Shortening the potential duration of unemployment benefits and labor market outcomes: Evidence from a natural experiment in Germany

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Abstract

This paper explores the effects of a major reform of unemployment benefits in Germany, implemented in 2006 as part of the so-called Hartz-Reforms. The reform induced a substantial reduction in the potential duration of unemployment benefits 1 for older workers, providing a natural experiment setting. This work analyzes the reform in a wider framework of institutional interactions, which allows us to distinguish between its intended and unintended effects. Our results provide causal evidence for a significant decrease in number of days in unemployment benefits 1 and increase in number of days in employment. However, they also suggest a significant increase in number of days in unemployment benefits 2, granted upon exhaustion of unemployment benefits 1. Transitions to unemployment benefits 2 represent rather an unintended effect, limiting the success of a policy change that aims to increase labor supply via reductions in the generosity of the unemployment insurance system.

JEL-codes: I1, J2, J65

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1. Introduction

Since the Lisbon Strategy, launched in March 2000, promotion of employment has become a priority for European policy makers. Since then, a bunch of reforms with the purpose to reestablish incentives to work and to delay withdrawals from the labor market have been implemented. Among other policy changes, a profound rearrangement of unemployment insurance (UI) systems was initiated almost contemporaneously in many European countries. Thereby, older workers were the target population, because the rate of longer unemployment spells is generally higher for this age group, either due to poor employment outlooks or disincentives of reemployment. Economic theory hints at poor work incentives upon availability of unemployment benefits, whereby the generosity of the UI system matters (Mortensen 1970, Moffitt and Nicholson 1982). Theoretical literature suggests that a less generous UI scheme is related to increased job search effort of unemployed workers and shorter unemployment duration. Moreover, around the date of benefits exhaustion the intensity of their job search rises, which is associated with a "spike" in the unemployment exit rate at this point (Card and Levine 2000, Card et al. 2008). Finally, static labor supply model predicts lower reservation wages and higher probability to choose employment over unemployment in response to lower unemployment benefits. There is a growing literature with the primary interest in the optimal design of the UI scheme (Shavell and Weiss 1979, Hurd 1980, Wright 1986, Hopenhayn and Nicolini 1997, Cahuc and Lehmann 2000, Schmieder et al. 2012b). The core issues are potential decreases in UI benefits over the unemployment spell as well as increases in potential benefits durations (PBD) during recessions and for different demographic groups in the population. This literature explores the trade-off between the insurance function of and disincentives derived from UI, whereby welfare changes of affected individuals play an important role. On the one hand, UI ensures consumption smoothing for the unemployed, while on the other one, benefits-induced disincentives shape their job search effort and moral hazard behavior. Because unemployed individuals shrink from accepting job offers, delaying their reemployment, and because monitoring of job search behavior is limited, the optimal UI scheme provides incentives that discourage the unemployed from persistent unemployment.

In 2004 the German government announced a major reform of the UI system. The reform was implemented in February 2006 as part of the so-called Hartz-Reforms. It involved a substantial reduction in the potential duration of regular unemployment benefits (unemployment benefits 1, UB-1) with the purpose to stimulate employment among older workers by alleviating the disincentive effect of long compensation. The reform affected older age groups, while

younger workers were not exposed to the policy change. The design of the reform provides a natural experiment setting, on which our identification strategy relies. We exploit the 2006 reform of the German UI system in order to investigate the relationship between the potential duration of UB-1 and the labor market outcomes of the affected individuals. The contribution of this paper is threefold. To begin with, we propose a novel way to study the impact of unemployment compensation on covered workers, introducing an alternative measure of their response to an UI regulation In particular, as opposed to unemployment duration that is a well-established outcome in the UI literature, our outcome of interest is aggregated for the complete calendar year from spell data. Hence, unlike the previous research that focuses on the initial spell limiting the analysis of a policy change to its short-term outcome, our estimates present the combined effect from the incidence and duration of recurring spells within a well-defined time period, thus capturing the reform effects that go beyond the first unemployment spell. Indeed, short-term effects may either under- or overstate the total cost of the reform if these also impact the incidence and duration of future unemployment. The most closely related to our work is that of Schmieder et al. (2012a), where the outcome is aggregated over the first five years after the start of the initial UI spell.

Next, our sample consists of individuals with some health impairment. The use of routine data on labor market performance of participants in medical rehabilitation allows us to study the reform effects on this population group, opening an interesting perspective in the policy evaluation. Intuitively, a potentially positive impact of the policy change for this population group would encourage us to expect even larger effects for healthy individuals with no need for medical rehabilitation because their labor market opportunities should be better. In fact, empirical evidence suggests that work-limited workers with physical or nervous conditions suffer large and persistent declines in annual earnings as well as hours worked following work-limitation onset (Charles 2003, Mok et al. 2008). Furthermore, the post-onset annual hours contraction of individuals who are older at work-limitation onset chiefly stems from reduced probability of labor participation. Worse labor market prospects of participants in medical rehabilitation might also derive from the demand-side factors such as discrimination in the recruitment process or performance evaluation as well as from the coworkers' side that might adversely affect the workplace integration of work-limited workers (Colella and Bruyère 2011). Adverse treatment of work-limited workers hampers their return-to-work as well as job retention and, therefore, we expect individuals with no health impairment to be more responsive to the UI incentives, interpreting our estimates as lower and upper bounds for the treatment effects in the total population. Finally, we analyze the reform in a wider framework of

institutional interactions from the labor market perspective, neglecting the incidental fiscal effects of the policy change. We distinguish between the intended and unintended labor market effects of the reform and show that it had a structural impact on the distribution of unemployment and employment. Based on a difference-in-differences approach, our results provide causal evidence for a significant decrease in number of days in UB-1 and increase in number of days in employment subject to social insurance contributions. However, the findings also suggest a significant increase in number of days in unemployment assistance (unemployment benefits 2, UB-2), granted to unemployed jobseekers upon exhaustion of UB-1 with the purpose to provide them a living at the subsistence level. This result is consistent with recent work on effectiveness of more comprehensive reforms of labor market institutions as opposed to one policy reform at a time (Pellizzari 2006, Fremigacci 2010). From the labor market and social policy perspective, transitions to UB-2 represent rather an unintended consequence of the reform, limiting the success of a policy change that aims to increase labor supply via reductions in the generosity of the UI system. To the best of our knowledge, this is the first paper that explicitly investigates this important aspect of the major reform of UB-1 in Germany and is intended to fill the gap.

The remainder of the paper is organized as follows. Section 2 presents a literature review. Section 3 describes the institutional setting of the German UI system. Section 4 presents the data set and samples. Section 5 describes our econometric approach along with summary statistics for the variables of interest. Section 6 reports the estimation results. Section 7 concludes with a short summary and discussion of the main findings.

2. Literature Review

This paper explores the impact of PBD on the workers' labor market performance and, thus, contributes to the stream of literature analyzing the disincentives provided by the UI system. Empirical findings broadly support the predictions of job search models. Insurance protection offered by the UI system significantly affects exits from unemployment of benefits recipients. A large body of literature explores the impact of changes in UI parameters on the duration of the first nonemployment spell, limiting the analysis of a policy change to its short-term outcome, partially because the identification of precise longer-term effects is empirically challenging (Meyer 1990, Hunt 1995, Lalive 2007). However, in their recent contribution, Schmieder et al. (2012a) remedy this shortcoming. The authors exploit a regression discontinuity design in Germany and examine the long-term impact of an extension of UI duration captured by the total days receiving UI benefits and the total days in nonemployment in the

first five years after the start of the initial UI spell. Thus, the positive effect of longer UI duration on the sum of days spent in nonemployment combines the effect from the initial nonemployment spell and the incidence as well as duration of additional spells. Their results further indicate that a large part of the effect of UI extension is captured by a longer initial nonemployment spell and allowing for multiple spells reduces the impact of longer PBD. In other words, the effect of PBD on total nonemployment is smaller than the effect on the duration of the initial nonemployment spell. This implies that the long-term effect of UI on overall nonemployment is smaller.

Hunt (1995) investigates the impact of large increases in PBD for older workers in the former West Germany, distinguishing between escapes to employment and out of the labor force. Applying a difference-in-differences method, the author reports longer unemployment spells that lower the hazard rates to both exit destinations. A positive relationship between the generosity of the UI system and the duration of unemployment spells has become a stylized fact, although less is known about the underlying mechanisms. Higher UI benefits are associated with a strong negative effect on the probability of leaving unemployment that rises dramatically just prior to benefits exhaustion (Meyer 1990). This evidence has been generally attributed to moral hazard caused by a substitution effect, according to which UI alters the relative price of leisure and consumption, lowering the marginal incentive to search for a new job. More recent studies focus on the identification of the channels through which unemployment benefits affect search behavior. In a sample of US workers, Chetty (2008) examines the importance of moral hazard (substitution effect) versus liquidity (income effect), referring to the principle according to which, as with any uncompensated labor supply elasticity, the total labor supply response to a change in UI benefits incorporates both effects. Indeed, the author finds that more than half of the labor supply elasticity is due to a liquidity effect. This indicates that a large fraction of benefits recipients is liquidity constrained and that when unemployed workers are unable to smooth consumption due to a transitory income shock, unemployment benefits affect job search behavior through a liquidity effect in addition to the moral hazard mechanism highlighted in previous studies. However, when consumption can be smoothed perfectly, UI benefits raise unemployment durations essentially through moral hazard.

Lalive et al. (2006) show that replacement ratio (RR) as well as PBD are both important policy tools able to alter behavior, although they prompt rather different behavioral responses. On the example of an increased RR and extended PBD in Austria in 1989, the authors observe an

increase in unemployment duration, which is larger in case of a simultaneous increase in replacement rate and potential benefits duration compared to isolated increases in these UI parameters. Furthermore, they find a strong association between increases in PBD and exit rates from unemployment around the date of benefit expiration, while behavioral adjustments in response to an increase in RR follow a more uniform distribution over the unemployment spell. This pattern can be interpreted as indicating that an increased RR has the largest impact on the behavior from the start of the unemployment spell, while extended PBD does so around the date of benefits expiration. Indeed, in many studies, a large "spike" in the exit rate from registered unemployment at the point of benefits exhaustion is observed, which can be explained by the idea that as the remaining period of a benefits receipt declines, the value of remaining unemployed declines as well. The latter adversely affects the reservation wage and boosts job search intensity as workers approach benefits exhaustion. After UI benefits have expired, reservation wage and job search effort remain constant. As a result, employment hazard increases up to the date of benefits exhaustion and does not change afterwards (Mortensen 1977).

The observed "spike" in exit rates is mostly interpreted as a manifestation of non-stationary search behavior of benefits recipients who wait until their benefits expire to return to work, potentially accepting jobs with lower stability and lower wages compared to those unemployed who exit unemployment at an earlier stage of their unemployment spell (Caliendo et al. 2013). This strategic job search behavior is widely recognized to disclose the distortionary effects of the UI system and social insurance programs in general, although the effective moral hazard effect is supposed to be significantly lower in recessions than in booms (Feldstein 2005, Schmieder et al. 2012b). There is empirical evidence in favor of strategic job search behavior caused by more generous unemployment benefits durations. For instance, Lichter (2016) exploits an exogenous variation in PBD originated from a policy change in Germany. The reform, implemented in 2008, involved an extension of the potential benefits duration for workers of specific age groups. Applying a difference-in-differences technique, the estimates provide causal evidence of reduced search effort, measured by the number of job applications and the probability of applying for jobs in distant areas, in response to the extension of PBD. These findings are in line with the theoretical predictions of standard job search models. Furthermore, instrumental variables estimates show that the reduction in search effort, induced by the reform, caused a significant decrease in the short-run job finding rate. Evaluated at the mean, a 10 percent increase in the number of filed applications is associated with an increase in the short-run job-finding rate by about 1.3 percentage points. Additionally, individuals may make arrangements about the date of return to work. Empirical estimates support the hypothe-

sis that at an early stage of an unemployment spell firms and employees plan its ending date (Meyer 1990, Katz and Meyer 1990). Intuitively, if workers are bound to firms, for instance by implicit contracts, moving costs, or specific human capital, there is a strong incentive to tie recall decisions to the length of UI benefits.

A growing strand of literature focuses on transitions from employment into unemployment induced by the UI incentives (Winter-Ebmer 2003). In fact, unemployment entry and unemployment duration both explain the dynamics of the aggregate unemployment rate. Studying the unemployment incidence before and after the major reform of UB-1 in Germany in 2006, Dlugosz et al. (2013) apply a difference-in-differences method and find evidence for decreased unemployment inflows for individuals aged 52 and older. Moreover, the results indicate large anticipation effects of the reform in the three months before the policy change came into force, which greatly distorted the short-term effects of the reform. In particular, relative to younger workers, transition rates into unemployment of workers aged 52 and older substantially increased in the anticipation period, which suggests a change in the composition of the unemployed in response to the reform. In fact, for a limited period right after the introduction of the reform, unemployment inflow decreased in a more pronounced way than in absence of anticipation. This observation could be explained by the anticipation of dismissals and quits from the post-reform period to the pre-reform one. Nevertheless, the decrease in unemployment inflows following the reform far outweighed the anticipation effect. Based on this knowledge gain, Lo et al. (2015) focus on the age group with the smallest anticipation effect, for which no systematic decrease in unemployment inflows after the reform has been observed. The authors, further, exclude periods with unemployment inflows during the potential anticipation period in order to remove anticipation effects. Based on a sample of male unemployed with full-time employment before unemployment, they distinguish between transitions to desired destinations, such as non-low-wage full-time employment, transitions to less desired destinations, such as subsidized self-employment, low-wage full-time employment and transitions to other states, such as part-time employment, previous employer, secondary labor market or long training programs. A central contribution of their study consists in providing more detailed insights into the impact of the unemployment compensation system on labor market performance of the affected individuals. In particular, the authors conclude that (non) low-wage workers tend to take up (non) low-wage employment. The probability to experience a recall to the previous employer is higher for low-wage workers, while the probability to take up subsidized self-employment is higher for non-low-wage workers than for the low-wage

ones. Although the authors analyze the impact of the major German reform on transitions to important exit destinations, they disregard perhaps no less important transitions to UB-2.

