FDZ *Methodenreport*

Methodische Aspekte zu Arbeitsmarktdaten

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

August 2007

Melanie Arntz(ZEW Mannheim)
Simon M. S. Lo (University of Mannheim and ZEW Mannheim)
Ralf A. Wilke (University of Leicester)

Bundesagentur für Arbeit

Data access

The dataset described in this document is available for use by professional research-ers. For further information, see "Individual Data" on the website: http://fdz.iab.de/en/.

Non-technical summary

Administrative individual data does not contain complete information concerning individual employment trajectories. For this reason, there are gaps or unobserved periods and the individual labor market state is sometimes unknown. In this paper, we present an approach how to empirically analyze transition times to competing labor market states in the case of such missing information. As a solution to the resulting data-driven identification problem, we derive bounds for the observed destination-specific transition times. By performing a nonparametric analysis, we provide a flexible and descriptive tool for the analysis of observed risk-specific transition time distributions in presence of partially identified interval data. As an advantage over earlier attempts, our approach does not assume that the competing labor market states are independent.

We apply our framework to empirically evaluate the effect of unemployment benefits on observed migration probabilities in Germany. 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 ten 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. Given the partial identification problem in our data, we then apply our bounds framework to analyze the effect of the reform on migration and other destination states. The findings indicate that missing interval information in German individual administrative data at first precludes any clear result as the resulting bounds tend to be very wide. When introducing additional assumptions, 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, our results are indicative for a mobility-reducing effect of an extensive receipt of unemployment benefits.

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

Melanie Arntz[†]

Simon M. S. Lo[‡]

Ralf A. Wilke[§]

August 2007

Abstract

In this paper we derive nonparametric bounds for the cumulative incidence curve within a competing risks model with partly identified interval data. As an advantage over earlier attempts our approach also gives valid results in case of dependent competing risks. We apply our framework to empirically evaluate the effect of unemployment benefits on observed migration of unemployed workers in Germany. Our findings weakly indicate that reducing the entitlement length for unemployment benefits increases migration among high-skilled individuals.

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

JEL: C41, C14, J61

1 Introduction

In this paper, we present an approach how to empirically analyze models of competing risks in the case of partially identified interval data. As an example, administrative unemployment duration data from Germany provide only incomplete information concerning the duration until leaving unemployment to different states 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). Lee and Wilke (2005) bound a difference-in-differences treatment effect on the marginal survival probability over different definitions of the latent durations. They assume 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 censoring or dependent competing risks.

The contribution of this paper is to present a solution to the data-driven identification problem that gives also valid results in the case of dependent competing risks. For this purpose, we extend the bounds framework for partially identified data to a competing risks setting and derive bounds for the cumulative incidence curve (CIC) instead of assuming independent censoring. By using the CIC (see also Kalbfleisch and Prentice, 1980), our analysis bounds the risk-specific distribution of observed durations rather than latent durations. Our approach is fully nonparametric and thus we do not impose assumptions that may be violated in the real world.

Our work is relevant as partial identification is a common problem in merged administrative individual data from many countries. The literature using such data is growing in recent years and governments use this data to analyze policy reforms. In this light we illustrate our approach by analyzing the effect of unemployment compensation on the probability of migration. The findings of former work on this topic are not conclusive. A number of 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 compensation to raise reservation wages (Atkinson and Micklewright, 1991). The effect on migration, however, is 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 unemployment benefits 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, Antolin and Bover, 1997). Consistent with these findings,

Arntz (2005) and Arntz and Wilke (2007) provide some evidence that unemployed in Germany who are entitled to receive unemployment benefits (UNB) 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 UNB receipt. To some extent the findings of these studies may be driven by an unobserved selection of immobile individuals into unemployment benefits or an extensive receipt of UNB. In a study with individual fixed effects that should mitigate such biases, 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 possibly dependent 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. Our findings indicate that missing interval information in German individual administrative data at first precludes any clear result as the bounds tend to be very wide. By introducing additional assumptions, bounds can be tightened. For high-skilled individuals, for whom the threat of entitlement loss due to the 1997 reform is likely to be largest, our results are indicative for the mobility-reducing effect of an extensive receipt of unemployment benefits.

The paper is structured as follows. The following section presents the theoretical framework for the bounds analysis. Section three applies the proposed method to analyze the effect of unemployment benefits on the cumulative incidence of migration. Section four concludes.

2   Model

In this model we study state transition times with $k = 0, 1, \ldots, K$ different competing labor market states. We let T_{lk} be a random variable of the latent transition time from an original state $l = 0, 1, \ldots, K$, to a destination state $k = 0, 1, \ldots, K$. l denotes the original state and k denotes the destination state and $k \neq l$. We denote $k = 0$ as the state of unemployment whereas $k = 1, \ldots, K$ represents states such as employment in the local or a non-local area or being out of labor force. There are $i = 1, \ldots, n$ independent identically distributed realizations τ_{ilk} of T_{lk}. For simplicity, we suppress subscript i in the rest of this paper. In case $k = 0$, the end date of the last employment spell is normalized as $T_{l0} = 0$, for $l = 1, \ldots, K$. We are particularly interested in studying unemployment duration, i.e. the transition time T_{0k} from state $l = 0$ to state $k = 1, \ldots, K$, and its realization τ_{0k} with $k = 1, \ldots, K$. When there is no

ambiguity, we suppress the subscript of the original state 0. The latent transition time is then $T_{0k} = T_k$ and its realization is $\tau_{0k} = \tau_k$. We assume that $\tau_{lk} \neq \tau_{lm}$ for all l and all $k \neq m$. X is a vector of exogenous individual characteristics and T_{lk} are some unknown functions of X. T_{max} is an exogenous random variable that refers to the maximum observation period and t_{max} is its realization. In other words, we have independent right censoring at the end of the observation period. In what follows r is the exit state which has the shortest latent transition time among all τ_k with $k = 1, \ldots, K$, i.e.

$$
r = \begin{cases} 1 & \text{if} \quad \tau_1 = min_k\{\tau_k\}; \\ \quad \vdots \\ K & \text{if} \quad \tau_K = min_k\{\tau_k\}. \end{cases}
$$

Other labor market states such as becoming self-employed or being out of the labor force are not observable and thus produce observational gaps in the interval data. For simplicity, we pool all unobserved labor market states to one state K.[1] Since an observational gap after the end of unemployment compensation transfers may also refer to continued unemployment without receiving unemployment compensation, it is not clear whether an individual remains unemployed or leaves to one of the unobserved exit states K after the end of transfer receipt. The data structure thus implies that the unemployment duration and the transition to one of the other exit states $k = 1, ..., K - 1$ can unambiguously be identified only if the individual receives unemployment compensation during the entire time period of unemployment.

