} P[(\tilde{T}^{UB}, r) = t_l]$ and the monotone relation $P[(\tilde{T}^{LB}, r) < t_j] > P[(\tilde{T}, r) > t_j] < P[(\tilde{T}^{UB}, r) > t_j]$ implies $\acute{I}_r^{LB}(t_j) < I_r(t_j) > \acute{I}_r^{UB}(t_j)$. If the dependent risk z is treated as censored, bounds of the hypothetical risk-specific CIC will not have a meaningful interpretation because the information provided by the dependent risk z is dropped artificially.

To conclude the above argument, bounds analyses for the CIC and the survivor curve can generally be used by treating missing data as dependent or independent competing risks instead of as censoring. By doing so, we estimate the unbiased overall survivor curve and the risk-specific CIC.

Now consider a setting where the duration of interest is the unemployment duration. Moreover, an individual faces several risks: it may enter local employment, non-local employment, or it may leave the labor force, become self-employed or enter subsidized employment. Unfortunately, the data offers only limited information on certain exit states $R = 1, \ldots, z$, i.e. $(\tilde{T}_i, R)_{R=1,\ldots,z}$ can be observed while all other competing risks cannot be distinguished. Thus, for the indistinguishable other exit states denoted with $R = o$, we observe $(\tilde{T}_i, o) = min\{(T_i, o_1), (T_i, o_2), \ldots, (T_i, o_v)\}$, with (T, o_v) representing the latent failure times for the non-distinguishable exit states o_v.[2] Lower and upper bounds are denoted with $(\tilde{T}_i^{LB}, o)$ and $(\tilde{T}_i^{UB}, o)$. Duration is independently censored with time C_i. The duration to exit state o is dependent on that of exit state $R = 1, \ldots, z$. Treating the exit state o as a competing risk, bounds for a treatment effect on the overall survivor curve and on cause-specific cumulative incidence curves can be used without imposing the independence assumption.

Now, suppose there is a policy intervention in a natural experiment setting. We have two groups, the control group ($G = 0$) and the treatment group ($G = 1$), and two time intervals, the pre-reform period ($P = p_{t0}$) and the post-reform period ($P = p_{t1}$). The reform of interest is supposed to have an effect on the unemployment duration of the treatment group in the post-reform years and the effect of the reform can be estimated by a Difference-in-Differences estimator (DID) as

$$\Delta_{Ir}(t_j|p_{t0}, p_{t1}, x) = [I_r(t_j|1, p_{t1}, x) - I_r(t_j|0, p_{t1}, x)] - [I_r(t_j|1, p_{t0}, x) - I_r(t_j|0, p_{t0}, x)] \tag{15}$$

for $r = 1, \ldots, z$, where $I_r(t_j|g, p, x) = P(T \leq t_j, R = r, G = g, P = p, X = x)$ with X as further

[2]In an application, the pooling of all unidentified exit states aggravates its interpretability as changes in the failure time for certain exit states subsumed under the exit state o may actually oppose each other.

observable variables such as gender, age etc. and

$$\Delta_S(t_j|p_{t0},p_{t1}) = [S(t_j|1,p_{t1},x) - S(t_j|0,p_{t1},x)] - [S(t_j|1,p_{t0},x) - S(t_j|0,p_{t0},x)], \tag{16}$$

where $S(t_j|g,p,x) = P(T > t_j, G = g, P = p, X = x)$. Given that we observe bounds for the estimated survivor function as in (11) and bounds for the estimated cause-specific cumulative incidence curve as in (12) it is straightforward to derive bounds for $\hat{\Delta}_{Ir}$ and $\hat{\Delta}_S$ since they are bounded by intervals with endpoints $[l_{Ir}(t_j|p_{t0},p_{t1},x), u_{Ir}(t_j|p_{t0},p_{t1},x)]$:

$$\begin{aligned} l_{Ir}(t_j|p_{t0},p_{t1},x) &= \max[-1, \{I_r^{LB}(t_j|1,p_{t1},x) - I_r^{UB}(t_j|0,p_{t1},x)\} \\ &\quad - \{I_r^{UB}(t_j|1,p_{t0},x) - I_r^{LB}(t_j|0,p_{t0},x)\}] \qquad (17) \\ \text{and} & \\ u_{Ir}(t_j|p_{t0},p_{t1},x) &= \min[1, \{I_r^{UB}(t_j|1,p_{t1},x) - I_r^{LB}(t_j|0,p_{t1},x)\} \\ &\quad - \{I_r^{LB}(t_j|1,p_{t0},x) - I_r^{UB}(t_j|0,p_{t0},x)\}] \qquad (18) \end{aligned}$$

for $r = 1, \ldots, z$ and $[l_S(t_j|p_{t0},p_{t1},x), u_S(t_j|p_{t0},p_{t1},x)]$:

$$\begin{aligned} l_S(t_j|p_{t0},p_{t1},x) &= \max[-1, \{S^{LB}(t_j|1,p_{t1},x) - S^{UB}(t_j|0,p_{t1},x)\} \\ &\quad - \{S^{UB}(t_j|1,p_{t0},x) - S^{LB}(t_j|0,p_{t0},x)\}] \qquad (19) \\ \text{and} & \\ u_S(t_j|p_{t0},p_{t1},x) &= \min[1, \{S^{UB}(t_j|1,p_{t1},x) - S^{LB}(t_j|0,p_{t1},x)\} \\ &\quad - \{S^{LB}(t_j|1,p_{t0},x) - S^{UB}(t_j|0,p_{t0},x)\}]. \qquad (20) \end{aligned}$$

Note that the lower and upper bounds are restricted to be between -1 and 1. This is due to the fact that maximum variation of probabilities cannot be larger than 1 in absolute values. In the next section, we use simulations to demonstrate our major findings in this section.

3 Simulations

The following simulation framework is closely related to the empirical application which is concerned with transitions from unemployment. For the simulation, we thus assume that there are two observed exit states, employment ($r = e$) and other destinations ($r = o$) such as leaving the labor force and self-employment and no censoring.[3] $\tilde{T}_o$ is not directly observed. Let $\tilde{T}^{LB}$ be the

[3] In the application, we will further distinguish between employment in the local area and employment via migration.

observed lower bound of $\tilde{T}_o$. To simplify the issue, we use $\tilde{T}_e$ as a natural upper bound of $\tilde{T}_o$.[4] We assume that the latent failure times, T_e and T^{LB}, have the following multivariate lognormal distribution

$$log(T_r) \sim N(\mu_r + G \times c_{1r} + D_P \times c_{2r} + G \times D_P \times c_{3r}, \Omega),$$
$$\Omega = \begin{pmatrix} \sigma_e^2 & \rho_{eLB}\sigma_e\sigma_{LB} \\ \rho_{eLB}\sigma_e\sigma_{LB} & \sigma_{LB}^2 \end{pmatrix},$$

with $r = e, LB$. G is a group dummy equal to zero for the control group and equal to one for the treatment group. D_P is a period dummy equal to zero in the pre-reform period and equal to one in post-reform period. μ thus refers to the mean failure time for the control group in the pre-reform period. Values of the parameters $\{\mu_r, c_{ir}, \sigma_r\}$ for r=e, LB and i=$1, 2, 3$ are chosen to approximate the conditions in the empirical application and are summarized in Table 1.

Table 1: Chosen parameters for the simulation framework

r	μ	$c1$	$c2$	$c3$	σ
e	1.0	0.4	0.6	-0.4	2.5
LB	0.8	0.0	0.0	-0.5	1.5

This framework assumes that the true failure time for exit o, T_o, follows a uniform distribution in the interval $[T_{LB}, T_{LB}+5]$. Moreover, the treatment group is assumed to have longer latent failure times to all exit states in both the pre- and post-reform period than the control group ($c_1 > 0$). Due to factors unrelated to the reform, both the treatment and control group have longer failure times to all exit states in the post-reform period than in the pre-reform period ($c_2 > 0$). The interaction term $G \times D_P$ implies the assumption that the reform decreases the failure time to exit state e on the treatment group only ($c_3 < 0$). We first assume zero correlation between the failure time to exit state e and its lower limit, i.e. ρ_{eLB}=0, in order to study the case of independent competing risks before we relax this assumption later.

Using formulas (15)-(16), we make use of the fully identified competing risks, T_o, to compute the estimated change in the observed distribution for exit state e. We then compute the bounds for these changes for the situation that we observe only T^{LB}. We apply the formulas (4)-(20) and treat T^{LB} either as censored or as a competing risk. We run 500 simulations, each with $10,000$ failure times for each exit state to derive consistent estimates for the changes of the observed distribution functions and the corresponding bounds. The mean value and the bounds for the

[4] If $\tilde{T}_o$ was larger than $\tilde{T}_e$, it could not be observed and therefore did not exist.

effects are reported in Figure 2. In addition, the 5% (95%)-quintile of the estimated lower bounds (upper bounds) are also included.

As the parameters c_{3e} and c_{3o} are negative, failure times to all exit states are shortened after the reform. The expected positive treatment effect on the CIC for exit e and the expected negative effect on the overall survivor curves with fully identified data are shown as the black thick line in Figure 2(i)(a), (ii)(a) and Figures 2(i)(b), (ii)(b), respectively.[5]

In Figure 2(i)(a) & (b), results using the competing risks framework show that the bounds contain the estimated treatment effect on the CIC for exit e and the overall survivor curve. Using the censoring framework, as discussed in remark 1 in the last section, the upper and lower bounds coincide with each other in Figure 2(ii)(a) & (b) and are an unbiased estimate for the treatment effect on the latent marginal survivor curve of exit e. As T_{LB} and T_o are independent of T_e, the overall survivor curve and the CIC in the presence of T_o correspond to the estimated marginal survivor curve.

