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Bounds analysis of competing risks: a non-parametric evaluation of the effect of unemployment benefits on migration

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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 Classification C41 · C14 · J61

1 Introduction

Interregional migration rates differ drastically across countries with migration rates in the US and Australia clearly exceeding European levels despite being measured at

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a more aggregated regional classification (OECD 2005). Moreover, migration rates in Germany tend to exceed southern and eastern European levels, but are markedly below migration rates in some western European countries such as the UK and the Netherlands. In addition, migration rates of different skill groups vary markedly across countries (OECD 2005). Moreover, being unemployed has been found to increase the propensity to be mobile in the UK and the US (Pissarides and Wadsworth 1989; Jackman and Savouri 1992; Bailey 1993), while German evidence suggests the opposite (Arntz 2007). Differences in the design of the unemployment compensation (UC) system may thus be one potential explanation for such cross-country variation in migration patterns.

However, economic theory is not conclusive on whether unemployment benefits promote or inhibit migration of unemployed workers. On the one hand, higher levels of unemployment benefits (UB) generate negative disincentive effects and reduce the geographical job search horizons (Hassler et al. 2005). On the other hand, the financial resources provided by UB may enable individuals to bear the cost of migration which could enhance the willingness to accept a job offer that requires a move (Tatsiramos 2009). 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 UB 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 (2007) 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 a dominant disincentive effect of UB on migration. The findings of these studies, however, might be driven by an unobserved selection of immobile individuals into unemployment benefits. Tatsiramos (2009) 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 is positively affected by UB in Denmark and France, while no effect is found in the UK and Germany.

Motivated by this unsettled discussion, this article 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 by shortening the maximum entitlement lengths for specific groups of registered unemployed by several months in 1997 (later "the 1997 reform"). 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 use long-term information on the individual employment history to select comparable individuals for the treatment and control groups. 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 allows for a non-parametric analysis of the rather rare event of migration and even enables us to estimate heterogenous treatment effects. In particular, we distinguish individuals by education and marital status since these sub-groups are likely to be affected differently by the reform due to different wage replacement rates and migration costs.

As an additional methodological contribution, we develop a framework for bound analysis of incomplete duration data in the context of dependent competing risk model. Such a method is necessary because the administrative data contain unobserved periods during which an individual might be unemployed but not eligible for unemployment compensation or might have entered a labor market state that is not covered by the data. Since administrative data tend to be collected for a particular administrative purpose, such unobserved periods frequently occur in such data. This feature is not specific to the German case, but it applies to many other countries such as Spain and Britain. Incomplete interval duration causes major difficulties for the identification of unemployment duration. See Kruppe et al. (2008) for attempts to implement the ILO definition of unemployment in German administrative data. 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, see Angrist and Krueger (1999) for a short review. 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 survival probability for unemployment duration. However, their bounds framework is restricted to a 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 article presents slight but crucial extensions of Lee and Wilke's (2009) bounds framework to a dependent competing risks model. For this purpose, we derive bounds for the destination-specific cumulative incidence curve (CIC). Due to the non-identifiability of the competing risks model, the marginal distributions of the competing risks are not point identified without additional assumptions on the dependence structure between competing risks. See Lo and Wilke (2010) for an extensive treatment of this topic. Without additional assumptions, the marginal distributions can only be bounded and the so-called Peterson (1976) bounds tend to be wide in applications. A combination of the bounds due to missing data and the Peterson bounds will therefore result in even wider bounds and will likely not produce any informative results. To avoid this problem, we study the destination-specific CIC and focus on the bounds caused by the data problem. For this reason, the interpretability of our results is limited to these observable probabilities. The CIC is the distribution of the observed transitions for a particular risk, which is also called sub-distribution. It is useful for the analysis of competing risks models (Kalbfleisch and Prentice 2002), and it is of prime interest in clinical researches (Kim 2007). In contrast, cause-specific hazard function (or sub-hazard functions) and the cause-specific cumulative hazard function (or cumulative sub-hazard functions) are more popular in applied economics and econometrics (see, for example, Kiefer 1988). Although all these functions are observably equivalent and are algebraically interchangeable, the sub-hazard function and the cumulative sub-hazard cannot be bounded in a similar way as the CIC in the case of incomplete data. This is because the sub-hazard function is a conditional probability in contrast to the CIC. A bounds framework due to missing data problems is therefore suggested for the CIC. We perform a similar non-parametric DiD analysis as in Lee and Wilke (2009) to study the effect of UB on the competing incidence rates of local job finding and migration. Although we estimate non-parametric CICs in this article, the proposed bounds framework 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. In the current application, the identification problem due to missing data is much more severe than random sampling error. Additional assumptions on the nature of the missing data are required to derive any informative results. After imposing an independence assumption we obtain much tighter bounds.
- 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 non-parametric approach.

The article is structured as follows. The following section presents the details of the 1997 reform. Section 3 presents the data structure, the econometric framework, and the empirical results. Section 4 concludes.

2 The 1997 reform of UB

2.1 Basic features of the UC system

Until 2004, the UC system in Germany consisted of two main components: UB and unemployment assistance (UA). Whether an individual is entitled to receive UB as well as the maximum length of receipt, i.e., the potential UB duration (PUBD) is determined by the age of the claimant and the creditable working months (CWM), i.e., the number of months an individual has been working in a socially insured job within the relevant claim period (see Appendix A for details). 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 the insurance-based UB replaces 68 % (63 %) of former wage income, UA has an income replacement rate 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.

Table 1 PUBD for UB claimants by age at start of UB claim and work history, IAB-R01

CWM within extended claim period (months)	PUBD (months) before/after March 1997 by age				
	< 42	42–43	44	45–46	> 46
12	6/6	6/6	6/6	6/6	6/6
16	8/8	8/8	8/8	8/8	8/8
20	10/10	10/10	10/10	10/10	10/10
24	12/12	12/12	12/12	12/12	12/12
28	12/12	14/12	14/12	14/14	14/14
32	12/12	16/12	16/12	16/16	16/16
36	12/12	18/12	18/12	18/18	18/18
40	12/12	18/12	20/12	20/18	20/20
44	12/12	18/12	22/12	22/18	22/22

Source Plaßmann (2002)

2.2 The natural experiment, the control and treatment groups

In April 1997, a reform of the Employment Promotion Act (*Arbeitsförderungsgesetz*) shortened the PUBD for those aged 42 and above, while the wage replacement ratio of UB remained unchanged. The reduction in PUBD depends on the age when initially claiming unemployment and the employment history as is summarized in Table 1. Individuals below age 42 receive up to 12 months of PUBD depending on their CWM within the extended claim period prior to claiming UB. An individual aged 42 with a length of 28 or more working months within the claim period, however, received an extended PUBD of up to 18 months before the reform, but was eligible for a maximum of only 12 months after the reform. The shortening of PUBD was even stronger for those aged 44 since the maximum PUBD before the reform was 22 months compared to 12 months after the reform. The 1997 reform thus provides a natural experiment with a credible source of exogenous variations in PUBD that, together with an extension of PUBD in 1984 and 1986, has already been used to identify the effects on the overall duration of unemployment (Hunt 1995; Plaßmann 2002; Wolff 2003; Fitzenberger and Wilke 2010; Müller et al. 2007; Lee and Wilke 2009). However, this is the first paper that exploits this natural experiment to investigate its causal effect on the unemployment duration until reemployment in either the local or the distant labor market.

More formally, let T_k be the latent unemployment duration with destination state k including all labor market states other than unemployment, such as local, distant employment, self-employment, civil servant, subsidized employment, out-of-job training, and out of the labor force, etc. We also allow for independent right censoring at the end of the observation period but we ignore it in our notation to keep it simple. The destination state of the shortest latent duration is r, i.e., $r = \arg\min_k\{T_k\}$. We assume for simplicity that there are no ties in the latent durations for each individual.

We assume T_k to depend on a number of variables. Let $G = g_0$ denote the control group and $G = g_1$ the treatment group. $P = p_{t0}$ is the pre-reform period and $P = p_{t1}$ is the post-reform period. Let Z denote a vector of other observable individual variables. Our interest is to study the effect of the reform on the incidence rates of two destination states, local reemployment ($r = E$) and distant reemployment ($r = D$), within a DiD framework. For this purpose, we need to define comparable control and treatment groups with a common trend if the reform had not taken place.

