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 maximum entitlement length for unemployment benefits increases unemployment duration. 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 large administrative register based data with the drawback that registered unemployment is not directly observed. By exploiting the richness of the data 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. We identify a systematic increase in unemployment duration in response to the reform in samples that amount to about 15% of the unemployment spells for the treatment group.

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

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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 elderly was increased during the 1980s. This reform presents a unique opportunity to identify the effect of an increase in the maximum entitlement length in a natural experiment setup 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 up to date is that it was leading the path for massive early retirement at the costs of the unemployment insurance system. Both employers and elderly employees agreed in early retirement packages making redundant the stronger dismissal protection for the elderly 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 elderly unemployed who are not early retired, 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 did not increase in response to the reform. Biewen and Wilke (2005) apply semiparametric single spell duration models to the same data and they observe that one may identify an increase in unemployment duration of the less than 49 years old males but it remains unclear whether this is in response to the reform or due to a general change in the macro conditions. 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 at gaining 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. We identify a systematic increase in unemployment duration in response to the reform in samples that amount to about 15% of the unemployment spells for the treatment group.

The paper is organized as follows. ...

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 labor office is observed. 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. 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 labor 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.[1]

In this case we do not know whether the individual is still unemployed, out of labor force or maybe self-employed. With this definition of unemployment we include the periods of nonemployment (out of the labor 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 labor 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): only episodes between two employment spells during which an individual continuously receives UB, UA, or IMT payments have positive length. Interruptions of these payments can be up to four weeks – in the case of cut–off times: 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 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 continuous flow of unemployment compensation from the German federal employment 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 case there is 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 em-

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

ployment. This is a valuable property for the identification of the increase in early retirement as done by Fitzenberger and Wilke (2004) but in our analysis we may loose too much information, in particular for all individuals who do not enter employment anymore. This may prevent us from obtaining tight bounds for the treatment effect. In any case we have $UBJ \leq UPIT \leq NE$.

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).

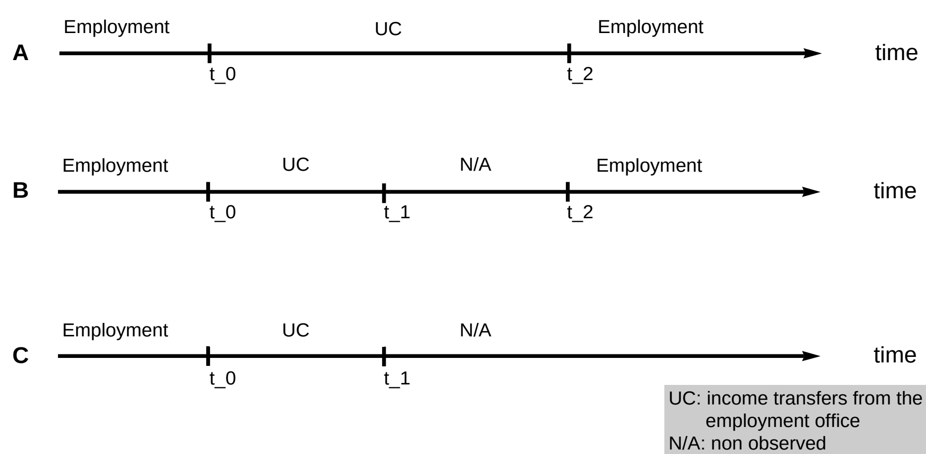

Figure 1: Three common examples of the data structure.

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 control 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 for unemployment benefits.[2] The length of the entitlement period depends on the length of the employment periods before the begin of the unemployment period and on the age of the unemployed. 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 calender years 1981-1994 into three categories:

- pre reform period: 1981-1983
- reform period: 1984-1986
- post reform period: 1987-1994

1984 is considered as reform year because unemployment spells starting in 1983 are the latest not affected at all by the reform. Many spells starting in 1984 were ex post extended in 1985. Anticipation behavior in 1984 may also affect our estimation results. Years before 1981 are not considered because of data quality issues[3]. As post reform years we use 1987 - 1994 (8 years). 1987 is included because the post reform system applies already to most of the unemployment spells starting in 1987. Years after 1994 are not considered because of the systematic censoring at the end of the data (December 1997).

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 previous (or expected) earnings.[4] Unemployed 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 is not high enough to cover the basic needs of the household. Households (and not individuals) are eligible for social benefits which are means 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 for

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

[3] The information on transfer payments seems to be incomplete in the data, see XY for details.

[4] In addition, unemployment assistance is means tested, i.e. it decreases with the income generated by other household members.