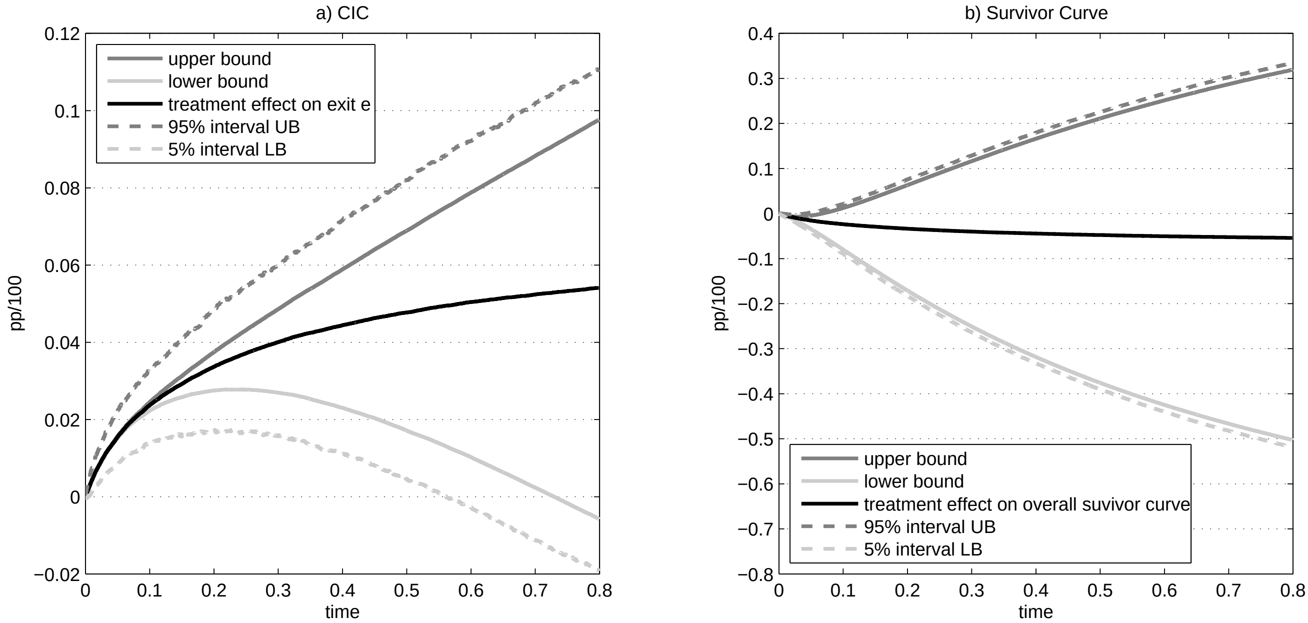
Next we allow a correlation of T_e and T^{LB} with ρ_{eLB}=0.05 and thus introduce a setting in which the failures times for the exit state e and the observed lower bound are dependent. In a competing risk framework, Figure 3(i) is very similar to Figure 2(i). The bounds contain the estimated treatment effect on both the overall survivor and the cause-specific cumulative incidence curve. In the presence of dependent competing risks, the bounds for the marginal survivor curve of exit e revert in direction when treating T^{LB} as censored as shown in Figure 3(ii)(b) and are thus not applicable. In fact, as discussed in remark 2 of the last section, the bounds are now bounding neither the treatment effect on the overall survivor curve nor the marginal survivor curve for the employment. When treating T^{LB} as censored, Figure 3(ii)(a) shows that a slight increase in the correlation ρ_{eLB} from zero to 0.05 suffices to dramatically widen the bounds from a single line to a wide bound. This bound differs from that without censoring in 3(i)(a) and, as discussed in remark 2 of the last section, does not have a meaningful interpretation.

We conclude that estimating the bounds on the CIC and treating T^{LB} as a competing risk is a preferable approach. In the case of independent competing risks, both bounds on the CIC and the survivor curve produce a consistent estimate of the latent marginal treatment effect. In the case of dependent competing risks, however, the bounds for the survivor curve are now bounding neither the treatment effect on the overall survivor curve nor the marginal survivor curve for the exit state employment. By contrast, the bounds for the CIC are still interpretable as an estimate of the effect on the observed distribution of failures, but - due to the non-identifiability of competing risks - do not any longer capture the latent marginal treatment effect. Since the degree of dependence

[5]In both the competing risk and the censoring setting, the estimated "true" reform effects are computed in the same way.

Figure 2: Simulated treatment effects and their bounds in case of uncorrelated failures types

(i) Treating T^{LB} as a competing risk



(ii) Treating T^{LB} as censored

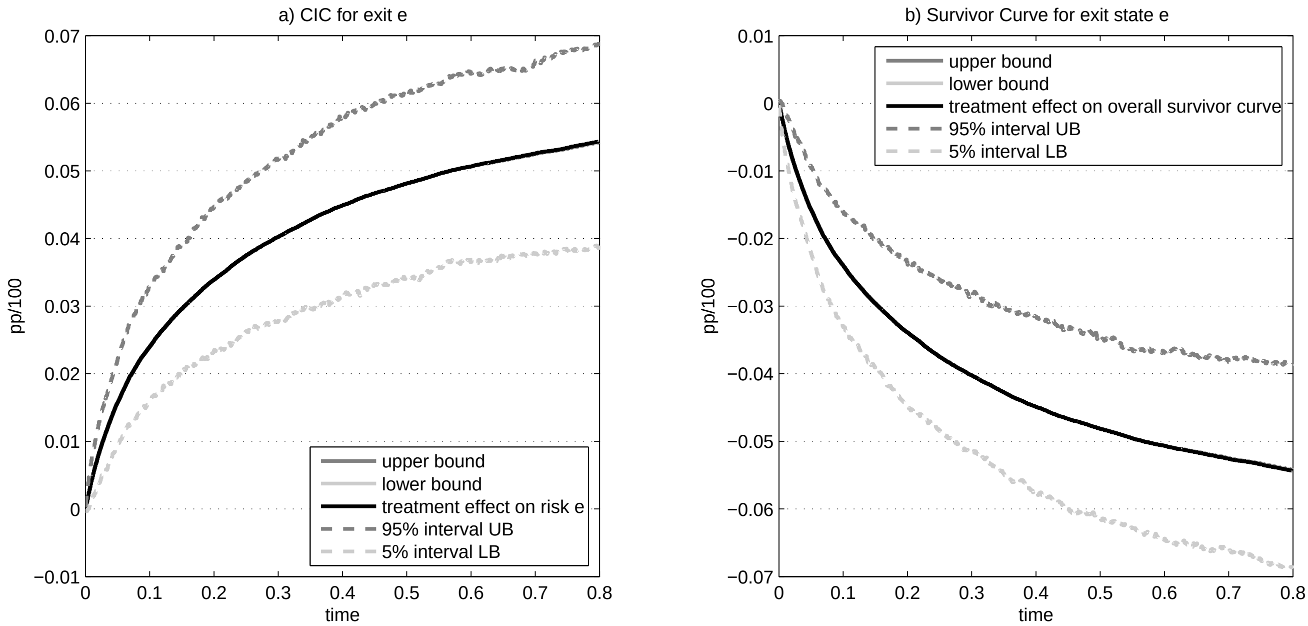
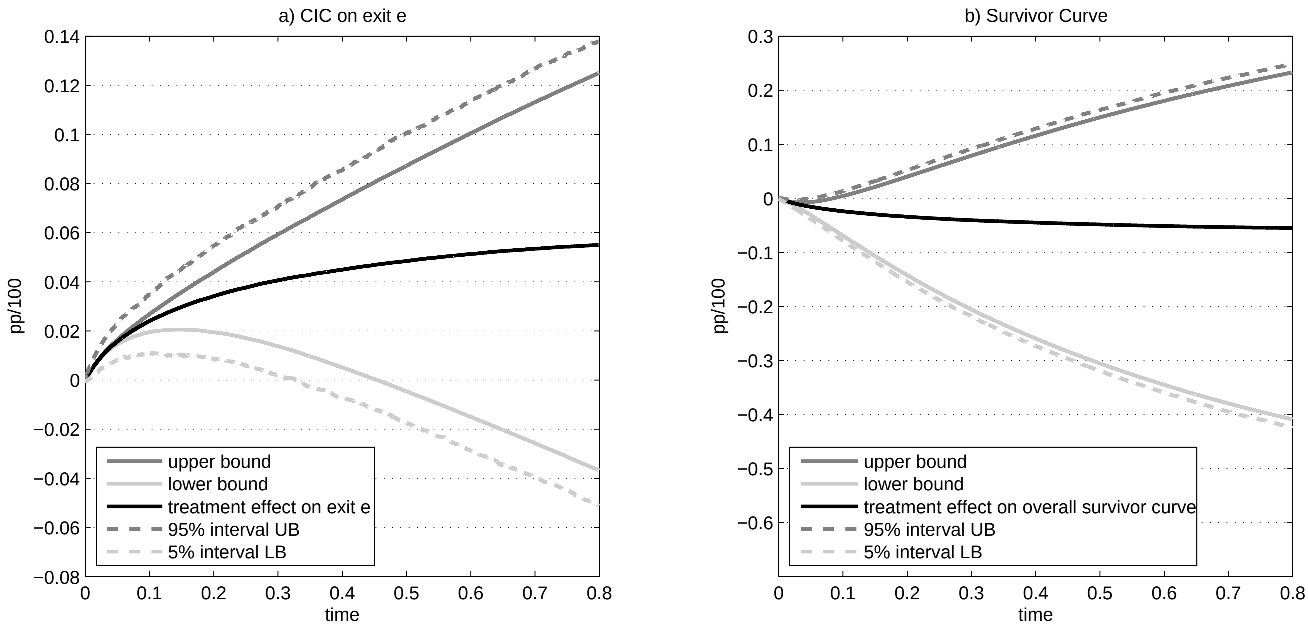
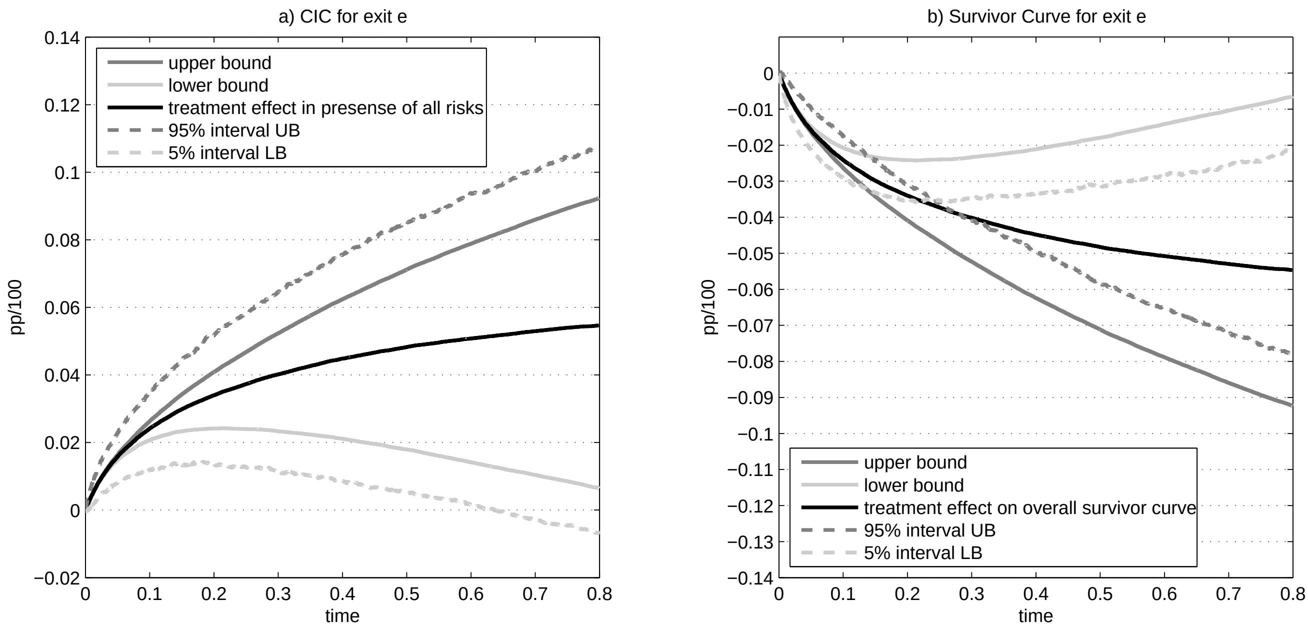


Figure 3: Treatment effects: mean estimations, ρ_{el}= 0.05

(i) Treating T^{LB} as a competing risk



(ii) Treating T^{LB} as censored



between competing risks is unknown, a bounds analysis of CICs is a preferable approach as it yields an interpretable result under all conceivable circumstances.