Our treatment group comprises individuals aged 42–44 at the start of unemployment and with more than 28 CWM. According to the reform design, these individuals were affected by shortened PUBD, but we do not include individuals above the age of 44 to avoid any complications that may arise from early retirement. According to previous studies, reforms in PUBDs have strong impacts on the incidence of unemployment for those aged 50 or above since reductions in PUBDs weaken the attractiveness of early retirements through the UC system (Müller et al. 2007; Fitzenberger and Wilke 2010). For individuals below 45 years of age it thus appears plausible that the reform did not considerably alter the inflow into unemployment in the post-reform period compared to the pre-reform.

In order to ensure the comparability of treatment and control groups, we first restrict the control group to individuals aged 36–41 to ensure that only individuals at similar stages of their life cycle are compared to each other. In addition, we exclude women from the analysis because life cycle effects are likely to be strong even within this rather homogeneous age group. In particular, women below the age of 40 are likely to differ in their fertility and employment decisions and how they interact with the UC system compared to women above the age of 40.

Among male claimants, we select only those individuals who would have been entitled to more than 12 months of UB given their CWM if they had been treated as an individual aged 42–44 according to the pre-reform regulations, i.e., whose counterfactual entitlement length exceeds 12 months. Since the counterfactual entitlement length depends on the previous employment history, 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 mitigates imbalances in the distributions of the employment history and ensures a common support. Moreover, the approach ensures that those in the treatment group have actually been treated by a shortened PUBD. Using similar data and adopting a similar identification strategy, Lee and Wilke's (2009) selection criteria were much broader so that estimated reform effects may have been biased toward zero.

Although neither the PUBD nor the counterfactual PUBD are available in the data, there are well-documented rules[1] describing how the individual employment history translates into CWM and PUBD (see Appendix A for details). Note that we cannot compute any PUBD for men from eastern Germany since the employment histories are truncated by reunification. We therefore restrict the analysis to western Germany. To sum up, the sample is selected as follows:

[1] See the Employment Promotion Act (*Arbeitsförderungsgesetz*) and the Social Welfare Act III (*Sozialgesetzbuch III*).

- Men with a previous full-time job and a job located in western Germany.
- Men with a counterfactual PUBD longer than 12 months aged 36–41 (control group, $G = g_0$) and aged 42–44 (treatment group, $G = g_1$) at the start of unemployment; and
- Unemployment spells starting between 1995 and 1996 (pre-reform, $P = p_{t0}$) and between 1999 and 2000 (post-reform, $P = p_{t1}$).

The choice of the pre- and post-reform periods tries to ensure the validity of the evaluation design. As regards the pre-reform period, we use only spells staring in 1995 and 1996 since there has been a policy change in 1994 that introduced stricter sanction rules and might thus confound the effects of the 1997 reform. Moreover, allowing for a gap between the end of 1996 and the implementation month in April 1997 reduces biases from anticipation effects. As regards the post-reform period, we skip the year 1998 since there have been some transitory regulations to cushion the introduction of the reform. In particular, the new regulations did not apply to new benefit claimants before March 1999.

Another threat to the validity of the evaluation design could result from macroeconomic developments that change labor market outcomes before and after the reform. 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 UB receipt due to non-compliance with eligibility rules have been found as an effective means of reducing unemployment (Boone et al. 2007, 2009). Since these new regulations were applied to all unemployed (i.e., the treatment and control groups), the use of a DiD estimator eliminates both a macroeconomic time trend as well as the effect of stricter sanction rules when assuming that both treatment and control groups experience the same time trends. We consider this a plausible assumption since the selection criteria for the treatment and control groups ensures that individuals are quite similar with regard to their employment history and PUBDs.

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 were prepared by the IAB to have the same structure as the IAB employment sub-sample 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.

Table 2 shows that claimants would be entitled to similar PUBDs had they been treated according to the pre-reform regulations for individuals aged 42–44. By selecting only those individuals with a counterfactual PUBD exceeding 12 months, we ensure that individuals aged 42–44 have been treated by a shortened PUBD while individuals in the control group have similar previous employment and claim histories. When applying the actual age- and period-specific regulations to the selected

Table 2 Counterfactual PUBD for unemployment spells in the pre- and post-reform period by age groups, IAB data

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

Sample defined as described in Sect. 2.2

sample, 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 some individuals have an actual PUBD of <12 months. This is due to the fact that individuals aged 42 or above reach a PUBD of >12 months with an employment history that for the younger age group would result in even <12 months of 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 1 month to a maximum of 10 months.

Table 6 in Appendix B shows the descriptive statistics for the observable characteristics 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. Given the balanced character of the rest of the observable characteristics, we consider it unnecessary to condition on further covariates. Moreover, since we want to apply a non-parametric framework, conditioning on further covariates would split the sample into too small sub-samples.

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

Table 3 Actual PUBD for claimants with counterfactual PUBD >12 months by group and period, IAB data (final sample)

PUBD (months)	Control group		Treatment group	
	Pre-1997	Post-1997	Pre-1997	Post-1997
6–8	2.2 %	1.6 %	–	1.6 %
9–11	7.2 %	5.6 %	–	5.3 %
12	90.6 %	92.8 %	–	93.1 %
13–14	–	–	6.9 %	–
15–16	–	–	7.5 %	–
17–18	–	–	58.6 %	–
19–20	–	–	2.6 %	–
21–22	–	–	23.3 %	–
Average	11.8	11.8	18.5	11.8
Total spells	104,069	94,309	39,434	36,104

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 UC system thus results in a heterogeneous treatment which is likely to affect the strength of the reform effect.

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 UC. 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. As we are only able to proxy for the eligibility for complementary social benefits, our estimated effects may be biased toward zero for the higher skilled group and biased away from zero for the lesser skilled group. An additional bias may arise due to having misclassification in the education variable, even though we have corrected and imputed evident inconsistencies.

As a robustness check, therefore, we repeat the empirical analysis for different pre-unemployment wage levels of the unemployed individual (similar to Lee and Wilke 2009). We do so because the previous wage level is an alternative proxy of the household's earning capacity that should also be related to the eligibility for complementary social benefits. Since we obtain almost identical results for individuals with increasing educational attainment and increasing pre-unemployment wages, we do not find evidence for instability of our results with respect to the choice of the proxy variable. At least, misclassification in the education variable does not seem to affect the results.

As the willingness of individuals to migrate is likely determined within the household context, we also 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 et al. 1996). The reduction of PUBD is less likely to provoke a higher incidence rate of migration within this group of households than among singles 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 single or married individuals. Based on our previous considerations, 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 and less likely to find a local job in response to the reform than their married counterparts. For less-skilled individuals, the treatment and thus its effect are expected to be rather small.

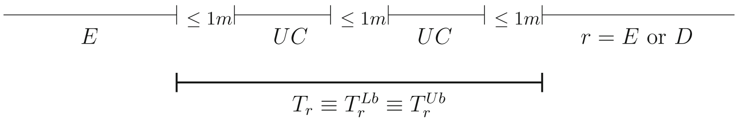
3 Incomplete duration data and bounds analysis

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 UC (UB or UA) from the Federal Employment Agency (*Bundesagentur für Arbeit*). We want to distinguish reemployment in a distant region which necessitates migration (“distant employment”, $r = D$) and reemployment in the local area (“local employment”, $r = E$). We can distinguish between distant and local employment in the data by comparing the location of the old and the new workplace at the level of the 440 German counties. In this article, 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 approximately corresponds to the average diameter of a German labor market region that is defined to minimize external commuting linkages. A distance of 100 km should thus in most cases necessitate residential mobility. A sensitivity test with an alternative threshold of 75 km did not yield qualitatively different results. We thus decided to use the 100-km threshold so that there is a transition to local employment if the distance between the two workplaces is $<$100 km.

