REFORM OF UNEMPLOYMENT COMPENSATION IN GERMANY: A NONPARAMETRIC BOUNDS ANALYSIS USING REGISTER DATA

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Abstract

Economic theory suggests that an extension of the maximum length of entitlement for unemployment benefits increases the duration of unemployment. Empirical results for the reform of the unemployment compensation system in Germany during the 1980s are less clear. The analysis in this paper is motivated by the controversial empirical findings and by recent developments in econometrics for partial identification. We use extensive administrative data with the drawback that registered unemployment is not directly observed. For this reason we bound the reform effect on unemployment duration over different definitions of unemployment. By exploiting the richness of the data we use a nonparametric approach without imposing critical parametric model assumptions. We identify a systematic increase in unemployment duration in response to the reform in samples that amount to less than 15% of the unemployment spells for the treatment group.

Keywords: unemployment duration, definition of unemployment, nonparametric bounds analysis, (quantile-) treatment effect

JEL: C14, C41, J64, J65

1 Introduction

Many empirical contributions consider the question whether unemployment durations increase with the entitlement length for unemployment benefits. This is suggested by economic theory which also predicts an increase with the level of the unemployment compensation. See Katz and Meyer (1990) for a summary. Some empirical evidence for that is observed for the US (Katz and Meyer, 1990) and for the UK (van den Berg, 1990).

In Germany the maximum entitlement length for unemployment benefits for the older employees was increased during the 1980s. This paper pre-investigates the reduction in maximum entitlement periods for unemployment benefits that will come into force in 2006, which basically withdraws the former reform. The reform of the 1980s is therefore highly relevant for possible outcomes of recent labour market reforms in Germany. It presents a unique opportunity to identify the effect of an increase in the maximum entitlement length in a natural experiment set-up since it only affects some groups (42 years old and older) of the population. It was already subject to several empirical investigations, see Biewen and Wilke (2005) for a summary. However, the only noncontroversial finding to date is that it was creating the conditions for massive early retirement at the expense of the unemployment insurance system. Both employers and older employees agreed to early retirement packages, thus negating the greater dismissal protection for older employees with long-term company affiliation. This typical win-win situation (Fitzenberger and Wilke, 2004) and additional costs due to the high unemployment in East Germany generated an enormous burden for the social security systems in Germany, which are nowadays close to collapse. However, the results are less clear when one focuses on the group of older unemployed who have not taken early retirement, i.e. who are still looking for new jobs. Empirical studies using household panel survey data do not have conclusive findings. Schneider and Hujer (1997) do not find increases in unemployment duration, whereas Hunt (1995) and Hujer and Schneider (1995) report such increases for some age groups. Using register data, Plaßmann (2002) finds strong effects but she ignores the early retirement issue. Fitzenberger and Wilke (2004) obtain rather different results for two definitions of unemployment. In particular using nonparametric techniques they find that unemployment duration of those who enter employment again seem not increase in response to the reform. Biewen and Wilke (2005) apply a series of semiparametric single-spell duration models to the same data and they identify an increase in unemployment duration for males aged under 49 but it remains unclear whether this is in response to the reform or due to changes in labour market conditions. For females they do not observe an increase at all. They conclude that further research is necessary.

The analysis in this paper is motivated by these controversial findings and by recent developments in econometrics for partial identification. The purpose of this paper is to revisit the analysis of the above mentioned papers by bounding the effect of the reform of the unemployment compensation system over different definitions of unemployment. We aim to gain robust insights into the extent to which the conducted reform in West Germany has increased unemployment spells by exploiting the extreme richness of the register-based data. In particular, we use a nonparametric approach in order to bound the reform effect on unemployment duration over different definitions of unemployment without imposing critical parametric model assumptions such as the proportionality of hazard rates. We identify a systematic increase in unemployment duration in response to the reform in samples that amount to less than 15% of the unemployment spells for the treatment group.

The paper is organized as follows. Section 2 provides a brief description of the reform and data. Section 3 describes our estimation strategy and Section 4 discusses our empirical findings. Section 5 concludes.

2 Data and Institutions

A comprehensive summary of the changes in the German unemployment compensation system can be found in Hunt (1995) and Plaßmann (2002). Details are therefore not presented here. For our estimations we use the IAB employment subsample (IABS) 1975-1997 which contains daily information about employment periods of about 500K individuals in West Germany. The data is a representative 1% sample of the socially insured workforce in Germany. For a general description of the data see Bender et. al (2000). A general advantage of this data is the large sample size and the daily register-based records which are assumed to be more precise than household interview- based data. A disadvantage of the IABS is the small number of observed variables and the missing information about registered unemployment, since only information about the receipt of unemployment compensation from the German federal labour office is observed. Until 2004 these were unemployment benefits (UB, Arbeitslosengeld)[1], unemployment assistance (UA, Arbeitslosenhilfe)[2] or income maintenance during further training (IMT, Unterhaltsgeld). For this reason Fitzenberger and Wilke (2004) proxy unemployment with two definitions. They introduce the nonemployment (NE) proxy as an upper bound for the unemployment duration and the unemployment between jobs (UBJ) proxy as a lower bound.

[1]Hunt (1995) refers to this as unemployment insurance (ALG).

[2]Hunt (1995) uses the abbreviation ALH.

In their analysis it is evident that the results strongly depend on the definition of unemployment.

The analysis in this paper intends to bound the effect of the reform of the unemployment compensation system over the proxies of unemployment that are extracted from the data. For this purpose we use the NE proxy of Fitzenberger and Wilke (2004) as the upper bound:

- **Nonemployment** (NE): all periods of nonemployment after an employment period which contain at least one period with income transfers by the German federal labour office. The nonemployment period is considered as censored if the last record involves a UB, UA, or IMT payment that is not followed by an employment spell.[3]

In this case we do not know whether the individual is still unemployed, out of the labour force or maybe self-employed. With this definition of unemployment we include the periods of nonemployment (out of the labour force, social benefits) which are not explicitly recorded in the data. This seems to be a natural approach since we cannot distinguish unemployment spells from periods of out of the labour market. It is therefore an upward biased proxy of the true unemployment duration. On the contrary we consider two proxies for the lower bound of unemployment duration: UBJ and UPIT, which are as follows:

- **Unemployment between jobs** (UBJ): all periods of nonemployment between two employment spells if there is a permanent flow of UB, UA, or IMT

[3]A nonemployment spell is treated as right censored if it is not fully observed.

payments. Interruptions of these payments can be up to four weeks – in the case of cut–off times[4]: six weeks. With this definition it is ensured that the individuals are continuously registered as unemployed. Note that in this sample many registered unemployed, who never exit again to employment, have an UBJ duration of length of 0. This is often the case for long term unemployed.

- **Unemployment with permanent income transfers** (UPIT): all periods of nonemployment after an employment period with a continuous flow of unemployment compensation from the German federal labour office. Maximum interruption in compensation transfers is one month – in the case of cut–off times: six weeks. An observation is marked as right censored at the last day of the duration before the transfers are interrupted for more than one month or in the event of there being no observation after the last compensation transfer.

We introduce the UPIT proxy because the UBJ proxy may be too narrow for our purposes. This is mainly because the latter conditions on the future exit to employment. This is a valuable property for the identification of the increase in early retirement as undertaken by Fitzenberger and Wilke (2004) but in our analysis we may lose too much information, in particular for all individuals who do not enter employment any more. This may prevent us from obtaining tight bounds for the treatment effect. In any case we have $UBJ \leq UPIT \leq NE$.

