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 2009

*We thank Simon Lee for helpful discussions and the participants of numerous seminars for helpful comments. 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”. We use a sample drawn from the Employee and Benefit Recipient History (V6.0) of the Institute of Employment Research (IAB). The data preparation and analyses were made possible by the Research Data Centre (FDZ) of the German Federal Employment Agency at the IAB because the project provides new insights on data quality. We thank selected staff members of the Research Data Centre (FDZ) of the German Federal Employment Agency at the IAB and especially Stefan Seth for the help with the data.

†ZEW Mannheim, E-mail: arntz@zew.de
‡Lingnan University, E-mail: simonlo@ln.edu.hk
§University of Nottingham, E-mail: ralf.wilke@nottingham.ac.uk
Abstract

By reexamining the effect of unemployment benefits on reemployment probabilities we make two contributions to the literature: First, we estimate separate effects for reemployment in the local or a distant region. Second, we address the problem of incomplete duration within a competing risks model. Our results confirm that missing data problems at first preclude any meaningful result even though we have access to daily individual data on 50% of the male workforce in Germany. When we impose additional assumptions, we obtain evidence that the treatment effect depends on the household context, the treatment intensity and the destination state.

Keywords: cumulative incidence curve, administrative data, difference-in-differences

JEL: C41, C14, J61

1 Introduction

Whether unemployment benefits promote or inhibit migration of unemployed workers is a highly debated policy question and economic theory is not conclusive in this respect. On the one hand, higher levels of unemployment benefits generate negative disincentive effects and reduce the geographical job search horizons (Hassler, Mora, Storesletten, and Zilibotti, 2005). On the other hand, the financial resources provided by unemployment benefits may enable individuals to bear the cost of migration which could enhance the willingness to accept a job offer that requires a move (Tatsiramos, 2008). Moreover, higher financial resources allow for additional expenditures to enhance the productiveness of job search (Barron and Mellow, 1979; Tannery, 1983). This theoretical disagreement has not yet been resolved by empirical studies. Studies estimating a binary choice model based on survey data find that unemployment benefits reduce the migration probability, see Goss and Paul (1990) for the US and Antolin and Bover (1997) for Spain. In order to take account of a possible duration dependence in the migration decision, Arntz (2005) and Arntz and Wilke (2009) extend these earlier approaches by employing duration models. Based on German administrative data they obtain similar evidence which suggests that the crude hazard rate of migration is reduced due to a dominant disincentive effect. The findings of these studies, however, might be driven by an unobserved selection of immobile individuals into unemployment benefits. Tatsiramos (2008) uses a binary choice panel data model to address both the issue of unobserved heterogeneity and duration dependence. He finds that the estimated conditional probability of migration (which is similar to a discrete-time crude hazard function of migration) is positively affected by unemployment benefits in Denmark and France, but no effect is found in the UK and Germany.

Motivated by this unsettled discussions, this paper reexamines this issue and adds several
contributions to this debate. First, we exploit a natural experiment in Germany that generates a credible exogenous variation of unemployment benefit receipt. In 1997, the maximum entitlement lengths for specific groups of registered unemployed were reduced by several months (later ‘the 1997 reform’). The 1997 reform essentially reversed a reform between 1984 and 1986 when entitlement lengths were extended. Both policy changes have already been subject to empirical investigations (Hunt, 1995; Plaßmann, 2002; Wolf, 2003; Fitzenberger and Wilke, 2010; Fitzenberger and Wilke, 2007; Müller, Wilke and Zahn, 2007; Lee and Wilke, 2009). However, this is the first paper which exploits this natural experiment to investigate the effect of unemployment compensation on migration and local reemployment separately while previous studies focus only on the overall duration of unemployment. Second, the results of previous studies may be affected by changes in the sample composition due to the reform. In order to control for possible sample selection issues, we construct a variable named counterfactual maximum length of UB entitlement which summarizes detailed long term information on the individual employment history. As this variable is similar to a proxy for the long term labor market behavior of an individual, its use is likely to resolve sample selection issues due to unobserved individual characteristics. Third, our analysis is based on very extensive daily administrative data encompassing 50% of the male working population in Germany, whereas previous studies using similar data have only access to 2% of the working population. The richness of this data is particularly important in the study of migration because the probability of migration is generally very small. Moreover, these large data allow us to perform a nonparametric analysis which avoids any parametric misspecification. In addition, we make use of the extensive data by not only estimating the average treatment effect, but by allowing for heterogeneous treatment effects.

As an additional methodological contribution, we develop a missing data method for multivariate competing risks models with interdependent states. Such a method is necessary because administrative data, despite being very large and detailed in many respects, also have important limitations. In particular, administrative data tend to be collected for a particular administrative purpose and thus often contain unobserved periods in an individual’s employment trajectory. Such unobserved periods frequently occur in register data from many countries and result in incomplete interval duration which causes major difficulties for the identification of model parameters. As an example, administrative individual data often contains claim periods for unemployment compensation while registered unemployment or job seeking is sometimes not available in the data. In this case, unemployed who are not eligible for unemployment compensation are not covered by the available data. This is highly relevant, for instance, for Spanish and British administrative data and for German data until 2000. Based on the newest generation of German merged administrative data, Kruppe et al. (2008), for example, implement various ILO definitions of unemployment
and obtain remarkable differences in the number of spells and lengths of unemployment periods. They observe that even though the data is very comprehensive, unobserved periods are still a major problem. Although methods to explicitly deal with these data limitations are still in its infancy, administrative data from many countries have been widely used by researchers in the last two decades (for short review see Angrist and Krueger, 1999). In order to improve applied research in this field, the development of missing data methods is thus of high relevance. As an example, Lee and Wilke (2009) deal with incomplete duration data by bounding a difference-in-differences (DiD) treatment effect on the overall survival probability for unemployment duration. However, they only derive the bounds framework in a univariate or single risk duration model with independent censoring.

In order to study the reform effect on different competing destination states such as employment in the local and a distant region, this paper presents a slight but crucial extension of Lee and Wilke (2009)’s bounds framework which covers multivariate competing risks models with possibly interdependent destination states. For this purpose, we derive bounds for the destination-specific cumulative incidence curve (CIC). CICs are useful for the analysis of competing risks models (Kalbfleisch and Prentice, 2002) and they are of prime interest in clinical researches (Kim, 2007). In contrast, hazard rate models are much more popular in applied economics and econometrics (see for example Kiefer, 1988). Although, these two approaches are mathematically equivalent, there are important differences in case of incomplete data. It is easy to see that the hazard rate by being a conditional probability is not a monotone transformation of its underlying duration variable. This hampers the derivation of meaningful bounds for the hazard function and the cumulative hazard function. In contrast, the CIC is a simple monotone probability function that is readily applicable to the bounds framework due to missing data problems. Hence, we perform a similar non-parametric DiD analysis as in Lee and Wilke (2009) to study the effect of unemployment benefits on the competing incidence rates of local job finding and migration. Note that the proposed bounds framework for the CIC can easily be carried over to parametric or semiparametric regression models for the CIC (Fine, 2001; Jeong and Fine, 2006; Klein, 2006).

In our empirical analysis we obtain the following main findings:

- We confirm that missing interval data is a highly relevant problem which, at first, precludes any unambiguous results. Bounds due to missing information are much wider than random sampling errors. Additional assumptions on the nature of the missing data are required to derive any meaningful results. By assuming some cross restrictions on the DiD parameter, bounds can be tightened to obtain significant results.

- There is strong evidence in favor of a heterogeneous effect of the reduction in unemployment benefits: The effect on the incidence of migration and local job finding hinges critically on the
household context and the wage replacement ratio. Interestingly, for certain groups we also observe different signs for the estimated effect at different durations. This demonstrates the usefulness of our flexible nonparametric approach and may explain why Tatsiramos (2008) does not find a significant average effect for Germany.

The paper is structured as follows. The following section presents the details of the 1997 reform. Section three presents the data structure, the econometric framework and the empirical results. Section four concludes.

