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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 on the labor market outcomes of individuals with some health impairment. The reform induced a substantial reduction in the potential duration of regular unemployment benefits for older workers. 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 the number of days in unemployment benefits and increase in the number of days in employment. However, they also suggest a significant increase in the number of days in unemployment assistance, granted upon exhaustion of unemployment benefits. Transitions to unemployment assistance represent 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.

Keywords: policy evaluation, labor market reform, unemployment insurance, difference-in-differences

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 aimed at re-establishing incentives to work and delay withdrawals from the labor market have been implemented. Among other policy changes, a profound rearrangement of the unemployment insurance (UI) systems was initiated almost contemporaneously in many European countries. 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 when unemployment benefits become available, indicating that 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 benefits are exhausted, the intensity of recipients' job searches rises, which is associated with a spike in the unemployment exit rate at this point (Card and Levine 2000, Card et al. 2008). Finally, a 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 investigating 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. 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 efforts 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 that was implemented in February 2006 as part of the Hartz-Reforms. It involved a substantial reduction in the potential duration of regular unemployment benefits (unemployment benefits 1, UB-1) 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 subject

to the policy change. The design of the reform provides a natural experiment setting, on which our difference-in-differences identification strategy relies to investigate the causal relationship between the potential duration of UB-1 and the labor market outcomes of the affected individuals. The contribution of our paper is threefold. To begin with, we use an alternative outcome measure. In particular, as opposed to unemployment duration, which 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 in the middle-run 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), in which 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, who underwent medical rehabilitation treatments. The main objective of rehabilitation measures directed to the working age patients is to retain their working capacity, to facilitate their reintegration into the labor market, and to avoid early retirement (German Statutory Pension Insurance 2014a). 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 evaluating the policy. 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 demand-side factors such as discrimination in recruiting 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. Therefore, we expect individuals with some health impairment to be less responsive to the UI incentives, interpreting our estimates as lower and upper bounds for the treatment effects in

the total population. Official statistics on medical rehabilitation treatments signals that this population group has gained more importance in recent years (German Statutory Pension Insurance 2014b). In fact, in 2012 the German Statutory Pension Insurance approved 1,097,538 applications for medical rehabilitation, which is 0.9% more in comparison to the previous year and 3.2% more with respect to 2010. Therefore, we believe that it is important to analyze the labor market performance of this group in response to policy changes and the findings from this study are intended to provide possible directions for future research in this area.

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 the number of days in UB-1 and increase in the number of days in employment subject to social insurance contributions. However, the findings also suggest a significant increase in the number of days in social assistance (unemployment benefits 2, UB-2) granted to unemployed jobseekers upon exhaustion of UB-1 to provide them a living at the subsistence level. This result is consistent with recent work on the 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 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.

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 and 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 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 use 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 and 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 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 identification of the channels through which unemployment benefits affect search behavior. In a sample of U.S. 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 la-

bor 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 that can alter behavior, although they prompt rather different behavioral responses. In 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 indicates 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 is observed in the exit rate from registered unemployment at the point of benefits exhaustion, suggesting 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 efforts remain constant. As a result, employment hazard increases up to the date of benefits exhaustion and does not change afterwards (Mortensen 1977).

The 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 (Caliendo et al. 2013). This strategic job search behavior discloses 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). Empirical literature finds evidence of strategic job search behavior caused by more generous unemployment benefits durations. For

instance, Lichter (2016) uses an exogenous variation in PBD originated from a policy change in Germany. The reform, implemented in 2008, involved an extension of 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 hypothesis 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 by issues such as 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 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 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 the absence of anticipation. This observation could be explained by the anticipation of dismissals and resignations 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. (2017) focus on the age group with the smallest anticipation effect, for which no systematic decrease in unemployment inflows after the reform has been observed. Furthermore, the authors exclude periods with unemployment inflows during the potential anticipation period to remove anticipation effects. Based on a sample of male unem-

ployed 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 is that it provides more detailed insights into the impact of the unemployment compensation system on the 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 of being recalled 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 low-wage ones. Although the authors analyze the impact of the major German reform on transitions to important exit destinations, they disregard transitions to UB-2 that are perhaps no less important.

In the European context, the Finnish 1997 reform decreased PBD for older workers to enhance employment incentives and cut expenditures on unemployment indemnity. Kyyrä and Ollikainen (2008) apply a difference-in-differences approach to analyze transitions to employment, which represent a targeted exit destination, and 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 in which 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, which is 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 in France 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 underscore the importance of how unemployment spells is 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 rank as complementary 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 average daily wages. In case of the reform in France, 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 focuses on the French 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 in 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, 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 discontinuity 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 may increase reemployment wages due to an increase in reservation wages or to stronger bargaining power. The second may reduce search effort, thus leading to longer nonemployment spells. If wage offers decrease over time due to stigma or human capital depreciation, workers will face a reduction in reemployment wages. Their results point to tiny reservation wage effects, leading 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. Contrary to this conclusion, Nekoei and Weber (2017), using 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 not 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, UB-1 (“Arbeitslosengeld 1”) and UB-2 (“Arbeitslosengeld 2”). UB-1 is funded by employee and employer contributions and is 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 contri-