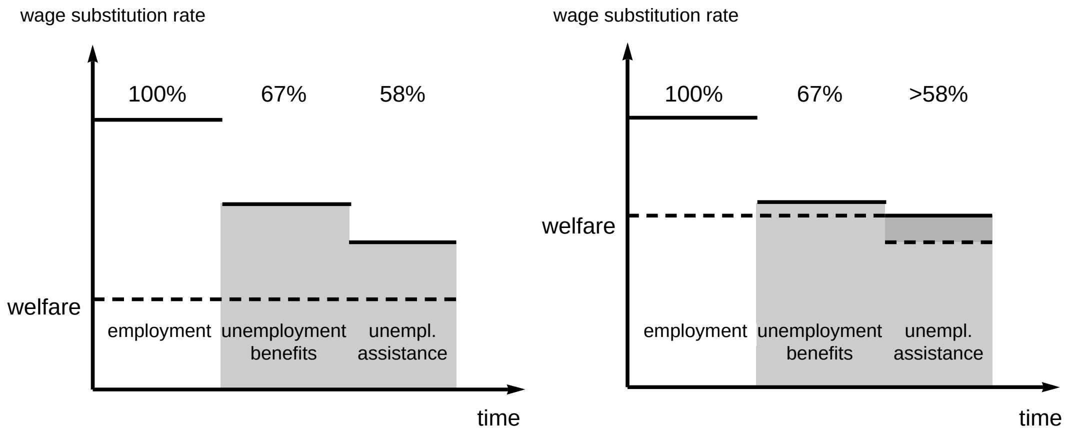

Figure 2: The level of income transfers in Germany is never below the welfare level: example for high (left) and low (right) pre-unemployment wages (in presence of children).

welfare support. The reform should therefore have a smaller effect on those with low pre-unemployment and low expected earnings because an increase in unemployment compensation would simultaneously decrease the level of additional social benefits remaining in a zero or very little net change. See figure 2 (right). Since we do not observe any receipt of welfare in the data, we can only try to control 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 also 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.[5]

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: aged 42-43 are

[5]The wage replacement rate also depends on the presence of 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.

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 already some evidence that early retirement starts within the age group 49-53 and we want to focus our analysis to 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[6] 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:[7]

- periods with unemployment benefits as first income transfer
- no receipt of any unemployment transfer during the past 12 months before the current unemployment period
- no recall to the former employer after the last unemployment period
- business sector "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 didn't receive any unemployment compensation within the year prior to unemployment. This does not ensure that unemployed indeed have maximum entitlement for unemployment compensation but we found that 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 less individuals get the maximum treatment. Tables

[6]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.

[7]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.

1 and 2 present the summary statistics for the pre and post reform samples. In total we have 20.297 unemployment spells in our sample of which 6.566 (32%) 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 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 zero. This means that more than 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.[8] There are no new ideas in our econometric framework; however, details of bounds analysis are newly developed to analyze difference-in-differences-type treatment effects under a natural experiment.[9]

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 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.

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

[9]See Honoré and Lleras-Muney (2004) for an application of bounds analysis to duration analysis in the context of competing risks models.

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 denotes time periods p_{t0} and p_{t1} (before and after a treatment) and T denotes age groups 0 and 1 (control and treatment groups). In our application, $p_{t0} = 1981, 1982, 1983$ and $p_{t1} = 1987, \ldots, 1994$. 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 analog of (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

$[l(y|x, p_{t0}, p_{t1}), u(y|x, p_{t0}, p_{t1})]$:

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

and

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

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, the lower bound is larger than zero or the upper bound is smaller than zero, then one can identify the sign of the effect.

Sample analog estimation of these bounds are 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 $u(y|x, p_{t0}, p_{t1})$ by the following sample analogs:

$$\hat{l}(y|x, p_{t0}, p_{t1}) = \max[-1, \{\hat{S}_1(y|1, p_{t1}, x) - \hat{S}_2(y|0, p_{t1}, x)\} - \{\hat{S}_2(y|1, p_{t0}, x) - \hat{S}_1(y|0, p_{t0}, x)\}] \tag{5}$$

and

$$\hat{u}(y|x, p_{t0}, p_{t1}) = \min[1, \{\hat{S}_2(y|1, p_{t1}, x) - \hat{S}_1(y|0, p_{t1}, x)\} - \{\hat{S}_1(y|1, p_{t0}, x) - \hat{S}_2(y|0, p_{t0}, x)\}], \tag{6}$$

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 under 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.[10] 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 young workers tend to have shorter durations than old workers while other things being 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

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

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

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

and

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

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

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

[10] 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.

where max and min are taken 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,[11] we define the τ-th quantile DID treatment effects to be

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

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 analogs.[12] Furthermore, the bounds can be tightened using similar independence and monotonicity assumptions. If we assume that $Q(\tau|0,p,x) \leq Q(\tau|1,p,x)$[13] and that bounds are not functions 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

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

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

[12] 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 upper quantiles may not be identified.