[4]Cut-off times sanction unemployed who have quit a job voluntarily, who reject acceptable job offers, who abort training measures or who do not comply with other regular responsibilities. For more details and empirical evidence about sanctions for the unemployed in West Germany see Wilke (2004).

Figure 1 presents three common samples of the data structure. In case A all proxies yield the same length for the unemployment duration: $t_2 - t_0$. In case B we obtain $UBJ = 0$, $UPIT = t_1 - t_0$ (right censored) and $NE = t_2 - t_0$ if the length of the non observed period is greater than one month otherwise we obtain case A. In case C we have $UBJ = 0$ and $UPIT = NE = t_1 - t_0$ (right censored).

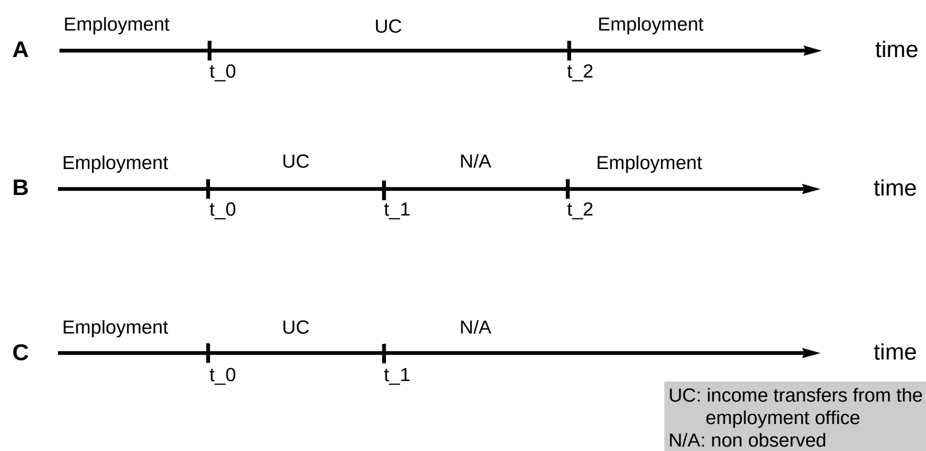

Figure 1: Three common examples of the data structure.

There is another important difference between the construction of our samples and the samples used in Fitzenberger and Wilke (2004). The latter extract samples of different size for their estimations. Their estimates may therefore be affected by sample selection issues. We make provision for that by comparing exactly the same samples. By construction UBJ and UPIT durations are less or equal to NE durations. In some cases a NE duration is not included in the UBJ and/or the UPIT sample. These observations are then added to UBJ and/or UPIT as a non censored zero duration. This corresponds to an observed zero length unemployment duration which is the natural lower bound. This implies that there exists a UBJ and UPIT duration for any NE duration.

In Germany, socially insured employees with a sufficient amount of working experience are entitled to unemployment benefits.[5] The length of the entitlement period depends on the length of the employment periods before the beginning of the unemployment period and on the age of the unemployed person. The maximum entitlement length for unemployment benefits was increased during the years 1985-1987. See table 1 in Hunt (1995) for an overview. For our analysis we classify the calendar years 1981-1988 into three categories:

- pre reform period: 1981-1983
- reform period: 1984-1986
- post reform period: 1987-1988

1984 is considered as a reform year because unemployment spells starting in 1983 are the latest not affected at all by the reform. The entitlement length in many spells starting in 1984 were extended in 1985 after the reform came into force. Anticipation behaviour in 1984 may also affect our estimation results. Years before 1981 are not considered because of data quality issues[6]. As post reform years we use 1987 - 1988 (2 years). 1987 is included because the post reform system already applies to most of the unemployment spells starting in 1987. Years after 1988 are not considered because of the systematic changes in labour market conditions during and after

[5]See Hunt (1995) for more details.

[6]The information on transfer payments seems to be incomplete in the data, see Bender et al. (1996) for details.

German unification.[7]

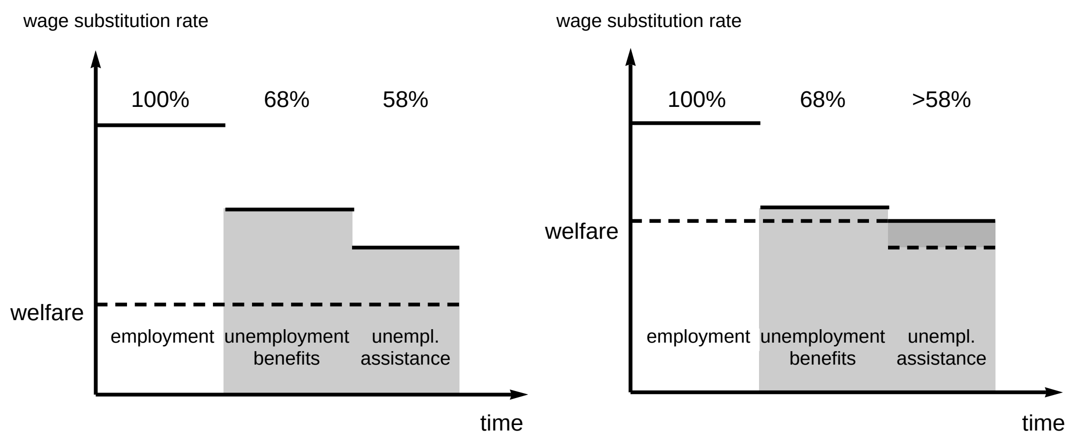

Figure 2: The level of income transfers in Germany is never below the welfare level: example for high (left) and low (right) pre-unemployment wages (where there are children involved).

It is also important to note that the extension of the maximum entitlement lengths has different implications for the unemployed depending on the levels of income transfers during the unemployment duration. The wage replacement rate for unemployment benefits (unemployment assistance) depends on previous earnings.[8] Unemployed persons with low pre-unemployment income may therefore obtain social benefits as additional income transfers. This is the case if income transfers from the employment offices are not high enough to cover the basic needs of the household. Households (and not individuals) are eligible for social benefits which are means

[7]We did estimations for different sets of post reform years until 1994. Figures are available on request.

[8]In addition, unemployment assistance is means tested, i.e. it decreases with the income generated by other household members and in some cases it depends on expected earnings.

tested and the level depends mainly on the community and on the demographic structure of the household. Any form of welfare support is paid by the communities and it is not observable in the data. If transfers from the employment office plus other household income is below this level the household is entitled to welfare support. The reform should therefore have a smaller effect on those with low pre-unemployment earnings because an increase in unemployment compensation would simultaneously decrease the level of additional social benefits resulting in a zero or very small net change. See figure 2 (right). Since we do not observe any receipt of welfare in the data, we can only try to make provision for that by using the level of pre-unemployment income. The same reasoning applies to individuals with high former income levels. See figure 2 (left). We may expect stronger reform effects for this group. The reform under consideration therefore implies a weak increase of the unemployment compensation level after twelve months unemployment duration. Unfortunately, we do not observe the level of unemployment compensation paid by the employment offices which leaves us the pre-unemployment earnings and the type of income transfers from the employment offices as the only observable determinants for the wage replacement rate.[9] For this reason we construct dummy variables indicating whether the pre-unemployment wage is located in the bottom (top) three (two) quintiles of the IABS population income distribution of full time employees in the year when the unemployment spells begins. The bottom (top) quintiles are

[9]The wage replacement rate also depends on whether the claimant has dependent children. Information about children is unreliable in the data and not available at all before 1983. For this reason we decided to ignore it in the analysis.

referred to as low (high) pre-unemployment earnings in this paper.