bution to the insurance scheme for at least 12 months within the last 24 months before a job loss, and its duration depends on the age and employment history of unemployed workers. PBD discontinuously increases with age to account for difficulties that older unemployed individuals might have in re-entering 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 their 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 is unconstrained by previous earnings and is granted without temporal restrictions. UB-2 is means-tested against household income and aims at providing a living at the subsistence level. Non-compliance with the rules can result in 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 called Hartz-Reforms. The reform was announced in 2004 and came into force in February 2006, affecting workers who lost their jobs after 31 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 promoting 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 six 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 receipts of UB-1. Under the old regime, workers were eligible if they had 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 for the level of benefits was not affected by the reform.

3.3 Other Relevant Policy Changes

UB-2 was introduced in January 2005. It largely replaced two previous components of the German unemployment compensation system, 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. This policy change aimed to reduce the dependence on benefits. To this end, the reform introduced strict rules aiming to motivate recipients to intensely cooperate with job centers and actively search for a job, on the one hand, and enhanced support to at least increase their employability by promoting education programs in skills valued on the labor market, on the other one. 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 PBD for older workers, the German old age pension system has also been redesigned. In particular, until 2003, workers unemployed for at least one 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 retire-

ment. 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 senior workers to leave the labor market before the legal retirement age (Kyyrä and Ollikainen 2008). This suggests that retirement can not be modeled separately from other spells out of work, justifying the exclusion of workers approaching retirement age in this study. Finally, during the period under study, the state legal requirements for the approval of applications for medical rehabilitation have not been modified so that the pre- and post-reform participants in rehabilitation treatments do not systematically differ, leaving no room for a composition effect.

4. Data Set and Samples

For our analysis, we use the routine data[1] 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 characterizing feature of medical rehabilitation consists in treating, among others, health deficiencies such as renal failure, disorders involving the metabolic and endocrine systems (e.g., diabetes mellitus), nervous system (e.g., migraine and sleep disorder), circulatory system (e.g., heart failure), respiratory system (e.g., asthma), digestive system (e.g., liver disorder), musculoskeletal system (e.g., back pain), mental and behavioral disorders (e.g., depression and alcohol abuse), and skin diseases (e.g., dermatitis. The most recurrent health disorder is the low back pain, which in 2013 accounted for 31.5% of all medical and other rehabilitative services provided by the German Statutory Pension Fund (German Statutory Pension Insurance 2013). In fact, it

[1] While the administrative data from the Sample of Integrated Labour Market Biographies (SIAB) contain daily spell information on employment periods subject to social security contributions, job search periods, participation in active labor market programs, and claim periods of UB-1 as well as UB-2, the advantage of our data over the SIAB data consists in the availability of information on individuals' health limitations and other important control variables.

is the largest finance provider of medical rehabilitation treatments for the employed individuals in Germany, followed by the statutory health insurance, and aims essentially at preventing costs connected with early retirement following the principle of rehabilitation before pension. 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 that 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 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 (approximately 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 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. Taking into account 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.

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. First, UB-2 was 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. Second, the exclusion of the year 2006 is motivated by the 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 restriction does not allow for controlling for pre-reform trends, it provides important insights into the impact of the policy change and results in an analysis free from any distortions that stem from anticipation effects of the reform. 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 shows 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: As for Sample A, we keep only years 2005 and 2007, but we 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 older 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 ordinary least squares (OLS). A robustness check adopting count data models reveals virtually 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 the treatment group specific effect (age trend), β_2 is the parameter for the time trend common to the control and the 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. 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 can not only be divided in employment and registered unemployment, but also in other sources of non-employment (e.g., family responsibility, early retirement). Apart from non-employment, we also exclude self-employment, minor employment, and civil service. 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. Because our outcome variables do not exhaust all possible labor market states, they do not sum up to 365 days.

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, indicat-

ing 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 the 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 older and amounts to 77% of individuals in our preferred sample A. Because reductions in the PBD implied by the policy change varied with respect to age categories, the treatment group is further defined according to these categories. 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). 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, so we expect a lower treatment effect for these age threshold groups.

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). It is noteworthy to mention that our estimates of the treatment effect are largely unaffected by the inclusion of the control variables, which indicates that our estimates are likely to be unbiased.