[13] 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 singles and/or females may be confounded by other factors such as introduction of parental leave benefits and higher labor 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. Bootstrap 5 % quantiles of lower bounds and bootstrap 95 % quantiles of upper bounds are also shown along with bounds estimates in Figure 3.[14] It can be seen that for married males with high pre-unemployment earnings, the bootstrap 5 % quantiles of lower bounds (in terms of both $\hat{l}(t|x)$ and $\tilde{l}(t|x)$) are above zero when the unemployment duration is larger than about 400 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.[15] On the other hand, there is little evidence on the existence of treatment effect for married males with low pre-unemployment earnings.[16] This supports our conjecture that the treatment is weak or even not present for this group.[17]

Now we consider bounds with UBJ proxy. Figure 9 show that estimated bounds

[14]In each bootstrap repetition, we resample data nonparamerically 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.

[15]For this group, the positive treatment effect persists after the end of the treatment. It starts shortly after the begin of the treatment and it reduces till 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 labor market conditions for very long-term unemployed married males aged 44-48 or it might be as well some sort of early retirement.

[16]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.

[17]This result suggests in addition that there is no general worsening of labor market conditions for the elderly during this period. This supports the conclusions of Fitzenberger and Wilke (2004).

with UBJ proxy are wide. 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 from their paper.

In addition, we report estimation results of quantile treatment effects. Figure 7 shows bounds of quantile treatment effects with UPIT proxy for married males. Again there is little evidence on the existence of the quantile treatment effect for married males with low pre-unemployment wages, while we can find evidence on the positive treatment effect at the upper quantiles for those with high pre-unemployment wages.[18]

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.[19] For single males and married females, we find little evidence on the existence of treatment effect. Results are not reported here, but they are available on request.[20] As already outlined by Fitzenberger and Wilke (2004) we do not observe that many unemployed wait until exhaustion of unemployment benefits before they accept a new job. Otherwise results would be clearer.[21] The sample size of group with positive treatment effect is small compared to all unemployment spells (about 15%) (see table 3). This implies that the treatment effect is small for the full population. Note that a very large share of the unemployment spells 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 these unemployed are not entitled for long lasting UB transfers. We did also some estimations for this group and did not find any remarkable changes for the treatment group. This supports the idea that there is

[18]It can be seen that for married males with high pre-unemployment earnings, treatment effects are strongest at the highest quantiles. This could mean that the long-term unemployed are most affected by the reform, but we do not want to exclude the possibility that this could be due to some other reasons than the reform.

[19]We need to be cautious to explain the results for the females because of some confounding factors such as introduction of parental leave benefits and higher employment participation of the females.

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

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

no general worsening of labor market situation for the mid 40 years old. If there is a general worsening in labor market conditions for the elderly, this would cause an upward bias in estimated reform effects. Thus, the true reform effect could be even smaller.