Figure 1 illustrates the fully identified case where the observed transition time refers to the transition time from state 0 (unemployment) to state $k \neq K$ such that $\tau_r = min_{k \neq K}\{\tau_k\}$. Thus, the transition time is point-identified with known exit state $r \neq K$.

Figure 1: A fully identified unemployment duration with $\tau_r = min_{k \neq K}\{\tau_k\}$.
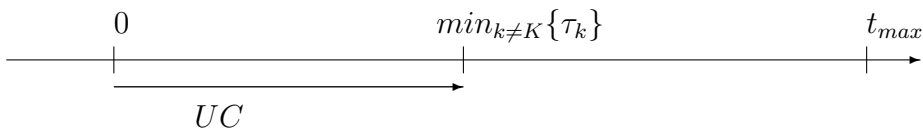
By contrast, if interval data is only partly identified, the true unemployment duration may not be point identified. As a consequence, parameters of interest can only be bounded (Lee and Wilke, 2005). Our modeling framework steps behind recent work by not assuming independence

---

[1] The model can be easily extended to more one unobserved labor market states but it is not interesting to distinguish between different unobservable risks.

between competing risks. Let us denote the beginning of the first unobserved period by the random variable C and its realization by ς. In our application, this is typically the end date of receiving unemployment compensation. In many cases, unemployment compensation stops because unemployment benefits have been exhausted and the individual does not pass a means-test for unemployment assistance. Other reasons are benefit sanctions for unemployed who did not comply with the eligibility criteria. Both cases can be hardly predicted and are typically not random. Since the earliest exit to K occurs at the beginning of an unobserved time period, it holds that $\varsigma \leq \tau_K$ and thus C and T_K are not independent, i.e. $C = T_K - \xi(T_K)$ where $\xi(T_K) \in [0, T_K]$ is some positive random function. Given the data structure, there are also observations where the receipt of unemployment compensation does not immediately start after the end of an employment period and thus there is an unobserved period starting at $T_{l0} = 0$ so that $\varsigma = 0$. Moreover, ς is observed only if $\varsigma \leq \tau_r$. In the case of our fully identified data in Figure 1, we therefore have $\tau_r = \min_{k \neq K}\{\tau_k\} < \varsigma \leq \tau_K$ and ς is not observed.

Figure 2: A partially identified unemployment duration for which ς is observed and $\varsigma \leq s_{k \neq K} \leq t_{max}$.
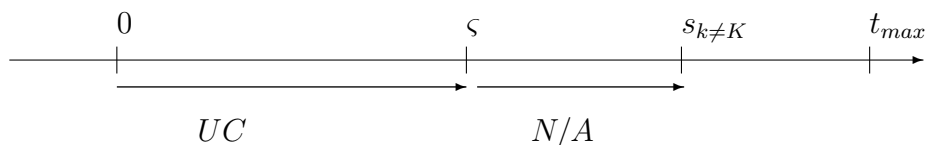

Figure 2 illustrates the case where we observe ς, i.e. $\varsigma \leq \tau_r$ due to an unobserved period after a period of unemployment compensation transfers.[2] In this case, τ_r cannot be point identified from the data and has to be bounded. For this purpose we denote the first observed transition time after an unobserved period as $s_{k \neq K}$. It is possible to construct worst-case bounds for τ_r:

1. The upper bound of τ_r can be obtained by assuming that there is no exit to K during the unobserved period. Instead, unemployment continues until $s_{k \neq K}$ and so τ_r is equal to $s_{k \neq K} = min_{k \neq K}\{\tau_k\}$. In other words, by ignoring ς, τ_r would be identified as in Figure 1.

2. The lower bound of τ_r can be obtained by assuming that there is an exit to K during the unobserved period, i.e. $r = K$. Then the earliest transition to K can occur at ς so that the lowest value of τ_r is ς. In this case, $s_{k \neq K}$ equals to $\tau_K + \tau_{Kk}$ and can be ignored.

---

[2]As discussed before, an unobserved period can also occur directly after an employment period. In this case $\varsigma = 0$. Moreover, there can be more than one unobserved period between records of unemployment compensation transfers.

If we observe ς, the true value τ_r lies always in the interval $[\varsigma, s_{k \neq K})$. We now define a variable δ to formalize the identification of τ_r from the data as follows:

$$\delta = \begin{cases} 0 & \text{if } \varsigma \text{ is not observed;} \\ 1 & \text{if } \varsigma \text{ is observed} \end{cases}$$

Events $\delta = 0, 1$ are disjoint and can be distinguished in the data. If ς is not observed in the data, we have $\delta = 0$, and τ_r is fully identified. If we have $\delta = 1$, the unemployment duration is partially identified and τ_r is unknown. If we knew that $r \neq K$, then τ_r would be known. But if $r = K$, we only know that $\tau_K \in [\varsigma, s_{k \neq K})$. The difficulty in an application is that for $\delta = 1$ we do not know whether $r = K$ or $r \neq K$.

In addition to the identification problem that arises from the uncertainty of τ_r in the case of $\delta = 1$, our competing risk setting also implies another identification problem which is related to the general identification problem of competing risks. If risks are not independent, the marginal distribution for each competing risk cannot be identified without additional parametric assumptions (Cox, 1962; Tsiatis, 1975). In light of this additional identification problem, partial effects and changes in the latent distributions can also only be bounded. Non-parametric bounds on the marginal distribution as have been proposed by Peterson (1976) are typically too wide to infer some causal interpretation. As an alternative, parametric assumptions can be imposed to tighten bounds or to achieve full identification. 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 in our application and hence leaves the fundamental identification problem unresolved. As its main contribution, however, it tackles the identification problem that stems from partial identification of interval data. Moreover, by using bounds on the cumulative incidence curve, it provides a non-parametric tool which has a meaningful interpretation also in presence of dependent competing risks.