4 Empirical Application

As briefly discussed in the introduction, we apply our bounds analysis to bound the effect of reducing the maximum duration of receiving unemployment benefits on the observed transitions from unemployment to local and non-local employment via migration. We begin this section with a brief description of the German unemployment compensation system and discuss the 1997 reform of unemployment benefit entitlements. This discussion is based on the Employment Promotion Act (*Arbeitsförderungsgesetz*), the Social Welfare Act III (*Sozialgesetzbuch III*) and several secondary sources such as Plaßmann (2002), Oschmiansky et al. (2001) and Wolff (2003). We then introduce the data and discuss the selection of treatment and control group, before we present the result of bounding the effect as described in the previous methodological section.

Basic features of the unemployment compensation system During the study period, the system of unemployment compensation in Germany consists of two main components: unemployment benefits (UB) and unemployment assistance (UA). As an insurance, unemployment benefits are limited in time depending on the length of socially insured employment during a period of seven years before the benefit claim. Moreover, the length of benefit receipt positively depends on age with a maximum UB receipt of 12 months for younger age groups and up to 32 months for older age groups in the years prior to the 1997 reform. After exhausting UB, unemployed individuals receive the tax-funded unemployment assistance if they pass a means-test. Both UB and UA are a percentage of former wage income with UB replacing 63% (68%) of former wage income and UA still reaching income replacement rates of 53% (57%) for individuals without (with) dependent children. For individuals with low pre-unemployment wages, income replacement rates may even be higher. If the unemployment compensation as a percentage of former wage income does not suffice to ensure the legally defined minimum standard of living, individuals receive complementary social benefits. As a result, income replacement rates for individuals with low pre-unemployment wages may be close to 100% and disincentives to take up a new job should be particularly severe for this group of unemployed. Consistent with such disincentive effects, there is empirical evidence that these groups experience longer unemployment durations and are less likely to leave for non-local jobs than unemployed individuals with higher pre-unemployment wages (Arntz and Wilke, 2006). The design of the unemployment compensation and welfare system in Germany thus implies that any changes concerning the length of entitlements to unemployment benefits are ineffective for unemployed individuals with complementary social benefits. Receiving unemployment assistance instead of unemployment benefits does not change the income replacement rate for these individuals and should thus not affect their job search strategy. By contrast, unemployed individuals without additional social benefits but with eligibility for the means-tested UA loose around 10% of their former wage income when switching from UB to UA. For this group, a shortening of UB is likely to have a small effect only. Individuals who do not pass the means test for receiving UA due to other income sources or private savings even loose all unemployment compensation after exhausting UB. The threat of entitlement loss should thus be strongest for this rather small group of unemployed.

1997 Reform In April 1997, a major reform of the Employment Promotion Act came into force to shorten the receipt of UB for some of the older age groups and to introduce stricter sanction rules for the non-compliance with certain eligibility requirements. The enforcement of stricter sanction rules in Germany after 1997 may have accelerated the transition from unemployment to employment because temporary reductions in UB due to non-compliance with eligibility rules have been found an effective means of reducing unemployment (Boone et al., 2002, 2004). Since these new regulations applied to all unemployed at the same time, however, our DID framework should eliminate this effect and still allow for the identification of the causal effect of shortening the UB receipt for some older age groups in 1997. In Germany, the potential UB duration (PUBD), i.e. the maximum duration of UB receipt at the beginning of the unemployment period, positively depends on the period of socially insured employment within the seven years prior to the benefit claim. This so called extended claim period is restricted by previous benefit claims and is thus shorter than seven years for individuals with a benefit claim within the previous seven years. In addition, the PUBD positively depends on age. During the 1980s, the PUBD had successively been expanded for older age groups. Thus, before the reform in 1997, entitlements to UB lasted up to 32 months for individuals above the age of 42, while the PUBD for individuals below this age range was only 12 months. A detailed description of these earlier reforms can be found in Hunt (1995). One well-documented result of these earlier reforms that demonstrates the disincentive effect of this system was the rapid increase of early retirees whose extremely long UB receipt allowed for bridging the gap between employment and retirement age. See Fitzenberger and Wilke (2004) for a nonparametric analysis using similar administrative data. In 1997, the PUBD was reduced for some of the older age groups by lowering the age limits for certain maximum entitlement length (see Table 2). As a consequence, the PUBD for individuals between 42 and 43 years of age was cut from 18 month before 1997 to 12 month after the 1997 reform. For individuals aged 44, UB was even cut from a maximum receipt of 22 to a maximum receipt of 12 months. Individuals aged below 42 years were unaffected by the reform as they always received a maximum of 12 month of UB. The 1997 reform thus provides a natural experiment with a credible source of variation in PUBD that can be used to identify its causal effect.

Table 2: Potential unemployment benefit duration (PUBD) for UB claimants up to age 47 by work history and age, IAB-R01

Soc. insured employment	**PUBD (in month)**	
during claim period	until 03/97	since 04/97
12 month	6	6
16 month	8	8
20 month	10	10
24 month	12	12
28 month	14 (age >42)	14 (age >45)
32 month	16 (age >42)	16 (age >45)
36 month	18 (age >42)	18 (age >45)
40 month	20 (age >44)	20 (age >47)
44 month	22 (age >44)	22 (age >47)

Source: Plaßmann (2002)

One problem of the 1997 reform that has to be taken into account, however, is that the implementation of the reform was partially cushioned. Until March 1999, new benefit claimants were treated according to the pre-reform regulations if there was a work history of more than one year during the three years prior to the benefit claim. Thus, the new regulations applied to all new benefit claims after March 1999 only. Two German studies already looked at the effect of the 1997 reform on transitions from unemployment to employment. Based on the socio-economic panel, Wolff (2003) only finds very weak positive effects of shortening the PUBD on the transitions to employment in eastern Germany. As previously discussed, this finding may reflect that the entitlement loss due to the 1997 reform was rather limited for most groups. Moreover, due to the limited sample size of the GSOEP data, the study pools unemployment spells starting between 1990 and 1999 and thus includes only a limited number of spells that were actually affected by the reform. In the subsequent analysis, we use an administrative data set that provides a much larger sample size and thus also allows for distinguishing between exits to local versus exits to non-local employment after migration. Based on the same data set, Müller et al. (2007) look at the effect of the 1997 reform on transitions to employment among older unemployed above the age of 52 for whom the 1997 reform also shortened the PUBD. They find evidence that the reform reduced the inflow into unemployment and drastically reduced the duration of unemployment among this group, a result that suggests that shorter UB durations lower the attractiveness of early retirement via the receipt of UB. Using the same administrative data set, we reexamine the effect of the PUBD on transitions to local and non-local employment. Moreover, we restrict the analysis to prime age individuals for whom early retirement should not be an issue. Moreover, prime age individuals are much more likely to migrate as a response to the 1997 reform than their older counterparts.

Data: IAB-R01 The analysis is based on the IAB employment subsample 1975-2001 - regional file (IAB-R01[6]). This register data set contains spell information on a 2 % sample of the population working in jobs that are subject to social insurance payments and thus excludes self-employed individuals and tenured civil servants. The data contains spell information on periods for which the individual received unemployment compensation (UC) from the federal employment office (*Bundesagentur für Arbeit*) such as unemployment benefits UB (*Arbeitslosengeld*), unemployment assistance UA (*Arbeitslosenhilfe*) and maintenance payments during training measures MP (*Unterhaltsgeld*). Thus, employment histories including periods of transfer receipt can be reconstructed on a daily basis. One major drawback of the data set is that the true unemployment duration is not known because the data only contains information on the receipt of UC. As a consequence, there is a gap in the IABS-R01 record whenever an individual continues to be unemployed after exhausting unemployment benefits without receiving unemployment assistance. Since such a gap in the IABS-R01 record is indistinguishable from other unobserved labor market states, such as being out of the labor force or self-employed, there is uncertainty about the true duration until leaving unemployment to one of these other destination states (o). As a consequence of this partially missing information problem, it is necessary to define unemployment spells according to a suitable bound (Fitzenberger and Wilke, 2004, Lee and Wilke, 2005). For the following analysis, we use two proxies, an upper and a lower bound that can be used for the bounds analysis as discussed in the methodological section. In our case $\tilde{T}^{LB}$ and $\tilde{T}^{UB}$ are defined as follows:

- **Unemployment with permanent income transfers**: The lower bound ($\tilde{T}^{LB}$) closely follows the receipt of UC. It requires an individual to receive UC within 1 month after the end of employment and continue to receive UC with intermediate gaps of less than 4 weeks. If such an intermediate gap or the gap between the end of UC receipt and employment is longer than 1 month, we consider this as an exit to an unknown destination (o) since these exits encompass exits to out of labor force as well as exits to self-employment.

[6]See Hamann et al. (2004) for a detailed description of the IAB-R01.

- **Nonemployment**: The upper bound ($\tilde{T}^{UB}$) closely follows a non-employment definition. It requires at least one receipt of UC after an employment spell, but does not impose further restrictions. The resulting spells of unemployment are considered as exits to an unknown destination (o) only if an individual does not exit to employment until the end of the observation period.