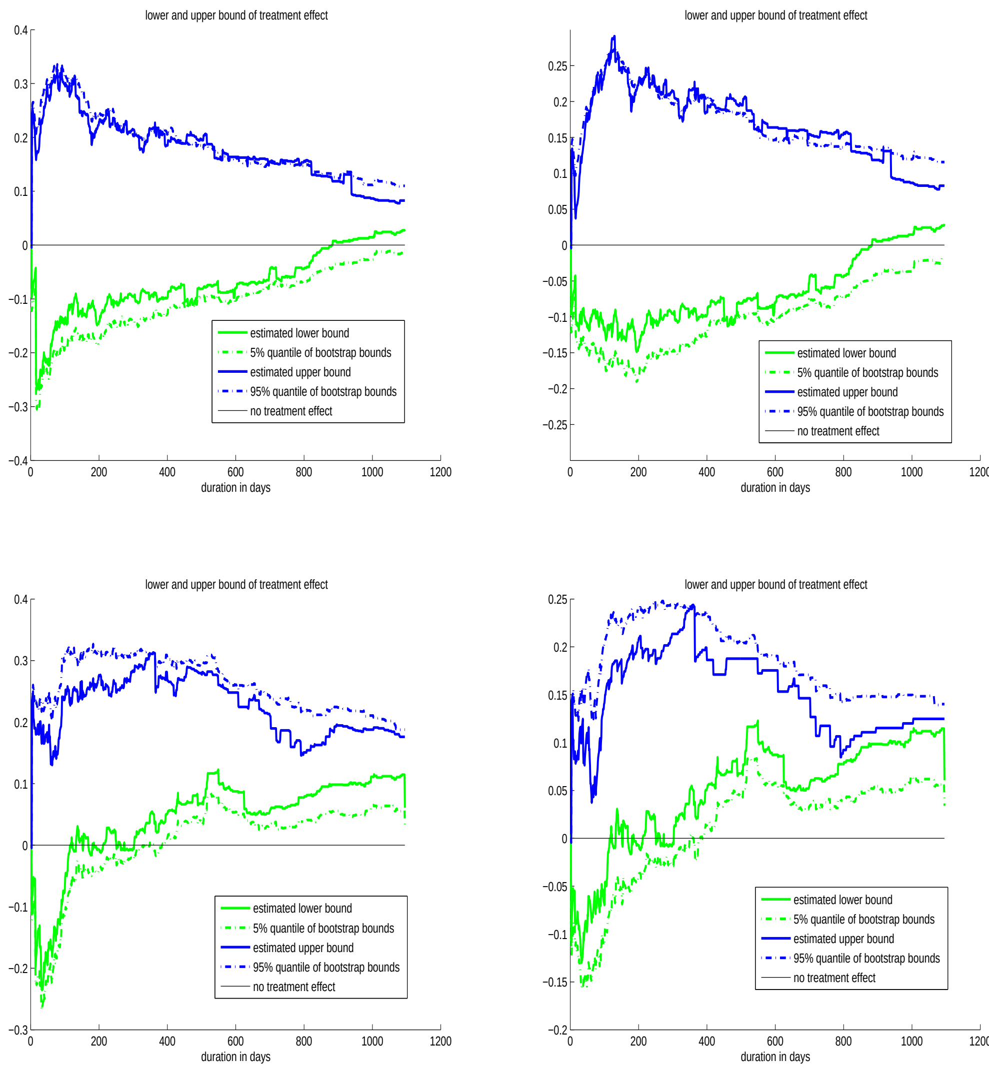

Figure 3: UPIT: $\hat{l}(t|x)$, $\hat{u}(t|x)$ (left) and $\tilde{l}(t|x)$, $\tilde{u}(t|x)$ (right) for low (top) and high (bottom) pre unemployment wages. Sample restricted to married males.

4.2 Inflow to unemployment

It is also possible to bound changes in the age group compositions of inflow to unemployment.[22] This allows us to detect whether the lay off behavior of the firms has changed. The estimation results in the previous section suggest that we expect differences regarding the level of pre-unemployment earnings.

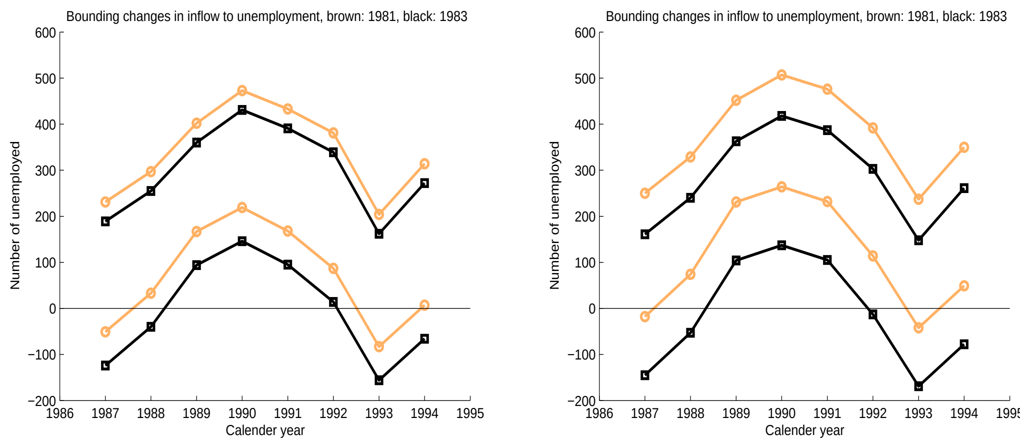

Figure 4: sample restricted to married males with low (left) and high (right) pre-unemployment earnings

Figure 4 presents the resulting bounds for the specific sample of married males with high or low unemployment income. Changes in inflow compositions are affected by the business cycle. Results are depending on the pre- and on the post-reform year. 1981 is similar to 1992 in terms of macro conditions, since there were recessions both in 1982 and in 1993. 1983 is similar to 1987. The changes over the business cycle tell us that the younger employees (control group) are more likely to be laid off in years of economic crisis than the older employees (treatment group). However, even when comparing years with similar macro conditions we observe an increase of the number of spells in the treatment group both for low and high pre-unemployment earnings. It is difficult to draw a conclusion form this figure but it seems that the increase is not due to the reform, since the low earners group is affected in the same way. We can support this by providing the inflow bounds for the single females (see appendix figure 10). For this treatment group we even observe a continuous compositional decrease in the inflow to unemployment. This part leaves some open questions, however, no strong evidence for change in lay off behavior due to the

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

reform. Observed changes are likely due to other reasons, e.g. increase in the labor force participation rate of the females.

5 Conclusion

This paper provides a detailed nonparametric analysis of the effects of the reform of the unemployment compensation system during the 1980s using large register data. We exploit the extreme richness of the data and avoid parametric assumptions. Under very 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 5). Lüdemann et al. (2004) do not observe an increase in unemployment duration for the 26-41 years old during the past decades despite a nearly doubling of the total unemployment rate during this period.