2 The 1997 reform of unemployment benefits

2.1 Basic features of the unemployment compensation system

Until 2004, the unemployment compensation (UC) system in Germany consisted of two main components: unemployment benefits (UB) and unemployment assistance (UA). UB is funded by the unemployment insurance. Employed individuals may be entitled to UB in case of unemployment. Their potential UB duration (PUBD) is the entitlement length for UB at the beginning of an unemployment period. PUBD is determined by the employment history, i.e. the previous employment duration and the length of unemployment benefit claims within the last seven years (see Appendix A for details), and the age of the claimant. After exhausting the PUBD, unemployed individuals who pass a means-test are eligible for a tax-funded UA. Both UB and UA correspond to a fixed ratio of former wage income. If the level of UB or UA is too low to ensure the legally defined minimum standard of living, individuals may be eligible for complementary social benefits which are funded by communal administrations. While UB replaces 68% (63%) of former wage income and UA has an income replacement rates of 57% (53%) for individuals with (without) dependent children. This implies that any kind of reduction in the PUBD will lead to a ceteris paribus reduction of the total expected present value of UC during the unemployment period as long as the individual is not eligible for complementary social benefits.

2.2 The natural experiment, the control and treatment groups

In April 1997, a reform of the Employment Promotion Act (Arbeitsförderungsgesetz) came into force to shorten the PUBD for the aged 42 and above, while the wage replacement ratio of UB remained unchanged. The reduction in PUBD depends on the age and the employment history as is summarized in Table 1. For instance, an individual aged 42 with a total employment duration of 28 months within the last seven years had a PUBD of 14 months which was reduced to 12 months after the reform. In contrast, an individual aged 42 with 20 month of prior employment had a
PUBD of 12 months before and after 1997 and was unaffected by the reform. The 1997 reform thus provides a natural experiment with a credible source of exogenous variations in PUBD that can be used to identify its causal effect. According to the reform design, it would be natural to select those aged 42 or above and with PUBD longer than 12 months as the treatment group; the control group would consist of all remaining individuals. However, the validity of this evaluation design could be compromised for the following reasons:

First, policy changes in 1994 that introduced stricter sanction rules may confound the effects of the 1997 reform. Similar confounding effects may be due to the fact that the implementation of the 1997 reform was partially cushioned by transitory regulations until March 1999. New benefit claimants were subject to pre-reform regulations if they had an employment duration of more than one year within the last three years. Thus, the new regulations did not apply to new benefit claimants before March 1999. We address these issues by a suitable choice of pre- and post-reform periods: We consider unemployment spells starting between 1995 and 1996 as pre-reform spells to exclude the influence of earlier interventions; the post-reform sample are those unemployment spells starting between 1999 and 2000 to avoid the complications caused by the transitory regulations. By allowing a gap between the end of 1996 and the implementation month in April 1997, the choice of the pre-reform period may reduce a potential anticipation effect between the announcement and the actual implementation of the reform.

Second, labor market outcomes before and after the reform may also change due to macroeconomic developments. In addition, stricter monitoring and sanction rules for non-compliance with eligibility requirements were introduced along with the 1997 reform (in addition to that in 1994). These policy changes may accelerate transitions from unemployment to employment because temporary reductions in the UB due to non-compliance with eligibility rules have been found as an effective means of reducing unemployment (Boone, Fredriksson, Holmlund, and van Ours, 2002; Boone, Sadrieh and van Ours, 2004). Since these new regulations were applied to all unemployed (i.e., the treatment and control group), the use of a difference-in-differences (DiD) estimator eliminates both a macroeconomic time trend as well as the effect of stricter sanction rules when assuming that both treatment and control group experience the same time trends. However, the disincentive effect of stricter sanctions is likely to be different for individuals with different PUBDs and thus for the treatment and control group. If this is the case, this disincentive effect might confound the effect of the reduction of PUBDs. We take account of this potential bias by conditioning our estimates on the PUBD. Although the PUBD is not available in the data, there are well documented rules describing how it can be computed based on the individual employment history (e.g., the Employment Promotion Act (Arbeitsförderungsgesetz) and the Social Welfare Act III (Sozialgesetzbuch III). The construction of PUBD is a rather laborious work (see
Annex A for details), but it contains valuable information. It not only controls the potential bias caused by the interactions of different PUBDs and stricter sanction rules, it also serves as a key variable to define the control and treatment group. The construction of PUBD effectively avoids a potential bias towards zero of the estimated reform effect that may be present in Lee and Wilke (2009). By not computing the PUBD, they tend to include unemployed in the treatment group who have not been affected by the reform.

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

<table>
<thead>
<tr>
<th>Duration of socially insured employment during the (extended) claim period</th>
<th>PUBD (in months)</th>
<th>before 03/97</th>
<th>after 04/97</th>
</tr>
</thead>
<tbody>
<tr>
<td>12 month</td>
<td>6</td>
<td>6</td>
<td></td>
</tr>
<tr>
<td>16 month</td>
<td>8</td>
<td>8</td>
<td></td>
</tr>
<tr>
<td>20 month</td>
<td>10</td>
<td>10</td>
<td></td>
</tr>
<tr>
<td>24 month</td>
<td>12</td>
<td>12</td>
<td></td>
</tr>
<tr>
<td>28 month</td>
<td>14 (age ≥42)</td>
<td>14 (age ≥45)</td>
<td></td>
</tr>
<tr>
<td>32 month</td>
<td>16 (age ≥42)</td>
<td>16 (age ≥45)</td>
<td></td>
</tr>
<tr>
<td>36 month</td>
<td>18 (age ≥42)</td>
<td>18 (age ≥45)</td>
<td></td>
</tr>
<tr>
<td>40 month</td>
<td>20 (age ≥44)</td>
<td>20 (age ≥47)</td>
<td></td>
</tr>
<tr>
<td>44 month</td>
<td>22 (age ≥44)</td>
<td>22 (age ≥47)</td>
<td></td>
</tr>
</tbody>
</table>

Source: Plaßmann (2002)

Third, an important conclusion from previous studies is that reforms in PUBDs have strong impacts on the incidence of unemployment (Fitzenberger and Wilke, 2010). Regarding the 1997 reform, Müller et al. (2007) confirm that for those aged 52 or above both unemployment duration and the inflow into unemployment were reduced by the reform. They reason that the main effect of PUBDs’ reductions is the weakening of the attractiveness of early retirements through the unemployment compensation system. In light of these results, it appears plausible that the reform altered the inflow into unemployment in the post- compared to the pre-reform period. By the same token, the stricter monitoring and sanctions rules introduced in 1997 might have had similar impacts on the compositions of the samples. In order to mitigate this selection on unobservables, we create a variable named counterfactual PUBD which is the hypothetical entitlement length a claimant would receive if he was treated as an individual aged 42-44 according to the pre-reform regulations. By applying the same regulations to every claimant, differences in this measure are only due to the individual heterogeneity in previous labour market decisions rather than in the
regulations. It thus summarizes an individual’s employment history into a single variable for a
given policy regime (see Appendix A for details) and may thus serve as a proxy for unobserved
individual heterogeneity. Since we are taking into account a rather long interval of the previous
employment history (seven years), the effect of the 1997 reform on this variable is negligible. The
PUBD is therefore independent of the reform and we can use it to alleviate a potential selection
bias.

In particular, we use this measure to select a comparable control and treatment group. Following
the previous reasonings we cannot simply select the control and treatment group based on
age and PUBD as it does not ensure comparability of previous working history and the implied
unobservable heterogeneity. We illustrate this with the following example. If we choose individ-
uals older than 42 with PUBD longer than 12 months to belong to the treatment group while
leaving all other unemployed just below the age of 42 in the control group, individuals in the treat-
ment group are more likely to have longer previous employment durations and shorter previous
UC claims than individuals in the control group. This is because, being in the treatment group
requires an employment duration of more than 28 months, while individuals in the control group
would have shorter employment durations on average (see Table 1). This creates an imbalance
which can be considered as a non-common support of the distributions of the employment history.
For this reason, we include only unemployed individuals with a counterfactual PUBD that exceeds
12 months. By applying the same selection criterion irrespective of whether someone belongs to
the treatment or control group and irrespective of whether an individual is observed in the pre-
or post-reform period, this approach mitigates imbalances in the distributions of the employment
history and ensures a common support. Using similar data and adopting a similar identification
strategy, Lee and Wilke (2009) did not address this potential issue.