The CIC refers to the observed probability of experiencing a transition to a specific state prior to a certain time in the presence of all competing risks. It therefore does not recover the underlying risk-specific marginal distribution of latent durations. Instead, it refers to observed transition probabilities. Related literature also refers to this as a subdistribution. In the following, we derive bounds on the identification region of the CIC which reflect the partial identification of τ_r only. These bounds do not resolve the identification problem of competing risks models. Equivalent bounds can also be derived for the overall survivor curve while bounds for other functions such as cause-specific hazard rate or the cause-specific cumulative hazard

7

rate cannot be derived. In the following, we restrict our attention to the observable risks $k = 1, \ldots, K - 1$.

The CIC for transition to state k at time t (see Moeschberger and Klein, 1995) can be decomposed:

$$
\begin{aligned}
I_k(t|x) &= P(T_k \leq t, r = k|x) \\
&= P(T_k \leq t, r = k, \delta = 0|x) + P(T_k \leq t, r = k, \delta = 1|x) \\
&= P(T_k \leq t, r = k, \delta = 0|x) \\
&\quad + P(T_k \leq t, r = k, r \neq K, \delta = 1|x) + P(T_k \leq t, r = k, r = K, \delta = 1|x) \\
&= P(T_k \leq t, r = k, \delta = 0|x) + P(T_k \leq t, r = k, r \neq K, \delta = 1|x)
\end{aligned}
\tag{1}
$$

for $k = 1, \ldots, K - 1$. The second part of (1) is not identified since we cannot identify r in presence of $\delta = 1$. Therefore, as discussed before, the second part of (1) can only be bounded. The lower bound relies on the assumption that unobserved periods correspond for sure to an unobserved labor market state $r = K$, i.e. $P(r \neq K|\delta = 1, x) = 0$. In this case, the second part of (1) is zero. By contrast, it is non-zero under the upper bound assumption that there is for sure continued unemployment during an unobserved period, i.e. $P(r \neq K|\delta = 1, x) = 1$. In this case, τ_r is identified and $P(T_k \leq t, r = k, r \neq K, \delta = 1|x)$ can be directly estimated from the data. Note that these worst case bounds assume the conditional probability $P(r \neq K|\delta = 1, x)$ to be either zero or one for all t. As suggested by Manski (2003), we bound the unknown probability $P(T_k \leq t, r = k, r \neq K, \delta = 1|x)$ by an interval which is identifiable with the available data structure. To see this, we rewrite (1) as:

$$
\begin{aligned}
I_k(t|x) &= P(T_k \leq t, r = k, \delta = 0|x) \\
&\quad + P(T_k \leq t, r = k|r \neq K, \delta = 1, x)P(r \neq K|\delta = 1, x)P(\delta = 1|x).
\end{aligned}
\tag{2}
$$

The worst-case lower bound on the identification region of the CIC of risk $k \neq K$ is then given by assuming $P(r \neq K|\delta = 1, x) = 0$:

$$
I_k^{LB}(t|x) = P(T_k \leq t, r = k, \delta = 0|x).
\tag{3}
$$

The worst-case upper bound for the CIC is obtained by assuming $P(r \neq K|\delta = 1, x) = 1$:

$$
\begin{aligned}
I_k^{UB}(t|x) &= P(T_k \leq t, r = k, \delta = 0|x) \\
&\quad + P(T_k \leq t, r = k|r \neq K, \delta = 1, x)P(\delta = 1|x)
\end{aligned}
\tag{4}
$$

It directly follows that $I_k^{LB}(t|x) \leq I_k^{UB}(t|x)$ for all t.

The bounds given in (3) and (4) can be estimated nonparametrically by using Kaplan-Meier type estimators, as the censoring time T_{max} is independent (see Kalbfleisch and Prentice, 2002).

8

Let $t_0 < \ldots < t_j < \ldots < t_J$ be the discrete times at which $\tau_{k \neq K}$, ς and t_{max} are observed. For the estimation of the lower bound (3) there are d_{kj}^{LB} observed exits to risk type $k \neq K$ at time t_j; d_{cj}^{LB} observed realizations of C at t_j; and d_{mj}^{LB} censored observations at $t_{max} = t_j$. For $k \neq K$, we have:

$$d_{kj}^{LB} = \sum_{i=1}^{n} \mathbb{I}(\tau_{ik} = min\{\tau_{ik}, \varsigma_i, t_{i,max}\})$$

$$d_{cj}^{LB} = \sum_{i=1}^{n} \mathbb{I}(\varsigma_i = min\{\tau_{ik}, \varsigma_i, t_{i,max}\})$$

$$d_{mj}^{LB} = \sum_{i=1}^{n} \mathbb{I}(t_{i,max} = min\{\tau_{ik}, \varsigma_i, t_{i,max}\})$$

with $\mathbb{I}(Y)$ is the indicator function of the event Y.

In contrast, ς can be ignored for the estimation of the upper bound (4). Moreover, in this case we have $\tau_r = s_{r \neq K}$. Then we define d_{kj}^{UB} and d_{mj}^{UB} analogously as

$$d_{kj}^{UB} = \sum_{i=1}^{n} \mathbb{I}(\tau_{ik} = min\{\tau_{ik}, t_{i,max}\})$$

$$d_{mj}^{UB} = \sum_{i=1}^{n} \mathbb{I}(t_{i,max} = min\{\tau_{ik}, t_{i,max}\}).$$

Let $d_j^{LB} = \sum_{k=1}^{K-1} d_{kj}^{LB} + d_{cj}^{LB} + d_{mj}^{LB}$ and $d_j^{UB} = \sum_{r=1}^{K-1} d_{kj}^{UB} + d_{mj}^{UB}$. The number of observations at risk just before t_j is then given by

$$n_j^{LB} = d_j^{LB} + \ldots + d_J^{LB} \quad \text{and} \quad n_j^{UB} = d_j^{UB} + \ldots + d_J^{UB}.$$