Treatment intensity: In the next step, we revise our DiD strategy to examine 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 under the age of 45 in 2007. But there was positive and heterogeneous treatment intensity for individuals aged 45 and older in 2007. Younger workers were affected to a lesser extent than older ones. In fact, the age group 45-46 suffered the smallest benefits cut by only six months, while age groups 52-54 and >56 suffered the largest reduction by 14 months. To address the fact that treatment intensity is correlated with age, we use different specifications of age as controls (age, age^2, age^3, age dummies). Furthermore, we run a placebo test for years 2004 and 2005 and act as if the UB-1 re-

duction 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, and 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 shows the regression results for the age treatment dummy, i.e., individuals younger than 45 years are the control group, and individuals equal to or older than 45 years are the treatment group. The general age trends (age≥45) indicate that individuals equal to 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) show that 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 AGENDA 2010. The treatment effects (age≥45×year2007) of the reduction of the potential duration of UB-1 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. The treatment effects are statistically significant and sizeable. If we put the absolute treatment effects simply in relationship to the sample mean outcomes, days in UB-1 have decreased by about 25%, days in UB-2 have increased by about 75%, and days in WORK have increased by about 5%. It should be kept in mind that the estimates in this study may not provide the true treatment effect for the entire eligible population. We argue that our estimated 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 average treatment effects in the total population. Also note that the treatment effects on our three outcome variables do not sum up to zero, be-

cause our three outcome variables do not exhaust all possible labor market states. In particular, we focus only on employment subject to social insurance contributions, excluding self-employment, minor employment, and civil service. Moreover, we exclude non-employment such as retirement due to health reasons or other labor market exits.

[Insert Table 3 about here]

In the next step, we replace the age treatment dummy with age treatment categories, i.e., we split the treatment group into age categories according to the different reductions of the potential duration of UB-1 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 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 four fewer days in UB-1 for the age group 45 to 46, which experienced a reduction of potential duration of UB-1 by six months. For the age group 47 to 51 (reduction by 10 months), the treatment effect is eight fewer days in UB-1. The age groups 52 to 54 (reduction by 14 months) and 55 to 56 (reduction by eight months) each have a treatment effect of about nine 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 three more days in UB-2 for the youngest treatment age group (45-46) and six 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 eight days, for the age group 47 to 51 by six 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. 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]

Finally, 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 1 to 3 present the predicted outcomes, and Figure 4 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 can not measure a significant treatment effect for the age threshold at 45 years. As mentioned, 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 1-4 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 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). Table 5 shows 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. More specifically, the coefficients of the interaction terms between age≥45 and the years 2005 and 2006[2] 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 5 to 7 also support the parallel trend assumption. Because we do not find evidence for a violation of the parallel trend assumption and can identify a structural break be-

[2] 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).

tween 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 5-7 about here]

6.3 Gender and Regional Differences for 2005/2007

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) as well as between West (including Berlin) and East German Federal States (see Table 7). The results for the separate samples support our previous findings for the complete sample in section 6.1. The small gender and regional differences[3] 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.

[Insert Table 6 about here]

[Insert Table 7 about here]

6.4 Unemployed and Non-employed before Rehabilitation for 2005/2007

In the previous sections, we focused on individuals who were employed before 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 medical rehabilitation.

[3] Estimates from the models with a triple interaction term (age≥45 × year2007 × female and age≥45 × year2007 × East, respectively) suggest that these differences are statistically insignificant.

Table 8 shows the average treatment effects for the unemployed sample of about six fewer days in UB-1, 10 more days in UB-2, and two fewer days in WORK, of which 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 into UB-2 and the reform seems to even have a negative effect on WORK in the unemployed and non-employed samples.

[Insert Table 8 about here]

6.5 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 that 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. Table 9 shows that treatment intensity is zero for all observations in 2005 and for all observations younger than 45 years in 2007, whereas treatment intensity is positive for all observations equal to or older than 45 years in 2007. Because treatment intensity is correlated with age, we estimate different specifications of age as controls (age, age², age³, 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 one 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. 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. Because 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 to re-establish incentives to work and 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. It is noteworthy to mention that, contrary to the middle-run framework adopted in our work, a long-run perspective of this reform evaluation might lead to more reassuring conclusions. In fact, a potential increase in the number of days in WORK registered in later years following the reform would encourage us

to conclude that the worse outcome estimated in the middle-run, as indicated by an increase in days in UB-2, might be an expected intermediate outcome, which serves as an additional incentive for reintegration into the labor market. In other words, the negative effect on days in UB-2 and days in WORK, observed in the middle-run for those less attached to the labor market, might be smaller in the long-run. Unfortunately, our data do not allow us to investigate the dynamics of UB-1, UB-2, and WORK in the long-run to test this hypothesis.

Due to data construction, our study presents some limitations with respect to measurement accuracy of eligibility conditions to qualify for the potential benefits duration. In practice, the duration of benefits 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 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 opens an interesting perspective in the reform evaluation, and positive effects of the reform for this population group that has gained more importance in recent years encourage us to predict even larger effects for healthy workers with no need for medical rehabilitation. Our expectation relies on several 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 policy-makers to learn from their neighbors' experience. In fact, only three years apart, the French government also enacted a major reform of the UI system that 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 the duration of unemployment benefits, a broader consideration of labor market performance upon exhaustion of regular unemployment benefits seems appropriate. Thus, the unemployment benefits reform in Slovenia achieved the intended decline in moral hazard induced by the UI system 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 unfounded if increased employment comes with a loss of job match quality in response to the reform and longer unemployment spells would have facilitated productive job searches. Furthermore, non-negligible exits from unemployment to non-employment as well as shifts from unemployment benefits to unemployment assistance or 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.