In addition to the employment history, we further restrict our samples based on various criteria
to ensure the comparability of the treatment and control group in the pre- and post-reform period.
We construct our estimation samples as follows:

- Men with a previous full-time job;
- Job location in western Germany since employment histories prior to 1991 are not observable
  for individuals from eastern Germany;
- Men aged 36 to 41 (control group) and men aged 42 to 44 (treatment group); this ensures
  a similar labour force attachment and avoids complications due to early retirements;
- Unemployment spells starting between 1995 and 1996 belong to the pre-reform period, spells
  starting between 1999 and 2000 to the post-reform period; and
2.3 Data and descriptive statistics

We use a sample drawn from the Employee and Benefit Recipient History (V6.0) of the Institute of Employment Research (IAB) which comprises 50% of the male working population. The data was prepared by the IAB to have the same structure as the IAB employment subsample 1975-2001 - regional file which is a 2% sample and available as a scientific use file (Hamann et al., 2004). As the access to the 50% sample is restricted, we did all the preliminary work and some sensitivity analysis with the 2% sample and switched to the 50% sample for the final estimations only.

For the sample chosen according to the above criteria, Table 2 confirms that the distribution of PUBD is very similar for the two age groups. Moreover, note that by using the pre-reform regulations that apply to individuals aged 42 to 44 as the basis of defining the counterfactual PUBD, our selection criteria ensure that unemployed who belong to the control group or the pre-reform treatment group have an actual PUBD of 12 or less months while those who belong to the post-reform treatment group have an actual PUBD of more than 12 months (compare Table 3). Note that not all individuals in our samples have the maximum age-specific PUBD although they fulfill the selection criteria based on the counterfactual PUBD. In the pre-reform period, the treatment group is entitled to 18.5 months PUBD, on average, whereas in the post-reform period the average PUBD of those aged 42-44 falls to 11.8 months which is the same as in the control group both before and after the reform. The 1997 reform therefore induced an average PUBD reduction of 6.7 months in our sample, whereas the reduction on individual level ranges from one month to a maximum of ten months.

Table 6 in Appendix B shows the descriptive statistics for the observable characteristic of the estimation samples. While the distribution for most of the variables are very similar in the four samples, there are small but notable differences in the skill level and marital status. Since we estimate the reform effect also by stratifying the sample with respect to these variables, we render this slight imbalance irrelevant.
Table 2: Constructed counterfactual PUBD for unemployment spells in the pre- and post-reform period by age groups, IAB data, partially restricted sample

<table>
<thead>
<tr>
<th>Counterfactual PUBD</th>
<th>Age 36-41</th>
<th></th>
<th>Age 42-44</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td># spells</td>
<td>%</td>
<td># spells</td>
<td>%</td>
</tr>
<tr>
<td>≤ 2 months</td>
<td>24,469</td>
<td>7.7</td>
<td>10,131</td>
<td>8.2</td>
</tr>
<tr>
<td>3-4 months</td>
<td>18,340</td>
<td>5.7</td>
<td>7,289</td>
<td>5.9</td>
</tr>
<tr>
<td>5-6 months</td>
<td>19,133</td>
<td>6.0</td>
<td>7,706</td>
<td>6.2</td>
</tr>
<tr>
<td>7-8 months</td>
<td>19,659</td>
<td>6.2</td>
<td>7,783</td>
<td>6.3</td>
</tr>
<tr>
<td>9-10 months</td>
<td>20,556</td>
<td>6.4</td>
<td>8,079</td>
<td>6.5</td>
</tr>
<tr>
<td>11-12 months</td>
<td>18,354</td>
<td>5.8</td>
<td>6,969</td>
<td>5.6</td>
</tr>
<tr>
<td>13-14 months</td>
<td>18,748</td>
<td>5.9</td>
<td>7,091</td>
<td>5.7</td>
</tr>
<tr>
<td>15-16 months</td>
<td>18,824</td>
<td>5.9</td>
<td>6,920</td>
<td>5.6</td>
</tr>
<tr>
<td>17-18 months</td>
<td>161,045</td>
<td>50.4</td>
<td>61,592</td>
<td>49.9</td>
</tr>
<tr>
<td>Total</td>
<td>319,128</td>
<td>100.0</td>
<td>123,560</td>
<td>100.0</td>
</tr>
</tbody>
</table>

* Included are previously full-time employed male born in West Germany.

Table 3: Constructed PUBD with counterfactual PUNB >12 months in the pre- and post-reform period by treatment and control group in pre- and post-reform period, IAB data, final sample

<table>
<thead>
<tr>
<th>PUBD</th>
<th>Control group</th>
<th></th>
<th>Treatment group</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>6-8 months</td>
<td>2.2%</td>
<td>1.6%</td>
<td>-</td>
<td>1.6%</td>
</tr>
<tr>
<td>9-11 months</td>
<td>7.2%</td>
<td>5.6%</td>
<td>-</td>
<td>5.3%</td>
</tr>
<tr>
<td>12 months</td>
<td>90.6%</td>
<td>92.8%</td>
<td>-</td>
<td>93.1%</td>
</tr>
<tr>
<td>13-14 months</td>
<td>-</td>
<td>-</td>
<td>6.9%</td>
<td>-</td>
</tr>
<tr>
<td>15-16 months</td>
<td>-</td>
<td>-</td>
<td>7.5%</td>
<td>-</td>
</tr>
<tr>
<td>17-18 months</td>
<td>-</td>
<td>-</td>
<td>58.6%</td>
<td>-</td>
</tr>
<tr>
<td>19-20 months</td>
<td>-</td>
<td>-</td>
<td>2.6%</td>
<td>-</td>
</tr>
<tr>
<td>21-22 months</td>
<td>-</td>
<td>-</td>
<td>23.3%</td>
<td>-</td>
</tr>
<tr>
<td>Average months</td>
<td>11.8</td>
<td>11.8</td>
<td>18.5</td>
<td>11.8</td>
</tr>
<tr>
<td>Total spells</td>
<td>104,069</td>
<td>94,309</td>
<td>39,434</td>
<td>36,104</td>
</tr>
</tbody>
</table>
2.4 Heterogeneous reform effects

A major reason for expecting heterogeneous rather than homogeneous reform effects is that the treatment itself is heterogeneous depending on the financial loss induced by exhausting UB. If individuals are eligible for complementary social benefits due to low pre-unemployment wages and little sources of non-labor income, the income replacement rate can even exceed 100% and is independent of whether receiving UB or UA since a higher level of social benefits compensates for the loss in UA compared to UB. In this case, the reform effect is expected to be weak or even zero for recipients of complementary social benefits. In contrast, the strongest reform effect can be expected for those who are not eligible for the means-tested UA due to having other income sources, while for those receiving both UA and UB without complementary social benefits, the loss due to exhausting UB amounts to a change in the wage replacement ratio from 68% (63%) to 57% (53%) for individuals with (without) dependent children. The design of the German unemployment compensation system thus results in a heterogeneous treatment which is likely to affect the strength of the reform effect. Hence, the German unemployment compensation system results in a heterogeneous treatment of the 1997 reform.

Unfortunately, the IAB data does not contain the household information necessary to identify individuals who are eligible for complementary social benefits. In our analysis, we use the skill level as a proxy for the earning capacities that affects the probability of receiving social benefits in addition to unemployment compensation. We define the high-skilled group as individuals having either a tertiary education or a qualification of master craftsmen, while the remaining individuals belong to the less-skilled group. When we repeat the empirical analysis for different wage levels (similar to Lee and Wilke, 2009) we obtain the same result patterns.