As discussed in Sect. 1, there are gaps between the observable records which prevent us from identifying the actual labor market state during these periods. Gaps occur whenever an individual is unemployed without receiving UC (Reason 1); or the individual leaves unemployment by entering into one of the labor market states which are not observable in the data (Reason 2). The only two destination states which are observable in the data are local ($r = E$) and distant reemployment ($r = D$). As they are always observed, the gaps in the data cannot belong to state E or D. Gaps in the data can be also due to Reason 1 followed immediately by Reason 2 because an unemployed who is not claiming UC can well make a transition into an unrecorded labor market state but this transition and its timing are not observed. Due to this incomplete data structure the unemployment duration is not fully observable. Although Reasons 1 and 2 cannot be distinguished from the data, a classification of the missing information would have important implications for the length and the risk type: if the entire unobserved period is only caused by Reason 1, the unknown unemployment duration spans over the entire unobserved period until it is terminated by a transition into an observed labor market state E or D. If an unemployed makes a transition into an unrecorded labor market state at any time during the unobserved period, the unemployment spell is terminated at that time with a labor market state other than E or D. To incorporate the latter into our model, we define a pooled residual risk ("other states", $r = U$) which covers all possible unrecorded destination states. Unemployment can be therefore terminated by three risks ($r = E$, D, and U) in our model. In the rest of this article, we will not examine the reform effect on the incidence rate of risk U as this residual pooled risk does not have a clear interpretation. A referee raised concerns whether pooling all unobservable risks into one remainder risk U affects the estimates for the CICs for risk E and D. As the CIC describes the observed distribution for a risk, they are not sensitive to the definition of alternative risks. However, estimates for the underlying marginal distributions can be biased unless the dependence structure between competing risks belongs to an Archimedean copula (compare Lo and Wilke 2010). As we focus on the analysis of cumulative incidences in this article, we do not require any assumption on the dependence structure between risks in our framework.

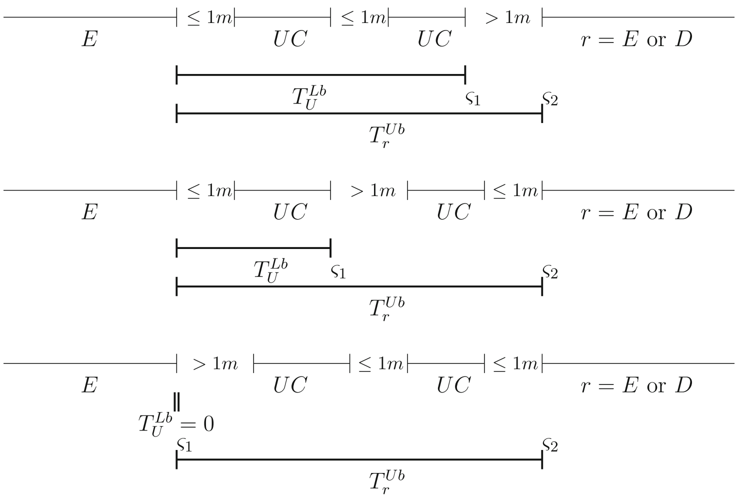
3.1 Constructing bounds for unemployment duration

An unemployment duration T_r is fully observed, denoted as $\delta = 0$, if it is immediately preceded and followed by an employment spell with observable states $r = E$ or D, and the individual receives either UB or UA without interruptions throughout the whole unemployment period. While the administrative record in this case provides full information, there are many unemployment spells that are interrupted by very short gaps of <1 month. Such short gaps need not reflect a new labor market state, but may simply point toward frictions when moving from employment to unemployment or back. Similarly, short interruptions of the receipt of UC are unlikely to point toward a new labor market state since any social security protection such as health insurance is lost if the interruption exceeds one month. We therefore relax the definition of a fully observed unemployment period as follows (see Fig. 1): unobserved gaps between subsequent administrative records may not exceed 1 month if they are between the end of employment and the start of UC receipt, between two UC claim spells or before a subsequent observable employment state. As observable employment consists only of local and distant employment, the fully observed unemployment duration T_r either ends with local ($r = E$) or distant employment ($r = D$). The lower bound (Lb) and upper bound (Ub) of the unemployment duration are the same in the case of $\delta = 0$. Accordingly, when $\delta = 0$, there is no unemployment spell with risk U. As a referee pointed out, this implies that there are only transitions to two risks when $\delta = 0$. This pattern is due to the nature of the missing data and not due to a restriction on the risk set. It is therefore not an assumption which may affect results for risks E and D.



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

Fig. 1 An observed unemployment duration T_E or T_D, $\delta = 0$.



Note: E: Spell of local employment; D: Spell of distant employment; UC: Spell receiving UC; m: Month
ς_1: Time at which unobserved period begins; ς_2: Time at which unobserved period ends

Fig. 2 Examples of Lb and Ub definitions of incompletely observed unemployment durations, $\delta = 1$.

If an unemployment spell does not meet the above conditions for $\delta = 0$, it is not fully observed, i.e., $\delta = 1$. Figure 2 illustrates different cases which occur in the data. They are unemployment durations with gaps at least longer than 1 month. As there are different possibilities during the gaps and there is no information in the data, we do not impose any restriction on the whereabouts of the individual during these periods. Instead, we define a Lb and an Ub for the transition time which reflect the uncertainty caused by the incomplete data.

In particular, each unemployment duration starts at the end of the employment spell preceding the receipt of UC. The Lb unemployment spell is terminated once there is a gap longer than 1 month between the following individual records. In this case, Lb assumes that the individual enters an unobserved labor market state ($r = U$) at ς_1 (see Fig. 2). Lb unemployment spells with $\delta = 1$ cannot be terminated by a transition to E or D, as if otherwise, E or D would be observed at ς_1. The Lb unemployment duration has length T_U^{Lb} with destination U. In contrast, the Ub of an unemployment duration assumes continued unemployment during the interval ς_1 and ς_2. The receipt of UB may end there because an individual does not pass a means-test for UA after exhausting UB despite continued unemployment, or there are benefit sanctions for the unemployed. Thus, irrespective of the length of the gap between the records, Ub

Table 4 Compositions of unemployment durations with different destination states under Lb and Ub definitions, IAB data (final sample)

	Control group (%)		Treatment group (%)	
	Pre-1997	Post-1997	Pre-1997	Post-1997
Lb unemployment spells				
Ends with local employment	50.1	50.8	48.1	49.4
Ends with distant employment	8.7	10.4	8.9	10.2
Ends with unknown state	41.2	38.8	43.0	40.4
Right-censored	0.0	0.0	0.0	0.0
Total	100.0	100.0	100.0	100.0
Ub unemployment spells				
Ends with local employment	75.2	71.1	72.5	69.6
Ends with distant employment	14.1	15.2	14.2	14.7
Ends with unknown state	–	–	–	-
Right-censored	10.7	13.7	13.3	15.7
Total	100.0	100.0	100.0	100.0
Total spells	104,069	94,309	39,434	36,104

assumes that there is no transition from unemployment to risk U during the gaps. The Ub unemployment spell ends when the next observable employment spell begins (ς_2). The Ub spell therefore corresponds to an unemployment duration of length T_r^{Ub} with destination state of $r = E$ or D. We only include spells in our sample if the Ub spell overlaps at least 1 day with a UC claim spell. Our analysis of the effect of UB is therefore restricted to those unemployed who claim benefits for at least 1 day while they are unemployed. To conclude, when an unemployment spell is incomplete, the Lb and Ub definitions differ in both the length of the unemployment spell and the destination states.

To further distinguish between the case of $r = E$ and D, we define a dummy variable δ_k with $\delta_k = 1$ if the unemployment spell is not fully observed ($\delta = 1$) and ends with risk $k = E, D$ at ς_2. If a spell is complete, we have $\delta = \delta_E = \delta_D = 0$. If a spell is incomplete, either $\delta = \delta_E = 1$ or $\delta = \delta_D = 1$, but not both. We thus have $\delta = \delta_E + \delta_D$.

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 (i.e., $\delta = 1$). 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 about 50 % (9–10 %) of the total unemployment spells are complete data ending with local (distant) employment, there are 20–25 % (5 %) of the total unemployment spells are incomplete and end with local, i.e., $\delta_E = 1$, (distant, i.e., $\delta_D = 1$) employment according to the Ub definition. This observation suggests that missing data problems are relevant and similarly pronounced for both the two exit states, as they

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

	Control group		Treatment group	
	Pre-1997	Post-1997	Pre-1997	Post-1997
Lb unemployment spells				
High-skilled singles	274	178	364	214
Less-skilled singles	243	183	303	214
High-skilled married men	184	123	233	165
Less-skilled married men	159	134	183	154
Overall	185	152	214	172
Ub unemployment spells				
High-skilled singles	488	278	648	369
Less-skilled singles	336	243	449	291
High-skilled married men	367	246	434	305
Less-skilled married men	211	177	245	200
Overall	258	204	307	232

concern about half of all spells. Moreover, the shares of different destination states are rather invariant across the control/treatment groups in pre-/post-reform period. Note that all spells are independently right censored at the end of the observation period in 2005. There is virtually no right censoring for the Lb spells as almost all right censored Ub spells are $\delta = 1$. This is not a feature of our model, but is driven by the data.

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 the control groups narrows after the reform. Also note that we do not obtain any evidence for an effect of the reform if we simply consider the overall median duration. This may indicate that it is important to run separate estimations for the four sub-samples.