By construction of the two unemployment definitions, we observe more UB spells[7] than LB spells because only the lower bound $\tilde{T}^{LB}$ conditions on the receipt of UC within four weeks after the end of employment. In order to aviod a potential sample selection issue if the excluded spells are not random with respect to the treatment effect, we extend the LB sample to match the size of the UB sample as in Lee and Wilke (2005). We do so by adding the missing UB spells to the LB spells. These spells have an observed unemployment duration of zero days and are considered as an exit to an unknown destination state o. This way of treating the added spells is in line with the lower bound definition.

For both unemployment proxies, right-censoring occurs in the case of continued UC receipt at the end of the observation period. For all unemployment spells that exit to employment, the IAB-R01 allows for identifying the location of the new workplace disaggregated to the level of microcensus regions. Thus, by comparing the previous and the new workplace location, it is possible to distinguish local from non-local exits to employment. In the following analysis, a movement between non-adjacent labor market regions (*Arbeitsmarktregionen*) is considered as migration. The 227 labor market regions (LMRs) in Germany comprise typical daily commuting ranges such that for the majority of individuals both residence and workplace are located within the LMR. Since individuals living at the fringe of an LMR may nevertheless easily commute to the adjacent LMR, what is considered a local job change has been extended to include all adjacent LMRs. Finding employment in a non-adjacent LMR should thus necessitate residential mobility in most cases. For each spell of unemployment, the analysis thus distinguishes exits to a local and a non-local job after migration from exits to other destination states.

For our analysis, we include inflow samples for a pre- and a post-reform era. Due to the implementation of stricter sanction rules in 1994, extending the pre-reform era beyond 1995, might mix different reforms. We therefore consider an unemployment spell starting between 1995 and 1996 as a a pre-reform spell. The post-reform era is predetermined by the fact that the implementation of new UB regulations did not start before 1999. The post-reform inflow sample thus consists of all unemployment spells starting in 1999 or 2000. Since the observation period of the IAB-R01 ends on 12/31/2001, the duration of a post-reform spell is between one and three

[7] We refer to UB (LB) spells or sample for the sample of unemployment spells that result when applying $\tilde{T}^{UB}$ ($\tilde{T}^{LB}$).

years only.

Choosing the treatment and control group The aim of the analysis is to identify the effect of being eligible for an extended UB duration on the transitions from unemployment to either a local job or a non-local job by using the natural experiment that is provided by the reform of the Employment Promotion Act in 1997. In particular, eligibility to an extended UB duration of more than 12 month was cut for individuals aged 42-44 years, while the PUBD of individuals below this age was unaffected by the reform. Thus individuals aged 36-41 years serve as the group to control for changing labor market conditions as well as changing sanction rules (see above) when comparing transitions to local and non-local employment before and after the reform. Since only individuals with long UB entitlements are affected by the reform, the exact choice of treatment and control group has to be conditioned not only on age, but also on the entitlement length at the beginning of the unemployment period. Moreover, the chosen selection rule should be the same for both treatment and control group to ensure that the groups are comparable with regard to their working history. Choosing, for example, all individuals with maximum UB entitlements in their respective age group results in a non-comparability of individuals in the control and treatment group as the criterium to reach this maximum entitlement is less strict for the younger cohort (see Table 2). For this reason, we compute counterfactual UB entitlements in addition to the actual UB entitlements at the beginning of each unemployment spell. Both information have to be computed based on the known employment history, age and the known regulations and changes across time (see Appendix A for details). Since the working history for individuals from eastern Germany is not known before 1991 which aggravates the comparability of computed entitlement length, we restrict the analysis to individuals from western Germany. For the counterfactual entitlements, we calculate the entitlement length in the absence of the 1997 reform had the individual been aged 42-44 at the time of benefit claim. As can be seen in Table 3, the resulting counterfactual UB entitlements are quite comparable for both age groups (Pearson chi2(9) = 14.9).

For the subsequent analysis, we choose all unemployment spells that begin with a receipt of unemployment benefits and whose counterfactual UB duration exceeds 12 month. Moreover, we condition on previous full-time employment to keep the sample more homogeneous in terms of labor force attachment. We also exclude unemployment spells of women because missing information on marital status and dependent children in the IAB-R01 aggravates the interpretation of corresponding results. For the chosen control group and the post-reform treatment group the estimated actual entitlement length as shown in Table 4 that is subject to the 1997 reform and the true age of the individual is up to 12 month[8] only. In the pre-reform era, the treatment group

[8]For some individuals who do not fulfill the criterium for the maximum entitlement length, but still pass the

is entitled to 18.5 month of UB receipt on average, while in the post-reform era this average UB duration falls to 11.8 month. This latter UB receipt is almost exactly the UB duration for the control group in the pre- and post-reform era. The average treatment thus is a reduction of UB entitlements of 6.7 month with the treatment ranging from a reduction of one to a reduction of ten month for individuals aged 44 with maximum UB entitlements.

Table 3: Estimated counterfactual UB entitlement length for unemployment spells in the pre- and post-reform era by age group[a], IAB-R01

	Age 36-41		Age 42-44	
UB duration	# spells	%	# spells	%
$\leq$ 2 months	1,226	6.8	551	7.6
3-4 months	1,212	6.7	473	6.6
5-6 months	1,061	5.9	423	5.9
7-8 months	1,092	6.1	440	6.1
9-10 months	1,194	6.6	463	6.4
11-12 months	1,017	5.6	444	6.2
13-14 months	1,182	6.6	486	6.7
15-16 months	1,026	5.7	435	6.0
17-18 months	9,008	50.0	3,505	48.6
Total	18,018	100.0	7,220	100.0

[a] Includes all previously full-time employed individuals born in West Germany whose unemployment spell starts with the receipt of unemployment benefits.

The chosen selection rule for the treatment and control group should ensure some comparability with respect to the working history that builds up claims to UB. As the working history strongly shapes labor market outcomes, this is quite important in order to minimize selection biases. For a DID approach to be valid, both groups should be comparable in both observed and unobserved characteristics which are likely to affect labor market outcomes. For the available information and some major indicators that can be calculated based on the employment history, Appendix B shows that treatment and control group are quite comparable in most characteristics. Unfortunately, characteristics such as the marital status and dependent children which are likely to affect the likelihood of migration are missing. The subsequent analysis thus rests on the assumption that the composition of treatment and control group in the pre- and post-reform era are as comparable with respect to these unobserved characteristics as they are with respect to the observed characteristics. Another assumption of the DID approach is that both treatment and control group experience

selection criterium, the true UB duration may be lower than 12 month.

similar changes in labor market conditions in the post- compared with the pre-reform era. This assumption could fail if older workers face more problems to exit unemployment in times of economic downturn than their younger counterparts because of stricter employment protection for older workers. This might be relevant as the post-reform era is characterized by slightly improving labor market conditions, while the pre-reform era rather falls into a period of economic downturn (Bundesanstalt für Arbeit, 2001). On the other hand, stricter employment protection for individuals above 40 only applies occasionally and generally requires a job tenure of more than 10 years. For jobseekers between 36 and 44, employment protection should thus be quite comparable and the better labor market conditions in the post-reform era should boost the transition to employment for both groups to a comparable extent.

Table 4: Estimated actual UB entitlement length for unemployment spells with counterfactual UB of >12 months in the pre- and post-reform era by treatment and control group, IAB-R01

	Control group		Treatment group	
UB duration	pre-1997	post-1997	pre-1997	post-1997
6-8 months	2.1%	1.3%	0.00%	1.5%
9-11 months	6.9%	4.5%	0.00%	5.6%
12 months	91.0%	94.2%	0.00%	93.0%
13-14 months	0.0%	0.0%	8.1%	0.0%
15-16 months	0.0%	0.0%	7.2%	0.0%
17-18 months	0.0%	0.0%	56.4%	0.0%
19-20 months	0.0%	0.0%	3.1%	0.0%
21-22 months	0.0%	0.0%	25.2%	0.0%
Average months	11.8	11.9	18.5	11.8
Total spells	4,294	3,577	1,557	1,436

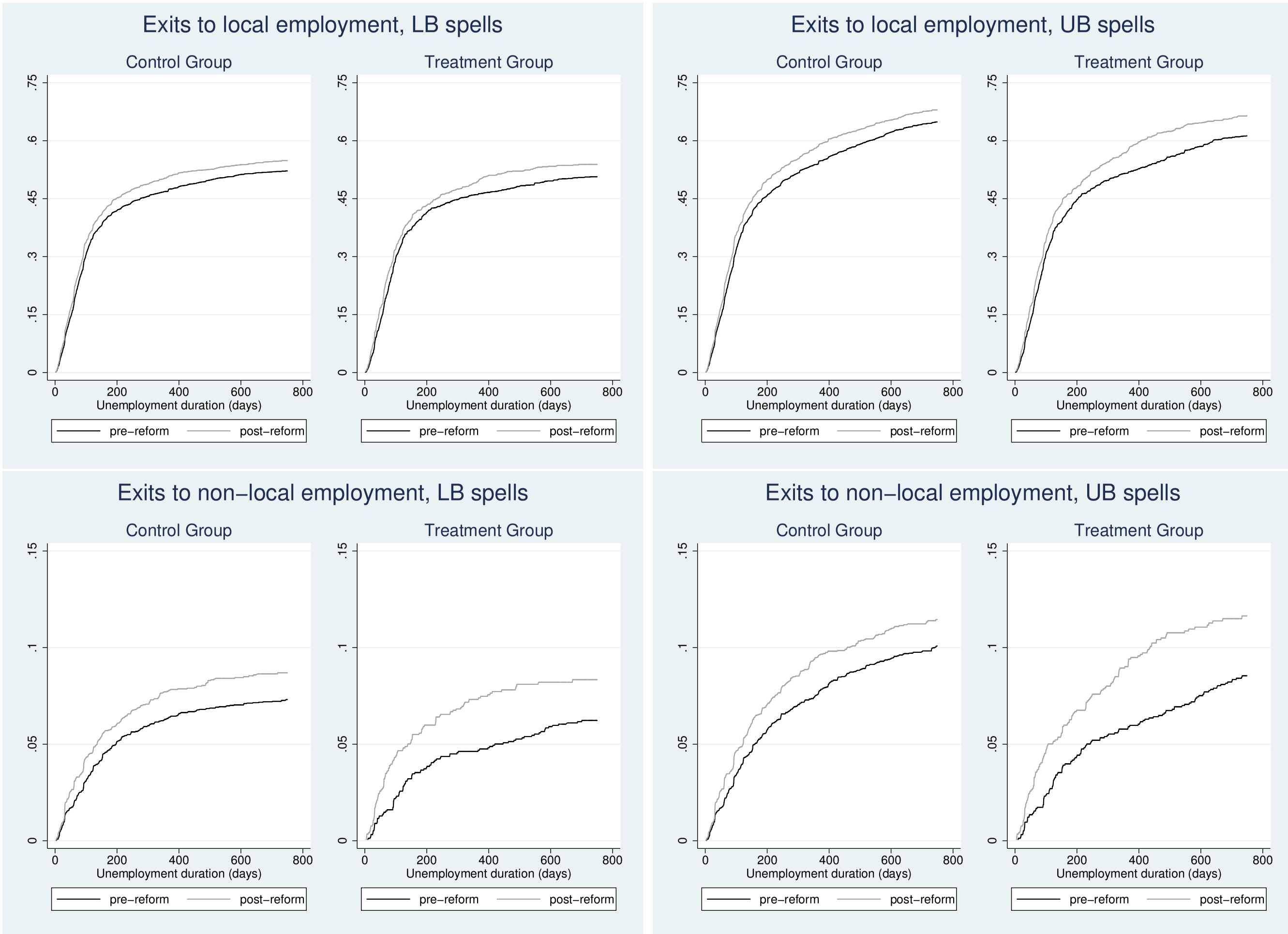
Table 5 shows exit types and median unemployment duration for both unemployment definitions. Due to the end of the observation period, the degree of censoring is more pronounced in the post-reform year. Moreover, exits to other destination states are much more likely for the LB spells. The simple descriptive statistics for both unemployment definitions suggest that the treatment group has a somewhat longer unemployment duration, but that the gap between treatment and control group almost disappears after the reform. Among the non-censored spells, both unemployment definitions suggest that the treatment group in the post-reform period almost catches up with the higher exit probability of the control group, especially for exits to local employment.