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.

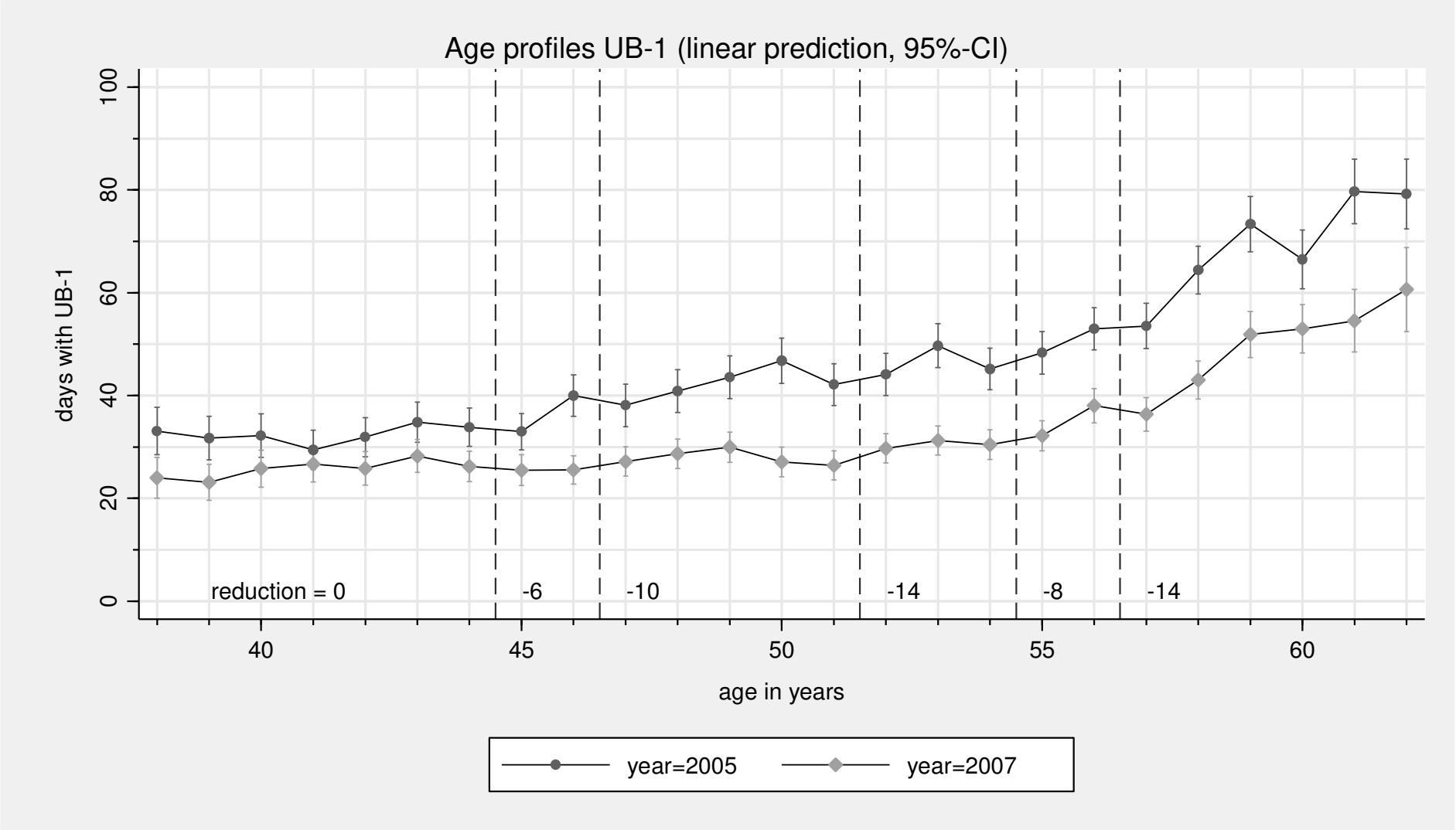

Figure 1: Age profiles UB-1 (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