We use individuals aged 36-41 as the control group in our analysis. These are the oldest individuals not affected by the reform. We select the individuals aged 44-48 as the treatment group. This is done for the following reasons: those aged 42-43 are excluded because the short extension of the maximum entitlement length implies a weak treatment for this group. Aged >48 are not considered because Fitzenberger and Wilke (2004) find there is already some evidence that early retirement starts within the age group 49-53 and we want to focus our analysis on individuals still looking for jobs. During the reform under consideration the maximum entitlement length for unemployment benefits increased from 12 to 22 months for the treatment group, whereby it remained constant for the control group.

For our empirical analysis we construct a sample of unemployment periods that is homogenous with respect to the work history of the individuals[10] in order to reduce sample selection issues at the inflow level to unemployment and to reduce the degree of unobserved components that may affect our nonparametric results. In addition the sample is chosen such that the individuals have long entitlement periods for unemployment benefits. In particular we restrict our sample to:[11]

- periods with unemployment benefits as first income transfer
- no receipt of any unemployment transfer during the past 12 months before the

[10]Using censored quantile regressions, Lüdemann et al. (2004) observe that work history variables have a strong explanatory degree for the length of unemployment duration in West Germany.

[11]We do not impose restrictions on the educational degree because in our analysis we find similar results for educational groups. For this reason we use a pooled sample.

current unemployment period

- no recall to the former employer after the last unemployment period

- the business sector of "agriculture" is excluded (last employment)

We do not observe the maximum entitlement length for unemployment benefits in the data and a construction of such a variable is laborious. For this reason we use the simple rule that the unemployed did not receive any unemployment compensation within the year prior to unemployment. This does not ensure that the unemployed persons actually do have maximum entitlement for unemployment compensation but we found that the median length of employment before unemployment is in the range of three years. This would imply a median entitlement length of about 18 months for the treatment group. The inclusion of individuals with shorter entitlement lengths results in a downward bias of the reform effect and at the same time the importance of the reform decreases since even fewer individuals get the maximum treatment. Tables 1 and 2 present the summary statistics for the pre and post reform samples. In total we have $9,631$ unemployment spells in our sample of which $6,566$ (68%) are recorded during the pre reform period. By definition the length of UBJ is shortest and the length of NE is longest. We observe that the average length of UPIT spells is about twice the length of UBJ and average NE length is about twice UPIT. We observe that median length of UBJ has decreased in the post reform years, UPIT remained almost unchanged whereby the median for NE duration increased, in particular for the treatment population. Interestingly, the median UBJ spell length for the treatment group in the post reform years is close to zero. This means that almost 50% of the unemployment spells do not meet the requirement for UBJ. For this reason we cannot expect high identification power by using UBJ. Just by looking at these crude numbers one may expect that the reform effect possibly varies across the unemployment proxies which motivates our analysis.

3 Econometric Framework

This section describes an econometric approach used in the paper. Our framework is based on bounds analysis (see a monograph by Manski (2003) for a review). In particular, we present bounds for treatment effects in the context of difference-in-differences. We also obtain tighter bounds using some plausible independence and monotonicity assumptions.[12] There are no new ideas in our econometric framework; however, details of bounds analysis are newly developed to analyse difference-in-differences-type treatment effects under a natural experiment.[13]

To describe our econometric model, assume that we observe interval data on the duration variable of interest, say Y. That is, we observe Y_1 and Y_2, where $Y_1 \leq Y_2$, and it is only known that latent duration Y is between Y_1 and Y_2. For example, if $Y_1 = Y_2$, then observed duration is a point and equal to Y; however, in general, we have $Y_1 < Y_2$, then Y is in the interval between Y_1 and Y_2. In our application, Y is

[12]See, for example, Manski and Pepper (2000) and Blundell, Gosling, Ichimura, and Meghir (2004) for implications of imposing some credible assumptions.

[13]See Honoré and Lleras-Muney (2004) for an application of bounds analysis to duration analysis in the context of competing risks models. See also Manski (1990, 1997) and Lechner (1999) for nonparametric bounds of treatment effects.

the unemployment spell, Y_1 is either UBJ or UPIT, and Y_2 is NE.

We consider two types of treatment effects, one on the survival probability of Y and the other on the quantiles of Y conditional on explanatory variables X. For simplicity, we assume that X is a vector of discrete random variables. Both treatment effects are defined as difference-in-differences (DID) in terms of survival probability and quantiles, respectively. It is plausible that the DID estimates can be regarded as treatment effects since the reform we consider can be thought of as a natural experiment.

First, we present bounds for the treatment effects in terms of survival probability. To do so, let P denote time periods p_{t0} and p_{t1} (before and after a treatment) and T denote age groups 0 and 1 (control and treatment groups). In our application, $p_{t0} = 1981, 1982, 1983$ and $p_{t1} = 1987, 1988$. Also, age group 0 consists of individuals aged 36-41 and age group 1 is composed of individuals aged 44-48. We define the effect of a reform to be

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

where $S(y|t, p, x) = P(Y > y|T = t, P = p, X = x)$. If Y were observed, then the treatment effect could be estimated by a sample analogue to (1). Obviously, this is infeasible since we have only interval data on Y. A natural approach is to bound $\Delta(y|x, p_{t0}, p_{t1})$ by combining bounds for four survival probabilities.

Define $S_1(y|t, p, x) = P(Y_1 > y|T = t, P = p, X = x)$, and $S_2(y|t, p, x) = P(Y_2 > y|T = t, P = p, X = x)$. Without imposing additional conditions, then the identification region for $S(y|t,p,x)$ is

$$S_1(y|t,p,x) \leq S(y|t,p,x) \leq S_2(y|t,p,x) \tag{2}$$

for $t = 0, 1$ and $p = p_{t0}, p_{t1}$. This is a worst case bound for $S(y|t,p,x)$. Since there are no cross restrictions over time periods and age groups, equation (2) implies that

$$\begin{aligned} S_1(y|1,p_{t1},x) - S_2(y|0,p_{t1},x) &\leq S(y|1,p_{t1},x) - S(y|0,p_{t1},x) \\ &\leq S_2(y|1,p_{t1},x) - S_1(y|0,p_{t1},x) \end{aligned}$$

and

$$\begin{aligned} S_1(y|1,p_{t0},x) - S_2(y|0,p_{t0},x) &\leq S(y|1,p_{t0},x) - S(y|0,p_{t0},x) \\ &\leq S_2(y|1,p_{t0},x) - S_1(y|0,p_{t0},x), \end{aligned}$$

which, in turn, implies that $\Delta(y|x,p_{t0},p_{t1})$ is bounded by an interval with endpoints $[l(y|x,p_{t0},p_{t1}), u(y|x,p_{t0},p_{t1})]$:

$$\begin{aligned} l(y|x,p_{t0},p_{t1}) = \max[-1, &\{S_1(y|1,p_{t1},x) - S_2(y|0,p_{t1},x)\} \\ &- \{S_2(y|1,p_{t0},x) - S_1(y|0,p_{t0},x)\}] \end{aligned} \tag{3}$$

and

$$\begin{aligned} u(y|x,p_{t0},p_{t1}) = \min[1, &\{S_2(y|1,p_{t1},x) - S_1(y|0,p_{t1},x)\} \\ &- \{S_1(y|1,p_{t0},x) - S_2(y|0,p_{t0},x)\}]. \end{aligned} \tag{4}$$

Note that the lower and upper bounds are restricted to be between -1 and 1. This is due to the fact that maximum variation of the survival probability cannot be larger than 1 in absolute values. If this interval is shorter than $[-1, 1]$, there is identifying power. In particular, if the lower bound is greater than zero or the upper bound is smaller than zero, then one can identify signs of the effect.