As suggested by one referee, we also looked for regional differences in mobility patterns that would motivate to examine heterogeneous treatment effects for different types of regions. However, controlling for the individual characteristics that we already take into account, we could not find evidence in support of significant regional differences. We therefore think that the individual characteristics are the driving forces behind heterogeneous treatment effects.

3.2 Bounds for the competing risks model

The bounds derived in the last section can be used as bounds for the unobserved true unemployment duration (Lee and Wilke 2009; Fitzenberger and Wilke 2010). In a framework with one risk, bounds for the unemployment duration directly translate into bounds for the functional of interests, e.g., the marginal survival curve. In our competing risks framework, the construction of bounds for risk-specific durations is hampered by the fact that the Lb for T_E (T_D) is not defined when $\delta_E = 1$ ($\delta_D = 1$). In contrast to the single risk case, we therefore do not bound the length of the risk-specific unemployment durations, but directly bound the CICs.

We bound the CIC for risk E and D by decomposing it into two parts, respectively. The first is due to fully observed durations ($\delta = 0$), and the second is due to incompletely observed durations ($\delta = 1$), i.e.,

$$\begin{aligned} I_k(t) &= P(T_k \le t, r = k) \\ &= P(T_k \le t, r = k, \delta = 0) + P(T_k \le t, r = k, \delta = 1) \\ &= P(T_k \le t, r = k, \delta = 0) + P(T_k \le t, r = k, \delta_k = 1) \end{aligned} \tag{1}$$

with $k \in \{E, D\}$. The third equation follows from the fact that in the case of $\delta = 1$ the CIC for risk E, for instance, can be broken down into two parts: $P(T_E \le t, r = E, \delta = 1, \delta_E = 1)$ and $P(T_E \le t, r = E, \delta = 1, \delta_D = 1)$. The latter part is zero by definition as an unemployment spell with $\delta_D = 1$ cannot be terminated by a transition to risk E.

We start with the so-called worst case bounds without imposing any restriction. In the Lb definition, we assume that no incomplete unemployment spell with $\delta_k = 1$ ends with risk E or D, and thus there is no intersection of spells with $\delta_k = 1$ and risk $r = k$. Accordingly, $P(r = k|\delta_k = 1) = 0$. As $P(r = k, \delta_k = 1) = P(r = k|\delta_k = 1)$ $P(\delta_k = 1)$, we have $P(r = k, \delta_k = 1) = 0$. Since $P(T_k \le t, r = k, \delta_k = 1) \le P(r = k, \delta_k = 1)$, we have $P(T_k \le t, r = k, \delta_k = 1) = 0$. Plugging the latter into (1), the Lb of the CIC for risk $k = E, D$ becomes

$$I_k^{\mathrm{Lb}}(t) = P(T_k \le t, r = k, \delta = 0). \tag{2}$$

For the Ub definition, we assume $P(r = k|\delta_k = 1) = 1$, i.e., all incomplete spells with $\delta_k = 1$ end with risk k. This implies $P(r = k, \delta_k = 1) = P(r = k|\delta_k = 1)$ $P(\delta_k = 1) = P(\delta_k = 1)$. Consequently, we have $P(T_k \le t, r = k, \delta_k = 1) = P(T_k \le t, \delta_k = 1)$. The latter is identified in the data as it is the CIC for all observations with $\delta_k = 1$. The Ub of the CIC for risk $k = E, D$ becomes

$$I_k^{\mathrm{Ub}}(t) = P(T_k \le t, r = k, \delta = 0) + P(T_k \le t, \delta_k = 1). \tag{3}$$

Since $0 \le P(r = k|\delta_k = 1) \le 1$, it follows from (1), (2), and (3) that the bounds for the identification region of the CIC for risk $k \in \{E, D\}$ are given by

$$I_k^{\mathrm{Lb}}(t) \le I_k(t) \le I_k^{\mathrm{Ub}}(t). \tag{4}$$

In Appendix C, we show that the bounds in (4) are sharp for all t and for $k \in \{E, D\}$. Sharpness of bounds means that they do not include any infeasible values. In other words, for the bounds in (4) to be sharp, there exists a joint distribution of missing data, $P(r = E|\delta_E = 1)$ and $P(r = D|\delta_D = 1)$, that is consistent with any value of the CICs within the range of (4) for all t and for all $k \in \{E, D\}$ (Manski 2003, p. 13). As pointed out by one referee, the suggested bounds framework is not a conventional way to deal with missing data in the sense of Manski (2003).

Since we are interested in estimating the observable effect of the 1997 reform on unemployment duration, we use these bounds for 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 the treatment and control groups would have followed parallel paths without the reform, the effect of the reform can be estimated by DiD (see also Abadie 2005 for a review of non-parametric identification of DID models)

$$\begin{aligned}\Delta_{Ik}(t|z) = [I_k(t|g_1, p_{t1}, z) - I_k(t|g_0, p_{t1}, z)] - [I_k(t|g_1, p_{t0}, z) \\ - I_k(t|g_0, p_{t0}, z)]\end{aligned} \tag{5}$$

for $k \in \{E, D\}$. Given that we can only identify intervals for the risk-specific CICs, it is straightforward to bound the reform effect by the Lb, $l_{Ik}(t|z)$, and Ub, $u_{Ik}(t|z)$, of $\Delta_{Ik}(t|z)$ similar to Lee and Wilke (2009) as

$$\begin{aligned} l_{Ik}(t|z) &= I_k^{\mathrm{Lb}}(t|g_1, p_{t1}, z) - I_k^{\mathrm{Ub}}(t|g_0, p_{t1}, z) - I_k^{\mathrm{Ub}}(t|g_1, p_{t0}, z) \\ &\quad + I_k^{\mathrm{Lb}}(t|g_0, p_{t0}, z), \quad \text{and} \\ u_{Ik}(t|z) &= I_k^{\mathrm{Ub}}(t|g_1, p_{t1}, z) - I_k^{\mathrm{Lb}}(t|g_0, p_{t1}, z) - I_k^{\mathrm{Lb}}(t|g_1, p_{t0}, z) \\ &\quad + I_k^{\mathrm{Ub}}(t|g_0, p_{t0}, z) \end{aligned} \tag{6}$$

for $k \in \{E, D\}$. Note that the Lb and Ub are sharp (see Appendix C for details). One of the referees mentioned that when the reform effect is defined as a discrete change (as in this article), the marginal effect does not need to be bounded by -1 or 1. Therefore, although a probability change larger than 1 seems to be illogical, it is not necessary to restrict the bounds in (6) to $[-1, 1]$.

As the worst-case bounds in (6) can be rather wide, we now explore reasonable approaches to tighten them in order to obtain informative results. In addition to the monotonicity or independence assumption of Lee and Wilke (2009) (for details see Appendix D), one can make use of the probabilistic decomposition of the CIC to impose more targeted assumptions. From (1) and (5),

$$\Delta_{Ik}(t|z) = \Delta_{Ik}(t, r = k, \delta = 0|z) + \Delta_{Ik}(t, r = k, \delta_k = 1|z). \tag{7}$$

The first part of (7) is point identified and it is

$$\begin{aligned} &\Delta_{Ik}(t, r = k, \delta = 0|z) \\ &\quad = P(T_k \le t, r = k, \delta = 0|g_1, p_{t1}, z) - P(T_k \le t, r = k, \delta = 0|g_0, p_{t1}, z) \\ &\qquad - P(T_k \le t, r = k, \delta = 0|g_1, p_{t0}, z) + P(T_k \le t, r = k, \delta = 0|g_0, p_{t0}, z). \end{aligned} \tag{8}$$