As the willingness of individuals to migrate is likely determined within the household context, we expect heterogeneous treatment effects with respect to the household composition. Being married and having dependent children have typically been found to have higher migration costs (see Ghatak, Levine and Price, 1996). The reduction of PUBD is less likely to provoke a higher incidence rate of migration within this group of households than the singles with or without dependent children. As the data do not include reliable information on dependent children, we can merely distinguish the household composition by the marital status. In the following empirical analysis we investigate heterogeneous treatment effects by stratifying our sample into four groups: high-skilled or less-skilled and singles or married individuals. Based on our previous considerations derived from economic theory, we expect high-skilled individuals to react more strongly to a cut in entitlements in either direction: High-skilled singles are more likely to migrate than the married counterparts, while married high-skilled individuals are more likely than the single to find a local job.
3 Incomplete duration data and bounds analysis

The IAB data consist of administrative records which are provided as spells with a daily start and end date. These records include employment spells for jobs subject to social insurance payments as well as spells of receiving unemployment compensation (Ub or UA) from the Federal Employment Agency (Bundesagentur für Arbeit). However, as discussed in the introduction, the record for each individual is incomplete, i.e. there are gaps between the observable spells for which no information is available on the actual labor market state. Gaps occur whenever an individual is unemployed without receiving unemployment compensation or whenever the individual is in one of several unrecorded labour market states such as self-employment, being a civil servant, or being out of labor force. As a result of this incomplete data structure, the unemployment duration is not fully observable for each individual.

The methodological framework thus needs to address this missing data problem by taking into account that effects on the unemployment duration can only be bounded. The existing framework of Lee and Wilke (2009) for bounding effects in case of such incomplete data is a useful tool for univariate or single risk duration models. Our analysis, however, requires a multivariate competing risks model with possibly inter-dependent competing risks, i.e. a model that allows for different destination states after the unemployment period. In particular, we want to distinguish reemployment in a distant region which necessitates migration (‘distant employment’) and reemployment in the local area (‘local employment’). In order to capture all remaining unknown labor market states, we further distinguish a third unknown labor market state (‘unknown state’). We can distinguish between distant and local employment in the IAB data by comparing the location of the old and the new workplace at the level of the 440 German counties. In this paper we assume that a transition to a distant employment occurs if the distance between the county capitals of the old and the new workplace exceeds 100 km, a distance that should necessitate residential relocation in most cases.1 Similarly, there is a transition to local employment if the distance between the two workplaces is less than 100km.

3.1 Constructing bounds for unemployment duration

In order to implement the bounds framework for our dependent competing risk setting, we first need to define which unemployment spells can be considered fully observable and which need to be treated as incomplete. An unemployment duration is fully observed if it is immediately preceded and followed by an employment spell, and the individual receives either UB or UA without

---

1German labor market regions that minimize external commuting linkages, have a radius of about 75 km on average.
interruptions throughout the whole unemployment period. While the administrative record in this case provides full information about all labor market states, there are many unemployment spells that are interrupted by very short gaps of less than one month. Since such short gaps need not reflect a new labor market state, but may simply point towards frictions when moving from employment to unemployment or back. Similarly, short interruptions of the receipt of unemployment compensation are unlikely to point towards a new labour market state since unemployed with compensation entitlements lose social security protection (such as health insurance) if they do not claim benefits for more than a month. We therefore relax the definition of a fully observed unemployment period as follows (see Figure 1): 1. The gap between the preceding employment spell and the start date of UC (UB or UA) must not exceed one month; 2. A gap between two subsequent UC claim spells must not exceed one month; 3. The gap between the last UC claim spell and the subsequent new employment spell must not exceed one month. The fully observed unemployment duration either ends with local or distant employment.

Figure 1: An observed unemployment duration

\[ E \leq 1m \leq UC \leq 1m \leq UC \leq 1m \leq E \text{ or } D \]

\[ Lb \equiv Ub \]

Note:  \( E \): Spell of local employment; \( D \): Spell of distant employment; \( UC \): Spell receiving UC; \( m \): Month

If an unemployment spell does not meet all of the criteria listed above, we consider it as not fully observed. See Figure 2 for three typical examples. It illustrates important cases which occur in the data, although the top case which refers to a large gap between UC spell and subsequent employment is predominant in our data. There are several reasons why an individual stops claiming UC. For example, UB entitlements may have been exhausted and the individual does not pass a means-test for UA. Other reasons are benefit sanctions for unemployed or transitions to unobserved labor market states which do not correspond to unemployment. But in any case it is practically impossible to infer the actual reason from the available data. This is why we can only determine a lower and an upper bound for the unemployment duration. Our duration bounds explicitly address the issue of the ambiguous destination state in addition to the partial observation of the length.

Both the lower bound \( (Lb) \) and the upper bound \( (Ub) \) of an unemployment duration start at the end of the employment spell preceding the receipt of UC. The \( Lb \) unemployment spell stops once there is a gap longer than 1 month (or simply ‘large gap’) between the following individual
Figure 2: Examples of $Lb$ and $Ub$ definitions of unobserved unemployment durations

Note: $E$: Spell of local employment; $D$: Spell of distant employment; $UC$: Spell receiving $UC$; $m$: Month
$\varsigma_1$: Time at which unobserved period begins; $\varsigma_2$: Time at which unobserved period ends

records. In this case $Lb$ assumes that the individual enters an unknown labor market state at this point of time which is denoted as $\varsigma_1$ in Figure 2. The $Lb$ spell therefore ends with an exit to an unknown destination state. By contrast, the upper bound of an unemployment duration assumes continued unemployment at $\varsigma_1$. Thus, irrespective of the length of the gap in the record, $Ub$ never assumes a transition into an unknown state. The $Ub$ unemployment spell therefore ends at $\varsigma_2$ when the next observable employment spell starts. The $Ub$ spell therefore always ends with either a transition to local or distant employment, while $Lb$ spells always end with an unknown labor market state if the unemployment duration is not fully observed (Figure 2). In contrast, $Lb$ and $Ub$ have the same length and record the same observable transition to local or distant employment in case of a fully observable record as shown in Figure 1. Transitions from unemployment to an unknown labor market state therefore occur when applying the $Lb$ definition to an incomplete record only (and never in the $Ub$ definition). Owing to the fact that $Lb$ is always shorter than or equal to $Ub$, the $Lb$ and $Ub$ of the unemployment duration are bounds for the unobserved true unemployment duration. Note that these bounds would be reduced to point estimates if all observations were fully observable as shown in Figure 1.
Table 4: Compositions of unemployment durations with different destination states under $Lb$ and $Ub$ definitions, IAB data, final sample

<table>
<thead>
<tr>
<th></th>
<th>Control group</th>
<th></th>
<th>Treatment group</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>$Lb$ unemployment spells</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ends with local employment</td>
<td>50.1%</td>
<td>50.8%</td>
<td>48.1%</td>
<td>49.4%</td>
</tr>
<tr>
<td>ends with distant employment</td>
<td>8.7%</td>
<td>10.4%</td>
<td>8.9%</td>
<td>10.2%</td>
</tr>
<tr>
<td>ends with unknown state</td>
<td>41.2%</td>
<td>38.8%</td>
<td>43.0%</td>
<td>40.4%</td>
</tr>
<tr>
<td>right-censored</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>total</td>
<td>100.0%</td>
<td>100.0%</td>
<td>100.0%</td>
<td>100.0%</td>
</tr>
<tr>
<td>$Ub$ unemployment spells</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ends with local employment</td>
<td>75.2%</td>
<td>71.1%</td>
<td>72.5%</td>
<td>69.6%</td>
</tr>
<tr>
<td>ends with distant employment</td>
<td>14.1%</td>
<td>15.2%</td>
<td>14.2%</td>
<td>14.7%</td>
</tr>
<tr>
<td>ends with unknown state</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>right-censored</td>
<td>10.7%</td>
<td>13.7%</td>
<td>13.3%</td>
<td>15.7%</td>
</tr>
<tr>
<td>total</td>
<td>100.0%</td>
<td>100.0%</td>
<td>100.0%</td>
<td>100.0%</td>
</tr>
<tr>
<td>Total spells</td>
<td>104,069</td>
<td>94,309</td>
<td>39,434</td>
<td>36,104</td>
</tr>
</tbody>
</table>

It is also important to mention that all spells are independently right censored at the end of the observation period (which is at the end of 2005 for the 50% sample and at the end of 2001 for the 2% sample). Some descriptive summaries of $Lb$ and $Ub$ spells in our final sample are presented in Tables 4 and 5. Table 4 shows that under the $Lb$ definition about 40% of all unemployment spells end with a transition to an unknown state, which is also the percentage of incompletely observed unemployment durations. This highlights the importance of the missing data problem for our empirical analysis. The share of distant employment ranges from just 9% to 15% for all groups which suggests that a reliable statistical analysis of this exit state requires large data. While the $Lb$ spells with an unknown destination state represent about 20-25%(5%) of the total exits to local (distant) employment in the $Ub$ spell sample, the $Lb$ spells ending with local (distant) employment represent about 50% (9-10%) of the total spells. This observation suggests that missing data is a relevant problem for both of the two exit states and represents roughly the same proportions (i.e. 1/2) of spells without missing data problem. Moreover, the shares of different destination states are rather invariant across the control/treatment groups in pre-/after-reform period.