Sample analogue estimation of these bounds is straightforward. In most cases, Y_1 and Y_2 may be censored. To deal with this, we assume that Y_1 and Y_2 are censored independently given $(T, P, X) = (t, p, x)$. Then $S_1(y|t, p, x)$ and $S_2(y|t, p, x)$ can be estimated consistently by Kaplan-Meier estimators conditional on $(T, P, X) = (t, p, x)$. Therefore, we estimate $l(y|x, p_{t0}, p_{t1})$ and $u(y|x, p_{t0}, p_{t1})$ using the following sample analogues:

$$\begin{aligned}\hat{l}(y|x, p_{t0}, p_{t1}) &= \max[-1, \{\hat{S}_1(y|1, p_{t1}, x) - \hat{S}_2(y|0, p_{t1}, x)\} \\ &\quad - \{\hat{S}_2(y|1, p_{t0}, x) - \hat{S}_1(y|0, p_{t0}, x)\}] \qquad (5)\end{aligned}$$

and

$$\begin{aligned}\hat{u}(y|x, p_{t0}, p_{t1}) &= \min[1, \{\hat{S}_2(y|1, p_{t1}, x) - \hat{S}_1(y|0, p_{t1}, x)\} \\ &\quad - \{\hat{S}_1(y|1, p_{t0}, x) - \hat{S}_2(y|0, p_{t0}, x)\}], \qquad (6)\end{aligned}$$

where $\hat{S}_1(y|t, p, x)$ and $\hat{S}_2(y|t, p, x)$ are Kaplan-Meier estimators of $S_1(y|t, p, x)$ and $S_2(y|t, p, x)$ conditional on $(T, P, X) = (t, p, x)$.

The lower and upper bounds in (3) and (4) are obtained by applying a few assumptions; however, these may not be very informative in some cases. It would be useful to compare these bounds with those obtained by imposing more restrictions. In particular, we obtain tighter bounds using some plausible independence and monotonicity assumptions. The first assumption we explore is that the treatment effect $\Delta(y|x, p_{t0}, p_{t1})$ is not a function of p_{t0} and p_{t1}. That is, $\Delta(y|x, p_{t0}, p_{t1}) = \Delta(y|x)$. This independence assumption is palatable since time effects cancel out for the DID estimates.[14] Under this additional assumption, the lower and upper bounds can be tightened:

$$\hat{l}(y|x) = \max_{p_{t0}, p_{t1}} \hat{l}(y|x, p_{t0}, p_{t1}) \tag{7}$$

and

$$\hat{u}(y|x) = \min_{p_{t0}, p_{t1}} \hat{u}(y|x, p_{t0}, p_{t1}), \tag{8}$$

where max and min are taken over all possible combinations of p_{t0} and p_{t1}.

The second assumption we consider is that $S(y|0, p, x) \leq S(y|1, p, x)$ for all p and x. Roughly speaking, this means that the durations for young workers tend to be shorter than for old workers where other things are equal. This is reasonable in our application since young workers may be more mobile than old workers. Under this additional assumption,

$$\begin{aligned} \max\{0, S_1(y|1, p_{t1}, x) - S_2(y|0, p_{t1}, x)\} &\leq S(y|1, p_{t1}, x) - S(y|0, p_{t1}, x) \\ &\leq S_2(y|1, p_{t1}, x) - S_1(y|0, p_{t1}, x) \end{aligned}$$

and

$$\begin{aligned} \max\{0, S_1(y|1, p_{t0}, x) - S_2(y|0, p_{t0}, x)\} &\leq S(y|1, p_{t0}, x) - S(y|0, p_{t0}, x) \\ &\leq S_2(y|1, p_{t0}, x) - S_1(y|0, p_{t0}, x). \end{aligned}$$

[14] Of course, only separable time effects cancel out. If there were any nonseparable time effects, then our estimates could be biased estimates for 'true' treatment effects.

This implies that $\Delta(y|x, p_{t0}, p_{t1})$ is bounded by an interval with endpoints:

$$\tilde{l}(y|x, p_{t0}, p_{t1}) = \max[-1, \max\{0, S_1(y|1, p_{t1}, x) - S_2(y|0, p_{t1}, x)\} \\ - \{S_2(y|1, p_{t0}, x) - S_1(y|0, p_{t0}, x)\}]$$

and

$$\tilde{u}(y|x, p_{t0}, p_{t1}) = \min[1, \{S_2(y|1, p_{t1}, x) - S_1(y|0, p_{t1}, x)\} \\ - \max\{0, S_1(y|1, p_{t0}, x) - S_2(y|0, p_{t0}, x)\}].$$

The first and second assumptions can be imposed together to yield tighter bounds. They are:

$$\tilde{l}(y|x) = \max_{p_{t0}, p_{t1}} \tilde{l}(y|x, p_{t0}, p_{t1}) \tag{9}$$

and

$$\tilde{u}(y|x) = \min_{p_{t0}, p_{t1}} \tilde{u}(y|x, p_{t0}, p_{t1}), \tag{10}$$

where max and min take over all possible combinations of p_{t0} and p_{t1}.

Now we present bounds for the treatment effects in terms of conditional quantiles. Notice that (2) can be rewritten in terms of conditional quantile functions:

$$Q_1(\tau|t, p, x) \leq Q(\tau|t, p, x) \leq Q_2(\tau|t, p, x), \tag{11}$$

where $Q(\tau|t, p, x)$ is the τ-th quantile of Y conditional on $(T, P, X) = (t, p, x)$ and $Q_j(\tau|t, p, x)$ is the τ-th quantile of Y_j conditional on $(T, P, X) = (t, p, x)$ for $j = 1, 2$. Again invoking difference-in-differences strategy to identify quantile treatment effects,[15] we define the τ-th quantile DID treatment effect to be

$$\Delta_Q(\tau|x, p_{t0}, p_{t1}) = [Q(\tau|1, p_{t1}, x) - Q(\tau|0, p_{t1}, x)] - [Q(\tau|1, p_{t0}, x) - Q(\tau|0, p_{t0}, x)].$$

[15] See, for example, Athey and Imbens (2002) for the DID method in nonlinear settings.