The Kaplan-Meier type estimators for the cause specific hazard rate and the overall survivor curve for the distribution of observed transition to state $k \neq K$ are

$$\hat{\lambda}_k^b(t_j|x) = d_{kj}^b/n_j^b \text{ with } b \in \{LB, UB\} \quad \text{and} \quad \hat{\lambda}_c^{LB}(t_j|x) = d_{cj}^{LB}/n_j^{LB};$$

$$\hat{S}^{LB}(t_j|x) = \prod_{u=1}^{j-1} \left(1 - \sum_{k=1}^{K-1} \hat{\lambda}_k^{LB}(t_u) - \hat{\lambda}_c^{LB}(t_u)\right) \quad \text{and} \quad \hat{S}^{UB}(t_j|x) = \prod_{u=1}^{j-1} \left(1 - \sum_{k=1}^{K-1} \hat{\lambda}_k^{UB}(t_u)\right) \quad (5)$$

Note that that these estimators are consistent as the right censoring is independent. A consistent estimator for the bounds given in (3) and (4) is then:

$$\hat{I}_k^b(t_j|x) = \sum_{u=1}^{j} \hat{\lambda}_k^b(t_u|x)\hat{S}^b(t_u|x) \quad \text{with} \quad b \in \{LB, UB\} \quad (6)$$

and $k \neq K$.

In analogy to Lee and Wilke (2005), we use the monotone relations given in (3) and (4) to bound a difference-in-differences estimator. Suppose there is a policy intervention in a natural experiment setting and we have $X = (G, P, Y)$. There are two groups, the control group ($G = g_0$) and the treatment group ($G = g_1$), and two time intervals, the pre-reform period ($P = p_{t0}$) and the post-reform period ($P = p_{t1}$). Y is a vector of other observable individual variables such as gender, age etc. The reform of interest is supposed to have an effect on the observed risk-specific transition distribution of the treatment group in the post-reform years. Under the assumption that the CIC of treatment and control group would have followed parallel paths without the reform, the effect of the reform can be estimated by a difference-in-differences estimator (DID) as (see also Abadie, 2005 for a review of nonparametric identification of DID models)

$$\Delta_{Ik}(t_j|y) = [I_k(t_j|g_1, p_{t1}, y) - I_k(t_j|g_0, p_{t1}, y)] - [I_k(t_j|g_1, p_{t0}, y) - I_k(t_j|g_0, p_{t0}, y)] \qquad (7)$$

for $r = 1, \ldots, K - 1$, where $I_k(t_j|g, p, y) = P(T_k \leq t_j, r = k|G = g, P = p, Y = y)$. Given that we can only identify intervals for the risk-specific cumulative incidence curve it is straightforward to bound Δ_{Ik} (Lee and Wilke, 2005):

$$
\begin{aligned}
l_{Ik}(t_j|y) = \max[-1, &\{I_k^{LB}(t_j|g_1, p_{t1}, y) - I_k^{UB}(t_j|g_0, p_{t1}, y)\} \\
&- \{I_k^{UB}(t_j|g_1, p_{t0}, y) - I_k^{LB}(t_j|g_0, p_{t0}, y)\}]
\end{aligned}
\qquad (8)
$$

and

$$
\begin{aligned}
u_{Ik}(t_j|y) = \min[1, &\{I_k^{UB}(t_j|g_1, p_{t1}, y) - I_k^{LB}(t_j|g_0, p_{t1}, y)\} \\
&- \{I_k^{LB}(t_j|g_1, p_{t0}, y) - I_k^{UB}(t_j|g_0, p_{t0}, y)\}]
\end{aligned}
\qquad (9)
$$

for $k = 1, \ldots, K - 1$. Note that the lower and upper bound are restricted to be between -1 and 1. This is due to the fact that the maximum variation of probabilities cannot be larger than 1 in absolute values. The reform effect is estimated by replacing the upper and lower bounds by consistent estimators as defined in (6).

From (3)-(4) it can be seen that the width of the bounds of the DID changes in (8)-(9) depends on $P(r \neq K|\delta = 1, g, p, y)$ and $P(\delta = 1|g, p, y)$. As these worst-case bounds can be wide, there are several approaches to tighten them. In addition to monotonicity or independence assumptions as in Lee and Wilke (2005) one could use economic reasoning to tighten the feasible interval for $P(r \neq K|\delta = 1, g, p, y)$ in (2). With this respect it is important to note that $P(r \neq K|\delta = 1, g, p, y)$ can be modelled as a function of time, while in (3) and (4) it was assumed to be constant. As an example, $P(r \neq K|\delta = 1, g, p, y)$ could be assumed to decrease with an increasing gap after ς. Another approach would be to increase the share of fully identified unemployment durations by determining an appropriate sample of unidentified

spells that can be excluded from the estimations without changing the estimates for the CICs in (3) and (4). Formally, this approach reduces $P(\delta = 1|g, p, y)$.

Another possibility is to assume that the conditional probability $P(r \neq K|\delta = 1, g, p, y)$ is independent of G and P, i.e. $P(r \neq K|\delta = 1, g, p, y) = P(r \neq K|\delta = 1, y)$ which only depends on other individual characteristics Y. The DID changes of the CIC from (7) can then be decomposed as follows:

$$\Delta_{Ik}^c(t_j|y) = \Delta_{Ik}(t_j, \delta = 0|y) + P(r \neq K|\delta = 1, y)\Delta_{Ik}(t_j, \delta = 1|y) \tag{10}$$

with the effect of the reform on the CIC for different values of δ defined as :

$$\begin{aligned}
\Delta_{Ik}(t_j, \delta = 0|y) &= I_k(t_j, \delta = 0|g_1, p_{t1}, y) - I_k(t_j, \delta = 0|g_0, p_{t1}, y) \\
&\quad - I_k(t_j, \delta = 0|g_1, p_{t0}, y) + I_k(t_j, \delta = 0|g_0, p_{t0}, y), \text{ and} \tag{11} \\
\Delta_{Ik}(t_j, \delta = 1|y) &= I_k(t_j, \delta = 1|r \neq K, g_1, p_{t1}, y) - I_k(t_j, \delta = 1|r \neq K, g_0, p_{t1}, y) \\
&\quad - I_k(t_j, \delta = 1|r \neq K, g_1, p_{t0}, y) + I_k(t_j, \delta = 1|r \neq K, g_0, p_{t0}, y). \tag{12}
\end{aligned}$$

In order to determine bounds we have to minimize and maximize (10) by assigning appropriate $P(r \neq K|\delta = 1, y)$ at each t_j: If $\Delta_{Ik}(t_j, \delta = 1|y) > 0$, set $P(r \neq K|\delta = 1, y) = 1$ and if $\Delta_{Ik}(t_j, \delta = 1|y) < 0$, set $P(r \neq K|\delta = 1, y) = 0$ to maximise (10). The minimum is attained in the reversed way. Thus the lower bound of $\Delta_{Ik}^c(t_j|y)$ is always smaller than the upper bound and the width of the bound is $|\Delta_{Ik}(t_j, \delta = 1|y)|$ which is tighter than the worst-case bound. Moreover, $P(r \neq K|\delta = 1, y)$ is now a function of t_j. Under the additional assumption, (11) and (12) are thus obtained by bounding the DID changes instead of bounding the CIC as in the case of the worst-case bounds in (8) and (9). A common difficulty of all approaches to tighten the bounds is to verify their validity in an application.