Table 5 shows the median unemployment duration by skill group and marital status. Note that the difference between the median duration under the $Lb$ and $Ub$ definitions ranges from 50 to 100
days which is a very substantial variation. This highlights again the importance of the missing data problem for our empirical analysis. Table 5 suggests some evidence for a positive reform effect for high-skilled singles as the gap in median unemployment duration between the treatment and control group narrows after the reform. Note also that we do not obtain any evidence for an effect of the reform if we simply consider the overall median duration. This may be suggestive that it is important to run separate estimations for different sub samples.

Table 5: Median unemployment duration (days) by sub-sample and definition of unemployment, IAB data, final sample

<table>
<thead>
<tr>
<th></th>
<th>Control group</th>
<th>Treatment group</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>pre-1997</td>
<td>post-1997</td>
</tr>
<tr>
<td></td>
<td>post-1997</td>
<td>pre-1997</td>
</tr>
<tr>
<td>Lb unemployment spells</td>
<td></td>
<td></td>
</tr>
<tr>
<td>high-skilled singles</td>
<td>274</td>
<td>178</td>
</tr>
<tr>
<td>less-skilled singles</td>
<td>243</td>
<td>183</td>
</tr>
<tr>
<td>high-skilled married men</td>
<td>184</td>
<td>123</td>
</tr>
<tr>
<td>less-skilled married men</td>
<td>159</td>
<td>134</td>
</tr>
<tr>
<td>overall:</td>
<td>185</td>
<td>152</td>
</tr>
<tr>
<td>Ub unemployment spells</td>
<td></td>
<td></td>
</tr>
<tr>
<td>high-skilled singles</td>
<td>488</td>
<td>278</td>
</tr>
<tr>
<td>less-skilled singles</td>
<td>336</td>
<td>243</td>
</tr>
<tr>
<td>high-skilled married men</td>
<td>367</td>
<td>246</td>
</tr>
<tr>
<td>less-skilled married men</td>
<td>211</td>
<td>177</td>
</tr>
<tr>
<td>overall:</td>
<td>258</td>
<td>204</td>
</tr>
</tbody>
</table>

3.2 Bounds analysis of competing risks model

Let us now formalize the framework in terms of a competing risks model with incomplete duration data. Based on a standard latent failure model with $k$ different destination states, we define the latent unemployment duration as $T_k$, with destination state as local employment ($k = E$), distant employment ($k = D$), or unknown state ($k = U$). As mentioned above, the administrative record only provides information on transitions to local or distant employment while information on the unknown state is not recorded. In our empirical application we have independent right censoring at the end of the observation period, but we ignore it for now to keep the notation simple. As
assumed in the previous section and confirmed by the summary statistics, the four sub-samples which are the control \((G = g_0)\) and treatment groups \((G = g_1)\) in the pre-reform \((P = p_{t0})\) and post-reform period \((P = p_{t1})\), need to be comparable in terms of observable and unobservable variables \((Z)\) if our analysis wants to identify the reform effect. We thus assume that the only source of variation between the four groups is due to the two selection variables: age and PUBD. We further assume that \(T_E\) and \(T_D\) are unknown functions of \(X' = [G', P', Z']'\).

Let us denote \(r\) as the destination state of the shortest latent duration, i.e. \(T_r = \min_k\{T_k\}\), and \(\delta = \mathbb{I}\{\text{unemployment duration is not fully observed}\}\). We assume for simplicity that there are no ties in the latent durations for each individual. As discussed above, an unemployment duration \((T_k)\) is not fully observed whenever there is a large gap between two administrative records. In presence of an unobserved interval \([\varsigma_1, \varsigma_2)\) (see Figure 2), we have to distinguish between two potential cases: (1) continuous unemployment despite long interruptions until the unemployment ends at \(\varsigma_2\) with an exit to either local employment \((r = E)\) or distant employment \((r = D)\). In this case the latent duration variable \(T_E\) or \(T_D\) respectively is observed and it is \(\varsigma_2\); or (2) terminating the unemployment period by entering an unknown state \((r = U)\) at some time between \([\varsigma_1, \varsigma_2)\). In this case the latent duration variable \(T_U\) is unobserved but somewhere in \([\varsigma_1, \varsigma_2)\). When an employment duration is unobserved \((\delta = 1)\), the \(Lb\) definition of this unemployment duration thus takes the value of \(r = U\) and \(T^{Lb} = \varsigma_1\), while the \(Ub\) definition of this unemployment duration has the length of \(T^{Ub} = \varsigma_2\) and the new labor market state is determined by the subsequent employment spell, which may either be \(r = E\) or \(r = D\).

We are now able to determine bounds for the identification region of the risk-specific cumulative incidence curves (CICs). Equivalent bounds cannot be determined for cause-specific crude hazard function or the cause-specific cumulative hazard function as they are non-monotone transformations of probability functions. It is straightforward, however, to construct bounds for the overall survival curve for unemployment duration \(T\), provided that we observe only the lower \((T^{Lb})\) and the upper bound \((T^{Ub})\) of \(T\):

\[
T^{Lb} \leq T \leq T^{Ub} \\
\Leftrightarrow P(T^{Lb} \geq t) \leq P(T \geq t) \leq P(T^{Ub} \geq t) \\
\Leftrightarrow S^{Lb}(t) \leq S(t) \leq S^{Ub}(t). \tag{1}
\]

Based on a univariate model studied by Fitzenberger and Wilke (2010), Lee and Wilke (2009) use these bounds to determine the identification region of the survival curve in presence of partially observed durations. The equivalent construction of bounds for risk-specific durations, however, is hampered by the fact that the lower bound for \(T_D\) and \(T_E\) is not defined except in the trivial case.
that an employment duration is fully observed ($\delta = 0$). While the $Ub$ of, for example, $T_D$, is

$$T_{D}^{Ub} = T_D \times \mathbb{I}\{r = D\} + \infty \times \mathbb{I}\{r \neq D\},$$

the $Lb$ of a partially observed duration ($\delta = 1$), by definition, is a transition to an unknown state ($r = U$) so that $T_{D}^{Lb}$ does not exist. We therefore need to modify the bounds framework for the univariate case in order to obtain meaningful bounds for the risk-specific CIC.

For this purpose, we decompose the CIC for risk $k = E$ or $D$ at time $t$ into two parts. The first is due to fully observed durations ($\delta = 0$) and the second is due to incompletely observed durations ($\delta = 1$):

$$I_k(t) = P(T_k \leq t, r = k) + P(T_k \leq t, r = k, \delta = 1) = P(T_k \leq t, r = k, \delta = 0) + P(T_k \leq t, r = k, r \neq U, \delta = 1). \tag{2}$$

The second part of (2) cannot be estimated since we do not observe $r$ if $\delta = 1$. Therefore, as discussed before, the second part of (2) can only be bounded. Similar to the ideas outlined by Manski (2003), we bound the unknown probability $P(T_k \leq t, r = k, r \neq U, \delta = 1)$. We start with the so-called worst case bounds by not imposing any restriction. We rewrite (2) as:

$$I_k(t) = P(T_k \leq t, r = k, \delta = 0) + P(T_k \leq t, r = k | r \neq U, \delta = 1)P(r \neq U | \delta = 1)P(\delta = 1). \tag{3}$$