Table 5: Descriptive summary of full sample, IAB-R01

	Control group		Treatment group	
	pre-1997	post-1997	pre-1997	post-1997
LB spells				
median duration (days)	79	73	88	73
exit to local job	54.5% (54.9%)	53.8% (57.0%)	53.4% (53.9%)	52.9% (56.2%)
exit to non-local job	7.7% (7.8%)	8.4% (8.9%)	6.7% (6.8%)	8.2% (8.7%)
exit to other destination	37.1% (37.3%)	32.1% (34.1%)	39.0% (39.3%)	32.9% (35.0%)
total exits	99.3% (100.0%)	94.3% (100.0%)	99.1% (100.0%)	94.0% (100.0%)
UB spells				
median duration (days)	161	124	185	130.5
exit to local job	75.1% (77.4%)	65.9% (74.6%)	72.1% (75.1%)	64.8% (74.1%)
exit to non-local job	12.8% (13.1%)	11.0% (12.5%)	12.5% (13.0%)	11.2% (12.8%)
exit to other destination	9.2% (9.5%)	11.4% (12.9%)	11.4% (11.9%)	11.5% (13.1%)
total exits	97.1% (100.0%)	88.3% (100.0%)	96.0% (100.0%)	87.5% (100.0%)
Total spells	4,294	3,577	1,557	1,436

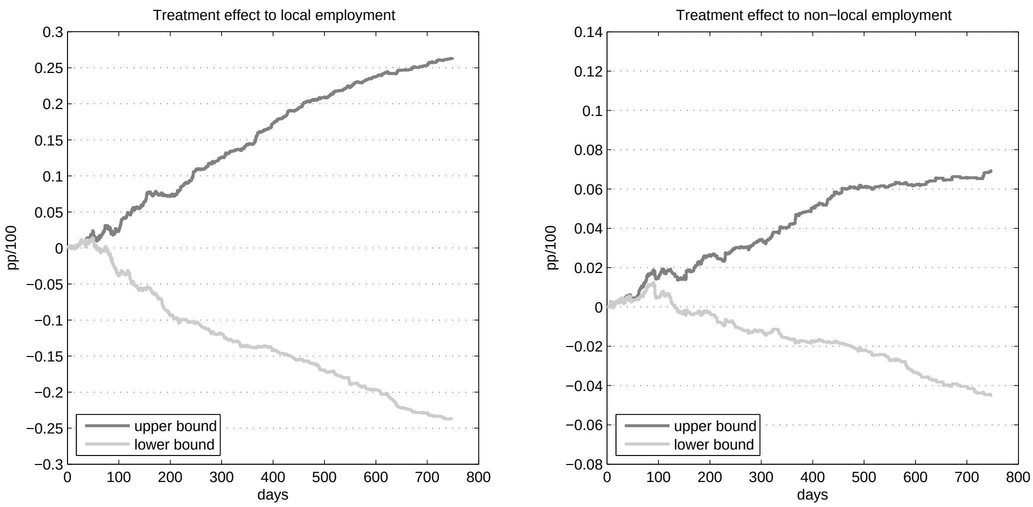
Cumulative incidence in the pre- and post-reform era Figure 4 shows the cumulative incidence curves for exits to local and non-local employment by treatment and control group in the pre- and post reform years. First of all, note that exit probabilities increase for all groups and exit types in the post-reform years. As has been discussed previously, this may reflect a combination of better labor market conditions compared to the pre-reform years as well as the stricter sanction rules that applied to both the control and the treatment group. More importantly, the figures suggest that the increase in both the local and the non-local exit probability via migration is more pronounced among the treatment group, especially as we reach the end of the shortened UB duration of 12 month. For exits to local employment, we even observe a small kink for the treatment group after approximately one year of unemployment in the post-reform years. In the pre-reform years where UB durations have been above 12 month for the treatment group, no such kink can be detected. This timing of events is indicative for a causal relationship of these observed changes to the shortening of UB receipt in the post-reform years. For exits to non-local employment no clear kinks can be detected, but the increase in the probability of non-local exits seems to be strongest as we reach one year of unemployment. In particular, the cumulative incidence curves for non-local exits in the pre-reform era appear flatter after one year of unemployment for the treatment than for the control group. In the post-reform era, the cumulative incidence curve of migration is as steep or even steeper for the treatment compared to the control group. Thus, this may also indicate some positive effect of the shortening of UB receipt for the treatment group on the observed probability of migration.

Figure 4: Cumulative incidence curves in the pre- and post reform era by treatment and control group for both unemployment definitions, IAB-R01



Bounds Analysis I As discussed previously, neither of the two unemployment definitions reflects the true unemployment duration. As a consequence, the effects implied by Figure 4 do not capture the true effect of the reform on the cumulative incidence of local and non-local employment. We therefore apply formulas ()-() to bound the true effect of the reform by the cumulative incidence curves for these two unemployment definitions. As a first interesting observation, we find that the resulting bounds do not coincide with the point estimates for the lower and upper bound of the latent variable. As shown in Appendix E, point estimates for the different definitions of the unemployment duration data do not span the full width of our estimated bounds. This suggests that a sensitivity analysis based on different failure time definitions alone may be misleading. As regards the reform effects on local employment and migration, Figure 5 suggests that the data insecurity involved due to the unobserved duration until exiting unemployment to one of the unknown exit states precludes any clear inferences. At the very beginning of unemployment, the upper bound for both exit types is above zero which suggests the same direction of effects as in Figure 4. However, after 50-100 days of unemployment bounds are too wide to deduct any treatment effect on exits to local or non-local employment. One reason for the bounds to be so wide is that the natural lower and the natural upper bound for the definition of unemployment that we apply result in many lower bound spells of length zero. These spells are generally uninformative and result in large differences between the distribution of $\tilde{T}^{LB}$ and $\tilde{T}^{UB}$.

Figure 5: Lower and upper bound of treatment effect on the cumulative incidence of local and non-local exits to employment, IAB-R01



Bounds Analysis II As an approach to tighten the bounds, we impose an additional assumption. Instead of expanding the LB spells to match the size of the UB sample, we restrict the analysis to the spells that are included in both definitions. Given the definitions for $\tilde{T}^{LB}$ and $\tilde{T}^{UB}$ this means that we restrict the analysis to unemployed individuals who receive unemployment compensation within one month after the end of employment. This approach is only valid if the exclusion of spells with a later start of UC receipt is a random sample in the sense that the joint distribution of the true unemployment duration remains unchanged. Intuitively, this implies independence of the treatment effect with respect to the exclusion of these uninformative spells.

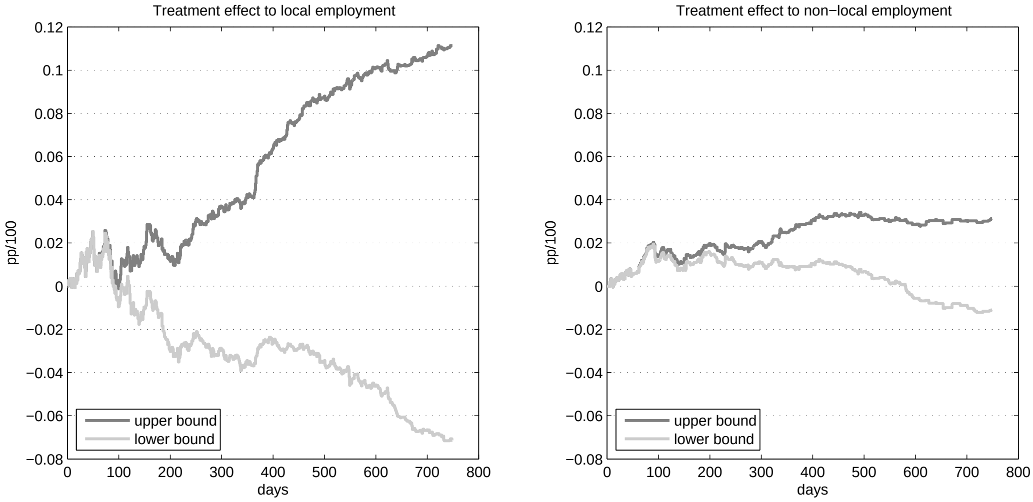
Table 6: Descriptive summary of restricted sample, IAB-R01

	Control group		Treatment group	
	pre-1997	post-1997	pre-1997	post-1997
LB spells				
median duration (days)	107	93	118	95
exit to local job	68.4% (68.9%)	65.2% (70.0%)	66.9% (67.6%)	64.7% (69.8%)
exit to non-local job	9.7% (9.8%)	10.2% (11.0%)	8.5% (8.5%)	10.1% (10.9%)
exit to other destination	21.1% (21.3%)	17.8% (19.1%)	23.6% (23.8%)	18.0% (19.4%)
total exits	99.2% (100%)	93.2% (100.0%)	98.9% (100.0%)	92.7% (100.0%)
UB spells				
median duration (days)	117	99	126	104
exit to local job	78.0% (79.7%)	69.4% (76.4%)	75.5% (78.0%)	69.3% (76.2%)
exit to non-local job	12.1% (12.4%)	11.1% (12.2%)	11.4% (11.8%)	11.1% (12.2%)
exit to other destination	7.7% (7.9%)	10.3% (11.3%)	9.9% (10.2%)	10.6% (11.6%)
total exits	97.9% (100.0%)	90.8% (100.0%)	96.9% (100.0%)	91.0% (100.0%)
Total spells	3,426	2,952	1,243	1,174

As can be seen in Table 6, using the restricted sample of spells yields similar descriptive patterns than before. Moreover, the cumulative incidence curves in the pre- and post reform era for the restricted sample in Appendix C indicate comparable shifts than for the full sample. This suggests that restricting the sample to spells with UC receipt within one months does not considerably alter the treatment pattern. A selection issue thus does not seem to be of major concern and the introduction of the sample restriction may be a valid way of tightening the bounds. Bounds for the restricted sample tend to be tighter because the distribution of T^{LB} and T^{UB} are more similar after eliminating one major source of data insecurity by assumption.