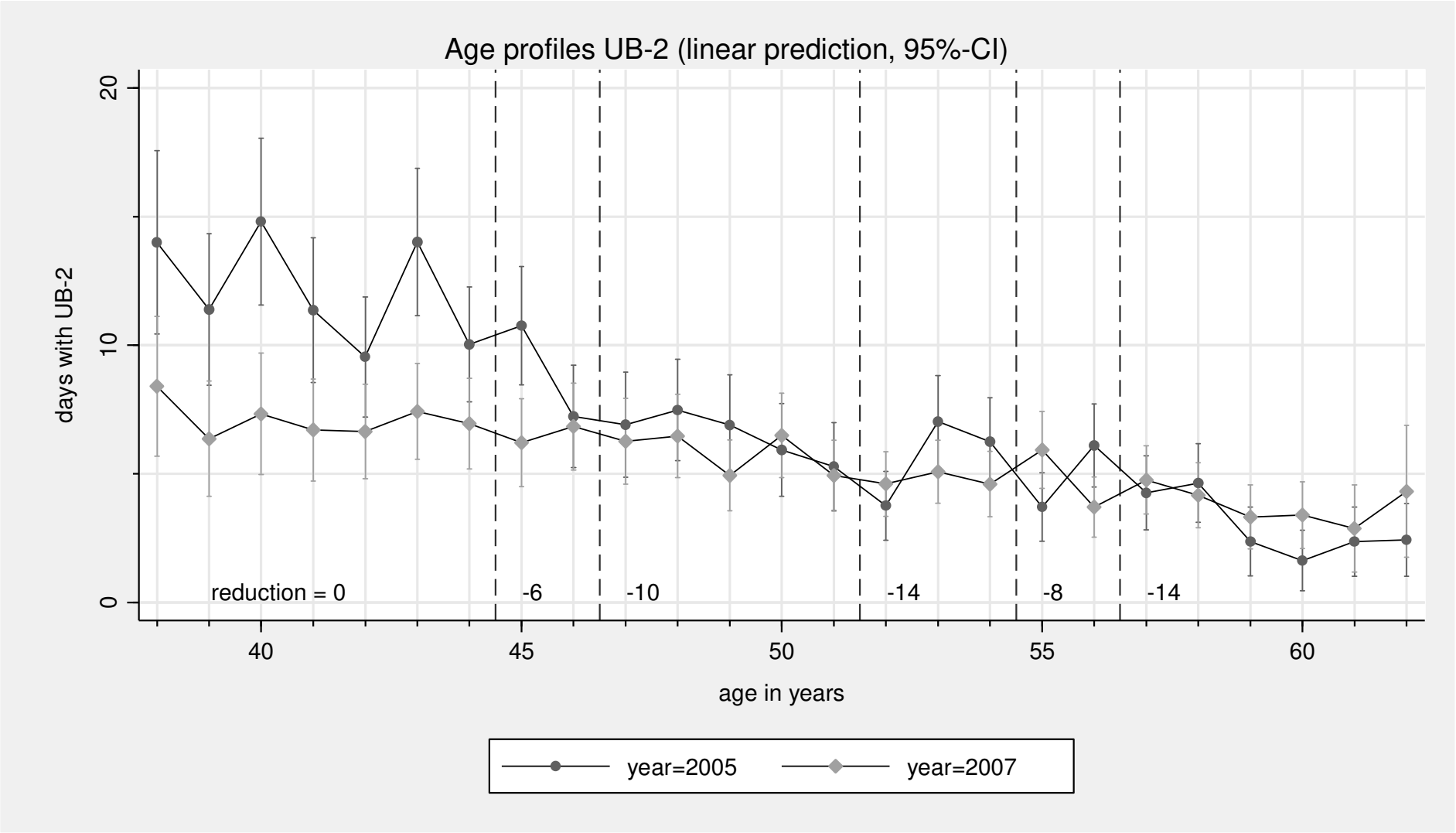

Figure 2: Age profiles UB-2 (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