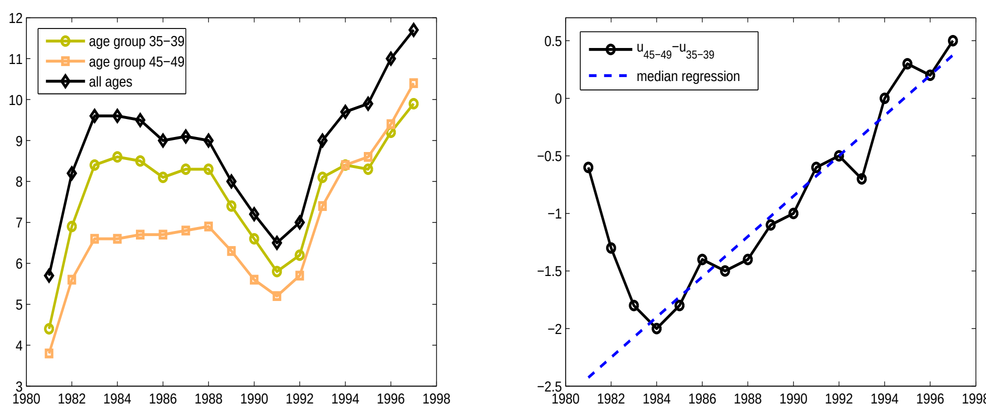

Figure 5: Evolution of unemployment rates (left) and difference in unemployment rates of treated and untreated (right). Source: IAB Nuremberg, own calculations

In our analysis we do not find any evidence supporting that the relative increase in the unemployment rate of the 44-48 years old is mainly due to the reform of the unemployment compensation system during the 1980s. We also do not observe a general worsening of the labor market conditions for 44-48 years old during the 1980s and 1990s 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 about 15% of the treated unemployment spells only. In particular we detect a systematic increase in unemployment spells between duration days 365 and 660 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 2.5 years and longer) is likely to be a substantial contribution for the increase in the unemployment rate for this group but this was not subject to detailed investigation in this paper. For the married males we identify an increase in the inflow to unemployment but it is not clear yet whether this is due to the reform or due to other reasons because for other treatment groups, such as single females, one can even observe a decrease.

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 that $N(t, p, x) \leq N_2(t, p, x)$ 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

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

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

$$N_1(t, p, x) \leq N(t, p, x) \leq N_2(t, p, x) \tag{13}$$

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 $[l_C(x, p_{t0}, p_{t1}), u_C(x, p_{t0}, p_{t1})]$:

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

and

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

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 $(0 - 40\%)$	50%	51%
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	8,284	5,447
mean/median spell length UBJ	117/14	109/0
mean/median spell length UPIT	230/107	278/122
mean/median spell length NE	486/283	562/353
censored (UPIT)	27%	31%
censored (NE)	22%	31%
female	46%	47%
married	68%	70%
low wage $(0 - 40\%)$	54%	52%
high wage $(60 - 100\%)$	21%	24%
mean age (in years)	38.4	46.0

Table 3: 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		16,583	
aged 44-48	5,287		11,043	
Sample with positive treatment effect:				
married males with high income transfers or single females				
aged 36-41	1,032	(16%)	2,414	(15%)
aged 44-48	752	(14%)	1,675	(15%)