The second part of (7) is

$$
\begin{aligned}
&\Delta_{Ik}(t, r=k, \delta_k=1|z) \\
&\quad = P(T_k \le t, r=k, \delta_k=1|g_1, p_{t1}, z) - P(T_k \le, r=k, \delta_k=1|g_0, p_{t1}, z) \\
&\qquad - P(T_k \le t, r=k, \delta_k=1|g_1, p_{t0}, z) + P(T_k \le t, r=k, \delta_k=1|g_0, p_{t0}, z), \quad (9)
\end{aligned}
$$

which is not identified as the risk type is unknown for incomplete observations. From above, we know that the width of the bounds of the DID changes in (8) and (9) depends on the unknown probability $P(r = k|\delta_k = 1, g, p, z)$. One could use economic reasoning to restrict the feasible range for this probability. Or one could impose some parametric assumption such as, for instance, a decreasing exponential function of the size of the gap $\varsigma_2 - \varsigma_1$. While these approaches can squeeze the bounds to the degree of our discretion, the economic implications of these assumptions are often vague. Another possibility is to impose a shape constraint on the unknown probability. The following independence assumption is rather mild as it leaves the unknown probability function mainly unspecified:

$$
P(r = k|\delta_k = 1, g, p, z) = P(r = k|\delta_k = 1, z) \quad (10)
$$

for $k \in \{E, D\}$. Assumption (10) also implies $P(r = U|\delta = 1, g, p, z) = P(r = U|\delta = 1, z)$ because $P(r = U|\delta_k = 1, g, p, z) = 1 - P(r = k|\delta_k = 1, z)$ for $k \in \{E, D\}$. Although empirically non-testable, this assumption has a straightforward economic meaning. Take $k = E$ as an example. We assume in (10) that the propensity for an incomplete unemployment spell ($\delta_E = 1$) to end with risk E rather than risk U is independent of the period and the age group when z is controlled for. Compared with Fig. 2, $P(r = E|\delta_E = 1, g, p, z)$ is the probability that an incomplete unemployment spell ($\delta_E = 1$) continues at ς_1 but unemployment compensation is no longer claimed until there is a transition to a local job ($r = E$) at ς_2. It is difficult to imagine that this probability varies across our age groups because of their proximity. Also, we are not aware of any other institutional feature that discriminates between these age groups and thus violates the results. It is less easy to justify period independence. While the general business cycle may affect the probability, it may also vary in response to policy changes which affect the attractiveness of unrecorded labor market states such as self employment or training courses. Although we are not aware of any important policy change which may have lead to a violation of this condition, we cannot directly verify this. Instead we have estimated two Probit models for the probability of observing a transition to risk $k \in \{E, D\}$ for the Ub definition of unemployment given that the same spell is incomplete according to the Lb definition. If Assumption (10) was true, it would be reasonable to expect the Ub of the propensity to also be independent of period and age group. In these models we have therefore included g, p, $g * p$ along with a number of individual controls z as independent variables. We found that the estimated coefficients on the former three are statistically insignificant in 5 out of 6 cases and significant at the 5 % level in one case. As the marginal effect of this variable on the probability is small ($< 3\,\%$), we do not obtain evidence for a strong association between the variables of interest and the probability that an unobserved period is terminated by a transition into one of the observed labor market states. Please note that it is not reasonable to perform a similar check for observing a transition for the Lb definition of unemployment because this is not related to transitions out of unobserved periods.

Technically, Assumption (10) imposes cross restrictions on the DiD terms, which precludes less likely scenarios. Typical research on unemployment duration with similar (incomplete) data uses only one definition of unemployment (the Lb or the Ub) for all z. This is therefore a special case of our assumption where each of the four DiD terms in (5) has the same Lb or Ub definition of unemployment for all z. In contrast, Assumption (10) is less restrictive as it still allows for different definitions of unemployment duration for the four DiD terms and for all z.

Similar as the bounds for the CIC, we consider two special case of the unknown probability $P(r = k|\delta_k = 1, g, p, z)$. If $P(r = k|\delta_k = 1, g, p, z) = 0$ for all age groups and periods, we have $P(T_k \leq t, r = k, \delta_k = 1|g, p, z) \leq P(r = k, \delta_k = 1|g, p, z) = 0$. All the four terms in (9) are zero, and (7) is $\Delta^c_{Ik}(t|z) = \Delta_{Ik}(t, r = k, \delta = 0|z)$ as in (8). Otherwise, if $P(r = k|\delta_k = 1, g, p, z) = 1$ for all age groups and periods, we have $P(T_k \leq t, r = k, \delta_k = 1|g, p, z) = P(T_k \leq t, \delta_k = 1|g, p, z)$ for all age groups and periods. They then equal to the CICs for incomplete spells ($\delta_k = 1$). In this case (7) becomes $\Delta^c_{Ik}(t|z) = \Delta_{Ik}(t, r = k, \delta = 0|z) + \Delta_{Ik}(t, \delta_k = 1|z)$ with

$$\begin{aligned}\Delta_{Ik}(t, \delta_k = 1|z) = P(T_k \leq t, \delta_k = 1|g_1, p_{t1}, z) &- P(T_k \leq, \delta_k = 1|g_0, p_{t1}, z)\\ -P(T_k \leq t, \delta_k = 1|g_1, p_{t0}, z)&\\ +P(T_k \leq t, \delta_k = 1|g_0, p_{t0}, z).& \qquad (11)\end{aligned}$$

Let us denote the bounds for (7) under the independence assumption (10) as $\Delta^c_{Ik}(t|z)$. In order to determine $\Delta^c_{Ik}(t|z)$ we minimize and maximize (11) by assigning values to the unknown $P(r = k|\delta_k = 1, z)$. To maximize $\Delta^c_{Ik}(t|z)$ we set $P(r = k|\delta_k = 1, z) = 1$ (the largest possible value) for all t if $\Delta_{Ik}(t, \delta_k = 1|z) \geq 0$. We set $P(r = k|\delta_k = 1, z) = 0$ (the smallest possible value) for all t if $\Delta_{Ik}(t, \delta_k = 1|z) < 0$. The minimum of $\Delta^c_{Ik}(t|z)$ is obtained in the reversed way. Take risk E as an example, the Lb and Ub of $\Delta^c_{Ik}(t|z)$ are,

$$\begin{aligned}&\Delta^{c,\mathrm{Lb}}_{IE}(t|z) = \Delta_{Ik}(t, r = E, \delta = 0|z), \quad \text{and}\\ &\Delta^{c,\mathrm{Ub}}_{IE}(t|z) = \Delta_{Ik}(t, r = E, \delta = 0|z) + |\Delta_{IE}(t, \delta_E = 1|z)| \qquad (12)\end{aligned}$$

respectively, when $\Delta_{IE}(t, \delta_E = 1|z)$ is positive at some t. The Lb and Ub of $\Delta^c_{Ik}(t|z)$ are,

$$\begin{aligned}&\Delta^{c,\mathrm{Lb}}_{IE}(t|z) = \Delta_{Ik}(t, r = E, \delta = 0|z) - |\Delta_{IE}(t, \delta_E = 1|z)|, \quad \text{and}\\ &\Delta^{c,\mathrm{Ub}}_{IE}(t|z) = \Delta_{Ik}(t, r = E, \delta = 0|z) \qquad (13)\end{aligned}$$

respectively, when $\Delta_{IE}(t, \delta_E = 1|z)$ is negative at some other t.

These bounds are sharp for $r \in \{E, D\}$ for all t (see Appendix C). Moreover, the resulting Lb of $\Delta^c_{Ik}(t|z)$ is always smaller than its Ub. The width of the bounds is

$|\Delta_{Ik}(t, \delta_k = 1|z)|$ which is not larger than the width of the worst-case bounds (6). This is because under assumption (10) the bounds for the reform effect are obtained by bounding the DiD changes in (7) rather than directly bounding the CIC as in the case of the worst-case bounds in (6).

The bounds given in (2) and (3) can be estimated non-parametrically using Kaplan–Meier-type estimators, as the censoring time $T_{\max}$ is independent (see Kalbfleisch and Prentice 2002). See Appendix E for details about the estimation procedure. Inference procedures for partially identified models were recently developed in econometrics. For example, Imbens and Manski (2004) and Bontemps et al. (2007) suggest to compute the confidence interval for the parameter of interest instead of the confidence interval of its upper and Lb. In this article, the parameter of interest is restricted by one Lb and one Ub, where the Ub is greater or equal than the Lb. The bounds for the parameter of interest can then be estimated by sample analogue estimation and the bootstrap procedure by Horowitz and Manski (2000) can be used for inference. Accordingly, we apply their bootstrap procedure to estimate the confidence intervals for the Lb and Ub of the DID reform effect for each risk. The bootstrap repetitions are 500.

3.3 Estimation 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 in (6). For identification of the reform effect, the Lb needs to be greater than zero or the Ub 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 estimated bounds contain the value zero (except for very short durations). Moreover, random sampling errors cause much less uncertainty for the results than the missing data problem. Due to having access to a large sample, the random sampling error seems negligible relative to the identification problem caused by the missing data.

As another interesting observation, we find that the width of the resulting bounds is much wider than the interval spanned by the point estimates for the Lb and Up definition of the unemployment duration (see Appendix F). 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 independence assumption given in (10).