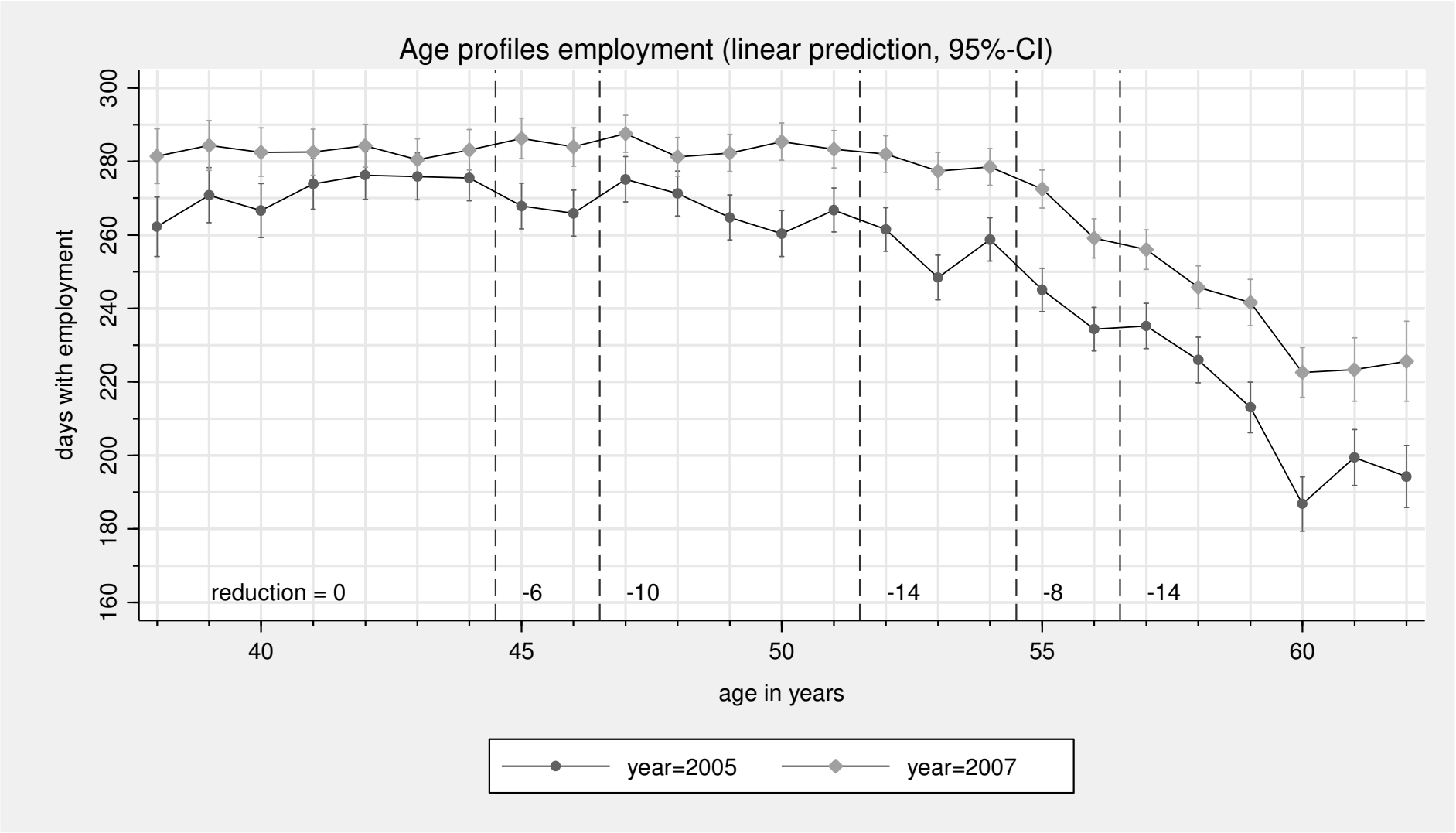

Figure 3: Age profiles employment (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