A III: Figures

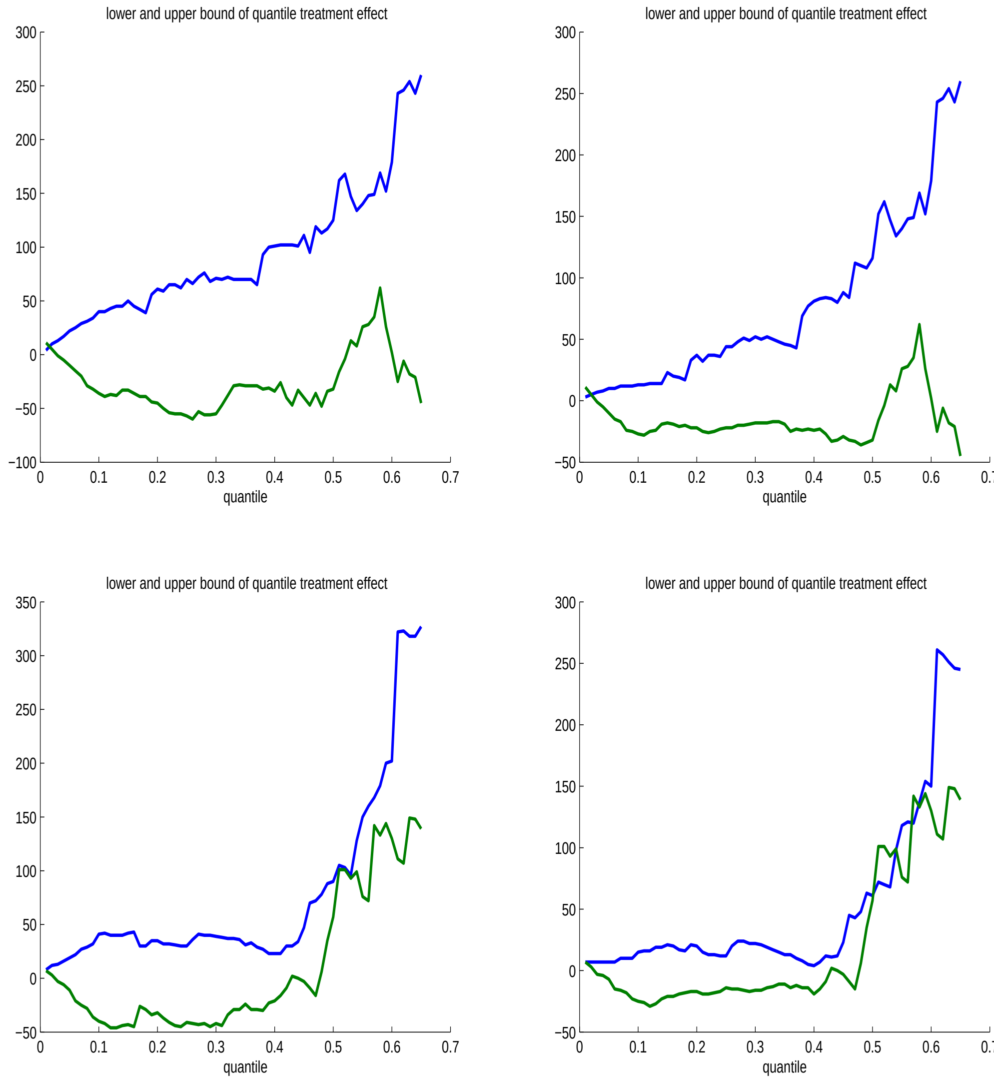

Figure 6: UPIT, sample restricted to married males. $\hat{l}_q(\tau|x)$, $\hat{u}_q(\tau|x)$ (left) and $\tilde{l}_q(\tau|x)$, $\tilde{u}_q(\tau|x)$ (right) for low (top) and high (bottom) pre unemployment wages.