The worst-case lower bound relies on the assumption that unobserved periods correspond for sure to an unobserved labor market state $r = U$, i.e. $P(r \neq U | \delta = 1) = 0$. In this case, the second part of (3) is zero. The worst-case lower bound of the CIC of risk $k \neq U$ is then given by:

$$I_{k}^{Lb}(t) = P(T_k \leq t, r = k, \delta = 0). \tag{4}$$

The worst-case upper bound assumes that there is for sure continuous unemployment during an unobserved period, i.e. $P(r \neq U | \delta = 1) = 1$ and is given by:

$$I_{k}^{Ub}(t) = P(T_k \leq t, r = k, \delta = 0) + P(T_k \leq t, r = k | r \neq U, \delta = 1)P(\delta = 1), \tag{5}$$

which can be estimated from the data. Note that the worst case bounds are attained if the conditional probability $P(r \neq U | \delta = 1)$ is either zero or one for all $t$. It follows directly from (3), (4) and (5) that the bounds for the identification region of the CIC are given by:

$$I_{k}^{Lb}(t) \leq I_k(t) \leq I_{k}^{Ub}(t). \tag{6}$$
Since we are interested in estimating the 1997 reform effect, we use these bounds of the CIC to determine an identification region for the reform effect. 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 difference-in-differences (see also Abadie (2005) for a review of nonparametric identification of DID models):

$$\Delta I_k(t|z) = [I_k(t|g_1, p_{t1}, z) - I_k(t|g_0, p_{t1}, z)] - [I_k(t|g_1, p_{t0}, y) - I_k(t|g_0, p_{t0}, z)]$$

for risk $k (= E$ or $D$). Given that we can only identify intervals for the risk-specific CIC’s it is straightforward to bound the reform effect by the lower-, $l_{I_k}(t|z)$, and upper bound, $u_{I_k}(t|z)$, of $\Delta I_k(t|z)$ similar to Lee and Wilke (2009) as:

$$l_{I_k}(t|z) = \max[-1, \{I_{Lb}^k(t|g_1, p_{t1}, z) - I_{Ub}^k(t|g_0, p_{t1}, z)\} - \{I_{Ub}^k(t|g_1, p_{t0}, z) - I_{Lb}^k(t|g_0, p_{t0}, z)\}]$$

and

$$u_{I_k}(t|z) = \min[1, \{I_{Ub}^k(t|g_1, p_{t1}, z) - I_{Lb}^k(t|g_0, p_{t1}, z)\} - \{I_{Lb}^k(t|g_1, p_{t0}, z) - I_{Ub}^k(t|g_0, p_{t0}, z)\}]$$

for $k = E, D$. Note that the lower and upper bound are restricted to be between -1 and 1 as it is not meaningful that a probability increases by more than one hundred percentage points.

As the worst-case bounds in (8) and (9) can be rather wide, we now explore plausible approaches to tighten them in order to obtain meaningful results. In addition to the monotonicity or independence assumption of Lee and Wilke (2009)(for details see Appendix C), one can make use of the probabilistic decomposition of the CIC to impose more targeted assumptions. From (3) it can be seen that the width of the bounds of the DID changes in (8) and (9) depends on three probabilities, $P(T_k \leq t, r = k|r \neq U, \delta = 1, g, p, z)$, $P(\delta = 1|g, p, z)$ and $P(r \neq U|\delta = 1, g, p, z)$.

While the first two can be directly estimated from the data, we can only impose restrictions on the unobserved conditional probability $P(r \neq U|\delta = 1, g, p, z)$ in order to achieve a tighter bound. One could use economic reasoning to restrict the feasible range for this conditional probability. Or one can assume that $P(r \neq U|\delta = 1, g, p, z)$ is a decreasing function of the size of the gap, which is $\varsigma_2 - \varsigma_1$.

Another possibility is to assume cross restrictions on the DiD terms to preclude the unlikely event that some of them attain their lower and the others their upper bound simultaneously. This can be done by assuming that the conditional probability $P(r \neq U|\delta = 1, g, p, z)$ is independent
of $G$ and $P$, i.e.

$$P(r \neq U|\delta = 1, g, p, z) = P(r \neq U|\delta = 1, z)$$ (10)

but it depends on the individual characteristics $Z$. Under this additional independence assumption (10) the DID changes of the CIC, $\Delta^*_t(t|z)$, can be decomposed as follows by substituting (3) into (7):

$$\Delta^*_t(t|z) = \Delta_{t}(t, \delta = 0|z) + P(r \neq U|\delta = 1, z)\Delta_{t}(t, \delta = 1|z)$$ (11)

with the reform effects on the CIC for different values of $\delta$ defined as:

$$\Delta_{t}(t, \delta = 0|z) = I_{k}(t, \delta = 0|g_{1}, p_{t1}, z) - I_{k}(t, \delta = 0|g_{0}, p_{t1}, z)$$

$$\Delta_{t}(t, \delta = 1|z) = I_{k}(t, \delta = 1|r \neq U, g_{1}, p_{t1}, z) - I_{k}(t, \delta = 1|r \neq U, g_{0}, p_{t1}, z)$$

In order to determine the bounds for $\Delta^*_t(t|z)$, we minimize and maximize (11) by assigning an appropriate value to the only unobserved term, $P(r \neq U|\delta = 1, z)$, for each $t$: If $\Delta_{t}(t, \delta = 1|z) > 0$, we set $P(r \neq U|\delta = 1, z) = 1$ and if $\Delta_{t}(t, \delta = 1|z) < 0$, we set $P(r \neq U|\delta = 1, z) = 0$ to maximize (11). The minimum is attained in the reversed way. The resulting lower bound of $\Delta^*_t(t|z)$ is always smaller than the upper bound. The width of the bounds is $|\Delta_{t}(t, \delta = 1|z)|$, which can be shown to be smaller than the width of the worst-case bounds (8) and (9). Under this additional independence assumption (10), bounds of the reform effect are obtained by bounding the DiD changes in (11) instead of bounding the CIC as in the case of the worst-case bounds in (8) and (9).

The bounds given in (4) and (5) can be estimated nonparametrically by using Kaplan-Meier type estimators, as the censoring time $T_{max}$ is independent (see Kalbfleisch and Prentice, 2002). We skip the details here as the estimation and inference procedures are straightforward. See Appendix D for details.

### 3.3 Estimations Results

We now present estimated bounds for the effect of the reform on the cumulative incidence of distant and local employment. We first estimate the worst case bounds (8)-(9). For identification of the reform effect, the lower bound needs to be greater than zero or the upper bound needs to be less than zero. Figure 3 shows the estimated bounds for samples stratified by skill level and marital status. It is apparent that the missing data problem precludes any informative result pattern
as in all cases the zero is part of the estimated identification (except for very short durations). Moreover, random sampling errors cause much less uncertainty for the results than the missing data problem. This demonstrates that even a large sample is not able to eliminate the problem of missing data.

As another interesting observation (see Appendix E), we find that the width of the resulting bounds is much wider than the interval spanned by the point estimates for the lower and upper bound definition of the unemployment duration (by ignoring the missing data problem). This suggests that a sensitivity analysis based on different transition time definitions alone may be misleading as it draws only an incomplete picture. Moreover, we 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.

In order to tighten the bounds, we at first impose the monotonicity and independence assumption of Lee and Wilke (2009). While the bounds based on the 2% sample become tighter, they are still too wide to draw interesting conclusions. For this reason, we do not report these results. As a next attempt, we tighten the bounds by imposing the additional independence assumption given in (10), i.e. $P(r \neq U|\delta = 1, g, p, z) = P(r \neq U|\delta = 1, z)$. While we cannot formally test this assumption, we believe that there is no obvious reason why the conditional probability should considerably vary across groups. Indeed, when looking at Table 4, we do not find evidence against it because the distribution of exit states is quite similar across groups. Moreover, since there is a rather high share of partially observed durations, i.e. $P(\delta = 1) \approx 0.4$, the aforementioned patterns in Table 4 may have some reliability due to the large number of observations.

Figure 4 shows the resulting bounds under the additional independence assumption which are much tighter. It provides evidence for considerable changes in observed exit probabilities for high-skilled job seekers for whom the threat of entitlement loss after exhausting UB is likely to be larger. Moreover, we find heterogeneous result patterns for high-skilled men depending on the marital status. While the bounds for single males - although only scratching the significance level - weakly suggest a higher probability of migration as a main reaction to a cut in PUBD, we find a strong and significant positive effect of the cut in PUBD on the probability of finding local employment among married men. For less-skilled individuals, however, we do not find evidence for effects irrespective of the marital status. For this reason, we only display the results for pooled educational degrees. Lee and Wilke (2009) also do not find evidence for a reform effect for low wage individuals.