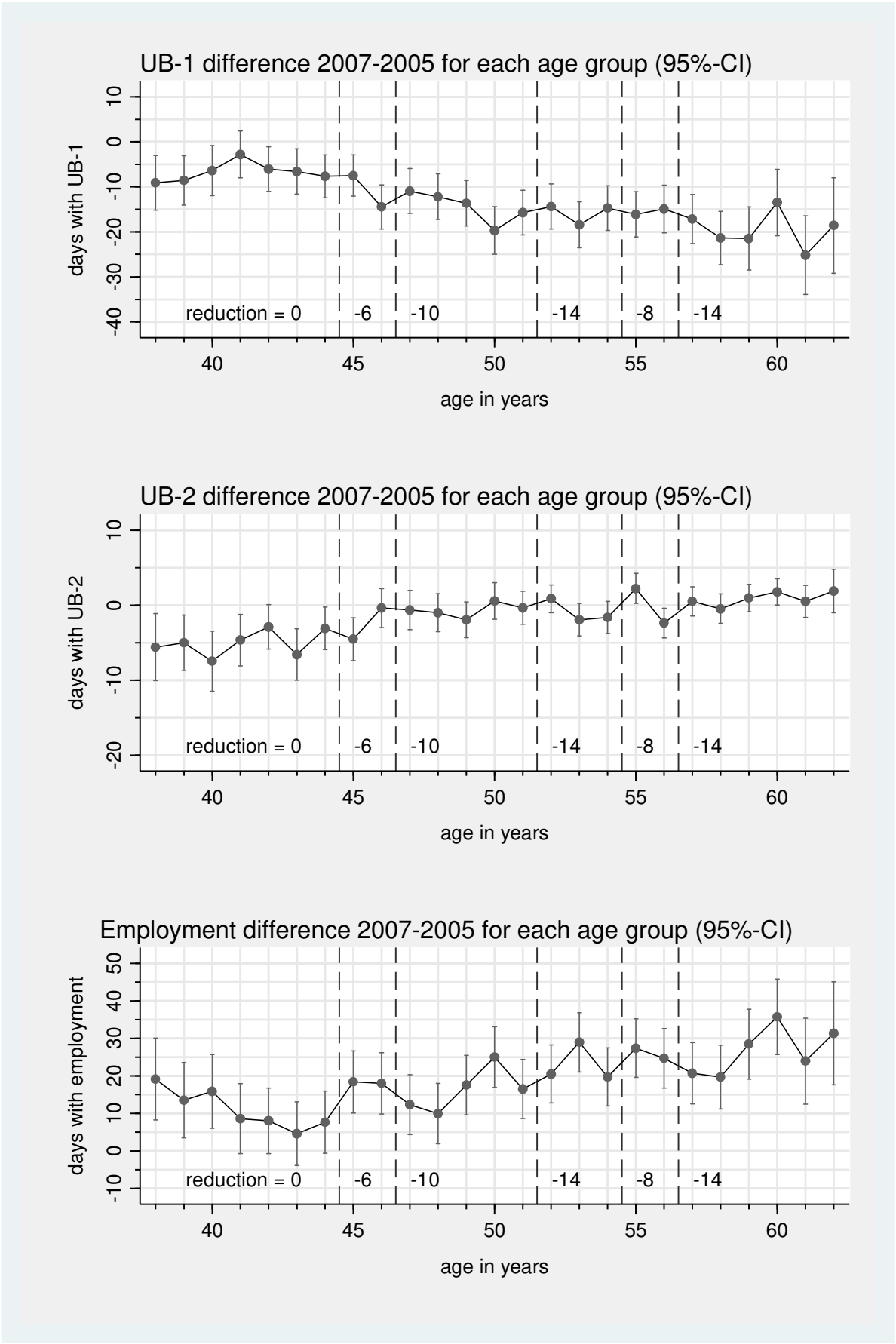

Figure 4: Differences 2007-2005 for each age group (Sample A: 2005/2007, employed before rehabilitation) (SE and CI by delta method)

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.