Figure 4 shows that the bounds are much tighter under Assumption (10). 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

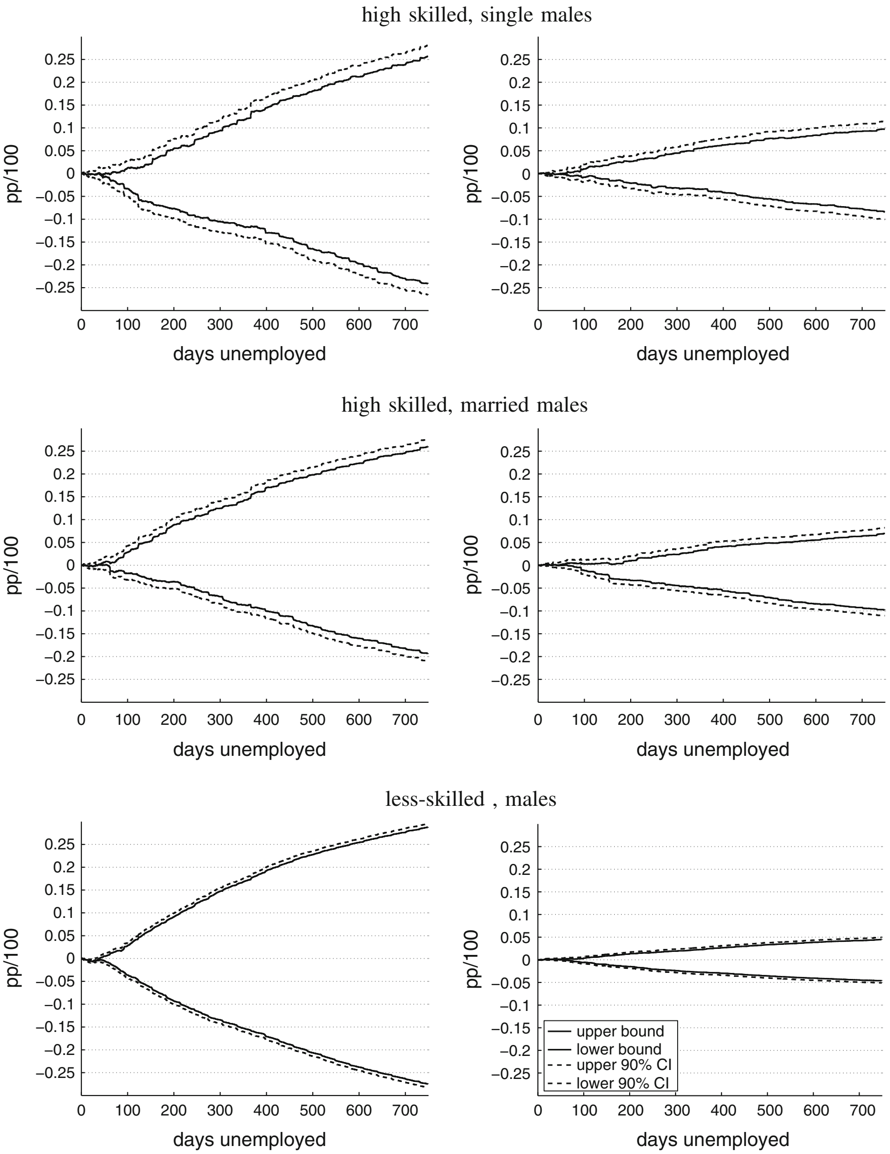


Fig. 3 Lb and Ub of the DiD changes of the cumulative incidence of local (*left*) and distant (*right*) employment among selected groups

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. The effect on the probability of distant job finding is negative, although insignificant and

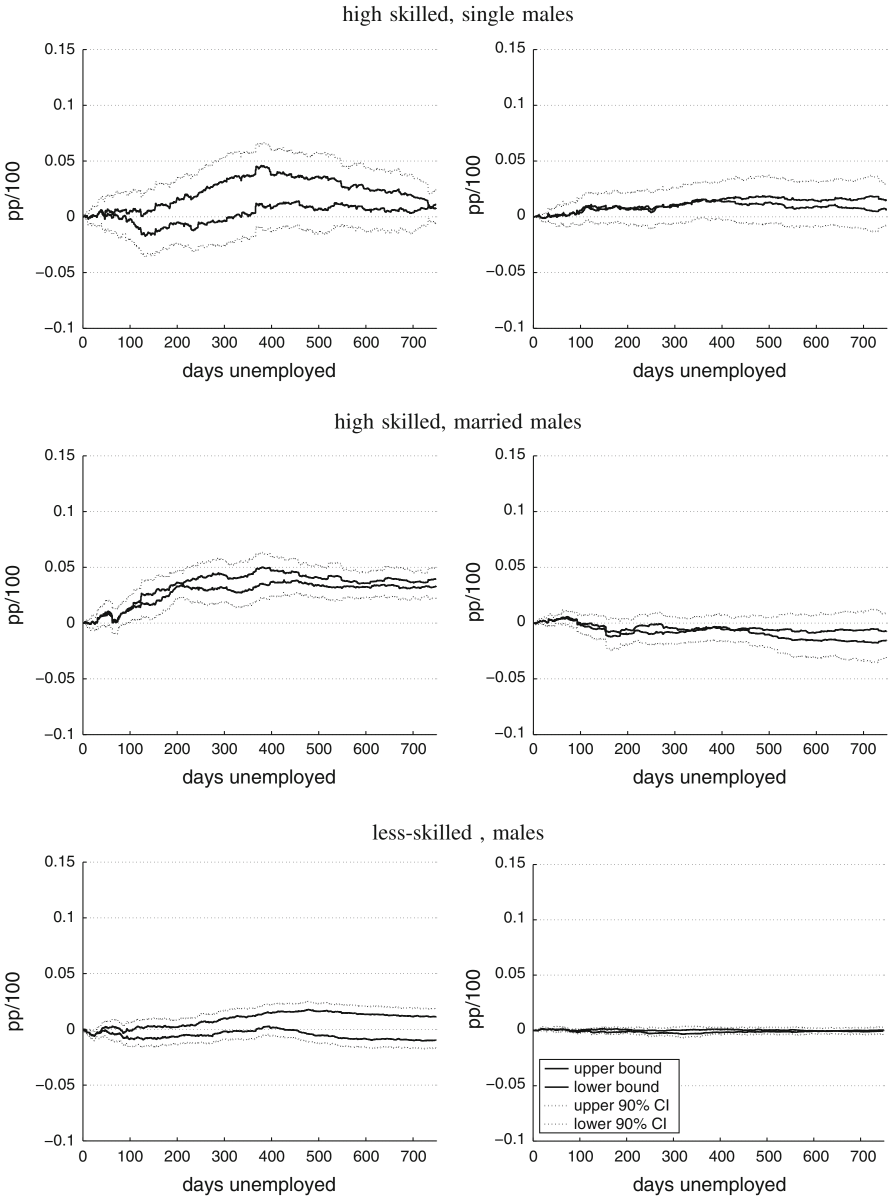


Fig. 4 Lb and Ub of the DiD changes of the cumulative incidence of local (*left*) and non-local (*right*) exits to employment among selected groups, additional assumption

mainly small. 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 individuals with low pre-unemployment wages.

We thus find some interesting evidence that the magnitude of the reform effect critically hinges on the household context. Our results provide new insights on the relevance of the household context for migration decisions. Without reporting the results, we have also estimated the average effect on distant job finding for the high-skilled men and find that the bounds are mainly centered around zero. This may explain why Tatsiramos (2009) did not find evidence for an average effect of UB on migration in Germany.

By comparing changes in cumulative incidence functions we also obtain insights whether longer benefit entitlements lead to longer unemployment periods by delaying the start of a new job. In this case, the effect on the cumulative incidence should be restricted to the interval where benefit entitlements are lost. Although, we do not observe changes for short durations less than 100 days for most groups, the estimated effect does not return to zero after the end of the treatment period. This finding suggests that the extensive unemployment benefit periods result mainly in a reduction of the share of unemployed who take up a new job at all, while the share of unemployed sliding into extreme long-term unemployment increases.

Finally, we would like to comment on the interpretability of the estimated reform effect in this article. 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 (2010) 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

The system of UC has been considered to be one potential explanation for the large variation in interregional migration rates of different groups of individuals across countries. In particular, migration rates in Germany tend to be lower for unemployed jobseekers as compared to direct job movers with migration rates being lowest among unskilled jobseekers for whom the UC system basically offers an unlimited wage replacement rate that may be close to 100 % of the previous wage level.