In the European context, the Finnish 1997 reform decreased the PBD for older workers with the purpose to enhance employment incentives and cut the expenditures on unemployment indemnity. Kyyrä and Ollikainen (2008) apply a difference-in-differences approach in order to analyze transitions to employment, being a targeted exit destination, as well as less desired transitions out of the labor force and into active labor market programs that are more relevant in the Nordic countries. Based on the altered flows into unemployment in anticipation of the policy change, the authors exclude the involved groups from the analysis. Their findings do not reveal large increases in the employment hazard around the date of benefits exhaustion. Instead, the hazard rates for labor market programs and non-participation present substantial rises. These results point to an important interaction between labor market institutions, where the UI system and the early retirement scheme represent an attractive pathway to labor market withdrawal prior to the regular old-age pension. The French reform of the UI system in 2003 also involved substantial shortenings in PBD for older workers (Fremigacci 2010). The findings point to increased transition rates out of unemployment in response to the policy change. A decomposition of the outflows from unemployment allows a closer insight into the exit destinations. It reveals a positive, although tiny, effect on exits to employment, but a substantial positive effect on transitions to unemployment assistance, granted to individuals who have exhausted the unemployment benefits or did not qualify to receive them. In fact, a common feature of the European institutions is the interaction of UI with other social security programs (Schmieder and von Wachter 2016). Thus, upon exhaustion of unemployment benefits, job seekers can shift to unemployment assistance or other basic income support programs. The author concludes that the major effect of the policy change was to shift job seekers from unemployment benefits to unemployment assistance. These non-negligible transitions represent an unintended consequence of the reform that might limit the success of a policy change aiming to increase labor supply via reductions in the generosity of the UI system (Pellizzari 2006).

The shift to unemployment assistance following a reduction in PBD largely explains the identified “spikes” in exits from unemployment around the date of benefits exhaustion and supports the relevant work of Card et al. (2008) on the true mechanisms behind these “spikes”. In their study based on a large sample of job losers in Austria, Card et al. (2008) show that the observed “spikes” may exaggerate the extent of moral hazard induced by UI. The authors un-

derline the importance of how unemployment spells are measured (time spent on the unemployment system vs. time to next job), which determines the magnitude of the "spikes" at benefits exhaustion. The results indicate that the hazard rate of reemployment accounts only for a small part of the exit rate from registered unemployment. This finding reveals that many unemployed workers leave the unemployment register around the date of benefits exhaustion without returning to work, which sheds light on the divergence between the two measures of unemployment spell.

Other significant contributions that can be understood as a complementary piece of research in this field analyze the impact of the UI reform in terms of job match quality. Contrary to the standard search models predictions, the evidence on the impact of UI benefits extension on post-unemployment job quality, as measured by earnings in the new job, is mixed. A large body of existing literature finds no effect. The study of Lalive (2007), based on a regression discontinuity approach, reveals that large extensions in UI benefits in Austria increased unemployment duration, reduced transitions to a regular job, and increased the duration until a new job, but did not affect the average daily wages. In case of the reform in France mentioned above, the results suggest that faster exits to jobs were not related to a decline in job stability, measured by transition rates from employment back to unemployment. Another study for France focuses on the reform in 2000 that induced a large extension in PBD conditional on past employment duration over a reference period. Based on a regression discontinuity design, Le Barbanchon (2016) finds that this policy change had a significantly large and negative impact on unemployment exits to work, but no improvement of the match quality captured by hourly wage and employment duration. No effects on the quality of jobs that workers found after periods of unemployment have been identified for Slovenia, where a change in the UI system entailed substantial reductions in PBD for selected age groups of workers (van Ours and Vodopivec 2006, van Ours and Vodopivec 2008). The results from this natural experiment indicate a positive effect on the exit rate from unemployment to new jobs without affecting the quality of post-unemployment job matches. In particular, no effect on wages, or on the distribution between permanent and temporary jobs, or on the duration of the post-unemployment jobs has been detected. These findings, based on a difference-in-differences approach, allow the authors to conclude that the unemployment benefits reform in Slovenia diminished the moral hazard induced by the UI system.

In contrast to these studies, statistically significant negative and positive UI wage effects have also been identified. In their recent paper, Schmieder et al. (2016) adopt a regression disconti-

nuity design and find negative effects of UI extensions on reemployment wages in Germany. Furthermore, they show that this effect results from the existence of several potentially offsetting components. The first one may increase reemployment wages due to an increase in reservation wages or to stronger bargaining power. The second one may reduce search effort leading to longer nonemployment spells. If the wage offers decrease over time due to stigma or human capital depreciation, workers would face a reduction in reemployment wages. Their results point to tiny reservation wage effects, which allows the authors to conclude that reductions in reemployment wages over the unemployment spell cannot arise from changes in reservation wages. If longer unemployment spells do not help workers find a better job due to a negligible positive reservation wage effect and a large negative duration effect, the optimal UI length should be shortened. Unlike this conclusion, Nekoei and Weber (2017), exploiting a regression discontinuity design in Austria, show that extensions of relatively short UI benefits cause higher reemployment wages persistent over time that do not substitute other valuable job characteristics. This evidence supports the idea that UI subsidizes productive job search and just unproductive leisure.

3. Institutional Setting

3.1 The German Unemployment Compensation System

Similar to other European countries (e.g., Pellizzari 2006), the unemployment compensation system in Germany relies on two main pillars, namely UB-1 ("Arbeitslosengeld 1") and UB-2 ("Arbeitslosengeld 2"). UB-1 are funded by employee and employer contributions and are administered by the Federal Employment Agency. All employees subject to social security contributions are covered by this UI. However, entitlement to receive UB-1 is conditioned on contribution to the insurance scheme for at least 12 months within the last 24 months before a job loss and its duration period depends on the age and the employment history of unemployed workers. The PBD discontinuously increases with age in order to account for difficulties that older unemployed individuals might have to re-enter the job market. Workers, who reached the statutory retirement age, are excluded from the UI coverage. Monthly benefits replace 60% (67% for claimants with children) of the last net salary (capped at the social security ceiling). Payments are usually annulled for up to 12 weeks if employees take the initiative to terminate the employment relationship, therefore reducing the maximum benefits duration. Furthermore, recipients of UB-1 are required to actively search for a job and to prove job

searching activities (applications and responses by potential employers) upon request from the local employment office. Lack of compliance with these requirements may lead to benefit cuts. Upon exhaustion of UB-1 or in case of no entitlement to them, needy unemployed jobseekers receive tax-financed UB-2, which are unconstrained by previous earnings and are granted without temporal restrictions. They are means-tested against household income and aim at providing a living at the subsistence level. Non-compliance with the rules is subject to benefit sanctions that reduce the compensation level.

3.2 The German Reform of Unemployment Benefits 1

Our study evaluates the major reform of UB-1 in Germany, originated from an institutional change known under the name of Hartz-Reforms. The reform was announced during 2003 and came into force in February 2006, affecting workers who lost their jobs after the 31st of January 2006. This major policy change implied a substantial reduction in the potential duration of UB-1 and largely annulled the extensions of the 1980s that were motivated by an increasing unemployment rate and long average spell duration among older workers in West Germany (Hunt 1995). The core motivation of this reform was poor labor market performance of workers above 50 (Dietz and Walwei 2011). Aiming at promotion of reemployment among seniors, the introduced innovations were particularly penalizing for older workers with the maximum reduction in the potential benefits duration by 14 months. However, this reform lasted only till December 2007. In fact, as early as in January 2008 the German government enacted a new reform of the UI scheme, re-extending the PBD for older age groups. The main driving force were fairness considerations, according to which workers with contributions to the UI system for a longer period should be granted longer benefits durations. Nevertheless, this was a minor policy change that did not lead to the pre-reform state. In fact, the maximum extension in the potential benefits duration amounted to only 6 months and only few age groups were affected by this adjustment. Table 1 illustrates the major and minor policy changes in the potential duration of UB-1 for each age category.

[Insert Table 1 about here]

The major reform in February 2006 also modified eligibility criteria and work history requirements for receipt of UB-1. Under the old regime, workers were eligible if they have worked at least 12 out of the 36 months preceding unemployment. After the reform, employment during at least 12 out of the last 24 months is required. Work history at the moment of the claim is crucial for qualification for the maximum benefits duration. Before the reform,

individuals must have worked during the previous 84 months for a number of months equal to at least twice the potential benefits duration. Under the post-reform regime, they must have worked for a number of months equal to at least twice the potential benefits duration within the last 36 months prior to unemployment. The replacement rate decisive for the level of benefits was not affected by the reform.

3.3 Other Relevant Policy Changes

UB-2 were introduced in January 2005. They largely replaced two previous components of the German unemployment compensation system, namely unemployment assistance ("Arbeitslosenhilfe"), granted to unemployed jobseekers upon exhaustion of unemployment benefits, and social assistance ("Sozialhilfe"), granted to all other needy individuals, in particular to those who have never been employed before. The intention of this policy change was to reduce the dependence on benefits. To this end, the reform introduced strict rules aiming to motivate the recipients to intensely cooperate with the job centers and to actively search for a job, on the one hand, and enhanced support with the purpose to at least increase their employability by promoting education programs to gain skills valued on the labor market, on the other one. The practical enforcement of new rules is achieved through a complex of benefits sanctions.

Apart from the minor reform of UB-1 in January 2008 that partially re-extended the PBD for older workers, the German old age pension system has also been redesigned. In particular, until 2003, workers unemployed for at least 1 year could take advantage of the early retirement scheme without pension shortenings at the age of 60. From 2004 on, entry into pension due to unemployment became possible at the age of 63 at the earliest, thus, postponing early retirement. The analysis of the effects of UI reforms for workers approaching retirement age differs from those for other workers and is not the focus of this paper. The rationale behind this choice lies in the specificity of this population, which requires a simultaneous consideration of the unemployment compensation system and the retirement scheme. Just before retirement age, search intensity severely decreases, which weakens the incentive effect of cutting unemployment benefits. Indeed, in many European countries extended benefits programs for long-term unemployed and early retirement schemes allow for senior workers to leave the labor market before the legal retirement age (Kyyrä and Ollikainen 2008). This issue suggests that

retirement cannot be modeled separately from other spells out of work, justifying the exclusion of workers approaching retirement age in this study.

4. Data Set and Samples

For our analysis we use the routine data collected by the German Statutory Pension Insurance. The longitudinal data set includes a random sample of 20% of all individuals who completed medical rehabilitation treatments granted by this insurer. A scientific use file of the data on completed rehabilitation in the course of insurance 2002-2009 was made available by the Research Data Centre of the German Pension Insurance (FDZ-RV 2012). The data set consists of three databases.

SUFRSDV09BYB: It is a pension insurance follow-up database, which provides information on insurance relationship and amount of contribution payments. Information on the outcome variables of interest in this research field such as number of worked days, days in UB-1, and days in UB-2 are also collected in the database and employed in this study.

SUFRSDV09MCB: It includes all the cases with at least one completed medical rehabilitation, which in single cases may be supported by vocational rehabilitation and / or followed by granted pension benefits. The following variables contain detailed information on rehabilitation events during the reporting period 2002-2009: type of granted rehabilitation, implementation form on an inpatient or outpatient basis, begin / end of the treatment and its duration in days, rehabilitation region and medical discharge diagnoses. Moreover, labor market related variables at the moment of or shortly before the application for a rehabilitation treatment such as labor status, most recent activity, and occupational status are also available.

SUFRSDV09KOB: Standard socio-demographic characteristics such as birth / death year, nationality, residence region, gender, marital status, and education of the sample complete the data.

We restrict our sample to individuals, aged between 38 and 62 in the outcome year, who participated in only one medical rehabilitation in the observation period (ca. 75% of the whole sample), either before the UB-1 reform or thereafter. In this way, the data set takes the form of pooled cross-sections with information before and after the rehabilitation. Our dependent variables measure days in employment subject to social insurance contributions and days in registered unemployment in the outcome year. Non-employment, such as retirement due to health reasons or other labor market exits, is not considered in the analysis. Taking into ac-

count the timing of the reform of the old age pension system, we keep only individuals with completed rehabilitations between 2003 and 2008, for whom we can observe labor market outcomes in the years between 2004 and 2009. Observations with missing values are dropped. Based on this general sample composition, we consider three distinct samples as follows.

Preferred Sample A: In our preferred sample A, we keep only years 2005 (pre-reform) and 2007 (post-reform), and focus on individuals employed before rehabilitation. This temporal restriction relies on the following considerations. Firstly, UB-2 were introduced only in January 2005, while the potential duration of UB-1 was partially re-extended as early as in January 2008. The enacted policy changes prompt us to exclude the years prior to 2005 and after 2007. Secondly, the exclusion of the year 2006 is motivated by potential transition period and anticipation effects of the reform. Indeed, most individuals with completed rehabilitation in 2005 and days in unemployment benefits measured in 2006 are more likely to have entered unemployment under the old regime. This is, however, not the case for rehabilitations finished in 2006 when the reform came into force. Although this specification does not allow for controlling for pre-reform trends, it provides important insights into the impact of the policy change and offers an analysis free from any distortions that stem from anticipation effects of the reform. In order to check whether the common trend assumption holds in our data, we extend the considered time period to years between 2004 and 2009 in sample B. We further restrict our preferred sample A to those employed at least 12 months in the two calendar years before rehabilitation and the year of rehabilitation, i.e., during three calendar years before the outcome year. Although imperfect in its nature due to data construction, this restriction is supposed to approximate the sample's fulfillment of eligibility criteria both under the old and the new regime. As a result, sample A reduces the total number of observations by less than 10% and adds up to 94,990 observations, of which 46% are female and 52% are in the post-reform period (year 2007). Table 2 provides information about the data structure for sample A.

[Insert Table 2 about here]

Extended Sample B: We extend our preferred sample A to outcome years 2004-2009, which enables us to compare the pre-reform and post-reform trends and, thus, verify the fulfillment of the common trend assumption. The final sample B consists of 306,230 observations.

Additional Sample C: In conclusion, we keep only years 2005 and 2007, and focus on individuals either unemployed or non-employed before rehabilitation. The subsample with the unemployed amounts to 15,857 observations, while that with the non-employed consists of 16,529 observations.

