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SHATTERED DREAMS: THE EFFECTS OF CHANGING THE PENSION SYSTEM LATE IN THE GAME*

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This article assesses the impact of a dramatic reform of the Dutch pension system on the mental health of workers nearing retirement age. The reform means that public sector workers born on 1 January 1950 or later face a substantial reduction in their pension rights while, for workers born before 1950 nothing changes. We employ a unique-matched survey and administrative dataset comprising male public sector workers born in 1949 and 1950 and find a strong deterioration in mental health for workers affected by the reform. These effects are stronger for married workers whose partner has no pension income.

This article assesses the mental health effects of a change in the Dutch pension system. Prior to 2006, public sector workers in the Netherlands could retire at age 62 years and three months with a replacement rate of 70% of their average yearly earnings since 2004.[1] As of 2006, those born before 1 January 1950 could continue to retire under the old rules, but for those born on or after 1 January 1950, the replacement rate is lowered to 64%. These younger workers need to work an additional one year and one month to obtain the 70% replacement rate enjoyed by counterparts who may be just a few days, weeks or months older. Two years after the policy change, we compared the mental health of workers born in 1949 (turning 59 years old in 2008) and 1950 (turning 58 years old in 2008). We find strong effects from the exogenous change in the retirement system: depression rates among the 1950 cohort were about 40% higher than among the 1949 cohort. To our knowledge, this is the first study to document large and persistent mental health effects from a change in a retirement system before affected individuals actually retire.

Our findings are relevant for a number of reasons. First, depression is a relatively common disorder, with prevalence rates of about 10% in the USA, the UK and the Netherlands. Depression is among the leading causes of disability worldwide (WHO, 2006), and it is associated with heart disease, diabetes, some forms of cancer and other diseases. Indeed, the healthcare expenditures of depressed individuals are about four times higher than those of non-depressed individuals. In addition to these direct effects on healthcare costs, the indirect costs of depression are substantial. Depression leads to lower productivity, workplace errors, faulty products, accidents and increased absenteeism and disability insurance expenditures. In fact, in the last decade, an increasing

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[1] Until 1 January 2002, pension benefits were calculated using wage earnings in the year prior to retirement. Since 2002, pension benefits have been calculated using average annual earnings.

share of disability insurance expenditure in the western world has been due to mental illnesses (OECD, 2008).

Second, our findings are relevant for public policy in the context of ageing. Most developed countries are currently encouraging prolonged working lives for older workers to mitigate the adverse effects of an aging population. Increasing labour force participation rates among older workers improves the fiscal stability of pension systems. However, a natural question that has been largely overlooked by policy makers concerns the effect of later retirement on individual well-being and, in particular, on health. Adverse (or positive) effects from later retirement on post-retirement health not only influence individual well-being but also directly affect healthcare costs after retirement. Our finding of persistent negative health effects prior to actual retirement from changes in the retirement system suggests that post-retirement health worsens when individuals are induced to extend their working lives.

Third, following up on the second point, there is a recent and growing body of literature on the health effects of retirement. Cross-sectional analyses usually find that those who retire early have worse post-retirement health. Tsai *et al.* (2005) compare mortality rates at later ages and find that post-65 mortality rates are higher for those who retire early. Dave *et al.* (2008) find that earlier retirement is associated with poor physical and mental health after retirement. It has been hypothesised that retirement in itself is a stressful event, or that retired people lose the physical and mental activity that is associated with work and/or that social networks associated with work decline. The policy implication of such findings indicates that increasing retirement age would lead to better individual health and well-being and may reduce the burden on (public) healthcare systems, as well as on pension systems. Alternatively, it may be true that aspects of work (stress or job characteristics) worsen health, leading to positive effects from retirement and negative effects from continued work. These alternative mechanisms illustrate that it is difficult to infer causation from a direct comparison of the health status of early and later retirees. Indeed, health may affect work and *vice versa*. Moreover, unobserved factors may confound the relation between health and work.