In order to examine whether the UC system generates disincentives for regional mobility, this article explored the effect of reducing the entitlement length for unemployment benefits on the incidence of finding 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 improve the selection of the estimation sample using a selection criterion that is based on an individual's work history to proxy for unobserved labor market behavior. Moreover, we deal with missing information on certain labor market states in our administrative data which is typical for data sources that serve a particular administrative purpose only. We have thus 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 CIC in a dependent competing risk model. It also derives sharp bounds for the DiD estimator.

Our results confirm that missing data is a big problem in administrative individual data. It can easily preclude any informative result if one is not willing to impose additional assumptions. Many former studies based on similar incomplete duration data do not model the incomplete data problem, but make implicit and non-testable assumptions about the unobservable periods instead. Our analysis thus reveals the severity of identification problems stemming from incomplete duration data. By imposing a reasonable independence assumption, however, we are able to considerably tighten the bounds and obtain several interesting results.

The estimated bounds suggest that shortening the entitlement length for UB only has an impact on observed exit probabilities for high-skilled individuals for whom the threat of entitlement loss after exhausting UB is likely to be largest. We observe a strong and significant increase in the cumulative incidence for local job finding for high-skilled married men while it is not significant for high-skilled single men. The cumulative incidence for migration increases for singles while it decreases for married men without being significant. These findings suggest that both conflicting theories with regard to the effect of UB on migration may have some validity, but that it depends on the household context and the corresponding migration costs which effect dominates.

For less-skilled individuals, we do not find any significant changes in observed exit probabilities that are related to the shortening of the entitlement length for UB. However, this is not surprising since the threat of entitlement loss after exhausting UB for this group should be rather small given the chance to receive the means-tested UA plus supplementary social benefits. For this reason, insignificant reform effects do not suggest that a cut in their entitlements would not have any mobilizing effect on this group. In fact, the significant effects for high-skilled individuals for whom there is a substantial treatment suggest that this is likely the case. However, our findings also indicate that the outcome of such cuts would strongly depend on the family background. At least for singles with low migration costs, however, the UC system in Germany may indeed hamper mobility and may thus partially be responsible for low mobility rates among certain groups of workers.

Since our analysis focuses on cumulative incidences, it is not directly informative about changes in marginal distributions of latent variables. As these marginal distributions are not point identified without additional assumptions, it would require the application of double bounds due to the non-identifiability of the model and the missing data problem. This could be achieved, for example, by combining the regression frameworks of Honoré and Lleras-Muney (2006) or Lo and Wilke (2011) with the missing data problem of this article.

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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.

Appendix A: computation of PUBD and counterfactual PUBD

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 (extended) claim period is a backward looking concept and encompasses at most (seven) 3 years preceding the current UB claim. Both periods are shortened to the time when the previous UB claim finished if this happened within the (seven) 3 years of the (extended) claim period.

The basic PUBD corresponds to 6 months and applies to all claimants who do not fulfill the criteria for an extended PUBD, but worked for at least 1 year within the claim period. For a claimant to fulfill the sufficient condition to receive a PUBD of more than 6 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 7 years. His last and only UB claim, however, was made only 12 months ago and lasted for 6 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 6 months at the end of his previous UB claim. Despite having accumulated 30 months of employment within the last 7 years, both the claim period and the extended claim period are shortened to only 6 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 7 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 6 months and he has a remainder of 6 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 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 4 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 5 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.

Appendix B

Table 6 Descriptive summary of sample characteristics, IAB data (final sample)

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

Appendix C: sharpness of bounds

1. Sharpness of (4) We prove this by finding the joint distribution of missing data, $P(r = E|\delta_E = 1)$ and $P(r = D|\delta_D = 1)$, that makes the CICs for all t and for all $k \in \{E, D\}$ attaining any values within the ranges of (4) (Manski 2003, p. 13). First, $P(T_k \leq t, r = k, \delta_k = 1)$ in (1) is a strictly increasing function of $P(r = k, \delta_k = 1)$. And as we have incomplete duration in the data, i.e., $P(\delta_k = 1) > 0$, $P(r = k, \delta_k = 1)$ is again a strictly increasing function of $P(r = k|\delta_k = 1)$. It is straightforward to see that there is value of $P(r = k|\delta_k = 1) \in [0, 1]$ that allows $I_k(t)$ to attain each value in (4). The remaining issue is whether the CIC of risk E and D jointly cover the full range of (4). We first consider the boundary cases. It is clear that when $[P(r = E|\delta_E = 1), P(r = D|\delta_D = 1)] = [1, 1]$, $I_E(t)$ and $I_D(t)$ equal to their Ub. Similarly, when $[P(r = E|\delta_E = 1), P(r = D|\delta_D = 1)] = [0, 0]$, $I_E(t)$ and $I_D(t)$ equal to their Lbs. When $[P(r = E|\delta_E = 1), P(r = D|\delta_D = 1)] = [1, 0]$, $I_E(t)$ equals to its Ub and $I_D(t)$ equals to its Lb. When $[P(r = E|\delta_E = 1), P(r = D|\delta_D = 1)] = [0, 1]$, $I_E(t)$ equals to its Lb and $I_D(t)$ equals to its Ub. Next, as $I_E(t)$ and $I_D(t)$ in (1) are strictly increasing in $P(r = E|\delta_E = 1)$ and $P(r = D|\delta_D = 1)$, there are corresponding pair of $[P(r = E|\delta_E = 1), P(r = D|\delta_D = 1)] \in (0, 1)^2$ for each pair of $[I_E(t), I_D(t)]$ laying inside (4). Because the events $\delta_E = 1$ and $\delta_D = 1$ are mutually exclusive and there are no cross-restrictions on the unknown $P(r = E|\delta_E = 1)$ and $P(r = D|\delta_D = 1)$ in the worst case bounds, all the joint distributions of $[P(r = E|\delta_E = 1), P(r = D|\delta_D = 1)]$ described above are feasible. This completes the proof.

2. Sharpness of (6). We show in paragraph 1 of this Appendix that bounds in (4) are sharp for $r = E, D$ and for all t. This result can be easily extended to (6). We do this by showing that there are distributions of the missing data which are compatible with the Lb and Ub. For the case of the Lb, it is required that $I_k(t|g_1, p_{t1}, z)$ and $I_k(t|g_0, p_{t0}, z)$ attain their lower bound value while $I_k(t|g_0, p_{t1}, z)$ and $I_k(t|g_1, p_{t0}, z)$ attain their Ub values. This is possible when $P(r = k|\delta_k = 1, g_1, p_{t1}, z) = P(r = k|\delta_k = 1, g_0, p_{t0}, z) = 0$ and $P(r = k|\delta_k = 1, g_1, p_{t0}, z) = P(r = k|\delta_k = 1, g_0, p_{t1}, z) = 1$. Such distributions are jointly feasible as there is no restricion on the relationship between these probabilities. The proof for the Ub is similar. It is straightforward to find a combination of the unknown probability $P(r = k|\delta_k = 1, g, p, z)$ that generates each of the other values within the Lb and the Ub as the CICs are all strictly increasing function of $P(r = k|\delta_k = 1, g, p, z)$ and all of the unknown probability $P(r = k|\delta_k = 1, g, p, z)$ is free to vary. This completes the proof.