The resulting bounds in Figure 7 are tighter and indicate a positive reform effect for observed transitions to non-local employment. After one year of unemployment, the cumulative incidence of migration for individuals entitled to 12 month of UB is 1-3pp higher than for individuals entitled to 16.8 month of UB on average. In light of the institutional design in Germany, this finding is

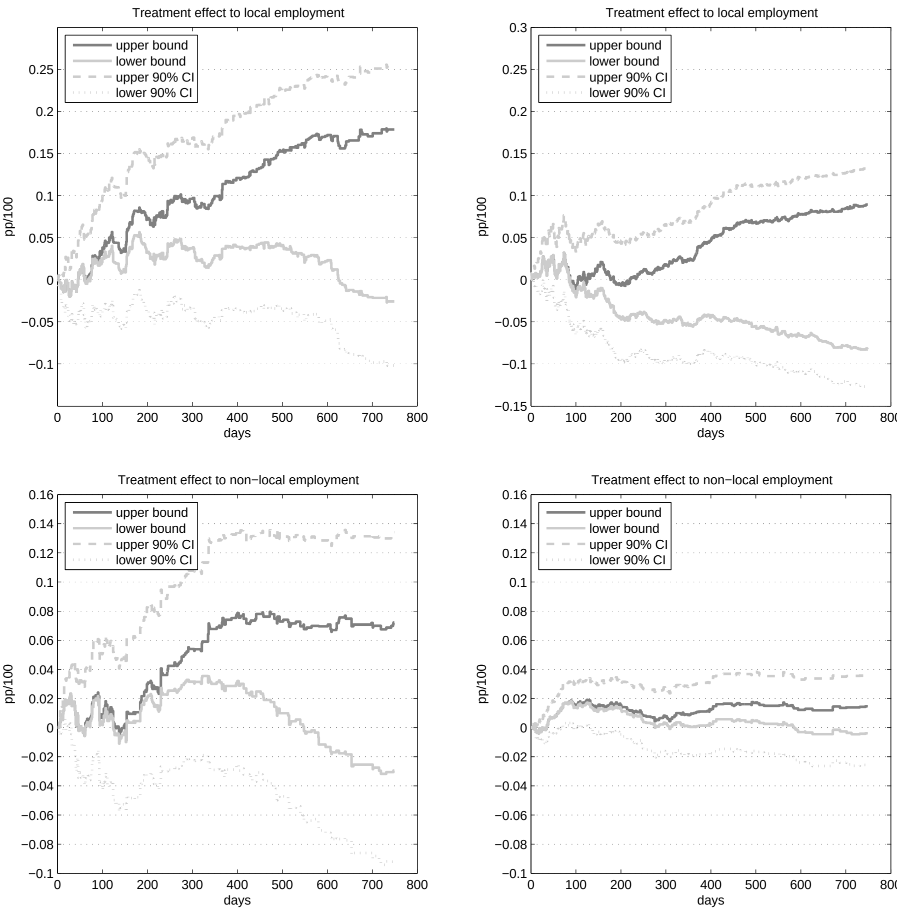
Figure 6: Lower and upper bound of treatment effect on the cumulative incidence of local and non-local exits to employment, restricted sample, IAB-R01



quite plausible as the counteracting resource effect suggested by Tatsiramos (2003) is likely to be small. This is because unemployed individuals irrespective of whether receiving UB or UA get financial support for search costs and moving costs. The negative effect of higher reservation wages in case of higher UB receipt should thus likely exceed any resource effect. Given the unresolved identification issue of the competing risks model, however, Figure 7 may only be considered as some tentative evidence that unemployment benefits reduce migration. Moreover, despite tighter bounds, the effect of shortening the receipt of UB on the observed transitions to local employment still cannot be identified from the data and the effect on observed non-local exits remains rather small. One reason for this weak finding may be that due to the institutional design the threat of entitlement loss from a reduction of the PUBD is likely to be large for a rather small group only. In the final section, we therefore take a look at the heterogeneity of the treatment effect.

Heterogeneous treatment effects As discussed before, the treatment effect of the reform is unlikely to be homogeneous. Individuals with complementary social benefits are not really affected by the length of UB receipt. Moreover, individuals who pass the means test for the receipt of UA only loose around 10% of there former wage income so that the impact of the reform should be limited. Individuals with other financial resources loose the entire unemployment compensation after exhausting unemployment benefits. Unfortunately, the IAB-R01 does not include enough information to actually distinguish between these three groups as the receipt of complementary social benefits is unknown. The wage information included in the IAB-R01 is only a rough indicator of complementary receipt of social benefits as the receipt of social benefits strongly depends on the household context which is unobserved in the IAB-R01. We therefore decided to compare two different skill groups instead because education is highly correlated with wage income and should thus capture some of the aforementioned differences. Less-skilled[9] workers are more likely to receive complementary social benefits or pass the means test for the receipt of UA than their high-skilled[10] counterparts. The reform effect is thus likely to be weaker for less-skilled individuals. Moreover, distinguishing between skill groups gave a clearer picture compared to looking at different wage quintiles as the latter results were not monotone across wage quintiles. Figure 7 therefore presents the findings by skill group. Moreover, we also add the asymptotically valid 90% joint confidence intervals for upper and lower bounds, which are computed following the bootstrap procedure of Horowitz and Manski (2000) and Lee and Wilke (2005). Sample sizes for the two skill groups can be found in Appendix D.

Figure 7: Lower and upper bound of treatment effect on the cumulative incidence of local and non-local exits to employment among high-skilled (left) and less-skilled (right) unemployed, restricted sample, IAB-R01



[9]Includes individuals who are either unskilled or have a vocational training and work as blue-collar workers.
[10]Includes individuals with a tertiary education or white-collar workers with at least a vocational training.

Despite the fact that none of the estimated lower bounds in Figure 7 is significantly above zero, the bounds are suggestive for a stronger reform effect on observed exit probabilities for the high-skilled segment for whom the threat of entitlement loss after exhausting UB is likely to be largest.[11] The point estimates for the bounds indicate that the observed post-reform probability of migration after one year of unemployment is 3-7pp higher for high-skilled individuals while the corresponding bounds for less-skilled individuals suggest a change of 0-1pp only. Moreover, point estimates also indicate an increasing observed transition probability to local employment after one year of unemployment of 2-8pp for high-skilled individuals only. For high-skilled individuals, the corresponding percentage change on the observed probability of migration is approximately 15-35% while the corresponding change for exits to local employment is around 5-20% only. Under the assumption of independent exit risks, these findings would have a causal interpretation in the sense that extensive unemployment benefits mainly allow for avoiding or postponing migration such that the reduction of UB entitlements primarily fosters the willingness to migrate. Due to the missing statistical significance which is probably due to the small sample size, however, all these findings are only weakly suggestive for some reform effects on leaving unemployment locally or non-locally and thus call for additional future research with a larger sample size.

[11]Due to the identification issue comparisons between groups are not unproblematic as both groups may experience different correlations between exit types which then also affect the resulting cumulative incidence curves.

5 Conclusion

This paper has presented an approach that allows for analyzing competing failure types in the case of partially missing information concerning the failure times. Partially missing data may occur whenever the state of an individual is partially unobserved such as in the case of unobserved periods in an individual's employment trajectory in administrative individual data. The non-parametric bounds analysis presented in this paper is thus a highly relevant approach for applied researchers who face similar data limitations. It extends the nonparametric bounds analysis for the single risk framework by Abadie (2005) and Lee and Wilke (2005) to a competing risk setting by deriving bounds for the risk specific cumulative incidence curve (CIC). One major advantage of the CIC compared to the marginal survivor curve is that it is still well defined in the case of dependent competing risks. In a simulation, we have demonstrated that this important property of the CIC also carries over to our bounds framework. Although our approach does not resolve the non-identifiability of competing risks and thus precludes a direct causal inference, it provides a flexible descriptive tool for the observed distribution of competing failures. In particular, our approach is fully nonparametric in the sense that we do not impose assumptions that may be violated in the real world. In an empirical application of our bounds framework, we have explored the effect of reducing the receipt of unemployment benefits on the observed transitions to either local or non-local employment via migration. For this purpose, we use the variation of unemployment benefit entitlements that is provided by the 1997 reform of entitlement length to unemployment benefits. Despite avoiding basically any identifying assumption in our bounds framework, we still obtain a number of interesting observations:

- Without showing statistical significance, the bounds are weakly suggestive for a stronger reform effect on observed exit probabilities for the high-skilled segment for whom the threat of entitlement loss after exhausting UB is likely to be largest.

- Under the assumption of independent competing risks, the treatment effect on migration clearly seem to exceed the positive treatment effect on exits to local employment in relative terms. This may suggest that extensive unemployment benefits mainly substitute for migration.

- In light of our findings, the current labor market reform in Germany (*Hartz IV*) is likely to foster migration and to accelerate exits to local employment among those for whom the threat of entitlement loss increased. First, the introduction of the means-tested social benefits II (SBII) decouples unemployment compensation after exhausting unemployment benefits from former wage income. This increases the threat of entitlement loss for approximately a quarter of all unemployed for whom the former unemployment assistance was more generous than the new SBII. In addition, the reduction of the maximum receipt of unemployment benefits for individuals above the age of 45 since 2003 is also likely to increase transition probabilities to both local and non-local employment. However, the transferability of our results to older age groups may be limited. For older age groups, more restrictive unemployment benefits may rather reduce early retirement, i.e. reduce the inflow into unemployment.