Recent articles in this area have tried to circumvent this endogeneity problem by using an Instrumental Variable approach. Charles (2004) and Neuman (2007) use age-specific retirement incentives provided by the US Social Security system to capture changes in labour force participation that are unrelated to health. Similarly, Bound and Waidmann (2007) employ age-specific retirement incentives of the UK Social Security system to gauge the effect of retirement on health. Coe and Lindeboom (2008) use the availability of retirement windows as an instrument. All these studies confirm that the cross-sectional association between health and retirement is positive; that is, those who retire later tend to be in better health. However, when the endogeneity of retirement is accounted for, the results change dramatically. Coe and Lindeboom (2008) find no negative effect from early retirement on male health; if anything, these authors report a temporary increase in self-reported health of highly educated workers. Bound and Waidmann (2008) find no evidence of negative health effects from retirement and some evidence that there may be a positive effect for males. Neuman (2007) finds, for subjective health measures, that retirement maintains health but finds no effect on objective health variables. Charles (2004) focuses on mental health and finds that the direct effect of retirement on mental well-being is positive.

Our findings of strong mental health effects for individuals who are still working are consistent with his findings.

Finally, the effects of the pension reform on mental health prior to actual retirement have implications for the literature on the determinants of retirement decision making. The larger part of this vast literature focuses on the role of financial incentives on retirement behaviour, with health included as an exogenous regressor; see the survey by Lumsdaine and Mitchell (1999). For the identification of the causal effect of financial incentives on retirement, it is generally believed that it is preferable to rely on exogenous changes in the retirement system. The negative health effects prior to retirement we find imply that changes in the retirement system will not only have an impact on budget constraints but will also influence mental health prior to retirement. This will confound both the health effects and the effect of financial incentives in retirement models.[2]

Our contribution is most closely related to the recent article by Falba *et al.* (2009), which examines the impact of a deviation of actual retirement dates from previous expectations on workers' mental health. The authors find significant effects on depression at age 62 from those working more than expected and from those working less than expected. However, our study differs from their article in three important ways. First, we are able to exploit a natural experiment that generates a drastic change in the retirement system that is independent of workers' health and that affects only a subgroup of workers. We link survey and administrative information from the pension fund. The survey was conducted in 2008 and consists of 5,274 observations of full-time working males born in 1949 and 1950.[3] The limited age difference between the treatment and control groups in our sample and the simple and transparent age criterion determining entitlement to the old or new pension rights guarantee the internal validity of the experiment. Furthermore, there have been no other institutional changes that differentially affected the 1950 (1949) cohort as opposed to the 1949 (1950) cohort. Our findings are therefore less likely to be confounded. Second, our study shows that there are negative effects on mental health quickly after the announcement of the reform and long prior to retirement, that these are substantial and that they persist over time. Third, our data allow, to some extent, for further analysis of the underlying mechanisms.

This article proceeds as follows: Section 1 presents a brief description of the institutional setting in the Netherlands and the policy change implemented in January 2006. Section 2 describes the data and examines the validity of our natural experiment, that is, whether individuals are aware of the reform and whether the treated and control groups are comparable with respect to other characteristics. Section 3 presents the results of the empirical analyses. Section 4 discusses and explores mechanisms that may explain the higher depression rate among workers affected by the reform. We close with a discussion of our conclusions.

[2] Part of the effect of financial incentives will be absorbed by the health effect if health changes prior to actual retirement. This suggests, moreover, that work has feedback effects on health, which in turn implies that health should be treated as an endogenous regressor in retirement models. See Bound and Waidmann (2008) for similar reasoning in the context of the effects of retirement on post-retirement health.

[3] We also use data collected in 2009 in robustness analyses. However, in 2009, an increasing share of respondents from the control group left the sample because of early retirement.