We thus find some interesting evidence that the magnitude of the reform effect critically hinges on the household context. This may explain why Tatsiramos (2008) did not find evidence for an average effect of unemployment benefits on migration in Germany. Our results provide some
Figure 3: Lower and upper bound of the DiD changes of the cumulative incidence of local (left) and distant (right) employment among selected groups.
Figure 4: Lower and upper bound of the DiD changes of the cumulative incidence of local (left) and non-local (right) exits to employment among selected groups, additional assumption.

- **High skilled, single males**
- **High skilled, married males**
- **Less-skilled, males**
interesting insights on the relevance of the household context for migration decisions. Finally, we would like to comment on the interpretability of the estimated reform effect in this paper. In a dependent competing risks model, the causal effect on the marginal distributions of the competing risks cannot be identified without imposing additional assumptions, e.g. on the shape of the marginal distributions or on the dependence structure. In a follow up paper, Lo and Wilke (2009) use the 2% sample to check the robustness of our result pattern with respect to the assumed dependence structure. They find that the sign of the estimated treatment effect is indeed quite robust. For this reason we believe that the sign of changes in the CICs in our application is likely to be the same as the sign of the causal treatment effect on the marginal distributions.

4 Conclusion

Unobserved periods belong to the nature of administrative individual data since these data are generated for a particular administrative purpose only. In this paper we have presented a non-parametric approach to analyze a competing risks model in presence of such missing interval information. Our model is highly relevant for applied researchers who face similar data limitations. It derives bounds for the risk-specific cumulative incidence curve in a dependent competing risk model. Our framework covers several attempts to tighten the bounds in an application. In particular, we suggest a plausible independence assumption which effectively tightens the bounds.

In our empirical application with German data, we have explored the effect of reducing the entitlement length for unemployment benefits on the incidence rate to either local or distant employment. We exploit a credible natural experiment which eliminates selection bias to a large extent. Compared with related previous studies, we make several further improvements for the selection of the estimation sample in order to better control for unobserved individual heterogeneity. In particular, we construct variables which make use of an individual’s work history to proxy for unobserved labor market behavior. Since we have access to very comprehensive data which covers 50% of the male working population, we are able to obtain reliable estimates for the typically small incidence of reemployment after migration.

Our results confirm that missing data is a big problem in administrative individual data. It can easily preclude any conclusive result if one is not willing to impose additional assumptions. 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. Many former studies based on similar incomplete duration data even lack such a sensitivity test, but make implicit and non-testable assumptions about the unobservable periods instead. Our analysis thus reveals the severity of identification problems.
stemming from missing interval information.

We obtain considerably tighter bounds by imposing additional assumptions. The resulting estimates are suggestive for a reform effect on observed exit probabilities for high-skilled individuals for whom the threat of entitlement loss after exhausting UNB is likely to be largest. Our results indicate that the effect of extensive unemployment benefits strongly depends on the family background. The cumulative incidence for migration increases for singles while it remains unchanged for married men. In contrast, we do not observe changes in the cumulative incidence for local job finding for singles while it increases significantly for married men.

Limitations of our paper point towards some interesting extensions. First of all, data with additional information on individual and household characteristics would be desirable to reexamine our empirical results. Such additional information would also allow us to distinguish groups for whom a shorter receipt of unemployment benefits implies different entitlement losses. Secondly, the availability of a sample of administrative data merged with survey data may allow for estimating the unknown conditional probability if the the survey data fully identifies the employment trajectory. Such an estimate could then be used to further tighten the bounds.
References


Appendix A - Computation of PUBD counterfactual PUBD

During the study period of this paper, the entitlement length at the beginning of an unemployment spell is not included in the data and has to be computed based on the known employment history, age and the regulations. For this purpose, we need to compute the claim period and the so called extended claim period of an individual. The claim period is a backward looking concept and encompasses at most three years preceding the current UB claim. The claim period is shortened to the time when the previous UB claim finished if this was happened less than three years ago. In this case, the claim period starts when the previous UB claim ended. The extended claim period encompasses at most seven years preceding the current UB claim. Again, the extended claim period is shortened to the time when the previous UB claim finished if this took place less than seven years ago.

The basic PUBD corresponds to six months and applies to all claimants who do not fulfill the criteria for an extended PUBD, but worked for at least one year within the claim period. For a claimant to fulfill the sufficient condition to receive a PUBD of more than six months, he must have worked for more than 12 months in a socially insured job during the claim period plus the required working duration as indicated in Table 1 in his extended claim period. In order to illustrate this rather complicated calculation, consider a claimant aged 40 who accumulated a total of 30 months of employment in the last seven years. His last and only UB claim, however, was made only 12 months ago and lasted for six months. According to the regulations shown in Table 1, he had a PUBD of 12 months at the time he made the previous UB claim, leaving a remaining claim of six months at the end of his previous UB claim. Despite having accumulated 30 months of employment within the last seven years, both the claim period and the extended claim period are shortened to only six months due to his last UB claim. As a consequence, he does not fulfill the sufficient condition to receive an extended PUBD. Nevertheless, the claimant can still receive an extended PUBD if he fulfills all of the following criteria:

C1 The previous UB claim started within the last seven years. The agent in our example fulfills this requirement.

C2 The maximum PUBD is the sum of the PUBD approved by the employment record within the claim period and the remaining months eligible for UB at the end of the previous UB claim. In our case, the PUBD according to the shortened claim period is six month and he has a remainder of six months of PUBD from his previous UB claim. He is thus eligible for 12 months of UB.

C3 Any PUBD cannot be longer than the age-specific PUBD (see Table 1). In our example, the claimant’s age-specific PUBD is 12 months which is equal to the eligibility according to
C2 and thus need not be cut in order to fulfil the last criterion.

In order to compute the PUBD, all changing regulations throughout the 1980s and 1990s need to be considered. For the calculation of the counterfactual PUBD, we apply the pre-reform conditions to the post-reform period and compute the PUBD as if all individuals had been 42 by the time of the benefit claim. More precisely, if an individual was 38 at the beginning of the unemployment period, we adjust his age over his whole history as if he had always been four years older. This adjustment alone does not ensure the comparability of the resulting counterfactual PUBD 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 PUBD for the post-reform period as if all the changes in the regulations had been shifted back by five years, i.e. the difference between the pre- and post-reform period. This procedure ensures a twofold: (i) the comparability of counterfactual PUBD for all age groups irrespective of whether their unemployment period starts in the pre- or post-reform period; and (ii) the equivalence of counterfactual and actual PUBD for the treatment group in the pre-reform period. As a consequence, the treatment group in the pre-reform period with counterfactual PUBD of more than 12 months actually has entitlements of more than 12 months while all others who fulfill this criteria actually receive PUBD for a maximum of 12 months only, but are comparable to the former group in terms of their employment history.
### Table 6: Descriptive summary of sample characteristics, IAB data, final sample