- We generally observe a smooth variation of the bounds with unemployment duration. This does not suggest any discontinuities in the hazard rates or survivor functions and thus supports the results of the non-stationary job-search theory of van den Berg (1990).
- As another interesting observation, we obtain that point estimates for the lower and upper bound of the latent variable do not span the full width of our estimated bounds. Therefore, a sensitivity analysis based on different definitions of the unemployment duration data alone may be misleading.

The limitations of our approach point towards some interesting future research needs:

- With regard to data limitations, data with more information on individual and household characteristics would be desirable to reexamine our empirical results. Such additional information would also allow to distinguish groups for whom a shorter receipt of unemployment benefits implies different entitlement losses. Moreover, repeating the analysis with a longer post-reform period or a larger sample size should be worthwhile to improve the statistical significance of the data.
- Due to the unresolved identification problem of the competing risk data, the causal inference from our empirical results is limited. Strictly speaking, our results can be interpreted causally only under the assumption of independent risk.

A promising route for future research thus is to combine our bounds framework for partially missing data with attempts to break the non-identifiability of dependent competing risks such as Honoré and Lleras-Muney (2006). However, as a disadvantage to our current bounds framework for cumulative incidence curves, such attempts necessitate additional assumptions.

Appendix A - Computation of actual and counterfactual UB entitlements

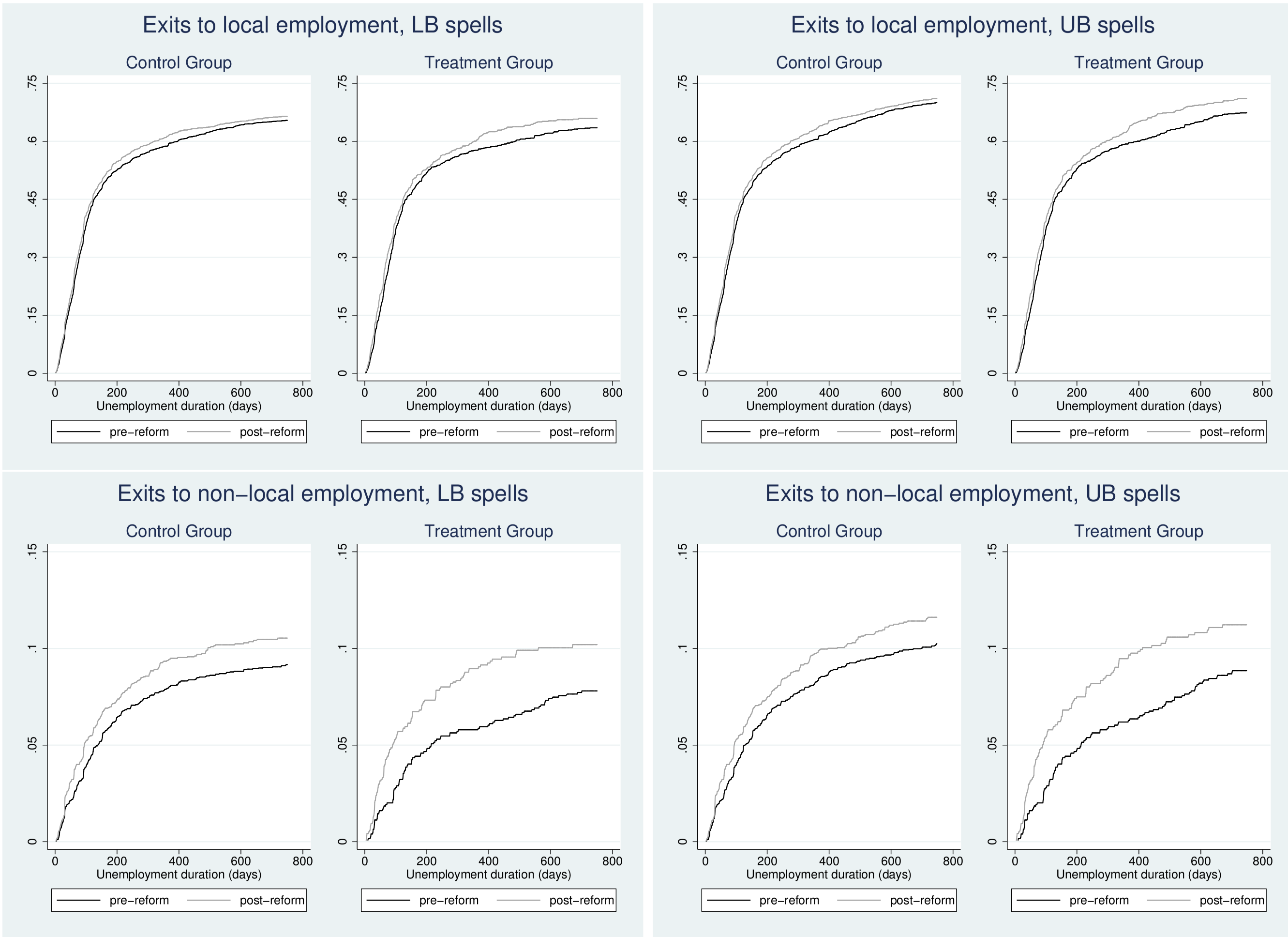
The entitlement length at the beginning of the unemployment spell is not included in the data and has to be computed based on the known employment history, age and the known regulations and changes across time. For this purpose, we compute the claim period which encompasses a maximum of three years prior to making the UB claim, but ends with a previous UB claim within this three years period. In the same token, we calculate the employment duration within the relevant extended claim period of up to seven years prior to making the claim. As previously mentioned, UB entitlements depend on the duration of socially insured employment within the relevant claim and the relevant extended claim period. Unemployment benefits exceeding 6 month necessitate at least 12 month socially ensured employment within the claim period. Thus, an individual with at least 12 month socially ensured employment within the claim period and 24 month within the extended claim period gets 12 month of UB. If there is a shortened claim period due to a previous UB claim, the new UB claim based on the employment periods after this last unemployment period may be extended up to the age-specific PUBD by remaining entitlements at the end of the previous unemployment period if the beginning of the last UB claim lies within the last seven years.

For the estimation of actual UB entitlements all changing regulations throughout the 1980s and 1990s have been applied. For the counterfactual UB entitlements, we apply the pre-reform conditions to the post-reform period and compute the UB entitlements as if all individuals had been 42 by the time of the benefit claim. More precisely, we adjust the whole age history of an individual as if, for example, an individual aged 38 at the beginning of the unemployment period had always been four years older. This adjustment alone does not ensure the comparability of the resulting counterfactual entitlements for the pre- and post-reform period because entitlements depend on the entire work history which is subject to all previous changes in regulations. We therefore compute the counterfactual entitlements for the post-reform period had all changes in regulations been shifted by five years, the difference between the pre- and post-reform period. This procedure ensures a twofold: (i) the comparability of counterfactual UB entitlements for all age groups irrespective of whether the unemployment period starts prior or after the reform and (ii) the equivalence of counterfactual and actual UB entitlements for the treatment group in the pre-reform era. As a consequence, the treatment group in the pre-reform period with counterfactual UB entitlements of more than 12 month actually has entitlements of more than 12 month while all others who fulfil this criterium actually receive UB for a maximum of 12 month only, but are comparable to the former group in terms of their employment history.

Appendix B - Descriptive summary of sample characteristics

	Control group		Treatment group	
Age (years)	38.3	38.3	43.0	43.0
High school degree	8.6	8.0	8.9	7.7
Vocational training	82.8	83.1	83.1	82.7
Tertiary education	8.6	8.9	8.0	9.5
1st wage quintile	22.0	22.1	23.9	23.9
2nd wage quintile	26.8	30.3	24.0	29.1
3rd wage quintile	20.3	22.9	20.7	20.2
4th wage quintile	17.0	14.8	16.8	14.0
5th wage quintile	14.0	9.9	14.6	12.8
Tenure prev. job (days)	1172.9	1128.6	1385.0	1244.7
Tenure in claim period (days)	1471.4	1434.3	1563.1	1439.2
Prev. recall	17.0	19.6	17.1	19.8
Skilled blue-collar	43.5	43.3	42.8	43.1
Unskilled blue-collar	32.8	31.8	33.7	31.0
White-collar	23.7	25.0	23.4	25.9
Prev. unemployment	73.8	78.9	70.4	77.3
Total spells	4,294	3,577	1,557	1,436

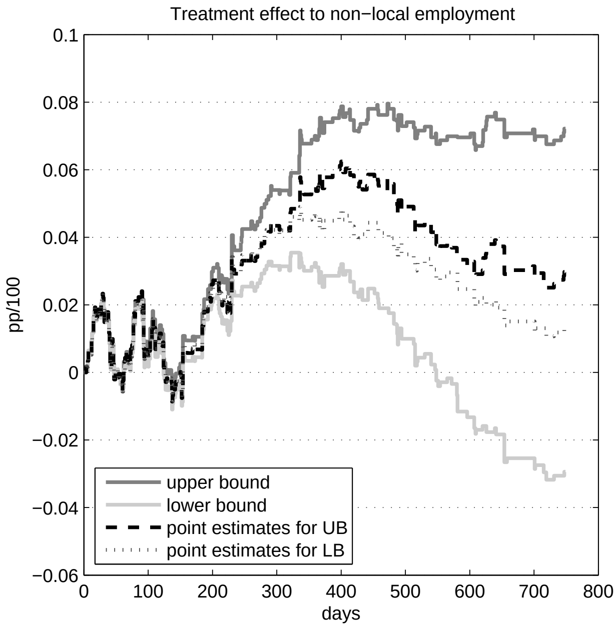
Appendix C - Cumulative incidence curves in the pre- and post reform era by treatment and control group for both unemployment definitions, restricted sample, IAB-R01