1. The Natural Experiment

1.1. *The Dutch Pension System*

The Dutch pension system consists of three pillars: at age 65, all residents are entitled to a state old age pension financed by contributions that are levied along with income tax; most employees are entitled to an (early) supplementary sectoral or firm pension of the defined-benefit type; individuals can voluntarily build up savings typically taken as annuities through an insurance company. However, due to the supplementary pensions in the second pillar, the third pillar is less well developed in the Netherlands. For nearly all employees, early retirement before the age of 65 is possible only through the sectoral pension systems in the second pillar. In general, for individual employees, participation in sectoral pension schemes is mandatory. These pension schemes are negotiated between unions and employer organisations at the sector or firm level and are officially set forth in collective agreements. The administration of these schemes is delegated to pension funds to which both employers and employees contribute. 'Algemeen Burgelijk Pensioenfonds' (ABP) is the pension fund for public sector workers in the Netherlands. Until 2006, sectoral pension schemes were facilitated by the government through preferential tax treatment that granted large tax advantages due to the progressive tax system (Euwals *et al.*, 2006).[4]

1.2. *Changes in the Pension System for Workers in the Public Sector*

The 2006 reform of the Dutch pension system provides the basis for our natural experiment. In line with its policy of stimulating the labour force participation of older workers, the government abolished the favourable tax treatment of early retirement schemes in the second pillar for all workers born after 1949.[5] As in other sectors, anticipation of the change in tax rules formed an input to collective bargaining on the introduction of a new pension scheme for the public sector in the summer of 2005 ('ABP Flexible Pension Scheme'). In the light of demographic changes, it had been acknowledged by then that reform of the pension system would be necessary. In that sense, a change in pension rights was not entirely unexpected. However, the timing of the reform as well as the particular implementation of a discontinuous assignment rule and the strong differential treatment of workers born around 1 January 1950 came as a surprise to public sector employees when it was announced on 5 July 2005.

The new pension scheme for public sector workers was launched on 1 January 2006. Workers born before 1950 remain entitled to the old, generous pre-pension rights if they have worked continuously in the public sector since 1 April 1997. This means that such workers can retire between age 55 and 65. Retirement at age 62 years and three months yields a pension benefit at a replacement rate of 70% of average yearly earnings since 2004. However, employees born after 1949 and workers born before 1950 who did not work continuously in the public sector in the 10 years prior to the pension reform

[4] Employees and employers were allowed to deduct their contribution to the sectoral early retirement schemes from their current pre-tax income.

[5] The abolition of favourable tax treatment was not limited to the public sector but also applied to workers in the private sector.

are subject to the new and less generous system. The new flexible pension system is characterised by: a drop in pension benefits; an increase in pension contribution payments to partly account for the drop in pension wealth resulting from a drop in pension benefits; stronger incentives to continue working, generated by penalties on pension income when retiring before commencement of the state pension at age 65 and by supplements for later retirement. Moreover, the eligibility age for pension benefits was increased to 60 years and workers may decide to continue working until their 70th birthday. For younger workers, the increase in pension contributions partly compensates for the decrease in pension benefits over time. However, public sector workers born just after 1949 do not have enough time to compensate for this drop in pension benefits. Therefore, as a consequence of both the abolition of the tax rules and the steeper early retirement scheme, workers born after 31 December 1949, are confronted with a substantial decrease in pension benefits if they wish to retire at age 62 and three months. More specifically, the replacement rate drops to 64% and they must work an additional 13 months to qualify for a pension at a replacement rate of 70%.[6]

In 2006, the Dutch government also introduced the 'Life Course Savings' programme (Levensloopregeling). This programme allows tax-free saving of up to 12% of annual earnings in a fund that can be used to finance periods of non-employment, such as a sabbatical or early retirement. Workers are allowed to save up to a cumulative amount of 210% of their annual earnings in this 'Life Course Savings' fund, which can be used to finance about two years of early retirement. Note, however, that at a savings rate of 12%, a worker needs to save for 17.5 years to reach the cumulative maximum of 210%. Special arrangements were made for older workers who were the most affected by the pension reform. Those born in 1950–1954 are allowed to save more than 12% of their annual earnings, so long as the cumulative maximum does not exceed 210% of annual earnings. It must be noted that workers of the 1950 cohort have to save approximately 14% of their annual earnings for seven years to finance an early retirement at age 62. It is likely that only a very small fraction of such workers are willing or capable of saving such a high proportion of their earnings each year before retirement.