As before, we obtain lower and upper bounds for $\Delta_Q(\tau|x, p_{t0}, p_{t1})$:

$$l_Q(\tau|x, p_{t0}, p_{t1}) = [Q_1(\tau|1, p_{t1}, x) - Q_2(\tau|0, p_{t1}, x)] - [Q_2(\tau|1, p_{t0}, x) - Q_1(\tau|0, p_{t0}, x)]$$

and

$$u_Q(\tau|x, p_{t0}, p_{t1}) = [Q_2(\tau|1, p_{t1}, x) - Q_1(\tau|0, p_{t1}, x)] - [Q_1(\tau|1, p_{t0}, x) - Q_2(\tau|0, p_{t0}, x)].$$

Again, these bounds can be estimated by sample analogues.[16] Furthermore, the bounds can be tightened using similar independence and monotonicity assumptions. If we assume that $Q(\tau|0, p, x) \leq Q(\tau|1, p, x)$[17] and that the quantile treatment effect is not a function of p_{t0} and p_{t1}, then for each τ, the lower and upper bounds for the quantile treatment effect $\Delta_Q(\tau|x)$ are given by

$$l_Q(\tau|x) = \max_{p_{t0}, p_{t1}} \tilde{l}_Q(\tau|x, p_{t0}, p_{t1})$$

and

$$u_Q(\tau|x) = \max_{p_{t0}, p_{t1}} \tilde{u}_Q(\tau|x, p_{t0}, p_{t1}),$$

where

$$\tilde{l}_Q(\tau|x, p_{t0}, p_{t1}) = \max[0, Q_1(\tau|1, p_{t1}, x) - Q_2(\tau|0, p_{t1}, x)] - [Q_2(\tau|1, p_{t0}, x) - Q_1(\tau|0, p_{t0}, x)]$$

and

$$\tilde{u}_Q(\tau|x, p_{t0}, p_{t1}) = [Q_2(\tau|1, p_{t1}, x) - Q_1(\tau|0, p_{t1}, x)] - \max[0, Q_1(\tau|1, p_{t0}, x) - Q_2(\tau|0, p_{t0}, x)].$$

[16] When Y_1 and Y_2 are censored, conditional quantiles can be estimated by inverting the Kaplan-Meier estimators of the conditional distributions of Y_1 and Y_2 conditional on $(T, P, X) = (t, p, x)$. It is possible that some of the upper quantiles may not be identified.

[17] Note that if this assumption holds for each τ, then that is equivalent to the previous assumption that $S(y|0, p, x) \leq S(y|1, p, x)$ for all y, p and x.

4 Empirical results of bounds analysis

4.1 Duration analysis

In this subsection, we report empirical findings of bounds analysis, applied to unemployment durations. We first begin with our main findings by describing bounds for the treatment effects in terms of survival probability. We focus on married males because this group is largest and effects of the reform on females may be distorted by other factors such as introduction of parental leave benefits and higher labour force participation of the females.

Top panels of Figure 3 show bounds with UPIT for married males with low pre-unemployment wages and bottom panels show those with high pre-unemployment wages.[18] Bootstrap 5 % quantiles of lower bounds and bootstrap 95 % quantiles of upper bounds are also shown along with bounds estimates in Figure 3.[19] It can be seen that for married males with high pre-unemployment earnings, the bootstrap 5 % quantiles of lower bounds (in terms of both $\hat{l}(y|x)$ and $\tilde{l}(y|x)$) are above zero when the unemployment duration is between 400 and 600 days. In view of the fact that treatment takes place between 365 and 660 days, this provides strong evidence on the significant positive treatment effect.[20] On the other hand, there is little evidence

[18]In addition to Figure 3, see Figure 7 in the Appendix for estimation results for married males.

[19]The number of bootstrap repetitions is 5,000. In each repetition, we resample data nonparametrically in each data cell and estimate the four survivor functions. This bootstrap procedure insures that we always have enough data points to estimate the survivor functions.

[20]When we use a pooled sample for the married males we cannot identify a positive treatment effect, since the group with high pre unemployment earnings is rather small (see figure 7). However, we made an interesting observation when we increased the set of post reform years until 1994. For

on the existence of a treatment effect for married males with low pre-unemployment earnings.[21] This supports our conjecture that the treatment is weak or even not present for this group.[22]

Now we consider bounds with UBJ proxy. The estimated bounds with UBJ proxy are wide (see Figure 7 in the Appendix). A positive treatment effect is not detectable for either group. We conclude that UBJ proxy does not provide enough identification power. Therefore, Fitzenberger and Wilke (2004) cannot draw strong conclusions as to whether the unemployed increased the length of search periods in response to the reform.

In addition, we report estimation results of quantile treatment effects. Figure 5 in the Appendix shows bounds of quantile treatment effects with UPIT proxy for married males. Again there is little evidence of the existence of the quantile

the group of married males with high pre-unemployment income, the positive treatment effect persists after the end of the treatment. It starts shortly after the beginning of the treatment and it reduces until the end of the treatment. However, after the end of the treatment the effect rises again. This could be due to the cumulative effect of the reform. However, it might be the case that something else was going on, e.g. worsening labour market conditions for very long-term unemployed married males aged 44-48 or it might also be some sort of early retirement.

[21]In the top panels of Figure 3, we can see that the distance between the lower and upper bounds is broader than the gap between the estimates and their bootstrap quantiles. This suggests that in our empirical analysis, partial identification due to missing information on the unemployment duration is a much more fundamental issue than random sampling errors.

[22]This result suggests in addition that there is no general worsening of labour market conditions for older employees during this period. This supports the conclusions of Fitzenberger and Wilke (2004) who use the full sample of the older unemployed.

treatment effect for married males with low pre-unemployment wages, while we can find evidence of the positive treatment effect at the upper quantiles for those with high pre-unemployment wages.

Finally, we report estimation results for other demographic groups briefly. For singles and females the results are often less clear. We find relatively weak positive treatment effects in terms of both the survivor function and quantiles for single females.[23] For single males (see figure 6) and married females, we find little evidence on the existence of a treatment effect. Surprisingly, even for single males with high pre-unemployment earnings we do not observe a positive treatment effect and therefore results for the pooled sample are presented.[24] Results for the married females are not reported here, but they are available on request.[25] As already outlined by Fitzenberger and Wilke (2004) we do not observe that many unemployed persons wait until they have exhausted their entitlement to unemployment benefits before they accept a new job. Otherwise results would be clearer.[26] The sample size of the group with a positive treatment effect is small compared to all unemployment spells (less than 15%) (see table 4). This implies that the treatment effect is small for the full population. Note that a very large share of the unemployment spells

[23]Caution is required when explaining the results for women because they are distorted by some factors such as the introduction of parental leave benefits and higher employment participation of females. Estimations are not conditional on the level of the former wage because the data contains too few single females with high pre-unemployment wages.

[24]Figures for samples conditional on the pre-unemployment wage level are available on request.

[25]Bounds cross or they are even reversed. There is no clear calendar time trend. Results jump between the years.

[26]An exception to this is married males with high pre-unemployment earnings.

in Germany are due to seasonal unemployment, temporary lay-offs or individuals with short employment spells before unemployment (up to 50%). These spells are excluded from our sample because such unemployed persons are not entitled to long lasting UB transfers. We also did some estimations for this group and did not find any remarkable changes for the treatment group. This supports the idea that there is no general worsening of the labour market situation for those in their mid forties. If there is a general worsening in labour market conditions for older employees, this would cause an upward bias in estimated reform effects. Thus, the true reform effect could be even smaller.

4.2 Inflow to unemployment

It is also possible to bound changes in the age group compositions of inflow to unemployment.[27] This allows us to detect whether the lay-off behaviour of the firms has been changed by the reform. In this subsection we may expect significant changes in respect of the inflow to unemployment just for that subpopulation who increased unemployment duration in response to the reform. For this reason we restrict the analysis to the married males with low or high pre-unemployment earnings. We use the number of positive UPIT durations as the lower bound for the inflow to unemployment and all NE durations as the upper bound.

Table 3 presents the resulting bounds for the specific sample of married males with high or low pre-unemployment income. Apart from the combination $p_0 = 1981$ and $p_1 = 1988$ we do not observe an increase in the number of spells in the treatment

[27] Details of how to bound these can be found in the Appendix A.I.

group both for low and high pre-unemployment earnings. It is difficult to draw a conclusion from this figure but it seems that there is no systematic increase due to the reform, since the low earners' group is affected in the same way.[28] This part does not provide evidence for change in lay-off behaviour due to the reform. Observed changes are likely due to other reasons, e.g. the business cycle, changes in the labour force participation rate or changes in the demographic structure.