3   Empirical Application

We apply the above framework 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 sections.

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 (UNB) and unemployment assistance (UNA). 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 UNB 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 UNB, unemployed individuals receive the tax-funded unemployment assistance if they pass a means-test. Both UNB and UNA are a percentage of former wage income with UNB replacing 63% (68%) of former wage income and UNA still reaching income replacement rates of 53% (57%) for individuals without (with) dependent children. For individuals with low pre-unemployment wages, income replacement rates irrespective of the type of unemployment compensation may even be close to 100% because of receiving complementary social benefits if the unemployment compensation as a percentage of former wage income does not suffice to ensure the legally defined minimum standard of living. Thus, the effect of shortening the length of entitlements to unemployment benefits is not homogeneous. In particular, recipients of complementary social benefits are not affected by a change in the length of UNB receipt. By contrast, unemployed individuals without additional social benefits but with eligibility for the means-tested UNA loose around 10% of their former wage income when switching from UNB to UNA. For this group, a shortening of UNB is likely to have a small effect only. Individuals who do not pass the means test for receiving UNA due to other income sources or private savings even loose all unemployment compensation after exhausting UNB. 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 UNB for some of the older age groups.[3] In Germany, the potential UNB duration (PUNBD), i.e. the maximum duration of UNB 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 thus may be shorter than seven years. In addition, the PUNBD positively depends on age. During the 1980s, the PUNBD had successively been expanded for older age groups. Thus, before the reform in 1997, entitlements to UNB lasted up to 32 months for individuals above the age of 42, while the PUNBD for individuals below this age

---

[3]In addition, the introduction of stricter sanction rules for the non-compliance with eligibility requirements may have accelerated transitions from unemployment to employment for all age groups after 1997 (Boone et al., 2002, 2004).

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 UNB receipt allowed for bridging the gap between employment and retirement age (Fitzenberger and Wilke, 2004). In 1997, the PUNBD was reduced for some of the older age groups by lowering the age limits for certain maximum entitlement length (see Table 1). As a consequence, the PUNBD 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, UNB 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 UNB. The 1997 reform thus provides a natural experiment with a credible source of variation in PUNBD that can be used to identify its causal effect. As a drawback, however, 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.

Table 1: Potential unemployment benefit duration (PUNBD) for UNB claimants up to age 47 by work history and age, IAB-R01

Soc. insured employment	PUNBD (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 $\geq$42)	14 (age $\geq$45)
32 month	16 (age $\geq$42)	16 (age $\geq$45)
36 month	18 (age $\geq$42)	18 (age $\geq$45)
40 month	20 (age $\geq$44)	20 (age $\geq$47)
44 month	22 (age $\geq$44)	22 (age $\geq$47)

Source: Plaßmann (2002)

Two German studies already looked at the effect of the 1997 reform on transitions from unemployment to employment. Based on the German socio-economic panel (GSOEP), Wolff (2003) only finds very weak positive effects of shortening the PUNBD on the transitions to employment in eastern Germany. As 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) find evidence that the 1997 reform reduced the inflow into unemployment and drastically reduced the duration of unemployment among individuals above age 52, a result that suggests that a shortening of the maximum UNB receipt lowers the attractiveness of early retirement. Using the same administrative data set, we reexamine the effect of the PUBD on transitions to local and non-local employment of prime age individuals for whom early retirement should not be an issue.

Data: IAB-R01   The analysis is based on the IAB-R01, the IAB employment subsample 1975-2001 - regional file (Hamann et al., 2004). This administrative data set contains information on a 2 % sample of the population working in jobs that are subject to social insurance payments concerning spells of employment and spells for which the individual received unemployment compensation (UC) from the Federal Employment Agency (*Bundesagentur für Arbeit*) such as unemployment benefits (UNB), unemployment assistance (UNA) and maintenance payments during training measures (MP). These administrative records are provided as spells on daily basis. Individual employment trajectories are only partly identified as there are gaps which can correspond to a period of unemployment without the receipt of unemployment compensation or to other unobserved labor market states. This is why the true unemployment duration is often not observed (see also Lee and Wilke, 2005). In this data right-censoring occurs at the end of the observation period. An unemployment period belongs to the case $\delta = 0$ if there is a permanent receipt of income transfers. It requires an individual to receive UC within one month after the end of an employment period and intermediate gaps between the receipt of UC or the gap between the end of UC receipt and employment do not exceed one month. Otherwise an observation belongs to the case $\delta = 1$. In this case we observe ς which is the beginning of an unobserved period.

For all unemployment spells that exit to employment, the IAB-R01 allows for comparing the microcensus region of the old and the new workplace. 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 job, exits to a non-local job after migration and 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 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 years only.

In order to take account of the heterogeneous treatment effect that comes with the 1997 reform, we distinguish between two skill groups because the reform effect should be weaker for less-skilled [4] workers as they typically have a lower wage. For this reason they are more likely to receive complementary social benefits or pass the means test for the receipt of UNA than their high-skilled[5] counterparts. Besides the household context, the wage determines the eligibility for complementary social benefits. Unfortunately, the IAB-R01 does not include enough information to actually identify recipients of complementary social benefits. In our analysis a lower educational degree is therefore a proxy for the eligibility of complementary social benefits.