The strong differential treatment of workers born around 1 January 1950 came as a surprise to public sector employees. Besides the pension reform, there are no institutional rules that differentially affect the 1949 and 1950 cohort. In essence we, therefore, have a sharp regression discontinuity design. However, for our empirical analyses it is important that workers born in 1950 are aware of the consequences of the new pension system for their individual situations. To publicise the introduction of the new pension system to participants, ABP launched a campaign in the second half of 2005 to explain the implications of the new system. A special newspaper was devoted to the new pension system; in it, unions, employer organisations and ABP jointly explained the new flexible pension scheme. All 1.2 million ABP participants received a letter on the core characteristics of the new scheme and a complete electronic service pack for public service employers was developed. Therefore, we can assume that on 1 January

[6] Nevertheless, a small minority of older employees born after 1950 can still retire early without experiencing a substantial drop in income: employees with burdensome jobs (firemen and ambulance and police personnel), who are eligible for special arrangements that allow early retirement against a replacement rate of at least 70% between ages 55 and 61. However, these workers are not included in our data.

2006, most public sector employees born after 1949 and their employers were familiar with the exogenous shock in their pension rights. Of course, this must be verified empirically. Since our data contain information on expectations about the replacement rate at age 62, we can check whether those born after 1949 indeed predicted their replacement rate to be lower than 70%. We return to this issue at the end of the next Section.

2. Data

2.1. *Matched Survey and Administrative Data*

We use matched survey data and administrative data for male full-time employees in the public sector who were born in 1949 or 1950.[7] The administrative data come from ABP, the pension fund for public sector employees in the Netherlands. The data contain detailed information on annual wage income, the number of years of contribution and establishment size.

The panel survey data are available for three years. The data in the initial wave were gathered in two stages one year after the introduction of the new pension system. In the first stage, all 27,719 male public sector workers in the Netherlands who were born in 1949 or 1950 were sent a request to participate in the survey and to provide their e-mail address.[8] In the second stage, those who gave permission (11,458 workers sent their e-mail address) received an e-mail with a link to the survey. Potential respondents were not informed about the experimental character of the study and no references were made to the shock in the pension system. In total, 8,516 individuals answered the questionnaire, of which 7,162 are assigned to the treatment or control group. Analyses based on the administrative data show that the 7,162 respondents form a representative sample of the 27,719 male public sector workers in the Netherlands born in 1949 or 1950. Importantly, the response rates to the survey for the two birth cohorts were virtually identical (30.5% and 31.0% for the treated and controls, respectively), and a simple linear probability model confirmed that selection into the survey was not related to the treatment (year of birth). More specifically, the linear probability model for participation in the survey resulted in a small and insignificant coefficient for being born in 1950 (−0.005 with a p-value of 0.332).

This study primarily relies on data from the second wave of the survey, which was held in March 2008 and includes 6,070 employees of the public sector. In this wave, detailed questions were asked on mental and physical health, job characteristics, retirement expectations and indicators for personal wealth.[9] We use the second wave

[7] We focus on male employees because for this cohort the male worker (aged 57 or 58) is generally the main family wage earner. Moreover, in the Netherlands, only a small fraction of women of this birth cohort are still working.

[8] This most likely does not affect the representativeness of the survey. At least 91% of the public workers aged 55 years or older have an Internet connection at home (TNS Nipo, 2006). Moreover, virtually all public sector workers have Internet access at work.

[9] For the second wave, all individuals who started the questionnaire in 2007 received an e-mail with a link to the survey in March 2008. Again, the invitation to the survey did not reveal the experimental character of the survey. The questionnaire did not contain questions about the shock in the pension system. To further diminish the possibility of priming effects, the block of questions concerning mental and physical health was carefully placed after a block of questions unrelated to retirement.

because the first wave does not have information on mental and physical health. The third wave did include health variables but a substantial share of respondents from the control group started to leave the sample in 2009 because of early retirement.[10]

The analysis is restricted to full-time employees who have worked continuously in the public sector since 1997 until 2006.[11] For these workers, the pension reform is clear and simple, since age is the only criterion that determines whether a worker is eligible for the restricted or the more generous retirement scheme, guaranteeing the internal validity of the experiment. The final sample consists of 5,274 men, of which 2,688 were born in 1950 (the treatment group) and 2,586 were born in 1949 (the control group). Table A1 shows the effect of different selections on the sample sizes and the cohort composition for all waves. Table A2 documents the results of the linear probability model for selection into the sample that indicate that the selection process is not different for the treatment and control groups.