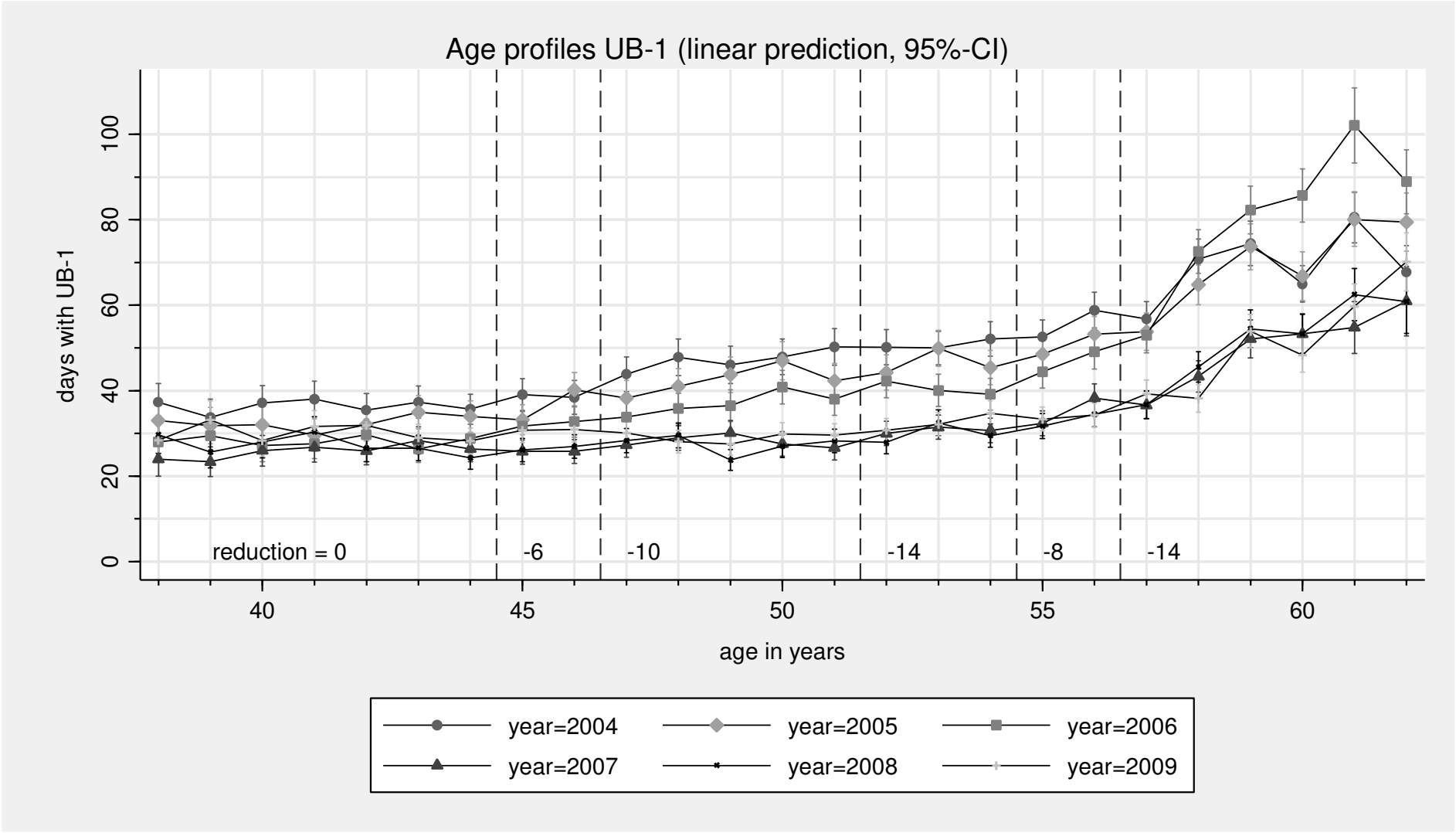

Figure 5: Age profiles UB-1 (Sample B: 2004-2009, employed before rehabilitation) (SE and CI by delta method)

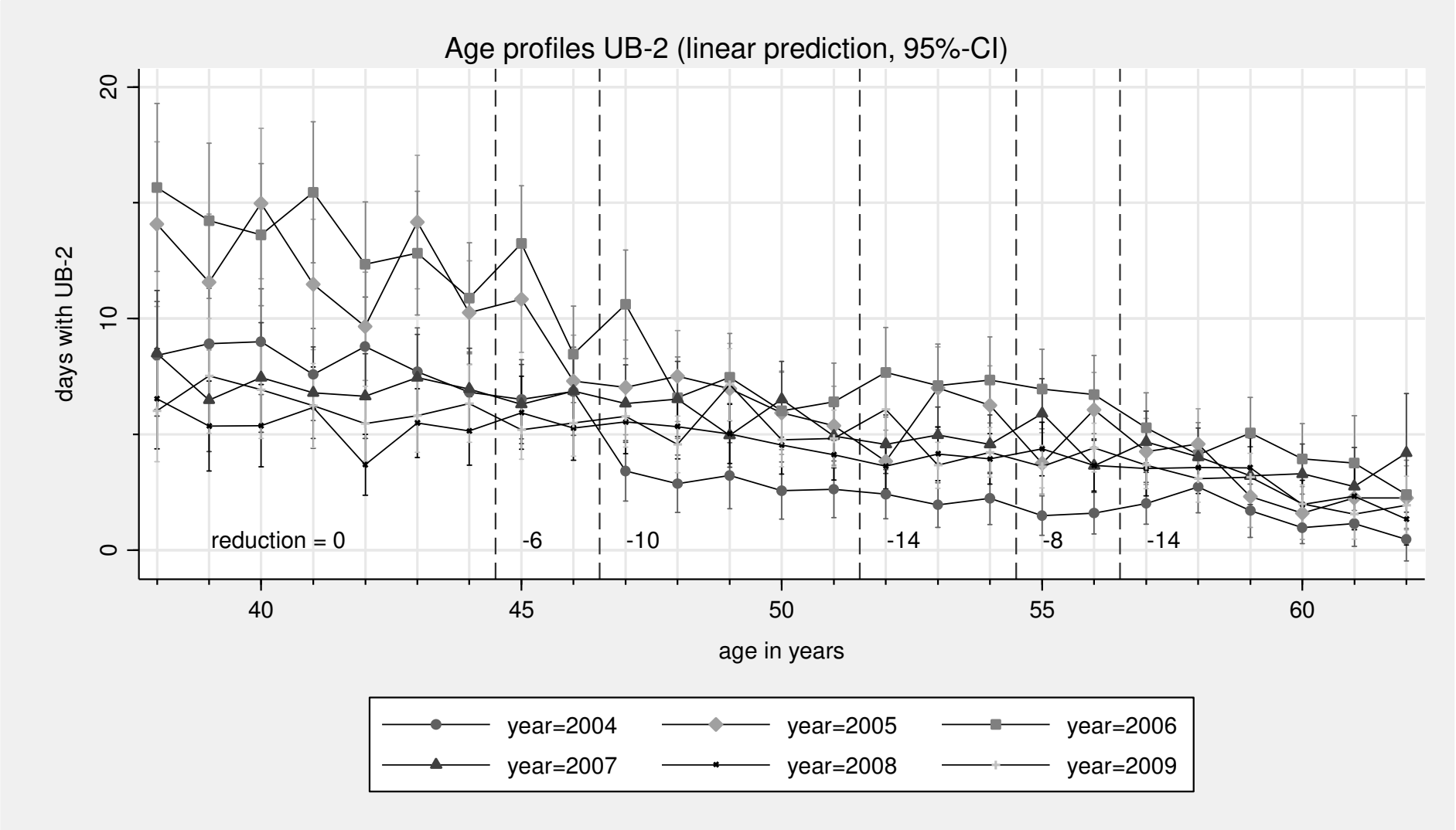

Figure 6: Age profiles UB-2 (Sample B: 2004-2009, employed before rehabilitation) (SE and CI by delta method)

Figure 7: 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.

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.

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.

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.

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