Appendix D - Descriptive summary of restricted sample by skill group

	Control group		Treatment group	
	pre-1997	post-1997	pre-1997	post-1997
High-skilled individuals				
LB spells				
median duration (days)	154	122	206	122
exit to local job	48.5% (48.9%)	46.5% (49.9%)	43.0% (43.3%)	46.3% (49.2%)
exit to non-local job	19.8% (20.0%)	19.8% (21.2%)	17.2% (17.3%)	18.8% (20.0%)
exit to other destination	30.9% (31.2%)	26.9% (28.9%)	39.2% (39.4%)	29.0% (30.8%)
total exits	99.2% (100.0%)	93.2% (100.0%)	99.4% (100.0%)	94.1% (100.0%)
UB spells				
median duration (days)	200	127	305	154
exit to local job	58.5% (59.8%)	51.8% (56.9%)	53.5% (55.3%)	51.9% (56.0%)
exit to non-local job	25.6% (26.2%)	21.8% (24.0%)	22.3% (23.0%)	21.8% (23.5%)
exit to other destination	13.7% (14.0%)	17.4% (19.1%)	21.0% (21.7%)	19.1% (20.6%)
total exits	97.8% (100.0%)	91.2% (100.0%)	96.8% (100.0%)	92.8% (100.0%)
Total spells	864	815	314	335
Less-skilled individuals				
LB spells				
median duration (days)	99	91	102	92
exit to local job	75.1% (75.7%)	72.3% (77.6%)	74.9% (75.9%)	72.0% (78.1%)
exit to non-local job	6.3% (6.3%)	6.6% (7.0%)	5.5% (5.6%)	6.6% (7.1%)
exit to other destination	17.8% (18.0%)	14.3% (15.3%)	18.3% (18.5%)	13.6% (14.8%)
total exits	99.2% (100.0%)	93.2% (100.0%)	99.7% (100.0%)	92.2% (100.0%)
UB spells				
median duration (days)	105	94	110	95
exit to local job	84.6% (86.4%)	76.2% (83.9%)	83.0% (85.7%)	76.3% (84.5%)
exit to non-local job	7.6% (7.8%)	7.0% (7.7%)	7.8% (8.0%)	6.8% (7.5%)
exit to other destination	5.7% (5.8%)	7.6% (8.4%)	6.1% (6.3%)	7.2% (7.9%)
total exits	97.9% (100.0%)	90.8% (100.0%)	96.9% (100.0%)	90.3% (100.0%)
Total spells	2,562	2,137	929	839

Appendix E - Point estimates for lower and upper bound of treatment effect on the cumulative incidence of non-local exits to employment among high-skilled unemployed, restricted sample.



The point estimations for the treatment effect using the lower bound and upper bound of the employment duration data are done by using the following formulas:

$$
\begin{aligned}
l_{Ir}(t_j|p_{t0}, p_{t1}, x) &= \{I_r^{LB}(t_j|1, p_{t1}, x) - I_r^{LB}(t_j|0, p_{t1}, x)\} \\
&\quad - \{I_r^{LB}(t_j|1, p_{t0}, x) - I_r^{LB}(t_j|0, p_{t0}, x)\} \\
&\text{and} \\
u_{Ir}(t_j|p_{t0}, p_{t1}, x) &= \{I_r^{UB}(t_j|1, p_{t1}, x) - I_r^{UB}(t_j|0, p_{t1}, x)\} \\
&\quad - \{I_r^{UB}(t_j|1, p_{t0}, x) - I_r^{UB}(t_j|0, p_{t0}, x)\}
\end{aligned}
$$

for $r = 1, \ldots, m$ and other notation is the same as in section 2.

References

[1] Bundesanstalt für Arbeit (2001), IAB-Kurzbericht Nr. 1/2001, Nürnberg: Institut fr Arbeitsmarkt- und Berufsforschung der Bundesanstalt fr Arbeit.

[2] Abbring, J.H. and G.J. van den Berg (2003), The identifiability of the mixed proportional hazards competing risks model, *Journal of the Royal Statistical Society B* Vol. 65, 701–710.

[3] Abadie, A. (2005), Semiparametric Difference-in-Differences Estimators, *Review of Economic Studies* Vol. 72, 1–19.

[4] Antolin, P. and O. Bover (1997), Regional Migration in Spain: The Effect of Personal Characteristics and of Unemployment, Wage and House Price Differentials Using Pooled Cross-Sections, *Oxford Bulletin of Economics and Statistics* Vol. 59, 215–35.

[5] Arntz, M. (2005), *The Geographical Mobility of Unemployed Workers*, ZEW Discussion Paper No. 05-34, Mannheim.

[6] Arntz, M. and R.A. Wilke (2006), *Unemployment Duration in Germany: Individual and Regional Determinants of Local Job Finding, Migration and Subsidized Employment*, ZEW Discussion Paper 06-92, Mannheim.

[7] Atkinson, A.B. and J. Micklewright (1991), Unemployment compensation and labor market transitions: A critical review, *Journal of Economic Literature* Vol. 29, 1679-1727.

[8] Barron, J.M. and W. Mellow (1979), Search Effort in the Labor Market, *The Journal of Human Resources* Vol. 14, 389–404.

[9] Boone, J., P. Fredriksson, B. Holmlund, and J.C. van Ours (2002) *Optimal Unemployment Insurance with Monitoring and Sanctions*, IFAU Working Paper 21, Uppsala.

[10] Boone, J., A. Sadrieh, and J.C. van Ours (2004), *Experiments on Unemployment Benefit Sanctions and Job Search Behavior*, IZA Discussion Paper No. 1000, Bonn.

[11] Card, D.E. and P.B. Levine (2000), Extended benefits and the duration of UI spells: Evidence from the New Jersey Extended Benefit Program, *Journal of Public Economics* Vol. 78, 107-138.

[12] Cox, D.R. (1962), *Renewal Theory*, London.

[13] Fitzenberger, B. and R.A. Wilke (2007), *New Insights on Unemployment Duration and Post Unemployment Earnings in Germany: Censored Box-Cox Quantile Regression at Work*, ZEW Discussion Paper No. 07-007, Mannheim.

[14] Fitzenberger, B. and R.A. Wilke (2004), *Unemployment Durations in West-Germany Before and After the Reform of the Unemployment Compensation System During the 1980s*, ZEW Discussion Paper No. 04-24, Mannheim.

[15] Goss, E. and C. Paul (1990), The Impact of Unemployment Insurance Benefits on the Probability of Migration of the Unemployed, *Journal of Regional Science* Vol. 30, 349–358.

[16] Hamann, S., G. Krug, M. Köhler, W. Ludwig-Mayerhofer, and A. Hacket (2004), Die IAB-Regionalstichprobe 1975-2001: IABS-R01, *ZA-Information* Vol. 55, 36–42.

[17] Hassler, J., J.V. Rodriguez Mora, K. Storesletten, and F. Zilibotti (2005), A positive theory of geographic mobility and social insurance, *International Economic Review* Vol. 46, 263-303.

[18] Heckman, J.J. and B.E. Honoré (1989), The identifiability of the competing risks model, *Biometrika* Vol. 76, 325–330.

[19] Honoré, B.E. and A. Lleras-Muney (2006), Bounds in Competing Risks Models and the War on Cancer, *Econometrica* Vol. 74, 1675–1698.

[20] Horowitz, J. L. and Manski, C. F. (2000), Nonparametric analysis of randomized experiments with missing covariate and outcome data (with discussion), *J. Amer. Statist. Assoc.* Vol. 95, 77–88.

[21] Hunt, J. (1995), The Effect of Unemployment Compensation on Unemployment Duration in Germany, *Journal of Labor Economics* Vol. 13, 88-120.

[22] Kalbfleisch, J.D. and R.L. Prentice (1980), The Statistical Analysis of Failure Time Data, *Wiley Series in Probability and Statistics.*

[23] Katz, L.F. and B.D. Meyer (1990), The impact of the potential duration of unemployment benefits on the duration of unemployment, *Journal of Public Economics* Vol. 41, 45-72.

[24] Lalive, R. and J. Zweimüller (2004), Benefit Entitlement and Unemployment Duration: Accounting for Policy Endogeneity, *Journal of Public Economics* Vol. 88, 2587–2616.

[25] Lee, S. and R.A. Wilke (2005), *Reform of Unemployment in Germany: A nonparametric Bounds Analysis using Register Data*, ZEW Discussion Paper 05-29, Mannheim.

[26] Manski, C.F. (2003), *Partial Identification of Probability Distributions*, New York.

[27] Moeschberger, M.L. and J.P. Klein (1995), Statistical Methods for Dependent Competing Risks, *Lifetime Data Analysis* Vol. 1, 195–204.

[28] Müller, E., R.A. Wilke, and P. Zahn (2007), Beschäftigung und Arbeitslosigkeit älterer Arbeitnehmer: Eine mikroökonometrische Evaluation der Arbeitslosengeldreform von 1997, *Jahrbücher für Nationalökonomie und Statistik* Vol. 227, forthcoming.

[29] Oschmiansky, O., S. Kull, G. and Schmid (2001), *Faule Arbeitslose? Politische Konjunkturen einer Debatte*, Discussion Paper FS 01-206, Wissenschaftszentrum Berlin für Sozialforschung.

[30] Pepe, M.S. (1991), Inference for Events With Dependent Risks in Multiple Endpoint Studies, *Journal of the American Statistical Association* Vol. 86, 770-778.

[31] Pepe, M.S. and M. Mori (1993), Kaplan-Meier, Marginal or Conditional Probability Curves in Summarizing Competing Risks Failure Time Data? *Statistics in Medicine* Vol. 12, 737–751.

[32] Peterson, A.V. (1976), Bounds for a Joint Distribution With Fixed Sub-Distribution Functions: Application to Competing Risks, *Proceedings of the National Academy of Science* Vol. 73, 11–13.

[33] Plaßmann, G. (2002), *Der Einfluss der Arbeitslosenversicherung auf die Arbeitslosigkeit in Deutschland. Beiträge zur Arbeitsmarkt- und Berufsforschung*, No. 255, Institut für Arbeitsmarkt- und Berufsforschung der Bundesanstalt für Arbeit (IAB), Nürnberg.

[34] Tannery, F.J. (1983), Search Effort and Unemployment Insurance Reconsidered, *The Journal of Human Resources* Vol. 18 432–440.

[35] Tatsiramos, K. (2003), *Geographic Mobility and Unemployment Insurance in Europe: a cross country comparison using the ECHP*, IZA Discussion Paper No. 1253, Bonn.

[36] Tsiatis, A. (1975), A Nonidentifiability Aspect of the Problem of Competing Risks, *Proceedings of the National Academy of Sciences* Vol. 72, 20–22.

[37] Van den Berg, G.H. (1990), Nonstationarity in Job Search Theory, *Review of Economic Studies* Vol. 57, 255–277.

[38] Van Ours, J.C. and M. Vodopivec (2006), How Shortening the Potential Duration of Unemployment Benefits Affects the Duration of Unemployment: Evidence from a Natural Experiment, *Journal of Labor Economics* Vol. 24, 351-378.

[39] Wolff, J. (2003), *Unemployment Benefits and the Duration of Unemployment in East Germany*, Sonderforschungsbereich 386 Discussion Paper, No. 344.