Concerning the experiment's external validity, the reform also affected private sector workers. The group of male public sector workers, however, differs from the group of private sector workers, for instance because they are, in general, higher educated. Since we do not have data on private sector workers, we cannot examine the extent to which our results are generalisable to the private sector but we will see in later analyses (Section 3) that observed characteristics do not affect the treatment effect. Furthermore, it should be recognised that the effects of the treatment on workers born after 1950 are not analysed and that the results may not be perfectly generalisable to younger cohorts. For younger workers, the increase in pension contributions will partly compensate for the decrease in pension benefits over time. Moreover, they may be less interested in their pension wealth and perceive themselves as less affected by the pension reform than those expecting to retire within a few years. Nevertheless, despite the potential limitations in the generalisations of the results to younger cohorts, the results found in this study remain highly relevant. The 1949 and 1950 birth cohorts belong to the baby boom generation, which is precisely the generation that will impose the highest costs on the pension system and on the working population that has to pay for these costs.

Our main interest lies in investigating how the change in the pension system affects the mental health of public sector workers. For measuring mental health, we use the CES-D8 indicator of depression, derived from CES-D (Radloff, 1977). The CES-D is a well-validated instrument for identifying populations at risk of developing clinical depression or anxiety disorders and has been used in numerous studies to assess depression risks in several populations (e.g. adolescents, the elderly and ethnic and clinical populations). Several validation studies have shown that the CES-D is strongly correlated to other self-report measures for depression, clinical ratings of depression and related variables that support its construct validity (Radloff, 1977; Prince *et al.*,

[10] In total, 555 individuals in the control group retired in 2009. For the third wave, respondents who initially did not participate in the first wave were re-approached. This led to 2,388 respondents entering the sample in the third wave who did not participate in the first or second wave. A total of 1,547 respondents of the second wave did not participate in the third wave.

[11] This resulted in the exclusion of 122 employees born in 1949 or 1959 who were not eligible for the old pension rights. This group is not large enough to serve as an additional control group.

1999; Perreira *et al.*, 2005). The CES-D8 indicator is a shortened version of the CES-D scale which originally contains 20 items. The CES-D8 consists of eight items, six of which are negatively phrased statements that reflect the presence of depressive symptoms (depression, that everything was an effort, restless sleep, inability to get going, felt lonely and felt sad). Two positively phrased statements reflect the absence of depressive symptoms (enjoying life and happiness). Several validation studies have shown that the performance of the CES-D8 scale is similar to that of the full CES-D scale in terms of measuring psychometric properties and that the reduction in the number of questions does not affect the structure and precision of the scale (Kohout *et al.*, 1993; Soldo *et al.*, 1997; Steffick, 2000; Han, 2002; Adams *et al.*, 2003). The CESD-8 scale is used in many large surveys such as the US Health and Retirement Survey (HRS) and is specifically used to assess the mental health of older adults (Han, 2002; Reyes-Gibby *et al.*, 2002).

To create the variable used in our analyses, we first dichotomise (yes/no) responses, similar to the original CES-D8 scale in the HRS and reverse the coding of the positively phrased items to achieve a count variable from 0 to 8, where higher values suggest worsening depressive symptoms. Subsequently, we construct a dummy variable that indicates whether workers are considered to be depressed. In accordance with the psychological literature, we used the suggested cut-off score of 4 and above which is consistent with probable clinical depression. Several validation studies have shown that the cut-off score of 4 for the CES-D8 scale is optimal for determining depression and is equivalent to the standard cut-off point of 16 for the full CES-D scale (Blazer *et al.*, 1991; Andresen *et al.*, 1994; Beekman *et al.*, 1995; Steffick, 2000; Reyes-Gibby *et al.*, 2002). Using lower cut-off scores leads to a substantial overestimation of the number of individuals with depressive symptoms, while the gains of applying a higher cut-off are limited (Beekman *et al.*, 1995).