5 Conclusion

This paper provides a detailed nonparametric analysis of effects due to changes in the German unemployment compensation system using extensive register data. We exploit the extreme richness of the data and avoid parametric assumptions. Under mild conditions for our econometric framework we address the important problem of missing information in the data by bounding reform effects according to what the data provide in terms of identification power. Surprisingly, we find that partial identification is a more serious problem than random sampling errors. We consider bounds for changes in the inflow and in the duration of unemployment for the treatment group aged 44-48 relative to the control group aged 36-41. There is some evidence for the past two decades that the unemployment rate of the treatment group continuously rose relative to the control group (see figure 4). Lüdemann et al. (2004) do not observe an increase in unemployment duration for the 26-41 age

[28] We can support our view that the reform did not systematically change the lay-off behaviour of firms by providing the inflow bounds for other groups on request. For other treatment groups we even observe a continuous compositional decrease in the inflow to unemployment.

group during recent decades despite a nearly doubling of the total unemployment rate during this period.

In our analysis we do not find any evidence to indicate that the relative increase in the unemployment rate of the 44-48 age group is mainly due to longer search periods of the unemployed in response to longer entitlement periods for unemployment benefits since the mid 1980s. We also do not observe a general worsening of the labour market conditions for the 44-48 age group since the unemployment durations did not uniformly elongate in all cells of the population. However, there is some evidence for an increase in the length of unemployment duration due to the reform. This can be observed for specific subsamples of the data which amount to less than 15% of the treated unemployment spells only. In particular we detect a systematic increase in unemployment spells lasting between 365 and 660 days for the married males with high pre-unemployment income and for single females. In several data cells we also identify a general increase in the highest quantiles of the unemployment duration distribution, i.e. after two years of unemployment and later. This rise in the length of *very* long-term unemployment (after several years) is likely to make a substantial contribution to the increase in the unemployment rate for this group but this was not subject to detailed investigation in this paper. However, the increase in extreme long- term unemployment may be related to early retirement programmes that were conducted during the period under consideration. It is an interesting topic for future research.[29] We do not identify a systematic increase in the inflow to

[29]In our estimations with post reform years up to 1994 we find that the increase in very long term unemployment is much stronger during the years 1991-1994.

unemployment for the group of unemployed who increased search periods after the reform. However, detailed investigation of this has not been covered in this paper and will need to be the subject of future research.

The recent reform of the German unemployment compensation system lead to the merger of UA, IMT and social benefits by the year 2005. The so called *new* social benefits (Arbeitslosengeld II) is means tested and it is generally at the level of welfare, i.e. it is independent of pre-unemployment income. In light of the future reform in 2006 this suggests that the decrease in the maximum entitlement length for UB will have a stronger effect on individuals with high pre-unemployment earnings than found in this paper. This is because the decline in the level of income transfers will be higher than in the old system with UA. However, the size of this group is pretty small compared to the total population of unemployed persons. We therefore conclude that the effect of the reform on population average search periods will be rather limited. However, as shown by Kyyrä and Wilke (2004) for Finland it will lead to an effective reduction in early retirement for individuals aged 55 or above.

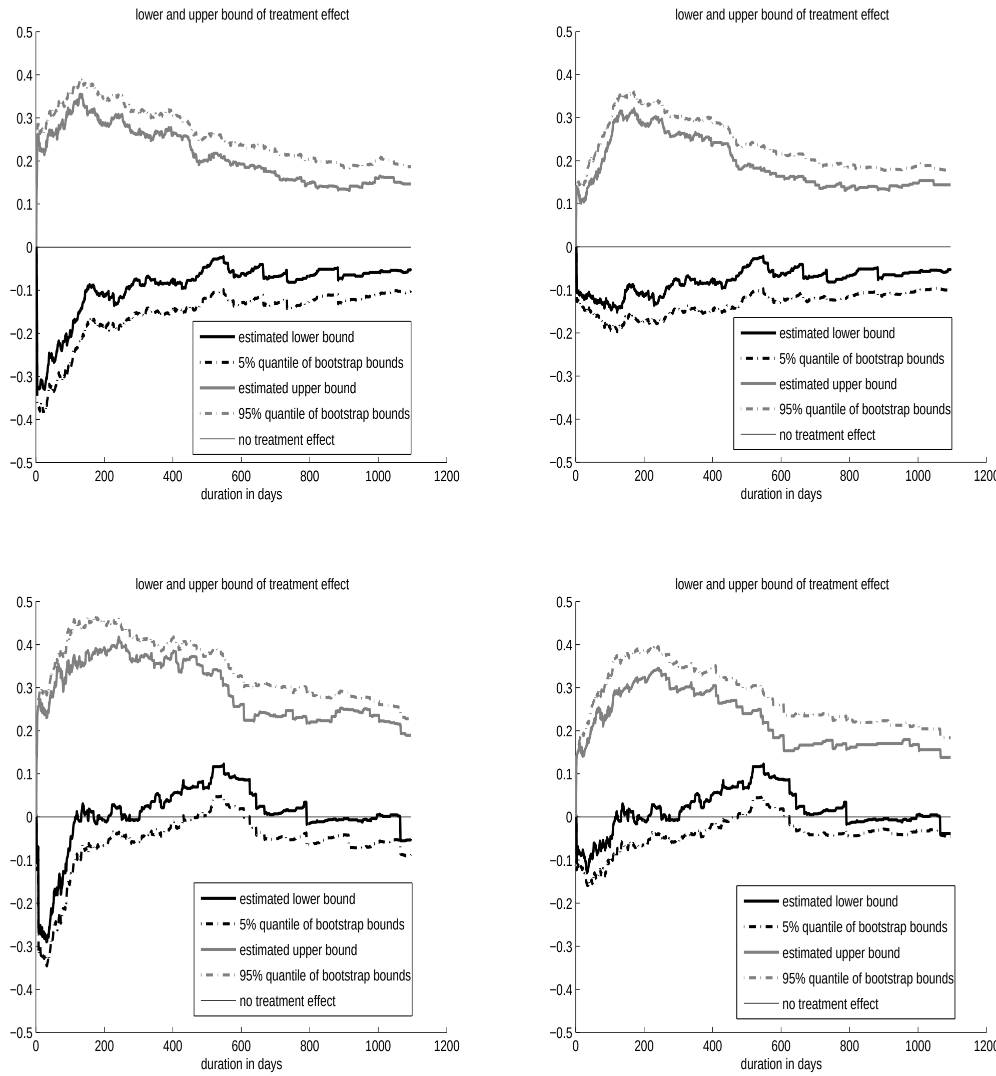

Figure 3: UPIT: $\hat{l}(y|x)$, $\hat{u}(y|x)$ (left) and $\tilde{l}(y|x)$, $\tilde{u}(y|x)$ (right) for low (top) and high (bottom) pre unemployment wages. Sample restricted to married males.