Choosing the treatment and control group   The aim of the analysis is to identify the effect of being eligible for an extended UNB duration on the transitions from unemployment to either a local job or a non-local job. Due to the reform in 1997, eligibility to an extended UNB duration of more than 12 month was cut for individuals aged 42-44 years, while the PUNBD 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 when comparing transitions to local and non-local employment before and after the reform. However, since only individuals with long UNB 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. This is because choosing the treatment and control group based on their actual UNB entitlements results in a non-comparability of

---

[4]Includes individuals who are either unskilled or have a vocational training and work as blue-collar workers.

[5]Includes individuals with a tertiary education or white-collar workers with at least a vocational training.

individuals in the control and treatment group with regard to their working history because the criterium to reach maximum entitlements, for instance, is less strict for the younger cohort (see Table 1). Therefore, a suitable 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. As a solution, we compute counterfactual UNB entitlements as the hypothetical UNB entitlement length in the absence of the 1997 reform had the individual been aged 42-44 at the time of benefit claim (see Appendix A for details). As can be seen in Table 2, the resulting counterfactual UNB 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 UNB duration exceeds 12 month. This selection rule thus ensures the comparability between the treatment and the control group and the existence of some minimum treatment for the treatment group. For the observable characteristics in the IAB-R01, Appendix B confirms that treatment and control group are quite comparable. Unfortunately, characteristics such as the marital status and dependent children which are likely to affect the likelihood of migration are missing in the data. The DiD approach in the subsequent analysis thus rests on the assumption that the composition of treatment and control group in the pre- and post-reform era are also comparable with respect to these unobserved characteristics. Moreover, the DiD approach assumes that both groups experience 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 in finding employment during economic downturn than their younger counterparts because of stricter employment protection. Since this is not the case for job seekers between 36 and 44, the improved labor market conditions in the post-reform era should boost the transition to employment for both groups to a comparable extent.

Table 2: Estimated counterfactual UNB entitlement length for unemployment spells in the pre- and post-reform era by age group[a], IAB-R01

UNB duration	Age 36-41		Age 42-44	
	# spells	%	# spells	%
≤ 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.

For the analysis, we restrict our sample to previous full-time employees 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. In addition, we restrict the analysis to individuals from western Germany because the working history for individuals from eastern Germany is not known before 1991 which aggravates the comparability of computed entitlement length. For the resulting control group and the post-reform treatment group the estimated actual entitlement length as shown in Table 3 that is subject to the 1997 reform and the true age of the individual is up to 12 month[6] only. In the pre-reform era, the treatment group is entitled to 18.5 month of UNB receipt on average, while in the post-reform era this average UNB duration falls to 11.8 month. This latter UNB receipt is almost exactly the UNB duration for the control group in the pre- and post-reform era. The average treatment thus is a reduction of UNB 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 UNB entitlements.

---

[6]For some individuals who do not fulfill the criterium for the maximum entitlement length, but still pass the selection criterium, the true UNB duration may be lower than 12 month.

Table 3: Estimated actual UNB entitlement length for unemployment spells with counterfactual UNB of >12 months in the pre- and post-reform era by treatment and control group, IAB-R01

	Control group		Treatment group	
UNB 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 4 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. Note also that median unemployment durations are shorter for all groups 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. Moreover, 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 4: 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

Bounds Analysis   We estimate bounds for the effect of the reform on the cumulative incidence of non-local and local re-employment for different skill groups by applying formulas (8)-(9). 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), also applied by Lee and Wilke (2005). The bootstrap repetitions are 500. Figure 3 shows that the partial identification problem of our interval data precludes any clear result pattern as none of the bounds cross the zero during the treatment period.

As another 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 C, 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 transition time definitions alone may be misleading.

Approaches to tighten the bounds   As indicated in the theoretical part there are several approaches to tighten the bounds. In a first attempt we assume monotonicity and independence as done by Lee and Wilke (2005). Bounds become tighter but for some time intervals they

Figure 3: Lower and upper bound of the DiD changes of the cumulative incidence of local (upper) and non-local (lower) exits to employment among high-skilled (left) and less-skilled (right) unemployed.
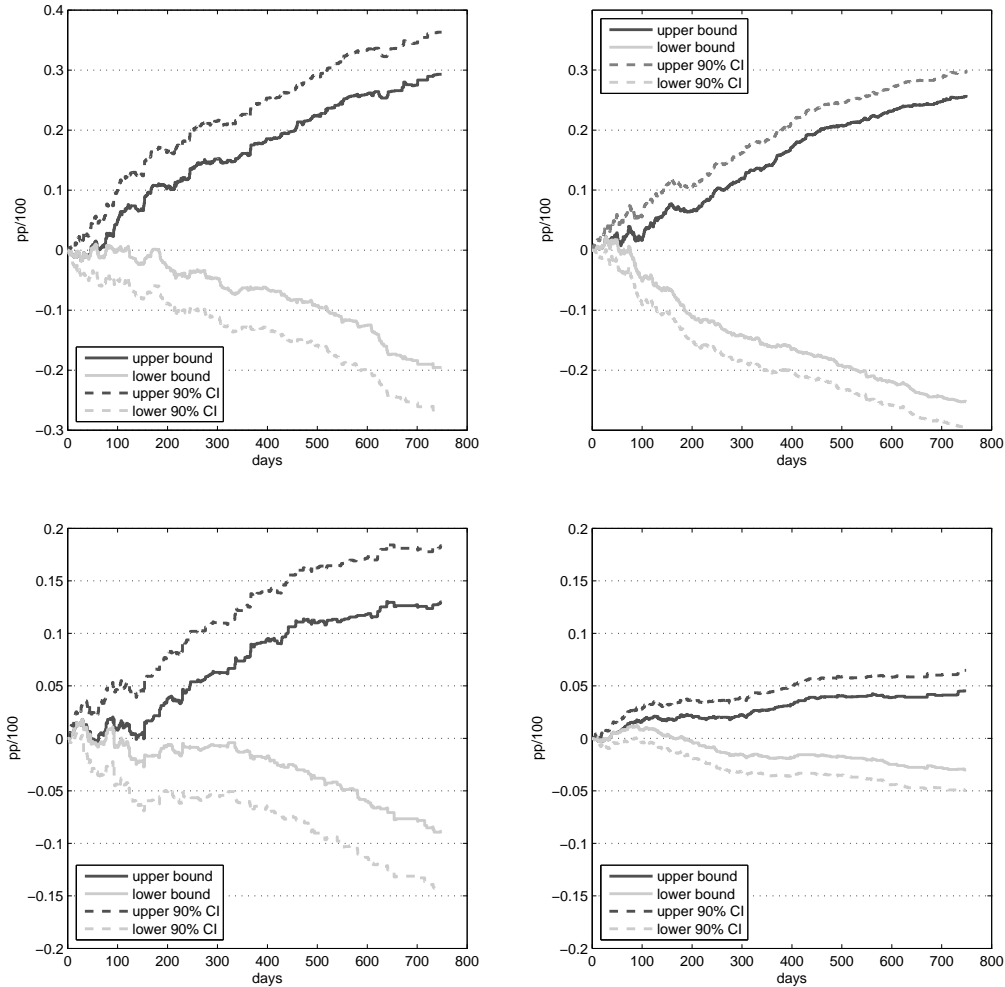
cross so that the assumptions do not seem to be valid in our empirical project. Another natural attempt to tighten the bounds is to restrict the interval for $P(r \neq K|\delta = 1)$. One could for example assume a shorter interval such as $[0.1, 0.9]$. Alternatively, one could also assume that $P(r \neq K|\delta = 1)$ decreases with the distance between ς and $s_{k \neq K}$, i.e. the length of the unobserved period. In other words, if the unobserved period is short, it is less likely that someone transits to an unobserved labor market state such as self-employment. As an example, one could assume that $P(r \neq K|\delta = 1)$ is an exponential density. We have estimated the bounds under several such scenarios. Resulting bounds are tighter and suggest that the change in the CICs is positive for the high-skilled group. Results are, however, not presented as it is difficult to prove the validity of these assumption.