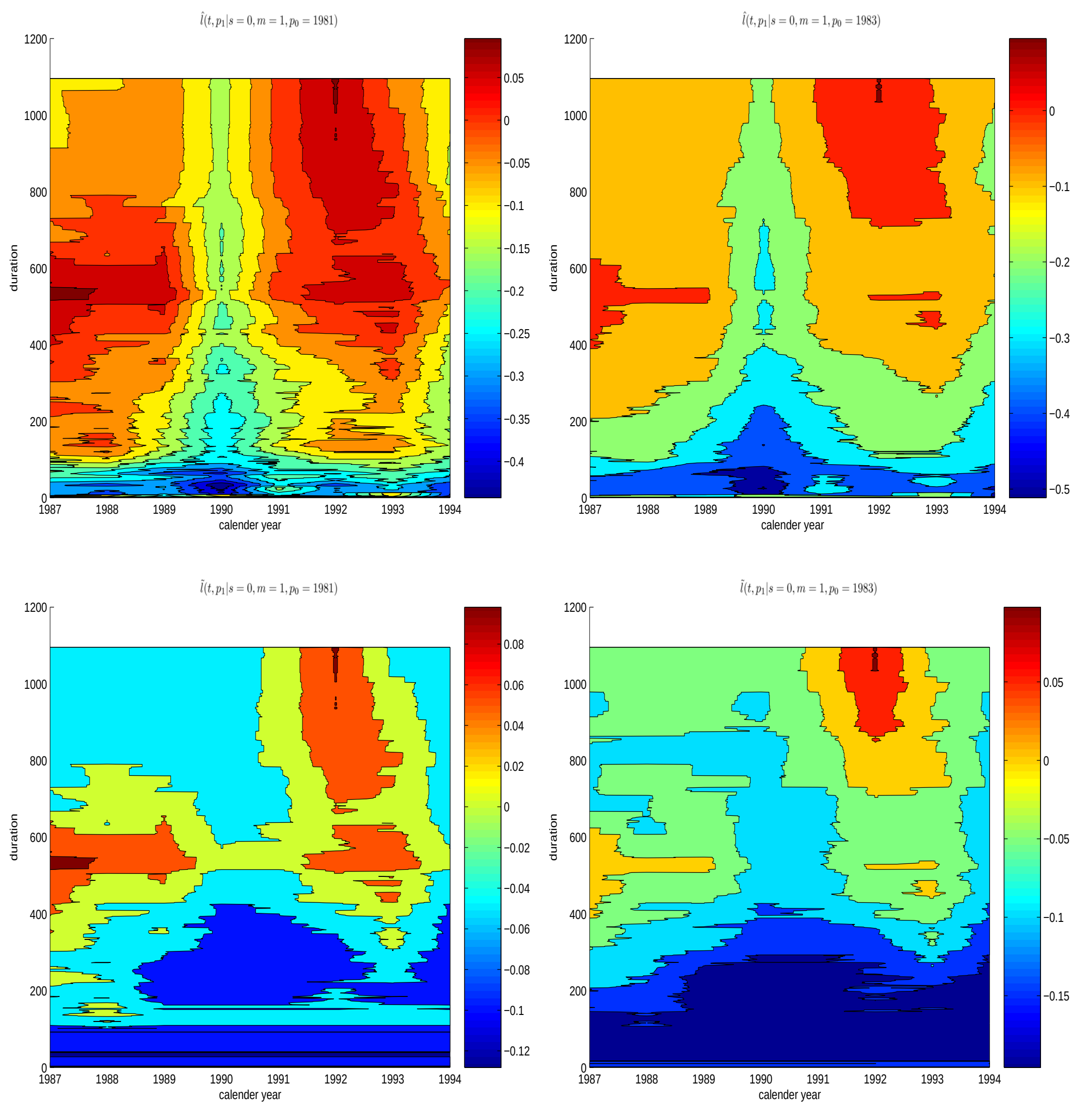

Figure 7: UPIT, sample restricted to married males with high pre-unemployment earnings. Contour plots of lower bounds in the calender-duration time space.

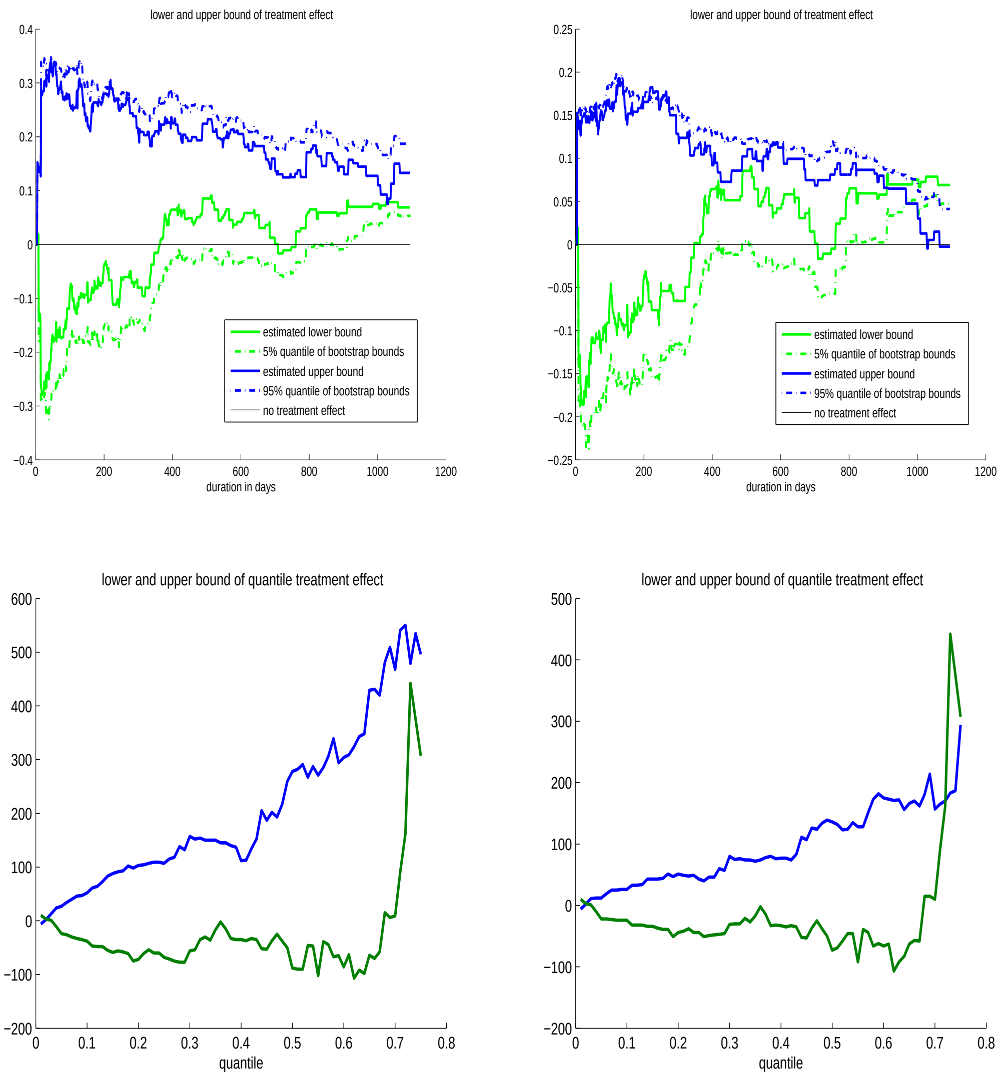

Figure 8: UPIT, sample restricted to single females: $\hat{l}(t|x)$, $\hat{u}(t|x)$ (left) and $\tilde{l}(t|x)$, $\tilde{u}(t|x)$ (top) and $\hat{l}_q(\tau|x)$, $\hat{u}_q(\tau|x)$ (left) and $\tilde{l}_q(\tau|x)$, $\tilde{u}_q(\tau|x)$ (bottom).