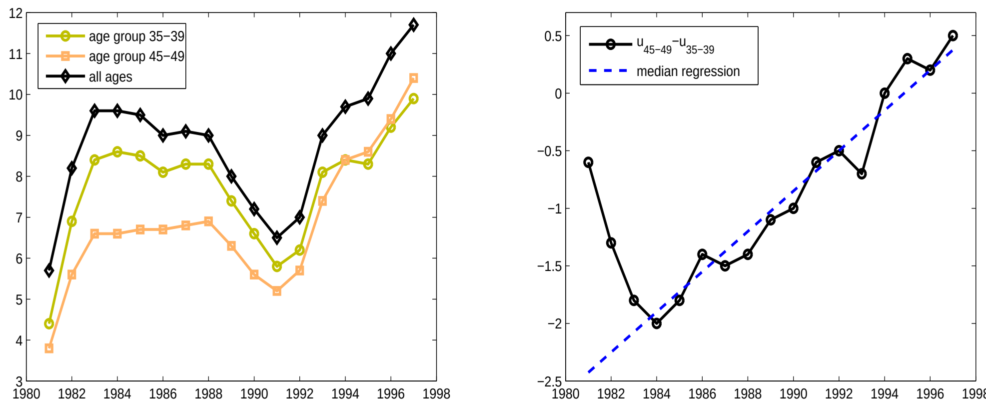

Figure 4: Evolution of unemployment rates (left) and difference in unemployment rates of treated and untreated (right). Source: IAB Nuremberg, own calculations

Appendix:

A I: Bounding changes in the age group composition of the inflow to unemployment

Let $N(t,p,x)$ denote the inflow to unemployment for age group $T = t$ in time period $P = p$ conditional on $X = x$. Since we only observe interval data on the duration variable Y, $N(t,p,x)$ is unobserved. However, as before, we can bound $N(t,p,x)$ in the following way. On one hand, for Y_2 (the upper bound of Y, in our applications $Y_2 =$ NE), we can compute the inflow to Y_2, denoted by $N_2(t,p,x)$. Notice $N(t,p,x) \leq N_2(t,p,x)$ since Y_2 may contain spells other than unemployment durations. On the other hand, for Y_1 (the lower bound of Y, in our applications $Y_1 =$ UBJ or UPIT), we can compute the inflow to strictly positive Y_1, denoted by $N_1(t,p,x)$. Notice that $N_1(t,p,x) \leq N(t,p,x)$ since positive Y_1 may not contain all unemployment spells. Also, notice that we consider the inflow to only positive Y_1 since the inflow to all Y_1 equals the inflow to Y_2.

We define the effect of a reform on the age group composition of the inflow to unemployment using the difference-in-differences (DID) framework. Specifically, the effect of a reform on the age group composition of the inflow to unemployment (denoted by $C(x, p_{t0}, p_{t1})$) is defined as

$$C(x, p_{t0}, p_{t1}) = [N(1, p_{t1}, x) - N(0, p_{t1}, x)] - [N(1, p_{t0}, x) - N(0, p_{t0}, x)]. \qquad (12)$$

Notice that the identification region for $N(t,p,x)$

$$N_1(t,p,x) \leq N(t,p,x) \leq N_2(t,p,x) \qquad (13)$$

for $t = 0, 1$ and $p = p_{t0}, p_{t1}$. Since there are no cross restrictions over time periods and age groups, equation (13) implies that $C(x, p_{t0}, p_{t1})$ is bounded by an interval with endpoints $[l_C(x, p_{t0}, p_{t1}), u_C(x, p_{t0}, p_{t1})]$:

$$\begin{aligned} l_C(x, p_{t0}, p_{t1}) &= \{N_1(1, p_{t1}, x) - N_2(0, p_{t1}, x)\} \\ &\quad - \{N_2(1, p_{t0}, x) - N_1(0, p_{t0}, x)\} \end{aligned} \tag{14}$$

and

$$\begin{aligned} u_C(x, p_{t0}, p_{t1}) &= \{N_2(1, p_{t1}, x) - N_1(0, p_{t1}, x)\} \\ &\quad - \{N_1(1, p_{t0}, x) - N_2(0, p_{t0}, x)\}. \end{aligned} \tag{15}$$

A II: Tables

Table 1: Descriptive summary of the sample: pre reform years

	aged 36-41 (control group)	aged 44-48 (treatment group)
number of spells	3,694	2,872
mean/median spell length UBJ	114/25	111/25
mean/median spell length UPIT	222/112	235/121
mean/median spell length NE	581/243	554/248
censored (UPIT)	27%	30%
censored (NE)	15%	21%
female	40%	38%
married	81%	82%
low wage ($0-60\%$)	75%	77%
high wage ($60-100\%$)	25%	23%
mean age (in years)	38.6	45.8

Table 2: Descriptive summary of the sample: post reform years

	aged 36-41 (control group)	aged 44-48 (treatment group)
number of spells	1,764	1,301
mean/median spell length UBJ	119/21	111/4
mean/median spell length UPIT	212/114	248/122
mean/median spell length NE	476/273	549/304
censored (UPIT)	27%	28%
censored (NE)	15%	23%
female	46%	47%
married	71%	70%
low wage $(0-60\%)$	78%	75%
high wage $(60-100\%)$	22%	25%
mean age (in years)	38.3	46.1

Table 3: Changes in inflow to unemployment, sample restricted to married males

		1981		1982		1983	
		l_c	u_c	l_c	u_c	l_c	u_c
1987	low wage	-51	231	-87	250	-124	189
	high wage	-53	235	-138	224	-210	131
1988	low wage	33	297	-3	316	-40	255
	high wage	29	298	-56	287	-128	194

Table 4: Number of spells in the sample, proportion of samples with positive treatment effect

	pre reform years	post reform years
Full sample IABS		
aged 36-41	6,609	3,880
aged 44-48	5,287	3,021
Sample with positive treatment effect:		
married males with high income transfers or single females		
aged 36-41	862 (13%)	434 (11%)
aged 44-48	651 (12%)	334 (11%)

A III: Figures

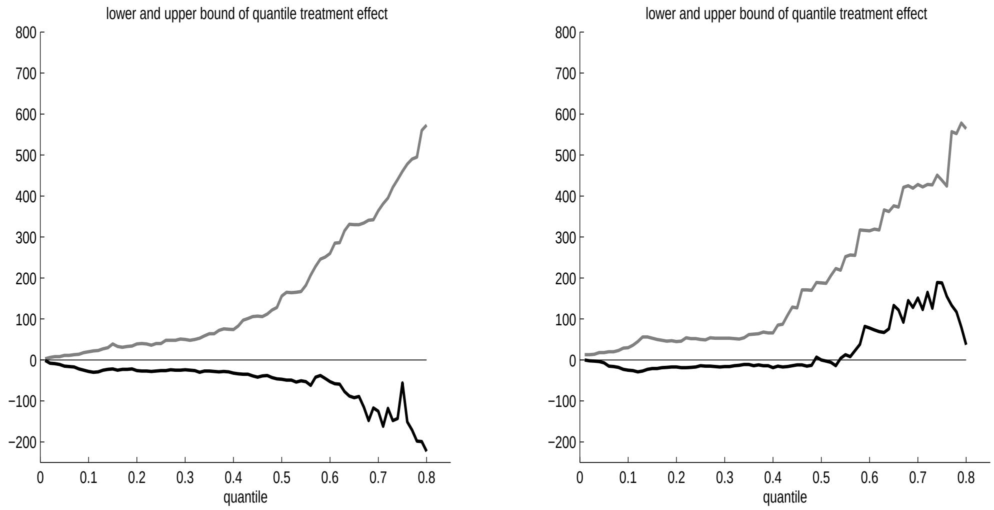

Figure 5: UPIT, sample restricted to married males. $\tilde{l}_q(\tau|x)$, $\tilde{u}_q(\tau|x)$ for low (left) and high (right) pre unemployment wages.