5. Difference-in-Differences (DiD) Design and Variables

The major reform of UB-1 affected only individuals aged 45 or above and had a more pronounced impact for older age groups. The natural experiment setting allows us to apply a standard difference-in-differences approach, with assignment to the treatment and control groups according to age. The general estimation framework for our specification as described in equation (1) can be estimated by using linear regressions with OLS (ordinary least squares). A robustness check adopting count data models reveals the same results.

$$Y = \alpha + \beta_1 AGE + \beta_2 YEAR + \beta_3 AGE \times YEAR + \delta X + \varepsilon \qquad (1)$$

β_1 is the parameter for treatment group specific effect (age trend), β_2 is the parameter for time trend common to control and treatment groups, β_3 is the parameter of interest that provides the DiD estimate of the average treatment effect on the treated (ATT). *X* is a vector of covariates defined below, α is a constant, and ε is the error term.

Outcome variables Y: Y denotes the outcome of interest measured in the calendar year after medical rehabilitation and indicates days in UB-1, days in UB-2, and days in employment subject to social insurance contributions (WORK). They are aggregated for the complete calendar year from spell data and range from 0 to 365 days, providing information on the combined effect from the incidence and duration of recurring spells within a well-defined time period, thus capturing the reform effects that go beyond the first unemployment spell. For our preferred estimation sample A (2005/2007, employed before rehabilitation), the number of days in UB-1 is on average 39.6, the number of days in UB-2 is 6.2, and the number days in employment is 261.7. Non-employment, such as retirement due to health reasons or other labor market exits, is not considered in the analysis. All three outcome variables are, of course, highly correlated with each other, because more working days, ceteris paribus, decrease the number of days in registered unemployment. Nevertheless, we think it is important to analyze all these outcome variables separately. In fact, the total number of days cannot only be divided in employment and registered unemployment, but also in other sources of non-employment (e.g., family responsibility, early retirement).

Time period YEAR: The time period dummy *YEAR* captures aggregate factors that would cause changes in *Y* even in the absence of a policy change. In our preferred specification (sample A), we include only years 2005 and 2007 so that *YEAR* is a dummy for 2007, indicating the post-reform period. In the extended specification (sample B), we examine the pre-reform and post-reform trends for the years 2004 to 2009, testing for the common trend assumption. Here *YEAR* is a set of dummy variables, while the year 2004 serves as reference group.

Specifications AGE: The dummy variable *AGE* captures possible differences between the treatment and control groups independent of the policy change. As a first step, the treatment group is defined by all individuals aged 45 or above and amounts to 77% of individuals in our preferred sample A. Because reductions in the maximum duration implied by the policy change varied with respect to age categories, the treatment group is further defined according to these categories. Then, for a more detailed picture, we estimate a specification with dummies for each year of age. Age distribution across years reveals at least 1,000 observations in each year for each year of age (see Table A.1). In order to facilitate the interpretation, we predict and plot non-linear age profiles instead of interpreting the coefficients. These definitions, however, might suffer from some imprecision. In particular, we only consider age in years so that in 2007 (post-reform period) individuals within a specific age threshold (e.g., age 45) are not affected by the reform for the complete calendar year. This measurement error would bias the coefficients toward zero, hence, we expect a lower treatment effect for these age threshold groups. In line with Hunt (1995), who finds slightly larger coefficients when the treatment group is defined more accurately, our estimates should be interpreted as lower bounds.

Control variables X: We further account for sex, marital status, nationality, education, job position, occupation, federal state, and rehabilitation diagnosis in our model. These are treated as control variables, which results are not further discussed. Table A.2 offers a closer look at the descriptive statistics of these variables for our preferred estimation sample A (2005/2007, employed before rehabilitation).

Treatment intensity: In the next step, we revise our DiD strategy exploiting the difference in the treatment intensity between age groups, implied by the reform design. The treatment intensity was null for all age groups in 2005 and for those aged under 45 in 2007. But there was a positive and heterogeneous treatment intensity for individuals aged 45 and above in 2007. Younger workers were affected to a lesser extent than the older ones. In fact, the age group 45-46 suffered the smallest benefits cut by only 6 months, while age groups 52-54 and >56

suffered the largest reduction by 14 months. In order to address the issue that treatment intensity is correlated with age, we use different specifications of age as controls (age, age², age³, age dummies). Furthermore, we run a placebo test for years 2004 and 2005 and act as if UB-1 reduction would have occurred in 2005, although 2004 and 2005 are both pre-reform years. To this end, we adopt the following specification in equation (2):

$$Y = \alpha + \beta_1 AGE + \beta_2 YEAR + \beta_3 REDUCTION2007 + \delta X + \varepsilon \qquad (2)$$

6. DiD Regression Results

6.1 Main Results for 2005/2007

In our main analysis, we focus on sample A, i.e., on individuals, who were employed before the rehabilitation and for whom we observe labor market outcomes in the pre-reform year 2005 and the post-reform year 2007. We use three different specifications of age (age treatment dummy for age≥45, age treatment categories according to different reductions implied by the reform, age dummies for each year to estimate age profiles), which indicate the treatment assignment and are interacted with the post-reform year 2007 in our DiD design.

Table 3 contains the regression results for the age treatment dummy, i.e., individuals younger than 45 years are the control group and individuals equal or older than 45 years are the treatment group. The general age trends (age≥45) indicate that individuals equal or older than 45 years have on average about 17.8 more days in UB-1, 6.5 fewer days in UB-2, and 25.3 fewer days in WORK than individuals who are younger than 45 years, which supports the view that older workers perform worse in the labor market. The general time trends (year2007) are positive, because individuals in 2007 have on average about 6.7 fewer days in UB-1, 5 fewer days in UB-2, and 10.6 more days in WORK than individuals in 2005, which might be driven by the overall labor market reforms induced by the AGENDA 2010. The treatment effects (age≥45×year2007) of the reduction of the potential UB-1 duration indicate on average about 10.5 fewer days in UB-1, 4.7 more days in UB-2, and 13.6 more days in WORK. Because our treatment effects measure average changes in days per calendar year for outcomes that do not exhaust all possible labor market states, the treatment effects do not sum up to zero. In particular, we focus only on employment subject to social insurance contributions, excluding self-employment, minor employment and civil servants. Moreover, we exclude non-employment, such as retirement due to health reasons or other labor market exits. The treatment effects are

statistically significant and sizeable. If we put the absolute treatment effects simply in relation to the sample mean outcomes, days in UB-1 have decreased by about one fourth, days in UB-2 have increased by about three fourth, and days in WORK have increased by about 5%. Figure 1 illustrates the regression results by predicting the outcomes for each of the 4 groups (control/ treatment group in 2005/ 2007) in the DiD design, for which we only discuss UB-1 as an example. Predicted average days in UB-1 are 33.5 in 2005 and 26.8 in 2007 for the control group and 51.3 in 2005 and 34.1 in 2007 for the treatment group. Thus, the difference between the post-reform year 2007 and the pre-reform year 2005 is -6.7 for the control group and -17.2 for the treatment group, leading to the estimated DiD treatment effect of -10.5 days in UB-1. It should be kept in mind that the estimates in this study may not provide the true treatment effect for the entire eligible population. For the reasons explained previously, we argue that our treatment effects for UB-1 represent the lower bounds in absolute value, for UB-2 the upper bounds and for WORK the lower bounds for the treatment effects in the total population.

[Insert Table 3 about here]

[Insert Figure 1 about here]

In the next step, we replace the age treatment dummy with age treatment categories, i.e., we split the treatment group in age categories according to the different reductions of the potential UB-1 duration induced by the reform. The results in Table 4 support the previous findings. Older workers have on average more days in UB-1, fewer days in UB-2, and fewer days in WORK. Days in UB-1 and UB-2 are lower and days in WORK are larger in 2007 than in 2005. More importantly, the treatment effects have the same signs as before. The different age categories allow us to further analyze in how far the treatment effects differ within the treatment group of older workers. The reference group is the control group consisting of individuals younger than 45 years. The treatment effects are 4 fewer days in UB-1 for the age group 45 to 46, which experienced a reduction of the potential UB-1 duration by 6 months. For the age group 47 to 51 (reduction by 10 months) the treatment effect is 8 fewer days in UB-1. The age groups 52 to 54 (reduction by 14 months) as well as 55 to 56 (reduction by 8 months) have each a treatment effect of about 9 fewer days in UB-1. The largest treatment effect is estimated for the age group older than 56 (reduction by 14 months), which has about 14 fewer days in UB-1. The treatment effects on days in UB-2 do not differ that strongly and range between 3 more days in UB-2 for the youngest treatment age group (45-46) and 6 more days in UB-2 for the oldest age treatment group (age>56). Days in WORK have increased for the age

group 45 to 46 by 8 days, for the age group 47 to 51 by 6 days, for the age group 52 to 54 by 13 days, for the age group 55 to 56 by 16 days, and for the age group older than 56 by 17 days after the reform. We present a further graphical illustration in Figure 2, which plots the predicted outcomes, and in Figure 3, which additionally presents the differences between 2007 and 2005 for each age treatment category. The DiD is then the difference between the age treatment categories in the differences between 2007 and 2005. Overall, we can conclude that the treatment effects on days in UB-1, days in UB-2, and days in WORK are significant for all age treatment categories and that the absolute treatment effects are larger for older individuals.

[Insert Table 4 about here]

[Insert Figure 2-3 about here]

At last, we use age dummies for each year that are interacted with the post-reform year 2007. This specification allows us to estimate and predict completely non-linear age profiles. Figures 4 to 6 present the predicted outcomes and Figure 7 summarizes the differences between 2007 and 2005 for each year of age. It can easily be seen for days in UB-1 and days in WORK that the differences between 2007 and 2005 are larger for the treatment groups than for the control groups, i.e., the treatment leads to a reduction of days in UB-1 and an increase of days in WORK. For days in UB-2 the differences between 2007 and 2005 are close to zero for the treatment groups and negative for the control groups, i.e., the treatment leads to an increase of days in UB-2. A further inspection of the non-linear age-profiles for days in UB-1 and days in UB-2 reveals that we cannot measure a significant treatment effect for the age threshold at 45 years. As mentioned earlier, we have a measurement problem at the age threshold values, because we only have age in years and not in months or even in days in our data. Thus, individuals turn 45 in the year 2007 and are not affected by the reform for the complete calendar year.

[Insert Figure 4-7 about here]

6.2 Pre-Reform and Post-Reform Trends for 2004-2009

Crucial for a causal interpretation of treatment effects in DiD designs is the parallel (common) trend assumption, i.e., the time trends must not differ between control groups (age<45) and

treatment groups (age≥45) in the absence of the reform in order to estimate unbiased treatment effects, which are the coefficients of the interaction terms between treatment groups and post-reform years. To check the pre-reform and post-reform trends, we repeat the analysis for the years 2005 and 2007 (Sample A, employed before rehabilitation) from the previous section with the full set of years from 2004 to 2009 (Sample B, employed before rehabilitation). The results in Table 5 show that the treatment effects are only observed in the post-reform years and not in the pre-reform years, which supports the parallel trend assumption. In more detail, the coefficients of the interaction terms between age≥45 and the years 2005 and 2006[1] do not differ significantly from zero and from the reference year 2004, whereas the coefficients of the interaction terms between age≥45 and the post-reform years 2007, 2008, and 2009 differ significantly from zero and from the pre-reform years 2004, 2005, and 2006. The more detailed age profiles in Figures 8, 9, and 10 also support the parallel trend assumption. As we do not find evidence for a violation of the parallel trend assumption and can identify a structural break between pre-reform years (2004-2006) and post-reform years (2007-2009), we are confident that our estimated treatment effects are not a statistical artifact.

[Insert Table 5 about here]

[Insert Figure 8-10 about here]

6.3 Gender and Regional Differences for 2005/2007

In order to check for potential gender and regional differences, we split our preferred sample A (2005/2007, employed before rehabilitation) between men and women (see Table 6 and Figure 11) as well as between West (including Berlin) and East German Federal States (see Table 7 and Figure 12). The results for the separate samples support our previous findings for the complete sample in section 6.1. The small gender and regional differences indicate, if anything, that the reform affected women and people living in East German Federal States slightly more positively, i.e., days in UB-1 decreased and days in WORK increased even more, whereas days in UB-2 did not increase that much.

[1] Although the coefficient of the interaction term between age≥45 and year 2006 is positive and statistically significant at $p<0.01$, which might indicate some anticipation effect of the reform, it is small in magnitude (3.4 days) and does not affect our main results from the preferred specification (sample A, 2005/2007).

[Insert Table 6 about here]

[Insert Figure 11 about here]

[Insert Table 7 about here]

[Insert Figure 12 about here]

6.4 Results Unemployed and Non-Employed before Rehabilitation for 2005/2007

In the previous sections, we focused on individuals who were employed before the medical rehabilitation. These individuals make up the majority of the complete sample. But there might also be a positive selection and their decisions to enter unemployment are likely to be more important than their decisions to exit unemployment for our analyzed labor market outcome variables. To give a more complete picture of the labor market reform, we repeat our previous analyses for individuals who were unemployed and non-employed before the medical rehabilitation (sample C, 2005/2007). The overall effects are less positive for these unemployed and non-employed samples than for individuals who were employed before the medical rehabilitation. The results in Table 8, which are also supported by the age profiles in Figure 13, indicate average treatment effects for the unemployed sample of about 6 fewer days in UB-1, 10 more days in UB-2, and 2 fewer days in WORK, whereby only the estimated treatment effect for days in UB-1 is statistically significant at $p<0.05$. The average treatment effects for the non-employed sample indicate about 10 fewer days in UB-1, 19 more days in UB-2, and 13 fewer days in WORK. Thus, the decreased days in UB-1 are largely due to a slip down in UB-2 and the reform seems to have even a negative effect on WORK in the unemployed and non-employed samples.