Additional assumptions concerning $P(r \neq K|\delta = 1)$ are thus able to tighten the bounds but in our case they may appear arbitrary. Since the share of partly identified spells ($\delta = 1$) in our sample is about 25%, another worthwhile approach is to reduce it. As an exercise, we exclude observations without receiving unemployment compensation within one month after the end of employment, i.e. we exclude spells with $\delta = 1$ and $\varsigma = 0$. This is done as these spells are least informative in the sense that the interval $[\varsigma, s_{k \neq K}]$ is often large. The resulting bounds are again tighter and suggest again some weak increase in the CICs for the high skilled while there are no apparent changes for the low skilled. As discussed above, this approach is only valid if the exclusion of spells is a random sample in the sense that the sample composition does not change and the shape of the conditional CICs in (3) and (4) does not change. As the latter condition is not well supported by the data, we decided not to proceed in this way.

Figure 4: Lower and upper bounds of the DiD changes of the cumulative incidence of local (upper) and non-local (lower) exits to employment among high-skilled (left) and less-skilled (right) unemployed, additional assumption
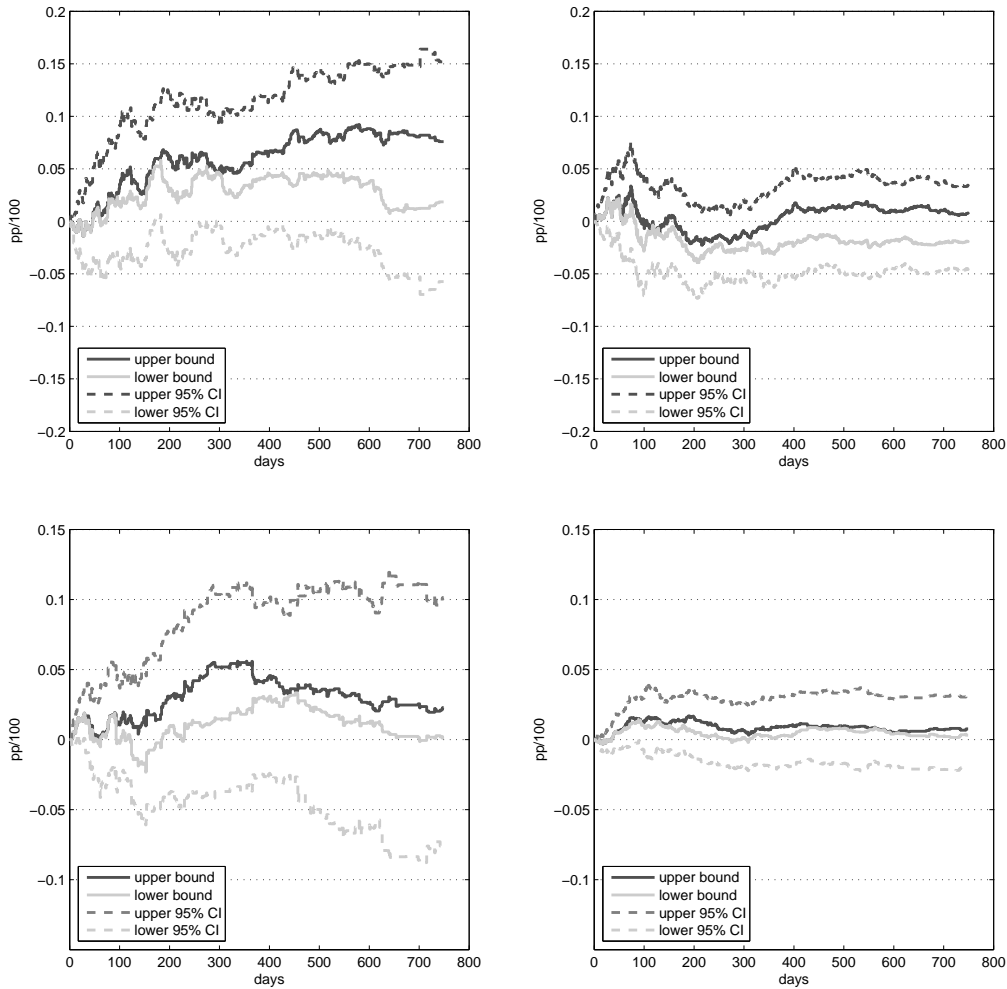
Our final attempt to tighten the bounds is therefore to impose the additional independence assumption $P(r \neq K | \delta = 1, g, p, y) = P(r \neq K | \delta = 1, y)$. The resulting bounds in Figure 4 are much tighter. They again suggest larger changes in observed exit probabilities for high-skilled job seekers for whom the threat of entitlement loss after exhausting UNB is likely to be larger. In particular, the figure shows that the observed post-reform probability of migration after one year of unemployment is 3-5pp 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 4-6pp for high-skilled individuals only. For high-skilled individuals, the corresponding relative change in the observed probability of migration is approximately 15-25% while the corresponding change for exits to local employment is around 10-15% only. If we would assume independent 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 UNB entitlements primarily fosters the willingness to migrate. In light of the institutional design in Germany, this finding is 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 UNB or UNA get financial support for search costs and moving costs. The negative effect of higher reservation wages in case of higher UNB receipt should thus likely exceed any resource effect. Also due to the missing statistical significance which is probably due to the limited sample size, however, all of 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. As another interesting observation, we generally observe a smooth variation of the bounds with the duration of unemployment. This does not suggest any remarkable jumps in the hazard rate or survivor function at the begin of the treatment. Our results therefore support the theoretical results of non-stationary job-search (van den Berg, 1990).