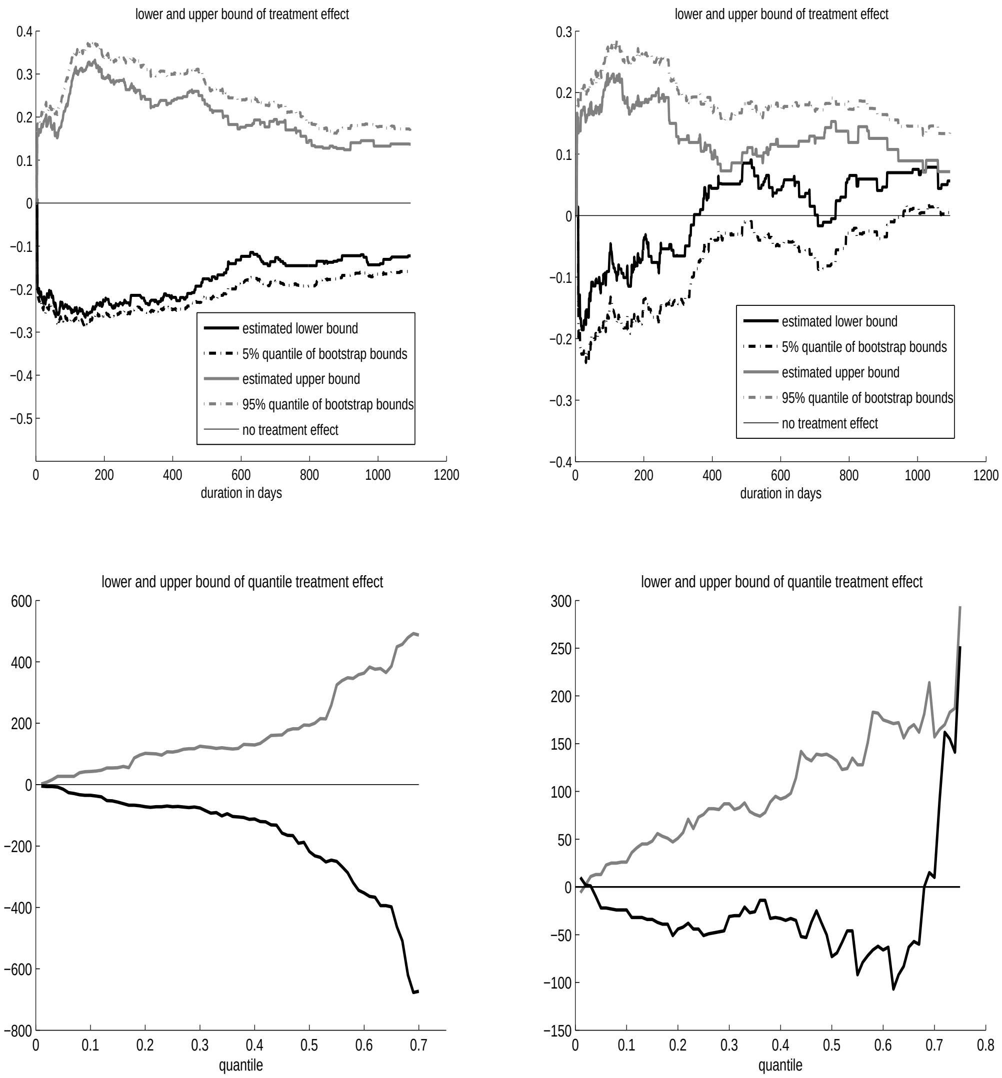

Figure 6: UPIT, sample restricted to single males (left) or single females (right): $\tilde{l}(y|x)$, $\tilde{u}(y|x)$ (top) and and $\tilde{l}_q(\tau|x)$, $\tilde{u}_q(\tau|x)$ (bottom).

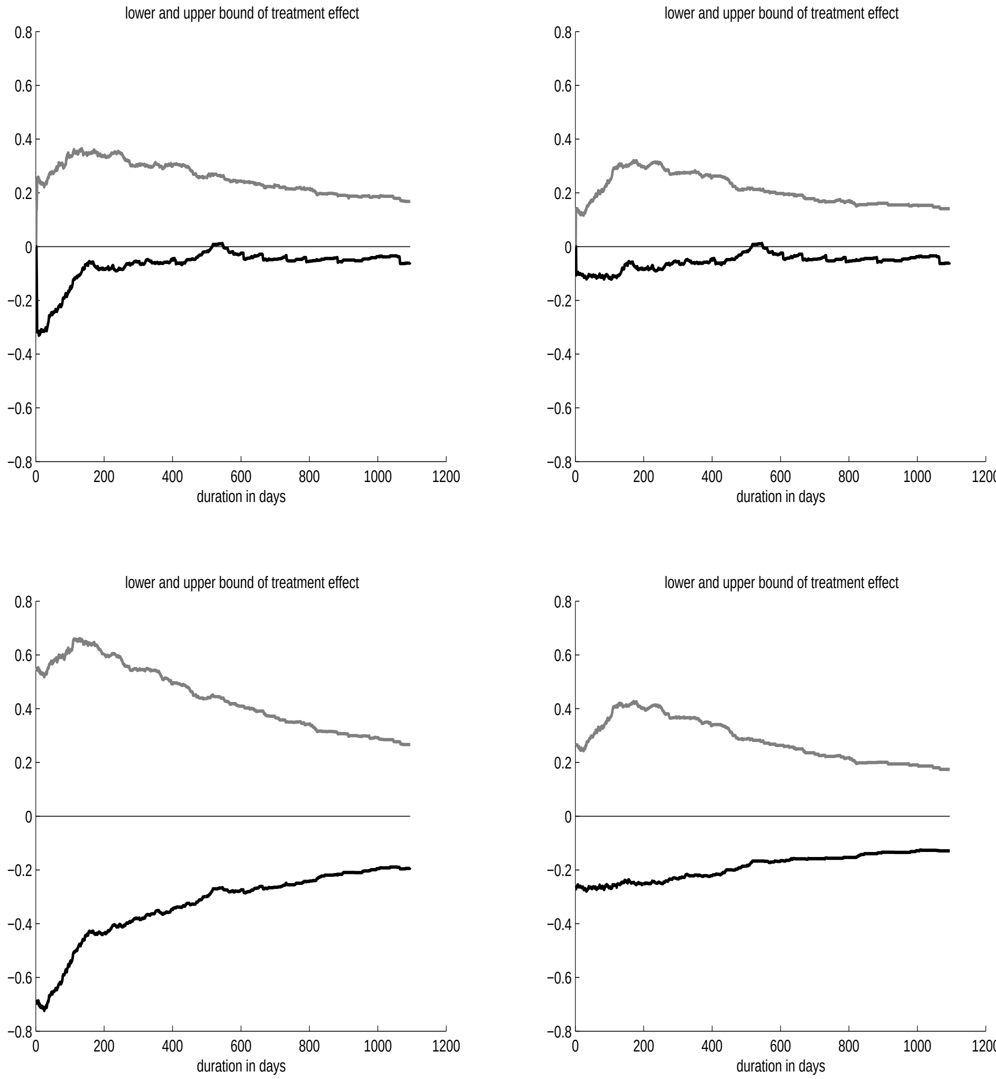

Figure 7: $\hat{l}(y|x)$, $\hat{u}(y|x)$ (left) and $\tilde{l}(y|x)$, $\tilde{u}(y|x)$ (right) for UPIT (top) and UBJ (bottom). Sample restricted to married males.

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