[Insert Table 8 about here]

[Insert Figure 13 about here]

6.5 Results Treatment Intensity for 2005/2007

We extend the standard DiD strategy for our preferred sample A (2005/2007, employed before rehabilitation) by replacing the interaction terms between age and the post-reform year

2007 with a treatment intensity variable, which measures the UB-1 reduction in months for the different age groups. The treatment intensity variable follows the DiD strategy, because it is in principle an interaction between treatment intensity and the post-reform year. As can be seen in Table 9, the treatment intensity is zero for all observations in 2005 and for all observations younger than 45 years in 2007, whereas the treatment intensity is positive for all observations equal or older than 45 years in 2007. Because the treatment intensity is correlated with age, we estimate different specifications of age as controls (age, age^2, age^3, age dummies). The results in Table 10 show that the size of the treatment intensity effect is indeed smaller if non-linearity of age is taken into account. Overall, the results in Table 10 support our previous findings, but allow a different quantitative interpretation. The reduction in potential duration of UB-1 by 1 month decreases UB-1 on average by more than 0.6 days per year, increases UB-2 on average by about 0.3 days per year, and increases WORK on average by more than 0.8 days per year. In order to rule out that our estimated treatment intensity effects are a statistical artifact, we perform a placebo test for the years 2004 and 2005 and act as if the UB-1 reduction in months would have occurred in 2005, although 2004 and 2005 are both pre-reform years. As the estimated coefficients in the placebo test are either not significantly different from zero or even have the opposite sign than the estimated treatment intensity effects for 2007, we are confident that the treatment intensity effects are not a statistical artifact.

[Insert Table 9 about here]

[Insert Table 10 about here]

7. Discussion and Concluding Remarks

This study offers a comprehensive evaluation of the major German reform of regular unemployment benefits (unemployment benefits 1, UB-1), enacted in February 2006 with the purpose to reestablish incentives to work and to delay withdrawals from the labor market. The policy change induced a substantial reduction in the potential benefits duration for older workers, thus alleviating the disincentive effect of long compensation provided by the UI system. Our estimation results, based on a difference-in-differences approach, reveal partially positive effects of the reform and, in line with the analyses of similar reforms in the European context, suggest the need for more complete evaluations of policy changes in general (Pellizzari 2006, Fremigacci 2010).

We find evidence that individual labor market attachment matters, so does the value of the UI recipients' outside options (employment, non-employment, unemployment assistance or other social programs). In particular, for our preferred sample of individuals who were employed before medical rehabilitation, we find that days in UB-1 decrease by 10.5 and days in employment increase by 13.6, which hints at a positive treatment effect. However, a deeper analysis indicates that days in unemployment assistance (unemployment benefits 2, UB-2), which is granted to unemployed jobseekers without temporal restrictions upon exhaustion of UB-1, increase by 4.7. Supplementary analysis of individuals who are less attached to the labor market (individuals who were unemployed or non-employed before medical rehabilitation) provides a more complete picture of the distribution of days in UB-1, days in UB-2, and days in employment. Although a positive impact on days in UB-1 is observed also for this population group, they perform worse on the labor market. In particular, days in UB-2 largely increase, while days in employment even decrease in response to the reform. These findings are consistent with the recent work on effectiveness of more comprehensive reforms of labor market institutions as opposed to one policy reform at a time. Indeed, from the labor market and social policy perspective, transitions to UB-2 upon exhaustion of UB-1 denote an unintended consequence of the reform, limiting the success of a policy change that aims to increase labor supply via reductions in the generosity of the UI system. To the best of our knowledge, this is the first paper that explicitly investigates this important aspect of the major reform of UB-1 in Germany and is intended to fill the gap.

Due to data construction, our study presents some limitation with respect to measurement accuracy of eligibility condition to qualify for the potential benefits duration. In practice, the duration of benefits receipt that an unemployed worker applying for unemployment compensation is entitled to, is calculated from his or her work history over a reference period just prior to job separation. In our work, however, due to lack of information on age in months and work history of the unemployed at the date of unemployment entry, we measure their labor market attachment within entire calendar years. One might argue that this imprecision could compromise our results. Nevertheless, we are able to restrict our samples in a way to approximate the fulfillment of eligibility criteria both under the old and the new regime. In line with Hunt (1995), who finds slightly larger coefficients when the treatment group is defined more accurately, our estimates should be interpreted as lower and upper bounds for the treatment effects in the total population. Furthermore, our sample consists of individuals with some health deficiency who participated in a medical rehabilitation program. We believe this aspect rather opens an interesting perspective in the reform evaluation and positive effects of the re-

form for this population group encourage us to predict even larger effects for healthy workers with no need for medical rehabilitation. Our expectation relies on a number of studies that focus on employment outcomes of work-limited workers and identify the underlying mechanisms of their adverse labor market prospects (Charles 2003, Mok et al. 2008, Colella and Bruyère 2011). Based on this literature, we expect individuals with no health impairment to be more responsive to the UI incentives, interpreting our estimates as lower and upper bounds for the treatment effects in the total population.

Our results hint at the importance to design labor market reforms in a wider framework of institutional interactions. In fact, a common feature of the European institutions is the interaction of unemployment insurance with other social security programs. Furthermore, institutional similarity in the European context may encourage the policy makers to learn from the neighbors' experience. In fact, only three years apart, the French government also enacted a major reform of the UI system, which significantly shortened the potential benefits duration for older workers. Fremigacci (2010) claims that the major effect of this policy change was to shift job seekers from regular unemployment benefits to unemployment assistance. When the objective of policy makers is to discourage moral hazard behavior via shortening of unemployment benefits duration, a broader consideration of labor market performance upon exhaustion of regular unemployment benefits seems appropriate. Thus, on the example of the unemployment benefits reform in Slovenia, the intended decline in moral hazard induced by the UI system is acknowledged to be achieved, because increased transitions from unemployment to new jobs did not occur at the expense of the quality of post-unemployment job matches (van Ours and Vodopivec 2006, van Ours and Vodopivec 2008). On the contrary, the claim of abated moral hazard behavior of benefits' recipients might be rather unfounded if increased employment comes along with a loss of job match quality in response to the reform and longer unemployment spells would have facilitated productive job search. Furthermore, non-negligible exits from unemployment to non-employment as well as shifts from unemployment benefits to unemployment assistance or to other welfare programs undermine potentially positive effects on duration of regular unemployment benefits, calling into doubt the success of proposed mitigation of the disincentive effect of long compensation provided by the UI system.

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Appendix A: Tables

Table A.1: Number of observations in year-age cells (employed before rehabilitation)

age	2004	2005	2006	2007	2008	2009	Total
38	1,375	1,140	1,206	1,150	1,125	1,064	7,060
39	1,399	1,272	1,282	1,352	1,280	1,293	7,878
40	1,575	1,393	1,456	1,385	1,435	1,524	8,768
41	1,712	1,441	1,614	1,645	1,679	1,677	9,768
42	1,724	1,609	1,679	1,711	1,847	1,796	10,366
43	1,883	1,683	1,795	1,922	1,974	2,111	11,368
44	1,930	1,765	1,870	1,963	2,194	2,177	11,899
45	1,981	1,812	2,014	2,013	2,164	2,369	12,353
46	1,949	1,888	1,958	2,225	2,275	2,506	12,801
47	2,070	1,834	2,084	2,163	2,389	2,593	13,133
48	2,065	1,825	2,106	2,250	2,439	2,752	13,437
49	1,928	1,927	2,166	2,273	2,633	2,763	13,690
50	2,121	1,889	2,138	2,221	2,419	2,880	13,668
51	2,164	1,985	2,164	2,267	2,583	2,748	13,911
52	2,346	2,117	2,271	2,495	2,620	2,864	14,713
53	2,447	2,178	2,296	2,508	2,609	2,995	15,033
54	2,505	2,202	2,328	2,384	2,674	2,811	14,904
55	2,677	2,330	2,407	2,503	2,597	2,900	15,414
56	2,641	2,465	2,473	2,469	2,683	2,904	15,635
57	2,568	2,181	2,448	2,483	2,611	2,805	15,096
58	2,483	2,244	2,315	2,283	2,506	2,587	14,418
59	2,178	1,969	2,148	1,983	2,233	2,595	13,106
60	2,802	1,661	1,825	1,817	1,949	2,289	12,343
61	1,880	1,570	1,084	1,116	1,338	1,619	8,607
62	1,407	1,323	1,307	706	938	1,180	6,861
Total	51,810	45,703	48,434	49,287	53,194	57,802	306,230

Table A.2: Summary statistics (Sample A: 2005/2007, employed before rehabilitation)

	Mean	Std. dev.	Min	Max
OUTCOME VARIABLES (Y)				
days UB-1 in calendar year (UB-1)	39.5784	93.0501	0	365
days UB-2 in calendar year (UB-2)	6.1490	40.7390	0	365
days employed in calendar year (WORK)	261.6766	151.7951	0	365
DiD VARIABLES (AGE, YEARS)				
age in years	50.4924	6.5965	38	62
age≥45 (affected by reform)	0.7744	0.4180	0	1
year2005 (pre-reform)	0.4811	0.4996	0	1
year2007 (post-reform)	0.5189	0.4996	0	1
CONTROL VARIABLES (X)				
female (dummy)	0.4641	0.4987	0	1
MARITALSTATUS (dummies)				
Single (reference group)	0.1254	0.3312	0	1
Married	0.7147	0.4516	0	1
Divorced	0.1312	0.3376	0	1
Widowed	0.0287	0.1671	0	1
NATIONALITY (dummies)				
Germany (reference group)	0.9429	0.2321	0	1
Italy, Spain, Greece, Portugal	0.0108	0.1036	0	1
Former Yugoslavia	0.0122	0.1096	0	1
Turkey	0.0145	0.1196	0	1
Other EU and non-EU country	0.0167	0.1281	0	1
Stateless, unknown	0.0029	0.0541	0	1
EDUCATION (dummies)				
Unknown, not applicable (reference group)	0.1747	0.3797	0	1
Low/ medium secondary schooling degree without apprenticeship	0.1288	0.3350	0	1
Low/ medium secondary schooling degree with apprenticeship	0.6062	0.4886	0	1
High secondary schooling degree without apprenticeship	0.0039	0.0622	0	1
High secondary schooling degree with apprenticeship	0.0261	0.1594	0	1
University of Applied Science degree	0.0292	0.1684	0	1
University degree	0.0311	0.1735	0	1
JOBPOSITION (dummies)				
Unknown, not applicable (reference group)	0.0034	0.0579	0	1
Apprentice	0.0006	0.0249	0	1
Unskilled blue-collar worker	0.1076	0.3099	0	1
Low skilled blue-collar worker	0.1020	0.3027	0	1

Skilled blue-collar worker	0.2724	0.4452	0	1
Master craftsman, foreman	0.0132	0.1143	0	1
White-collar worker	0.4955	0.5000	0	1
Civil servant	0.0004	0.0195	0	1
Self-employed	0.0047	0.0687	0	1
OCCUPATION (dummies)				
Unknown, not applicable (reference group)	0.0621	0.2413	0	1
Agriculture, forestry and fishing	0.0124	0.1106	0	1
Mining and quarrying	0.0036	0.0595	0	1
Manufacturing	0.0332	0.1791	0	1
Metal-making and metal-working	0.1236	0.3292	0	1
Textile-making and textile-processing	0.0056	0.0745	0	1
Accommodation and food service activities	0.0255	0.1576	0	1
Construction	0.0885	0.2841	0	1
Professional, scientific and technical activities	0.0555	0.2290	0	1
Trade and transportation	0.1846	0.3880	0	1
Administrative and support service activities	0.2059	0.4043	0	1
Health care	0.1144	0.3183	0	1
Teaching and training	0.0244	0.1544	0	1
Other	0.0607	0.2387	0	1
FEDERAL STATE (dummies)				
Berlin (reference group)	0.0428	0.2024	0	1
Schleswig Holstein	0.0287	0.1669	0	1
Hamburg	0.0159	0.1252	0	1
Lower Saxony	0.0998	0.2997	0	1
Bremen	0.0060	0.0775	0	1
Northrhine-Westphalia	0.2032	0.4024	0	1
Hesse	0.0740	0.2617	0	1
Rhineland Palatinate	0.0471	0.2118	0	1
Baden-Wurttemberg	0.1419	0.3489	0	1
Bavaria	0.1596	0.3663	0	1
Saarland	0.0137	0.1162	0	1
Brandenburg	0.0319	0.1758	0	1
Mecklenburg-West Pomerania	0.0222	0.1473	0	1
Saxony	0.0537	0.2254	0	1
Saxony-Anhalt	0.0260	0.1590	0	1
Thuringia	0.0336	0.1801	0	1
REHABILITATION DIAGNOSIS (dummies)				
166 medical diagnoses				

Notes: Sample A (2005/2007, employed before rehabilitation). Number of observations N=94,990.

Tables and figures to be included in text

Table 1: Maximum duration (in months) of unemployment benefits in Germany

Age category	Before 2/2006	Reduction	2/2006-12/2007	Extension	Since 1/2008
< 45	12	0	12	0	12
45-46	18	6	12	0	12
47-49	22	10	12	0	12
50-51	22	10	12	3	15
52-54	26	14	12	3	15
55-56	26	8	18	0	18
57	32	14	18	0	18
>57	32	14	18	6	24

Table 2: Data structure (example for Sample A: 2005/2007, employed before rehabilitation)

<table>
<tr><td>Year
Group</td><td>2002</td><td>2003</td><td>2004</td><td>2005
[pre-reform]</td><td>2006
[reform]</td><td>2007
[post-reform]</td></tr>
<tr><td rowspan="2">2005
(pre-reform)</td><td colspan="3">sample restriction (full entitlement length):
working days 2002/03/04 ≥ 365</td><td></td><td colspan="2" rowspan="2"></td></tr>
<tr><td colspan="2"></td><td>- employed
-rehabilitation
- rehabilitation exit</td><td>outcomes</td></tr>
<tr><td rowspan="2">2007
(post-reform)</td><td colspan="2" rowspan="2"></td><td colspan="3">sample restriction (full entitlement length):
working days 2004/05/06 ≥ 365</td><td></td></tr>
<tr><td colspan="2"></td><td>- employed
- rehabilitation
- rehabilitation exit</td><td>outcomes</td></tr>
</table>

Table 3: DiD results for age treatment dummy (Sample A: 2005/2007, employed before rehabilitation)

	(1) UB-1	(2) UB-2	(3) WORK
age≥45	17.80***	-6.51***	-25.29***
	[0.97]	[0.59]	[1.57]
year2007	-6.66***	-4.96***	10.56***
	[1.01]	[0.67]	[1.79]
age≥45 × year2007 (post-reform)	-10.50***	4.65***	13.57***
	[1.22]	[0.72]	[2.06]
Control variables	Yes	Yes	Yes
R^2	0.11	0.07	0.19
Mean dep. variable	39.58	6.15	261.68
N	94,990	94,990	94,990

Notes: Sample A (2005/2007, employed before rehabilitation). Outcome variables are days per calendar year. OLS regressions. Robust standard errors in brackets. * p<0.05; ** p<0.01; *** p<0.001.