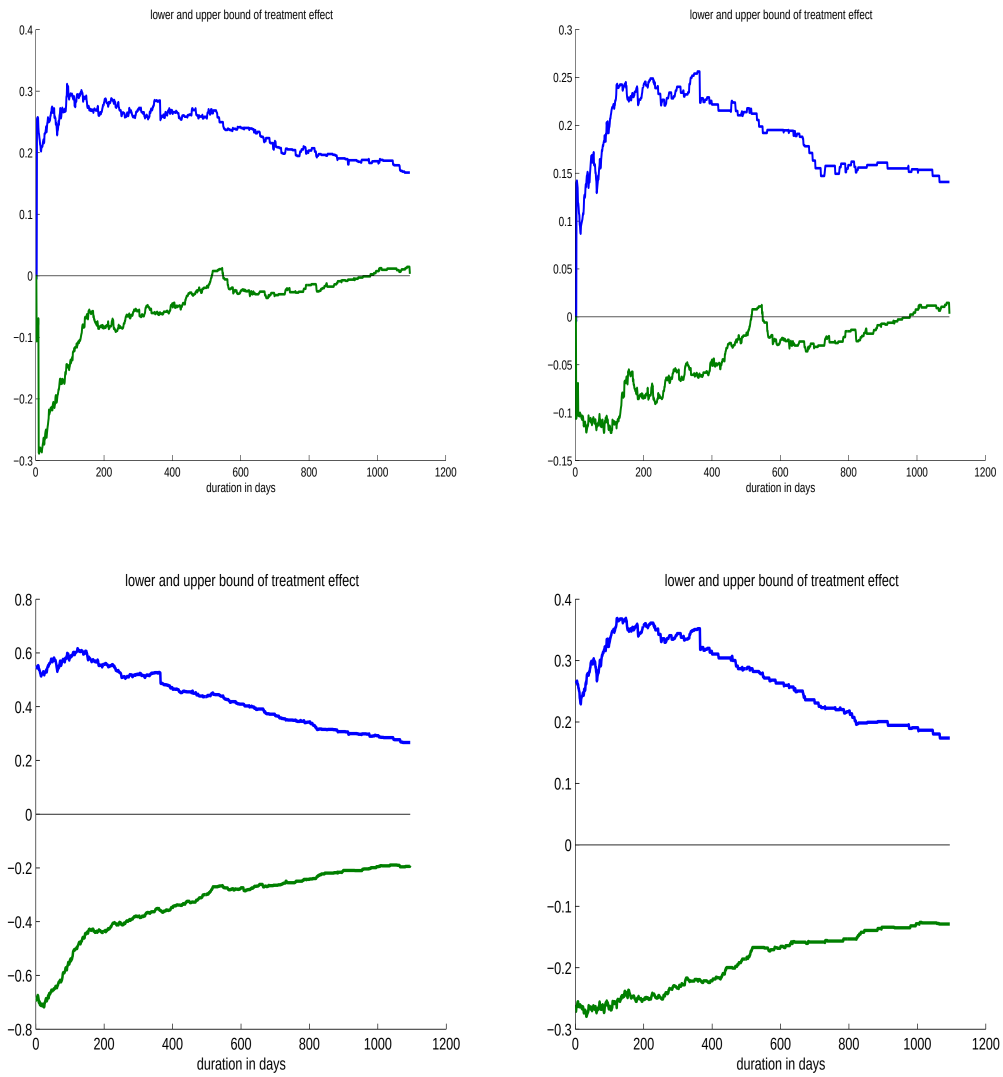

Figure 9: $\hat{l}(t|x)$, $\hat{u}(t|x)$ (left) and $\tilde{l}(t|x)$, $\tilde{u}(t|x)$ (right) for UPIT (top) and UBJ (bottom). Sample restricted to married males.

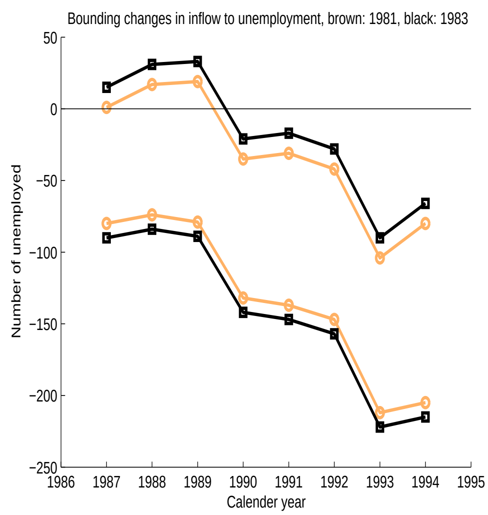

Figure 10: Bounds for the change in age group composition: sample restricted to single females.

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