3. Sharpness of (12) **and** (13). We show that the bounds of (7) under Assumption (10) are sharp by showing that there are distributions of the missing data which attain all the feasible values within the bounds. The bounds for (7) under (10) are

$$\Delta^{c,\mathrm{Lb}}_{Ik}(t|z) = \begin{cases} \Delta_{Ik}(t, r = k, \delta = 0|z), & \text{if } \Delta_{Ik}(t, \delta_k = 1|z) \geq 0 \\ \Delta_{Ik}(t, r = k, \delta = 0|z) - |\Delta_{Ik}(t, \delta_k = 1|z)|, & \text{otherwise.} \end{cases}$$

$$\Delta^{c,\mathrm{Ub}}_{Ik}(t|z) = \begin{cases} \Delta_{Ik}(t, r = k, \delta = 0|z) + |\Delta_{Ik}(t, \delta_k = 1|z)|, & \text{if } \Delta_{Ik}(t, \delta_k = 1|z) \geq 0 \\ \Delta_{Ik}(t, r = k, \delta = 0|z), & \text{otherwise.} \end{cases}$$

We now state the distributions of the data which jointly provide $\Delta_{I_E}^{c,\text{Lb}}(t|z)$ and $\Delta_{I_D}^{c,\text{Lb}}(t|z)$. We need $P(r = E|\delta_E = 1, z) = 0$ and $P(r = D|\delta_D = 1, z) = 0$ if $\Delta_{I_E}(t, \delta_E = 1|z) \geq 0$ and $\Delta_{I_D}(t, \delta_D = 1|z) \geq 0$; $P(r = E|\delta_E = 1, z) = 1$ and $P(r = D|\delta_D = 1, z) = 0$ if $\Delta_{I_E}(t, \delta_E = 1|z) < 0$ and $\Delta_{I_D}(t, \delta_D = 1|z) \geq 0$; $P(r = E|\delta_E = 1, z) = 0$ and $P(r = D|\delta_D = 1, z) = 1$ if $\Delta_{I_E}(t, \delta_E = 1|z) \geq 0$ and $\Delta_{I_D}(t, \delta_D = 1|z) < 0$; and finally $P(r = E|\delta_E = 1, z) = 1$ and $P(r = D|\delta_D = 1, z) = 1$ if $\Delta_{I_E}(t, \delta_E = 1|z) < 0$ and $\Delta_{I_D}(t, \delta_D = 1|z) < 0$. As there is no cross restriction on $P(r = E|\delta_E = 1, z)$ and $P(r = D|\delta_D = 1, z)$ and event δ_E and δ_D are mutually exclusive, these combinations of probabilities exist. The cases of $\Delta_{I_E}^{c,\text{Ub}}(t|z)$ and $\Delta_{I_E}^{c,\text{Ub}}(t|z)$ are similar. By substituting different values of $P(r = E|\delta_E = 1, z)$ and $P(r = D|\delta_D = 1, z)$, a similar approach can be used to prove all other cases, e.g., Lbs combined with the Ub, or other combinations within the sharp bounds. This completes the proof.

Appendix D: the independence and monotonicity assumptions

Lee and Wilke (2009) impose two assumptions to tighten the bounds. Under the independence assumption, the reform effect $\Delta_{Ik}(t|z)$ for $k = E, D$ in (5) does not depend on the calender time. By estimating the reform for samples stratified by calender 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 Lb in (6) 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 Ub can be reduced analogously.

Appendix E: non-parametric estimation and inference

Let $t_0 < \cdots < t_j < \cdots < t_J$ 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 Lb (2). There are d_{kj}^{Lb} observed exits to risk type $k \neq U$ at time t_j; d_{cj}^{Lb} observed realizations of ς_1 at t_j; and d_{mj}^{Lb} censored observations at t_j. For $k \neq U$, which are given by

$$d_{kj}^{\text{Lb}} = \sum_{i=1}^{n} \mathbb{1}(T_{ik} = \min\{T_{ik}, \varsigma_{1i}, T_{i,\max}\}, T_{ik} = t_j)$$

$$d_{cj}^{\text{Lb}} = \sum_{i=1}^{n} \mathbb{1}(\varsigma_{1i} = \min\{T_{ik}, \varsigma_{1i}, T_{i,\max}\}, \varsigma_{1i} = t_j)$$

$$d_{mj}^{\text{Lb}} = \sum_{i=1}^{n} \mathbb{1}(t_{i,\max} = \min\{T_{ik}, \varsigma_{1i}, T_{i,\max}\}, t_{i,\max} = t_j)$$

with $\mathbb{1}(Y)$ is the indicator function of the event Y. Note that these numbers can be also computed conditional on the observed covariates by restricting the sample accordingly.

For the estimation of the Ub (3) we have to compute equivalent numbers, although ς_1 can be ignored in this case and we have $T_r = \varsigma_2$. We define d_{kj}^{Ub} and d_{mj}^{Ub} as

$$d_{kj}^{\mathrm{Ub}} = \sum_{i=1}^{n} \mathbb{1}(T_{ik} = \min\{T_{ik}, T_{i,\max}\}, T_{ik} = t_j)$$
$$d_{mj}^{\mathrm{Ub}} = \sum_{i=1}^{n} \mathbb{1}(T_{i,\max} = \min\{T_{ik}, T_{i,\max}\}, T_{i,\max} = t_j).$$

Let $d_j^{\mathrm{Lb}} = \sum_{k=E,D} d_{kj}^{\mathrm{Lb}} + d_{cj}^{\mathrm{Lb}} + d_{mj}^{\mathrm{Lb}}$ and $d_j^{\mathrm{Ub}} = \sum_{k=E,D} d_{kj}^{\mathrm{Ub}} + d_{mj}^{\mathrm{Ub}}$. The number of observations at risk just before t_j is then given by

$$n_j^{\mathrm{Lb}} = d_j^{\mathrm{Lb}} + \cdots + d_J^{\mathrm{Lb}} \quad \text{and} \quad n_j^{\mathrm{Ub}} = d_j^{\mathrm{Ub}} + \cdots + d_J^{\mathrm{Ub}}.$$

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

$$\begin{aligned}
\hat{\lambda}_k^b(t_j|x) &= d_{kj}^b / n_j^b \quad \text{with} \quad b \in \{\text{Lb,Ub}\} \quad \text{and} \quad \hat{\lambda}_c^{\mathrm{Lb}}(t_j|x) = d_{cj}^{\mathrm{Lb}} / n_j^{\mathrm{Lb}};\\
\hat{S}^{\mathrm{Lb}}(t_j|x) &= \prod_{u=1}^{j-1} \left(1 - \sum_{k=E,D} \hat{\lambda}_k^{\mathrm{Lb}}(t_u) - \hat{\lambda}_c^{\mathrm{Lb}}(t_u)\right) \quad \text{and} \quad \hat{S}^{\mathrm{Ub}}(t_j|x)\\
&= \prod_{u=1}^{j-1} \left(1 - \sum_{k=E,D} \hat{\lambda}_k^{\mathrm{Ub}}(t_u)\right).
\end{aligned} \tag{14}$$

Note that that these estimators are consistent as the right censoring is independent. A consistent estimator for the bounds given in (2) and (3) is then ($k \neq U$)

$$\hat{I}_k^b(t_j|x) = \sum_{u=1}^{j} \hat{\lambda}_k^b(t_u|x) \hat{S}^b(t_u|x) \quad \text{with} \quad b \in \{\text{Lb,Ub}\}. \tag{15}$$

Appendix F

The point estimates for the reform effect in Fig. 5 are obtained using the following formulas:

$$\begin{aligned}
l_{Ik}(t_j|p_{t0}, p_{t1}, z) &= I_k^{\mathrm{Lb}}(t_j|g_1, p_{t1}, z) - I_k^{\mathrm{Lb}}(t_j|g_0, p_{t1}, z) - I_k^{\mathrm{Lb}}(t_j|g_1, p_{t0}, z)\\
&\quad + I_k^{\mathrm{Lb}}(t_j|g_0, p_{t0}, z),\\
u_{Ik}(t_j|p_{t0}, p_{t1}, z) &= I_k^{\mathrm{Ub}}(t_j|g_1, p_{t1}, z) - I_k^{\mathrm{Ub}}(t_j|g_0, p_{t1}, z) - I_k^{\mathrm{Ub}}(t_j|g_1, p_{t0}, z)\\
&\quad + I_k^{\mathrm{Ub}}(t_j|g_0, p_{t0}, z)
\end{aligned}$$

for $k = E, D$.

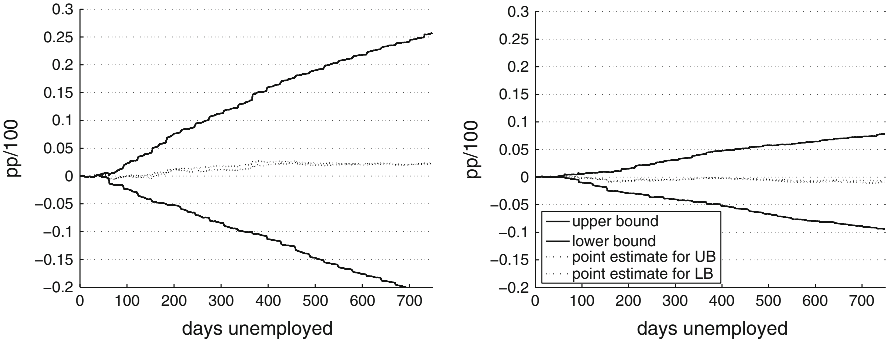

Fig. 5 Point estimates for Lb and Ub of reform effect on the cumulative incidence of local (*left*) and distant (*right*) exits to employment among high-skilled unemployed, married males

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