4   Conclusion

This paper has presented a nonparametric approach that allows for analyzing a competing risk model with partially identified interval data. Our bounds analysis is a highly relevant approach for applied researchers who face this data limitation. It extends the nonparametric bounds analysis by Lee and Wilke (2005) to a dependent competing risk setting and derives bounds for the risk-specific cumulative incidence curve. 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 risk-specific transition distribution. In particular, our approach is fully nonparametric and we avoid parametric assumptions on our duration model that may be violated in the real world. Moreover, we suggest several approaches to tighten the bounds in an application.

In our empirical application with German data, 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. Our results show that partial identification is a big problem in merged administrative individual data that may preclude any causal inference. Moreover, 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 the two point estimates alone may be misleading. By imposing additional assumptions we are able to tighten the bounds considerable. Without showing statistical significance, 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 UNB is likely to be largest. This indicates that extensive unemployment benefits mainly substitute for migration. The recent labor market reforms in Germany could therefore foster migration and accelerate exits to local employment.

The limitations of our approach point towards some interesting extensions. 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. In addition, the causal inference from our empirical results is limited because of the unresolved identification problem of the competing risks. 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. Moreover, a tightening of bounds could be achieved by estimating $P(r \neq K | \delta = 1)$ with additional information. This could be done for example if a sample of the administrative data would be merged with survey data that fully identifies the employment trajectories.

# Appendix A - Computation of actual and counterfactual UNB 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 UNB claim, but ends with a previous UNB 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, UNB 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 UNB. If there is a shortened claim period due to a previous UNB claim, the new UNB claim based on the employment periods after this last unemployment period may be extended up to the age-specific PUNBD by remaining entitlements at the end of the previous unemployment period if the beginning of the last UNB claim lies within the last seven years.

For the estimation of actual UNB entitlements all changing regulations throughout the 1980s and 1990s have been applied. For the counterfactual UNB entitlements, we apply the pre-reform conditions to the post-reform period and compute the UNB 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 UNB 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 UNB entitlements for the treatment group in the pre-reform era. As a consequence, the treatment group in the pre-reform period with counterfactual UNB entitlements of more than 12 month actually has entitlements of more than 12 month while all others who fulfil this criterium actually receive UNB for a maximum of 12 month only, but are comparable to the former group in terms of their employment history.

24

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 - Point estimates for lower and upper bound of treatment effect on the cumulative incidence of local(left) and non-local(right) exits to employment among high-skilled unemployed.
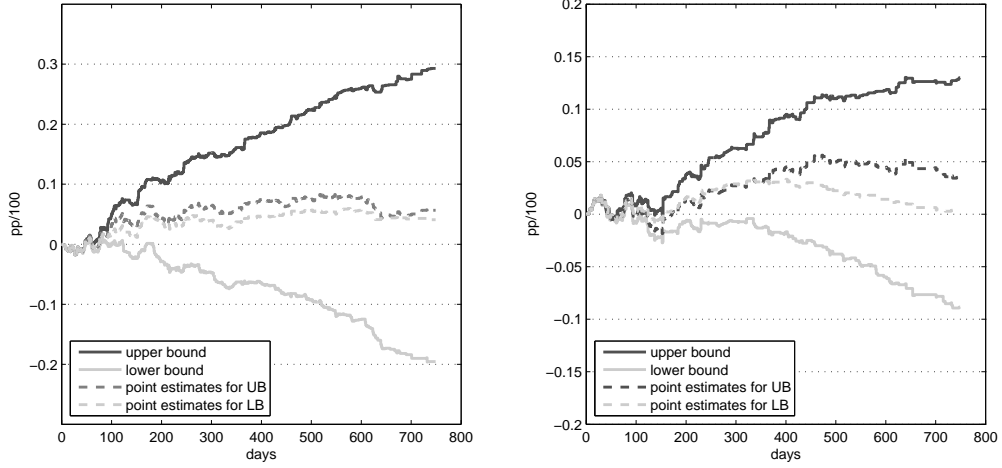
The point estimations for the treatment effect using the lower bound and upper bound of the employment duration data are done by using the following formulas:

$$l_{Ik}(t_j|p_{t0}, p_{t1}, x) = \{I_k^{LB}(t_j|1, p_{t1}, x) - I_k^{LB}(t_j|0, p_{t1}, x)\}$$
$$- \{I_k^{LB}(t_j|1, p_{t0}, x) - I_k^{LB}(t_j|0, p_{t0}, x)\}$$

and

$$u_{Ik}(t_j|p_{t0}, p_{t1}, x) = \{I_k^{UB}(t_j|1, p_{t1}, x) - I_k^{UB}(t_j|0, p_{t1}, x)\}$$
$$- \{I_k^{UB}(t_j|1, p_{t0}, x) - I_k^{UB}(t_j|0, p_{t0}, x)\}$$

for $k = 1, \ldots, K - 1$.

References

[1] Bundesanstalt für Arbeit (2001), IAB-Kurzbericht Nr. 1/2001, Nürnberg: Institut für Arbeitsmarkt- und Berufsforschung der Bundesanstalt für 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 (2007), Unemployment Duration in Germany: Individual and Regional Determinants of Local Job Finding, Migration and Subsidized Employment, *Regional Studies*, forthcoming.

[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 (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.

[14] 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.

[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, 65-86.

[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] 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.

[31] 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.

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

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

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

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

[36] 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.

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