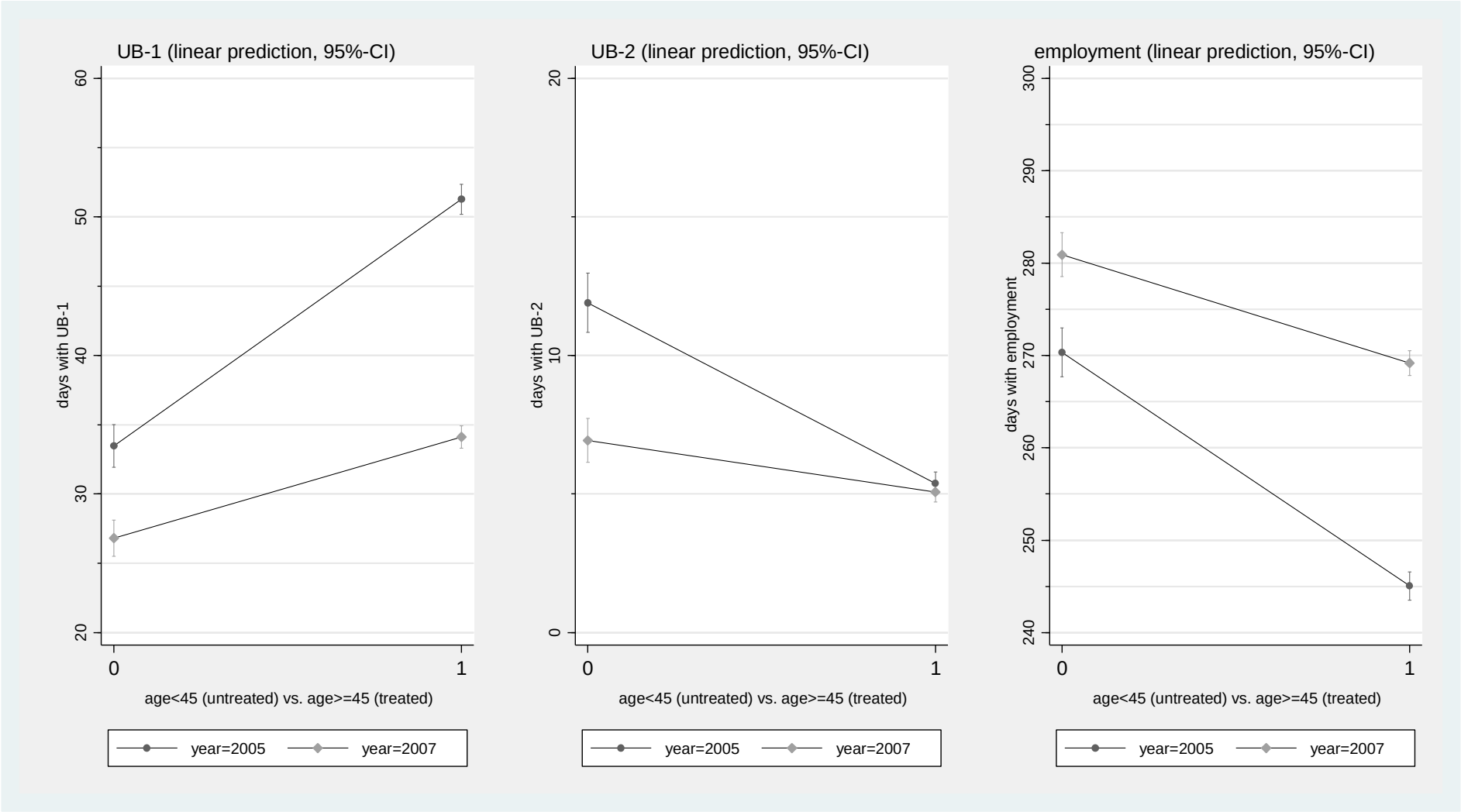

Figure 1: DiD results for age treatment dummy (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

Table 4: DiD results for age treatment categories (Sample A: 2005/2007, employed before rehabilitation)

	(1) UB-1	(2) UB-2	(3) WORK
age 45-46	4.00*	-3.08**	-5.32*
	[1.58]	[0.95]	[2.62]
age 47-51	9.74***	-5.55***	-4.58*
	[1.24]	[0.70]	[1.95]
age 52-54	13.74***	-6.32***	-15.94***
	[1.46]	[0.73]	[2.23]
age 55-56	18.08***	-7.08***	-32.55***
	[1.69]	[0.78]	[2.53]
age>56	35.45***	-8.90***	-60.08***
	[1.40]	[0.63]	[2.03]
year2007	-6.61***	-4.97***	10.46***
	[1.01]	[0.67]	[1.79]
age 45-46 (reduction -6 months) × year2007 (post-reform)	-4.44*	2.56*	7.72*
	[2.00]	[1.19]	[3.47]
age 47-51 (reduction -10 months) × year2007 (post-reform)	-7.87***	4.30***	5.88*
	[1.54]	[0.87]	[2.55]
age 52-54 (reduction -14 months) × year2007 (post-reform)	-9.28***	4.04***	12.68***
	[1.79]	[0.90]	[2.91]
age 55-56 (reduction -8 months) × year2007 (post-reform)	-9.04***	4.86***	15.85***
	[2.12]	[0.99]	[3.36]
age>56 (reduction -14 months) × year2007 (post-reform)	-14.22***	5.75***	17.16***
	[1.78]	[0.79]	[2.70]
Control variables	Yes	Yes	Yes
R^2	0.12	0.07	0.20
Mean dep. variable	39.58	6.15	261.68
N	94,990	94,990	94,990

Notes: Sample A (2005/2007, employed before rehabilitation). Outcome variables are days per calendar year. OLS regressions. Robust standard errors in brackets. * p<0.05; ** p<0.01; *** p<0.001.

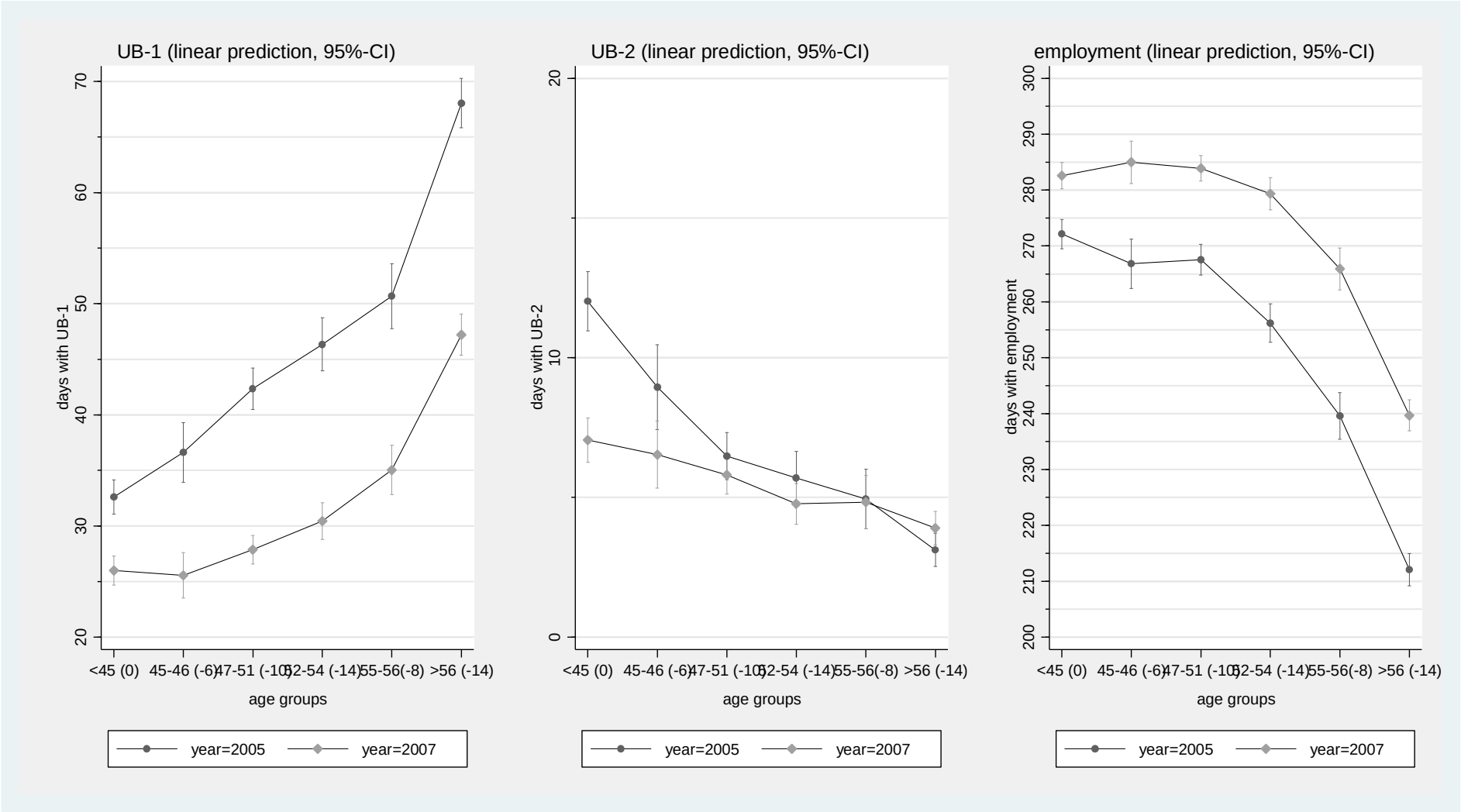

Figure 2: DiD results for age treatment categories (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

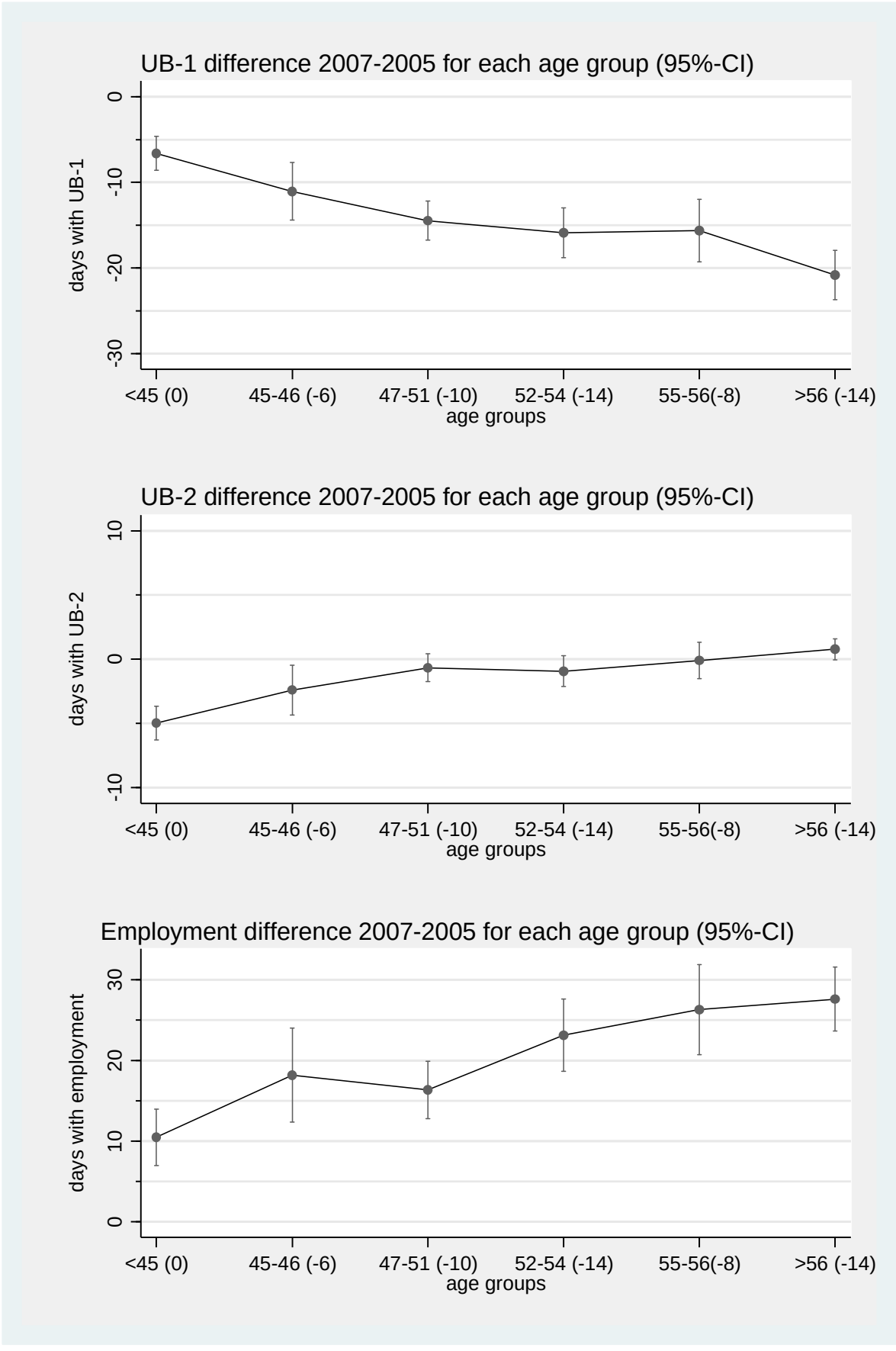

Figure 3: Differences 2007-2005 for each age treatment category (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