In addition to mental health, we collected information about physical health, using both objective and subjective measures. First, we asked public sector workers the following question: 'In general, how would you describe your current health?' Response categories ranged from 1 (very bad) to 5 (very good). Second, we asked them how often they visited their doctor in 2007 and whether they had been sick for more than 14 days. Lastly, we asked workers whether their health limits them in the kind and amount of work they are able to perform.

2.2. *Descriptives*

Figure 1 presents a scatter plot of the mean depression rates (for two birth months). It shows that there is a clear break in the depression rates around the 1 January 1950 threshold. This simple plot therefore gives a first indication of a negative effect from the shock in the pension system on the mental health of workers nearing retirement.

Our empirical analyses exploit the sharp discontinuity in pension treatment induced by the natural experiment. It is therefore important that the individuals in the treatment and control groups be sufficiently similar (apart from differential treatment in the pension system). Table 1 shows descriptive statistics for the treatment and control groups. The first two columns show the respective means; the last column gives the p-values of the treatment dummy from a regression of the variable in question on treatment and age.

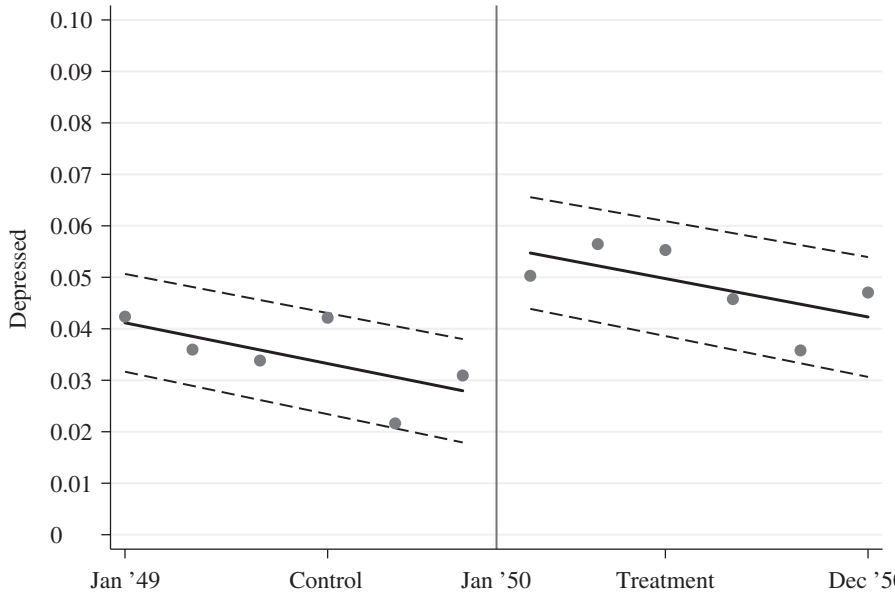

Fig. 1. *Mean Depression Rates Around 1 January* 1950
Notes. This Figure presents the mean depression rate (for two successive birth months). The vertical line marks the threshold dividing the control and treatment groups.

Table 1 shows that job and personal characteristics are similar across both cohorts. Concerning job characteristics, we observe that most public sector workers have mentally demanding work (67%) and that they spend approximately 29% of their working time on non-routine tasks.[12] Approximately 68% of public sector workers have a high education level and more than 91% are married.[13] Concerning the wealth variables, private savings (more than 15,000 euros), net housing wealth and annuity insurance are recurrent wealth components. Among the set of wealth variables, one variable is significantly different at conventional levels between the control and the treatment groups: the response to a question on whether individuals participated in the 'Life Course Savings' programme. Of the 1949 cohort, only 6% participate in this 'Life Course Savings' programme. Of the 1950 cohort, this fraction is more than double (about 16%). This is likely to be a response to the policy reform, since the programme allows for tax-free savings that can be used to finance early retirement.

Respondents were asked three questions concerning their retirement expectations: 'At what age did you expect to retire five years ago?'; 'At what age do you expect to retire

[12] The questions on physically/mentally demanding work are based on two survey questions that asked how well they identified themselves with the following statements: I have physically (mentally) demanding work. Answer categories ranged from 1 (very bad) to 5 (very good). For Table 1, the answer categories are dichotomised (1 corresponds to a score of 4 or 5 and 0 corresponds to a score of 3 or lower).