<table>
<thead>
<tr>
<th></th>
<th>Control group</th>
<th>Treatment group</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>pre-reform</td>
<td>post-reform</td>
<td>pre-reform</td>
<td>post-reform</td>
</tr>
<tr>
<td>Age (years)</td>
<td>38.3</td>
<td>38.3</td>
<td>43.0</td>
<td>43.0</td>
</tr>
<tr>
<td>Married</td>
<td>64.7</td>
<td>60.0</td>
<td>71.0</td>
<td>67.4</td>
</tr>
<tr>
<td>High school degree</td>
<td>19.9</td>
<td>17.7</td>
<td>18.5</td>
<td>18.2</td>
</tr>
<tr>
<td>Vocational training</td>
<td>72.9</td>
<td>75.1</td>
<td>74.8</td>
<td>75.1</td>
</tr>
<tr>
<td>Tertiary education</td>
<td>7.2</td>
<td>7.2</td>
<td>6.8</td>
<td>6.7</td>
</tr>
<tr>
<td>High-skilled single</td>
<td>2.7</td>
<td>3.2</td>
<td>1.9</td>
<td>2.3</td>
</tr>
<tr>
<td>Less-skilled single</td>
<td>32.6</td>
<td>36.9</td>
<td>27.2</td>
<td>30.3</td>
</tr>
<tr>
<td>High-skilled married</td>
<td>5.3</td>
<td>4.8</td>
<td>6.0</td>
<td>5.0</td>
</tr>
<tr>
<td>Less-skilled married</td>
<td>59.4</td>
<td>55.1</td>
<td>65.0</td>
<td>62.3</td>
</tr>
<tr>
<td>Skilled blue-collar</td>
<td>42.1</td>
<td>41.5</td>
<td>42.9</td>
<td>41.0</td>
</tr>
<tr>
<td>Unskilled blue-collar</td>
<td>32.3</td>
<td>31.8</td>
<td>30.3</td>
<td>32.0</td>
</tr>
<tr>
<td>White-collar</td>
<td>25.6</td>
<td>26.7</td>
<td>26.8</td>
<td>27.0</td>
</tr>
<tr>
<td>1st wage quintile</td>
<td>33.9</td>
<td>34.6</td>
<td>34.0</td>
<td>35.4</td>
</tr>
<tr>
<td>2nd wage quintile</td>
<td>23.0</td>
<td>24.1</td>
<td>21.8</td>
<td>22.9</td>
</tr>
<tr>
<td>3rd wage quintile</td>
<td>15.7</td>
<td>16.1</td>
<td>15.2</td>
<td>15.4</td>
</tr>
<tr>
<td>4th wage quintile</td>
<td>14.1</td>
<td>13.2</td>
<td>14.1</td>
<td>12.8</td>
</tr>
<tr>
<td>5th wage quintile</td>
<td>13.4</td>
<td>12.0</td>
<td>14.9</td>
<td>13.5</td>
</tr>
<tr>
<td>Tenure prev. job (days)</td>
<td>1253</td>
<td>1287</td>
<td>1489</td>
<td>1481</td>
</tr>
<tr>
<td>Previously unemployed</td>
<td>63.1</td>
<td>71.5</td>
<td>58.2</td>
<td>69.6</td>
</tr>
<tr>
<td>No. of prev. unempl. spells</td>
<td>2.8</td>
<td>3.0</td>
<td>2.5</td>
<td>3.0</td>
</tr>
<tr>
<td>Total spells</td>
<td>104,069</td>
<td>94,309</td>
<td>39,434</td>
<td>36,104</td>
</tr>
</tbody>
</table>
Appendix C - The Independence and Monotonicity Assumption

Lee and Wilke (2009) impose two assumptions to tighten the bounds. Under the independence assumption, the reform effect $\Delta_{Ik}(t|z)$ does not depend on the calendar time. By estimating the reform for samples stratified by calendar years for all combinations of pre and post reform years, they take the largest (smallest) estimate of the lower (upper) bound as lower (upper) bound of the reform effect.

Under the monotonicity assumption, the survival function of the younger individuals (control group) is lower than for the older individuals (treatment group) in the same period and in absence of a treatment. This assumption increases the lower bound in (8) by restricting $I_{k}^{Lb}(t|g_{1}, p_{t1}, z) - I_{k}^{Ub}(t|g_{0}, p_{t1}, z)$ to non-negative values. The upper bound can be reduced analogously.
Appendix D - Nonparametric Estimation and Inference

Let \( t_0 < \ldots < t_j < \ldots < t_J \) be the discrete times at which we observe \( T_E, T_D, \varsigma_1, \varsigma_2 \) and \( T_{\max} \). We first focus on the estimation of the lower bound (4). There are \( d_{kJ}^{lb} \) observed exits to risk type \( k \neq U \) at time \( t_j \); \( d_{cj}^{lb} \) observed realizations of \( \varsigma_1 \) at \( t_j \); and \( d_{mj}^{lb} \) censored observations at \( t_j \). For \( k \neq U \), which are given by:

\[
d_{kJ}^{lb} = \sum_{i=1}^{n} \mathbb{I}(T_{ik} = \min\{T_{ik}, \varsigma_{1i}, T_{i,\max}\})
\]

\[
d_{cj}^{lb} = \sum_{i=1}^{n} \mathbb{I}(\varsigma_{1i} = \min\{T_{ik}, \varsigma_{1i}, T_{i,\max}\})
\]

\[
d_{mj}^{lb} = \sum_{i=1}^{n} \mathbb{I}(T_{i,\max} = \min\{T_{ik}, \varsigma_{1i}, T_{i,\max}\})
\]

with \( \mathbb{I}(Y) \) is the indicator function of the event \( Y \). Note that these numbers can be also computed conditional to \( x \), by restricting the sample accordingly.

For the estimation of the upper bound (5) we have to compute equivalent numbers, although \( \varsigma_1 \) can be ignored in this case and we have \( T_r = \varsigma_2 \). We define \( d_{kJ}^{ub} \) and \( d_{mj}^{ub} \) as

\[
d_{kJ}^{ub} = \sum_{i=1}^{n} \mathbb{I}(T_{ik} = \min\{T_{ik}, T_{i,\max}\})
\]

\[
d_{mj}^{ub} = \sum_{i=1}^{n} \mathbb{I}(T_{i,\max} = \min\{T_{ik}, T_{i,\max}\}).
\]

Let \( d_{j}^{lb} = \sum_{k=E,M} d_{kJ}^{lb} + d_{cj}^{lb} + d_{mj}^{lb} \) and \( d_{j}^{ub} = \sum_{k=E,M} d_{kJ}^{ub} + d_{mj}^{ub} \). The number of observations at risk just before \( t_j \) is then given by

\[
n_{j}^{lb} = d_{j}^{lb} + \ldots + d_{j-1}^{lb} \quad \text{and} \quad n_{j}^{ub} = d_{j}^{ub} + \ldots + d_{j}^{ub}.
\]

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

\[
\hat{\lambda}_k(t_j|x) = \frac{d_{kJ}^{lb}}{n_{j}^{lb}} \quad \text{with } b \in \{Lb, Ub\} \quad \text{and} \quad \hat{\lambda}_c(t_j|x) = \frac{d_{cj}^{lb}}{n_{j}^{lb}};
\]

\[
\hat{S}^{lb}(t_j|x) = \prod_{u=1}^{j-1} \left( 1 - \sum_{k=E,M} \hat{\lambda}_k(t_u|x) - \hat{\lambda}_c(t_u|x) \right) \quad \text{and} \quad \hat{S}^{ub}(t_j|x) = \prod_{u=1}^{j-1} \left( 1 - \sum_{k=E,M} \hat{\lambda}_k(t_u) \right).
\]

Note that that these estimators are consistent as the right censoring is independent. A consistent estimator for the bounds given in (4) and (5) is then \((k \neq U)\):

\[
\hat{I}_k(t_j|x) = \sum_{u=1}^{j} \hat{\lambda}_k(t_u|x) \hat{S}^{lb}(t_u|x) \quad \text{with } b \in \{LB, Ub\}.
\]

We estimate joint confidence intervals for the lower and upper bound. We employ the bootstrap procedure of Horowitz and Manski (2000). The bootstrap repetitions are 500.
Appendix E

Figure 5: Point estimates for lower and upper bound of reform effect on the cumulative incidence of local(left) and distant(right) exits to employment among high-skilled unemployed, married males.

The point estimates for the reform effect in Figure 5 are obtained by using the following formulas:

\[
l_k(t_j|p_{t0}, p_{t1}, z) = \{I_{k}^{Lb}(t_j|g_1, p_{t1}, z) - I_{k}^{Lb}(t_j|g_0, p_{t1}, z)\}
- \{I_{k}^{Lb}(t_j|g_1, p_{t0}, z) - I_{k}^{Lb}(t_j|g_0, p_{t0}, z)\}
\]

\[
u_k(t_j|p_{t0}, p_{t1}, z) = \{I_{k}^{Ub}(t_j|g_1, p_{t1}, z) - I_{k}^{Ub}(t_j|g_0, p_{t1}, z)\}
- \{I_{k}^{Ub}(t_j|g_1, p_{t0}, z) - I_{k}^{Ub}(t_j|g_0, p_{t0}, z)\}
\]

for \(k = E, D\).