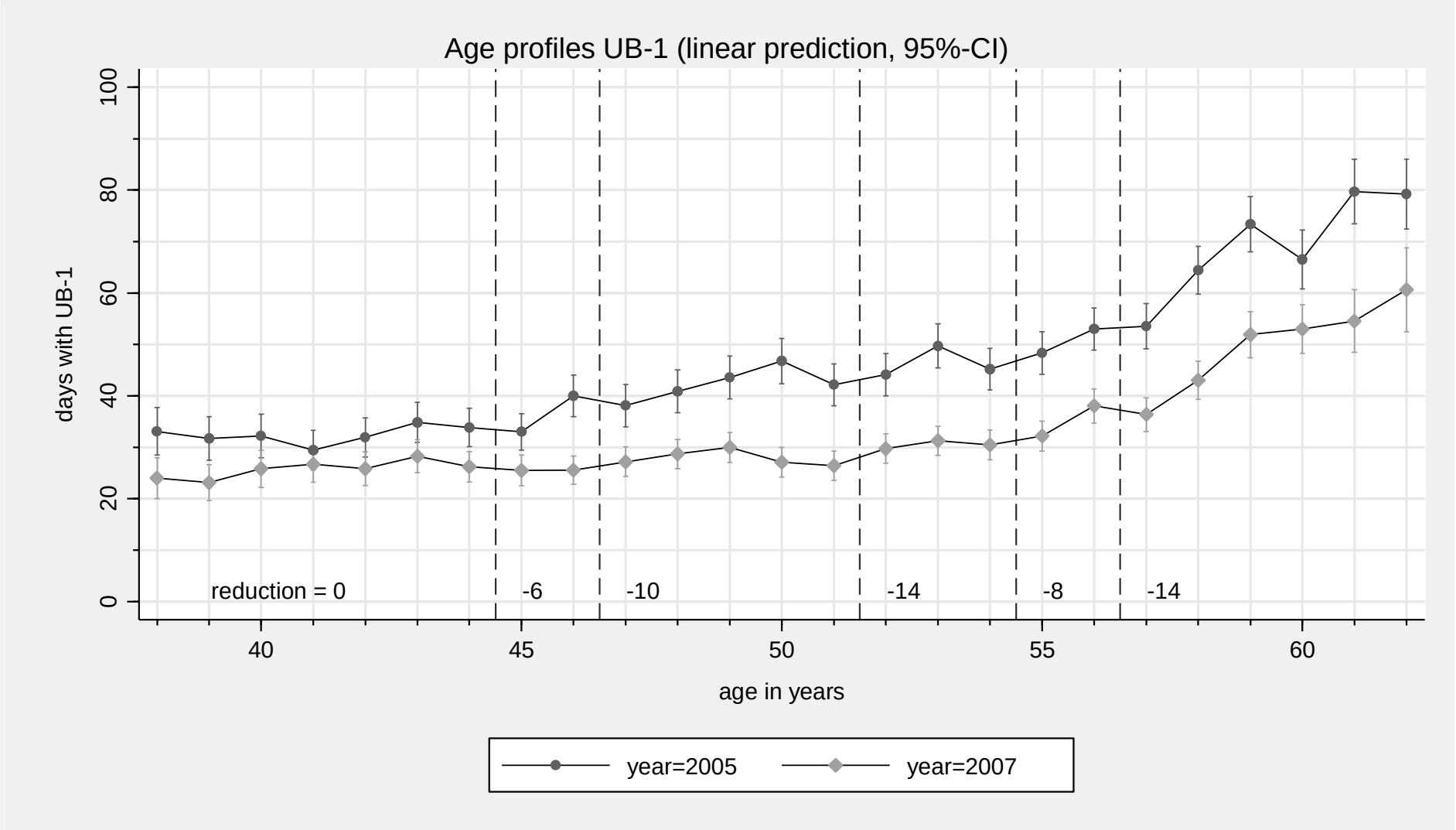

Figure 4: Age profiles UB-1 (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

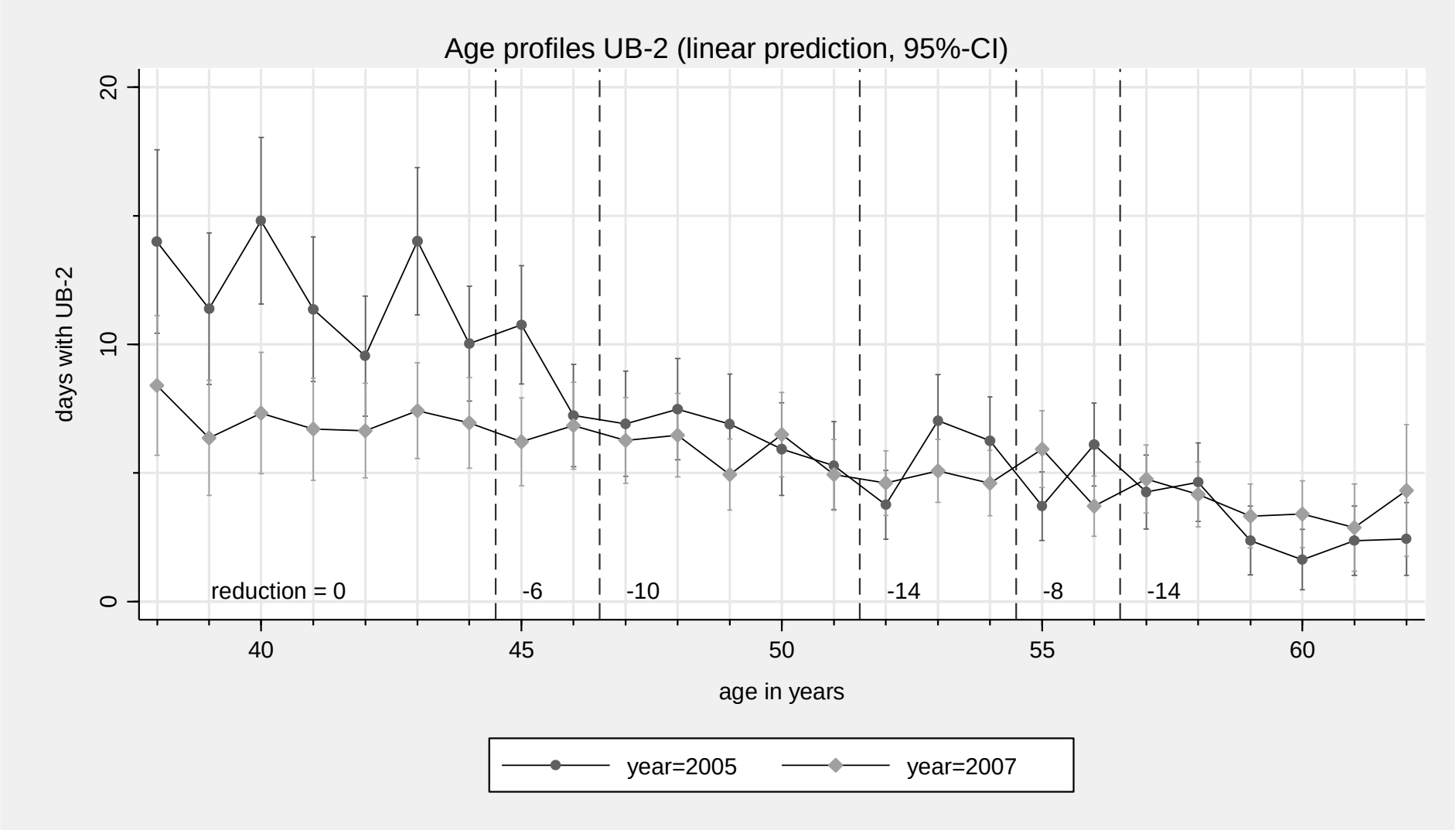

Figure 5: Age profiles UB-2 (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

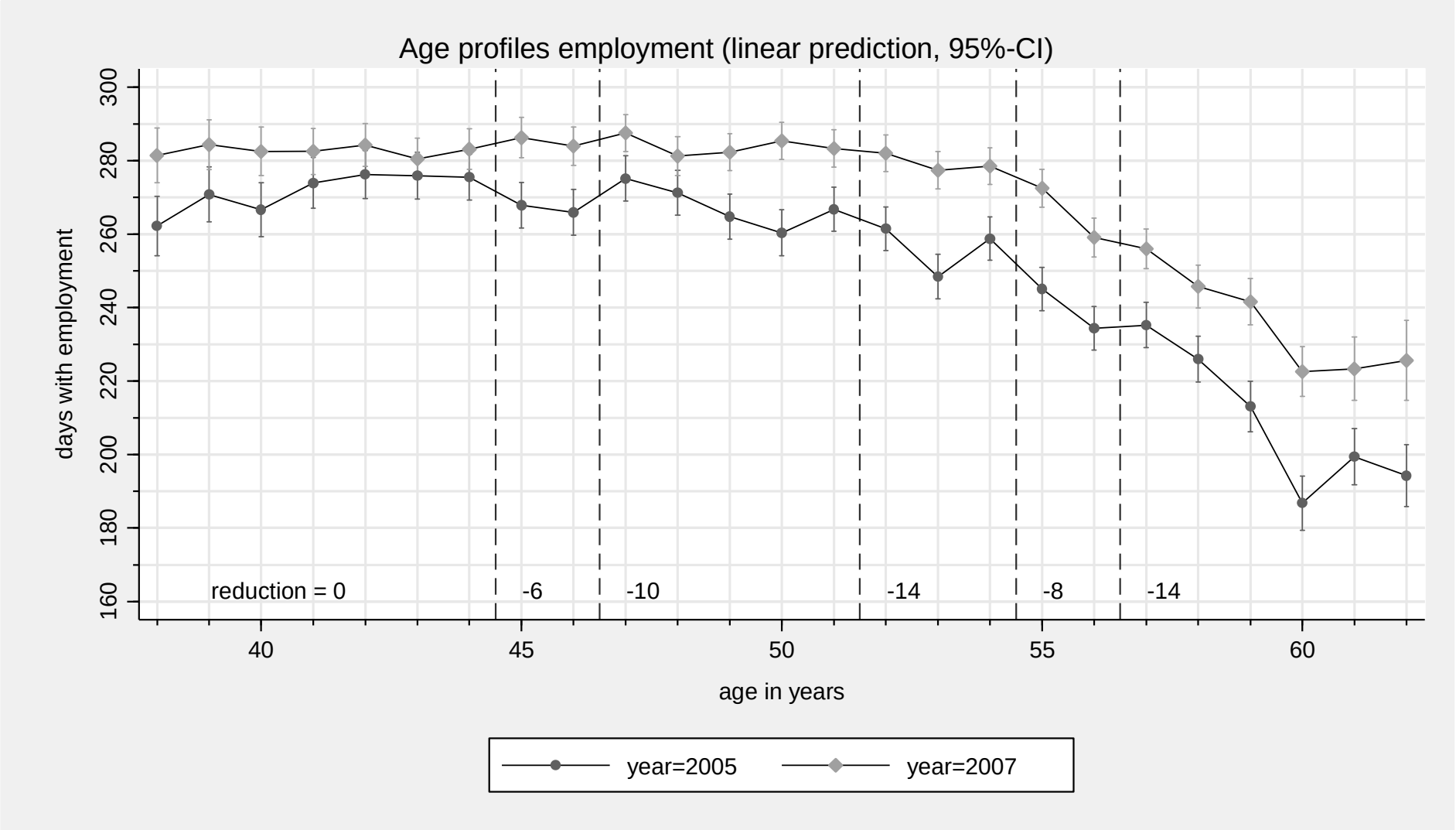

Figure 6: Age profiles employment (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

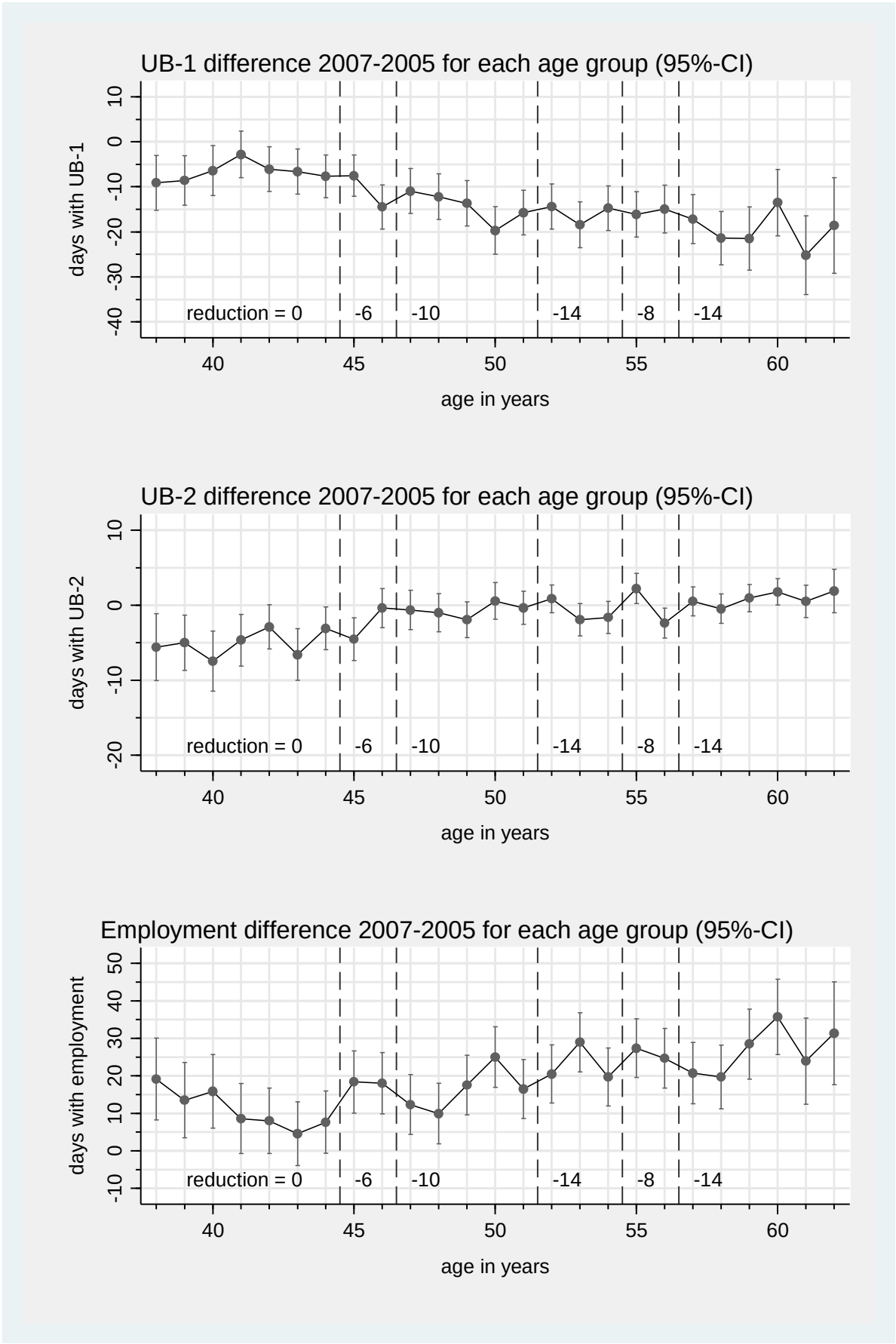

Figure 7: Differences 2007-2005 for each age group (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

Table 5: DiD results and trends for age treatment dummy (Sample B: 2004-2009, employed before rehabilitation)

	(1) UB-1	(2) UB-2	(3) WORK
age≥45	17.52***	-5.39***	-28.72***
	[0.94]	[0.43]	[1.48]
year2005	-3.73***	4.08***	4.84**
	[1.09]	[0.68]	[1.84]
year2006	-8.17***	5.27***	12.26***
	[1.04]	[0.69]	[1.79]
year2007	-10.34***	-0.98	15.33***
	[1.01]	[0.56]	[1.75]
year2008	-9.31***	-2.81***	15.48***
	[1.01]	[0.51]	[1.74]
year2009	-6.31***	-1.85***	9.06***
	[1.04]	[0.54]	[1.76]
age≥45 × year2005	0.38	-1.27	3.61
	[1.33]	[0.72]	[2.13]
age≥45 × year2006	3.35**	-1.05	4.61*
	[1.29]	[0.74]	[2.07]
age≥45 × year2007 (post-reform)	-10.14***	3.43***	17.17***
	[1.21]	[0.61]	[2.01]
age≥45 × year2008 (post-reform)	-10.99***	4.38***	19.74***
	[1.20]	[0.55]	[2.00]
age≥45 × year2009 (post-reform)	-12.61***	3.61***	23.25***
	[1.22]	[0.58]	[2.00]
Control variables	Yes	Yes	Yes
R^2	0.11	0.06	0.18
Mean dep. variable	40.47	5.51	261.43
N	306,230	306,230	306,230

Notes: Sample B (2004-2009, employed before rehabilitation). Outcome variables are days per calendar year. OLS regressions. Robust standard errors in brackets. * p<0.05; ** p<0.01; *** p<0.001.

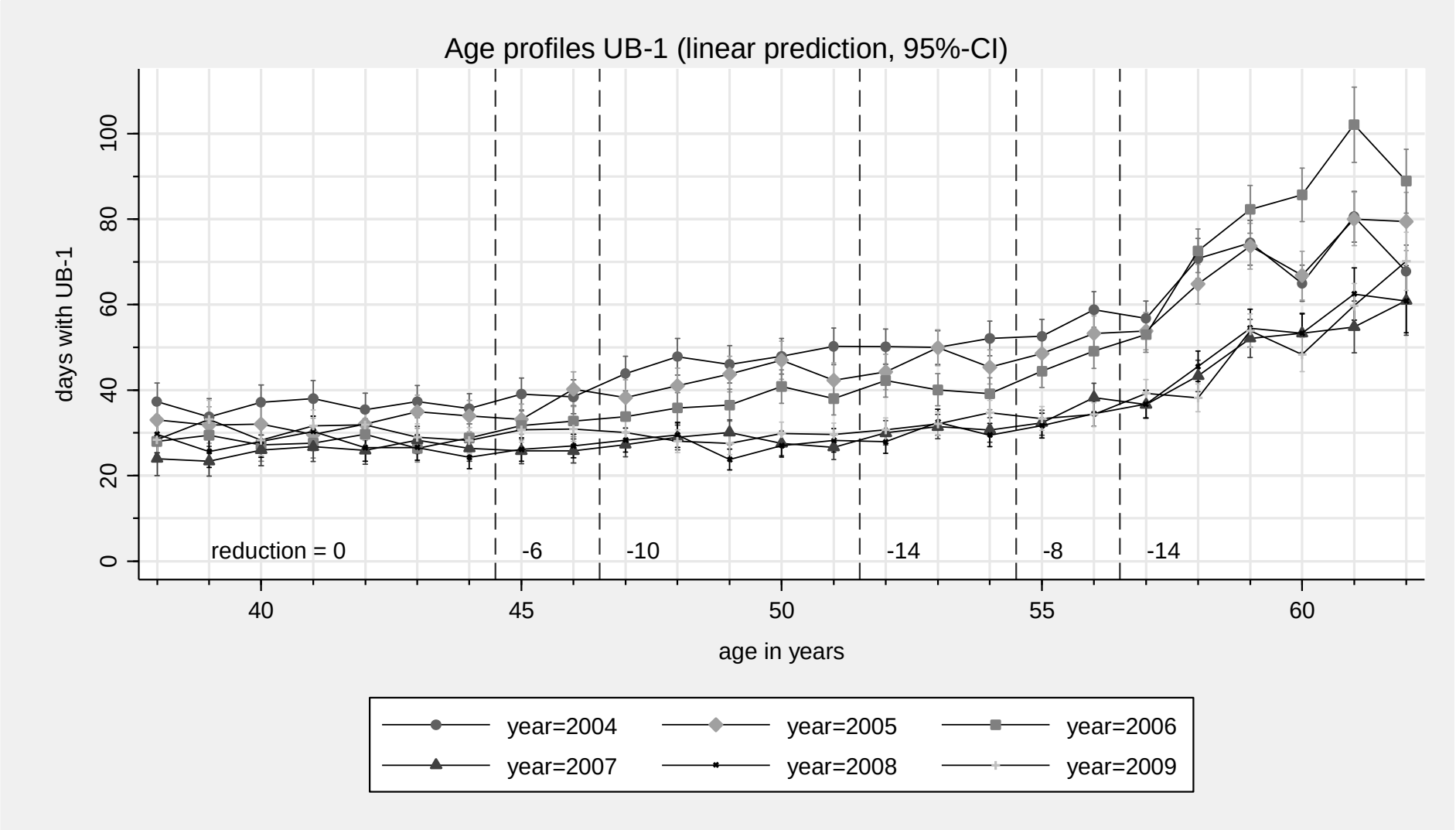

Figure 8: Age profiles UB-1 (Sample B: 2004-2009, employed before rehabilitation) (SE and CI by delta method)

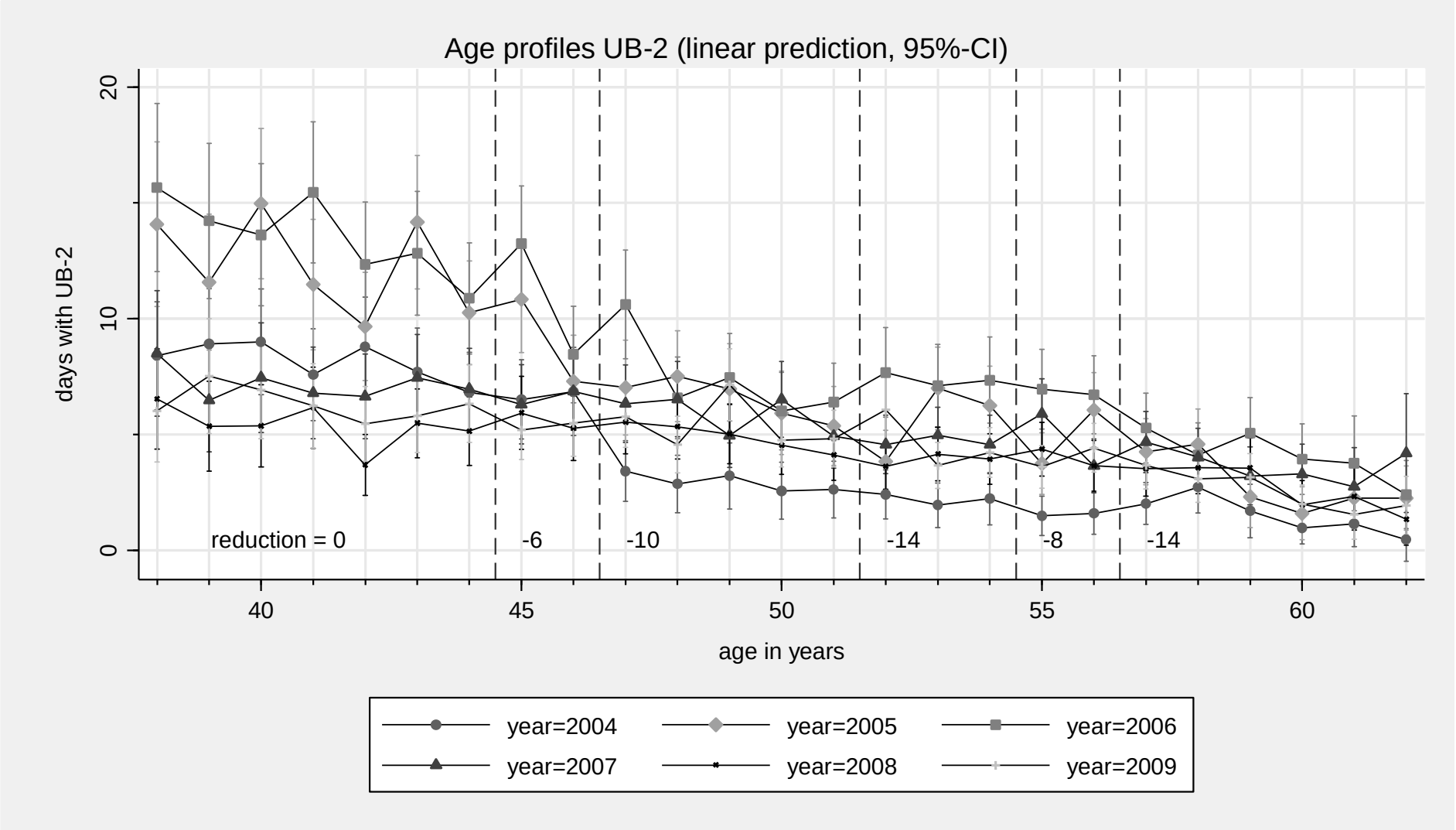

Figure 9: Age profiles UB-2 (Sample B: 2004-2009, employed before rehabilitation) (SE and CI by delta method)

Figure 10: Age profiles employment (Sample B: 2004-2009, employed before rehabilitation) (SE and CI by delta method)

Table 6: DiD results men vs. women for age treatment dummy (Sample A: 2005/2007, employed before rehabilitation)

	Men			Women		
	(1) UB-1	(2) UB-2	(3) WORK	(1) UB-1	(2) UB-2	(3) WORK
age≥45	18.21***	-6.89***	-28.79***	17.35***	-6.15***	-20.65***
	[1.32]	[0.79]	[2.11]	[1.44]	[0.88]	[2.36]
year2007	-7.96***	-5.44***	13.85***	-4.91**	-4.50***	6.15*
	[1.36]	[0.90]	[2.37]	[1.53]	[1.01]	[2.70]
age≥45 × year2007 (post-reform)	-8.76***	5.70***	10.13***	-12.76***	3.48**	17.77***
	[1.64]	[0.97]	[2.76]	[1.83]	[1.09]	[3.10]
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.12	0.08	0.20	0.11	0.07	0.18
Mean dep. variable	39.66	6.14	260.02	39.49	6.16	263.59
N	50,903	50,903	50,903	44,087	44,087	44,087

Notes: Sample A (2005/2007, employed before rehabilitation). Outcome variables are days per calendar year. OLS regressions. Robust standard errors in brackets. * p<0.05; ** p<0.01; *** p<0.001.

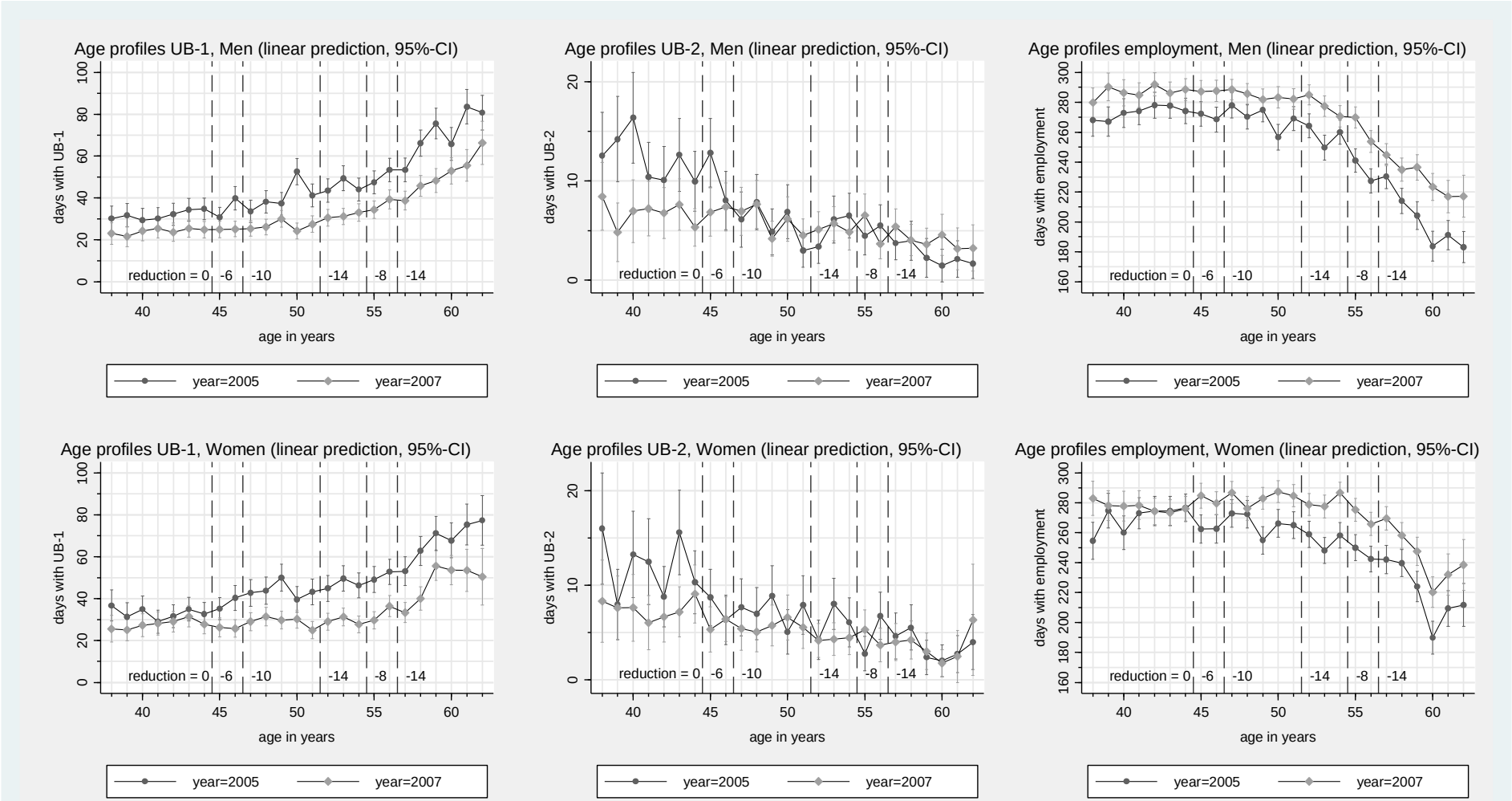

Figure 11: Age profiles men vs. women (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

Table 7: DiD results West vs. East for age treatment dummy (Sample A: 2005/2007, employed before rehabilitation)

	West			East		
	(1) UB-1	(2) UB-2	(3) WORK	(1) UB-1	(2) UB-2	(3) WORK
age≥45	16.60***	-6.74***	-23.53***	23.44***	-5.32***	-33.36***
	[1.06]	[0.63]	[1.71]	[2.44]	[1.60]	[3.95]
year2007	-6.48***	-4.48***	10.29***	-7.72**	-7.58***	11.73**
	[1.11]	[0.73]	[1.95]	[2.52]	[1.73]	[4.52]
age≥45 × year2007 (post-reform)	-10.15***	4.85***	12.68***	-12.07***	3.78*	18.31***
	[1.33]	[0.78]	[2.25]	[3.06]	[1.87]	[5.18]
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.11	0.08	0.18	0.14	0.06	0.22
Mean dep. variable	38.73	5.92	262.93	43.83	7.31	255.43
N	79,098	79,098	79,098	15,892	15,892	15,892

Notes: Sample A (2005/2007, employed before rehabilitation). Outcome variables are days per calendar year. OLS regressions. Robust standard errors in brackets. * p<0.05; ** p<0.01; *** p<0.001.

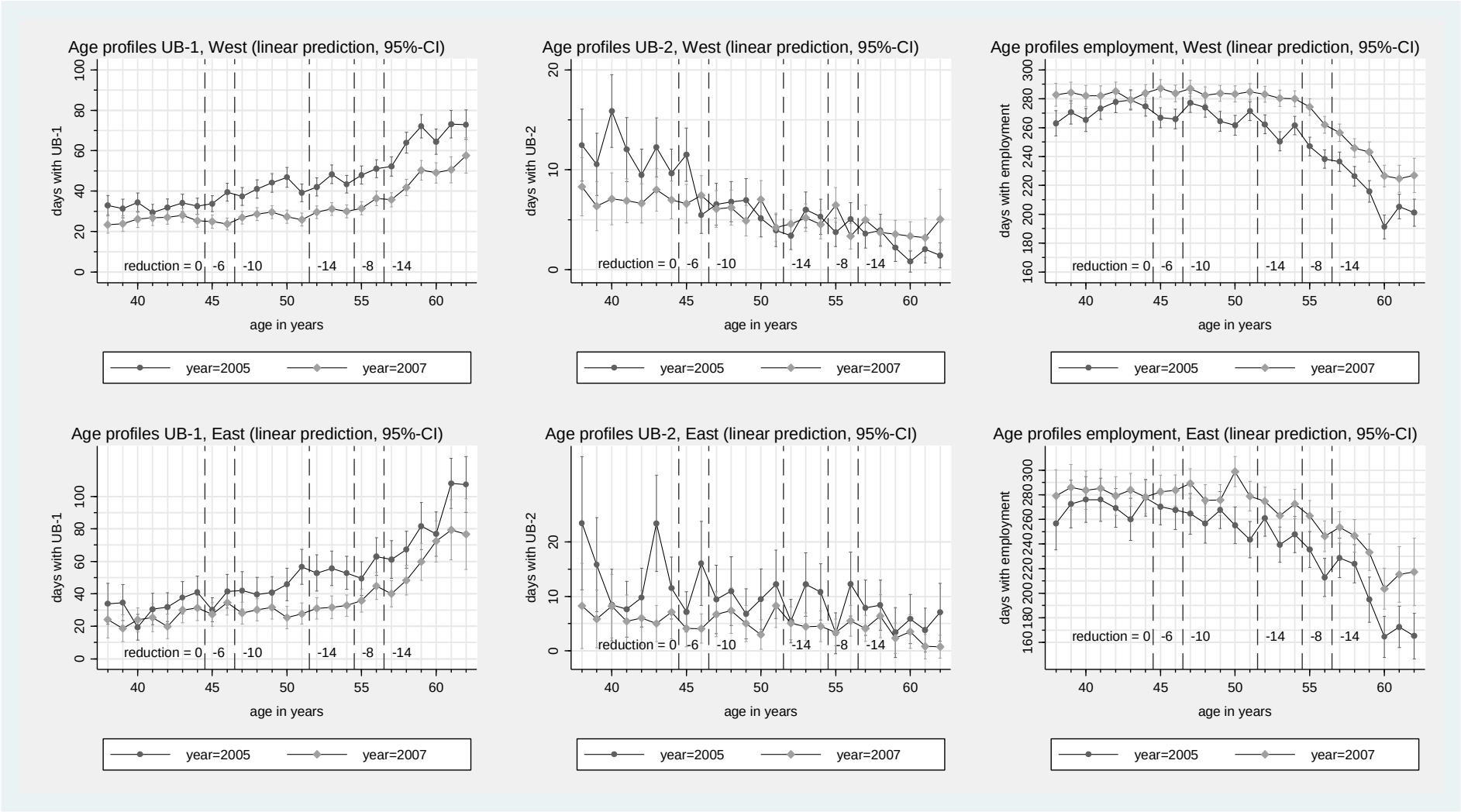

Figure 12: Age profiles West vs. East (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

Table 8: DiD results unemployed and non-employed before rehabilitation for age treatment dummy (Sample C: 2005/2007)

	Unemployed			Non-employed		
	(1) UB-1	(2) UB-2	(3) WORK	(1) UB-1	(2) UB-2	(3) WORK
age≥45	31.34***	-26.95***	-14.55***	20.83***	-23.86***	0.34
	[2.25]	[3.88]	[2.45]	[2.74]	[3.36]	[3.96]
year2007	-17.17***	14.30**	20.39***	-13.94***	-1.79	30.52***
	[2.20]	[4.84]	[3.41]	[2.67]	[3.78]	[4.46]
age≥45 × year2007 (post-reform)	-5.94*	9.62	-2.05	-9.93**	18.98***	-12.67*
	[2.93]	[5.53]	[3.77]	[3.27]	[4.21]	[5.07]
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.12	0.23	0.20	0.07	0.27	0.35
Mean dep. variable	55.93	159.31	42.86	47.47	61.23	146.31
N	15,857	15,857	15,857	16,529	16,529	16,529

Notes: Sample C (2005/2007, unemployed and non-employed before rehabilitation). Outcome variables are days per calendar year. OLS regressions. Robust standard errors in brackets. * p<0.05; ** p<0.01; *** p<0.001.

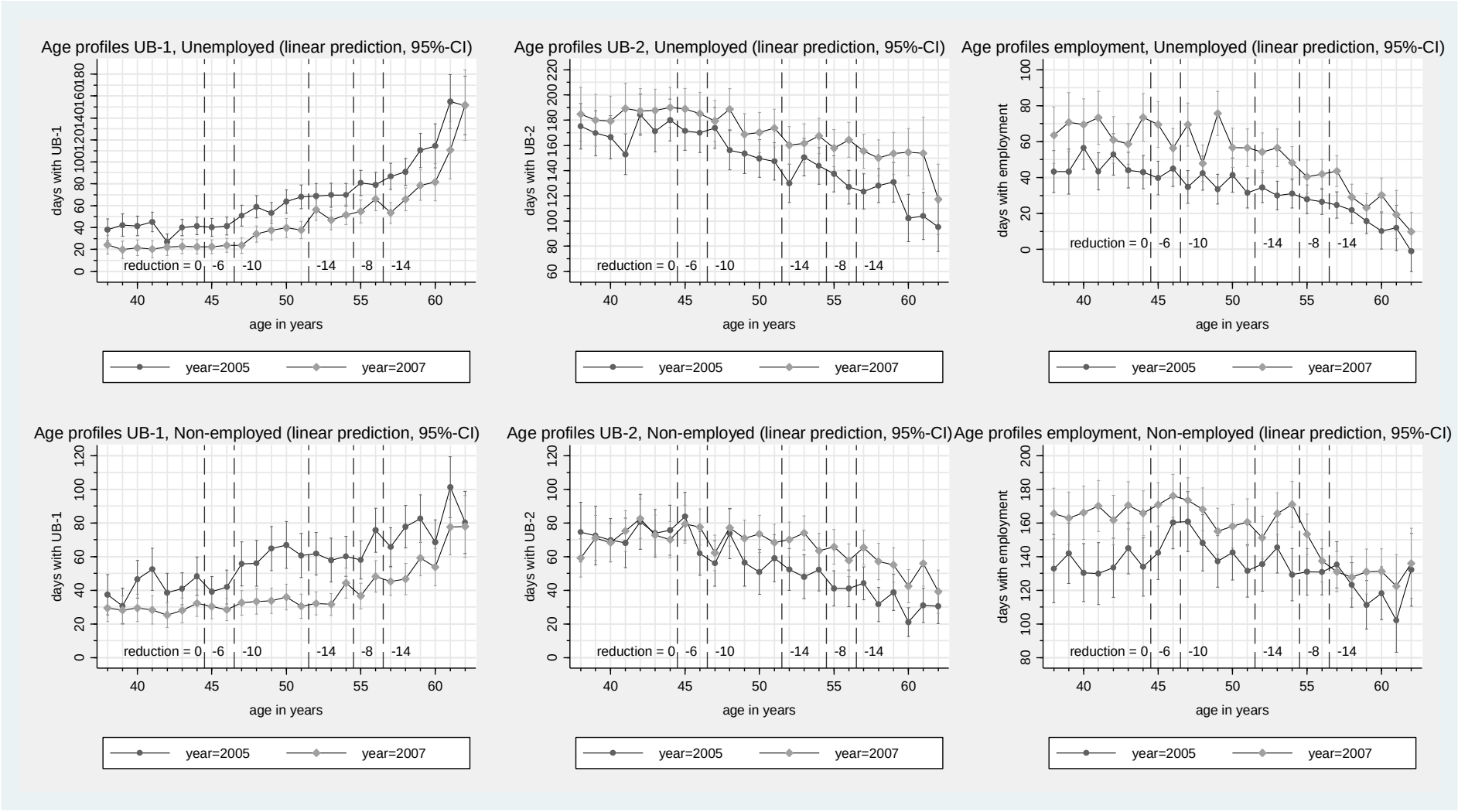

Figure 13: Age profiles unemployed and non-employed before rehabilitation (Sample C: 2005/2007) (SE and CI by delta method)

Table 9: Number of observations in treatment intensity variable UB-1 reduction in months

UB-1 reduction in months	2005 (pre-reform)	2007 (post-reform)	Total
0	45,703	11,128	56,831
6	0	4,238	4,238
8	0	4,972	4,972
10	0	11,174	11,174
14	0	17,775	17,775
Total	45,703	49,287	94,990

Table 10: DiD results for treatment intensity (Sample A: 2005/2007, employed before rehabilitation) and placebo tests (2004/2005)

	Different specifications of age			
	(1) age	(2) age, age²	(3) age, age², age³	(4) age dummies
Outcome: days UB-1				
UB-1 reduction in months	-0.96***	-0.65***	-0.60***	-0.81***
	[0.09]	[0.09]	[0.09]	[0.10]
R^2	0.12	0.12	0.12	0.12
Mean dep. variable	39.58	39.58	39.58	39.58
N	94,990	94,990	94,990	94,990
Coefficient placebo 2004/2005	-0.03	0.22*	0.23*	0.12
Outcome: days UB-2				
UB-1 reduction in months	0.24***	0.27***	0.26***	0.36***
	[0.04]	[0.05]	[0.05]	[0.05]
R^2	0.07	0.07	0.07	0.07
Mean dep. variable	6.15	6.15	6.15	6.15
N	94,990	94,990	94,990	94,990
Coefficient placebo 2004/2005	-0.18***	-0.14**	-0.14**	-0.12*
Outcome: days WORK				
UB-1 reduction in months	1.74***	0.86***	0.81***	0.95***
	[0.14]	[0.15]	[0.15]	[0.16]
R^2	0.20	0.20	0.20	0.20
Mean dep. variable	261.68	261.68	261.68	261.68
N	94,990	94,990	94,990	94,990
Coefficient placebo 2004/2005	1.08***	0.29	0.28	0.15

Notes: Sample A (2005/2007, employed before rehabilitation). Placebo tests for years 2004/2005 (N= 97,513) as if UB-1 reduction in months would have occurred in 2005. All control variables included. Outcome variables are days per calendar year. OLS regressions. Robust standard errors in brackets. * p<0.05; ** p<0.01; *** p<0.001.