[13] The public sector has an overrepresentation of highly educated workers. The fraction in our sample is consistent with the OSA labour supply panel, which is a representative panel survey of the working population in the Netherlands.

Table 1

Characteristics for Respondents Affected and Not Affected by the Policy Change

	Affected by policy change mean	Not affected by policy change mean	p-value (controlled for age)
Personal characteristics			
Lower secondary education	0.128	0.113	0.330
Higher secondary education	0.043	0.043	0.809
Vocational education	0.146	0.158	0.060
Higher education	0.684	0.686	0.381
Married	0.912	0.922	0.522
Sectors			
Government	0.444	0.421	0.978
Education sector	0.379	0.406	0.793
Energy, public transportation	0.145	0.143	0.856
Other (Judicial sector, utilities)	0.032	0.030	0.652
Job characteristics			
Fraction of non-routine tasks (as opposed to routine tasks)	0.296	0.292	0.597
Physically demanding work	0.092	0.092	0.143
Mentally demanding work	0.670	0.642	0.579
Log of yearly wage income	10.835	10.833	0.241
Wealth and income sources after retirement			
Number of years contributed to pension fund	32.088	32.677	0.236
Pension rights at other pension funds	0.122	0.136	0.463
Partner has pension or income	0.474	0.476	0.718
Net housing wealth	0.573	0.556	0.198
Inheritance	0.140	0.136	0.445
Annuity insurance	0.516	0.523	0.253
Life insurance	0.260	0.268	0.846
Savings account >15,000 euro	0.617	0.605	0.654
Investment	0.348	0.349	0.838
Life Course Savings programme	0.159	0.067	0.000
Other assets or pension savings	0.101	0.093	0.929
Extra savings for pensions in previous years	0.269	0.229	0.503
Retirement expectations			
Expectations about replacement rate	67.092	71.868	0.000
Expected retirement age	62.063	61.905	0.025
Expected retirement age five years ago	61.378	61.517	0.222

Notes. The fraction of non-routine tasks is based on the following survey question 'If you divided your working time into non-routine tasks and routine tasks, how much of your time do you spend on non-routine tasks?' The indictors for physically/mentally demanding work are based on two survey questions that asked how well workers identified themselves with the following statements: 'I have physically (mentally) demanding work'. Answer categories ranged from 1 (very bad) to 5 (very good). For this Table, the answer categories are dichotomised (1 corresponds to a score of 4 or 5 and 0 corresponds to a score of 3 or lower). The wealth and income sources after retirement are dummy variables that indicate whether the respondent have access to these respective sources.

now?'; and 'If you retired at the age of 62, how large would your pension benefit be as a percentage of your net wage income?' Interestingly, the average response to the first question does not differ significantly between the treated and the control groups. However, we do find a significant difference between the two groups for the second question (p-value is 0.025), although this difference is relatively small. Those born in 1949 expect to retire at age 61 years and eight months, while those born in 1950 expect

to retire at age 62 years and one month, on average. This small difference in expected retirement age could imply that many workers in the treatment group accept lower pension benefits because they do not want to change their retirement plans. An alternative explanation is that workers are not well-enough informed about their pension rights or that they increased their private savings. In the light of the latter variable, it is important to have a closer look at the response to the third question.

Do people understand the consequences of the changes in the pension system? The sample means in Table 1 show that respondents born in 1949 expect, on average, a pension income at the replacement rate of 72% at retirement age 62, while employees born in 1950 anticipate a replacement rate of only 67% at this age. This difference is highly statistically significant in a regression on the treatment dummy and age. These expectations are remarkably close to the actual replacement rates of 70% and 64%, respectively, that they are forecast to receive based on past and projected pension contributions. Figure 2 shows the expected replacement rates for workers born in 1949 and 1950. The dots represent expected average replacement rates for individuals born in a specific two-month period. Figure 2 shows that there is a clear break in expectations around the threshold date (1 January 1950). It seems reasonable to conclude from the Figure that employees are indeed familiar with the consequences of the new pension system with respect to their individual situations.

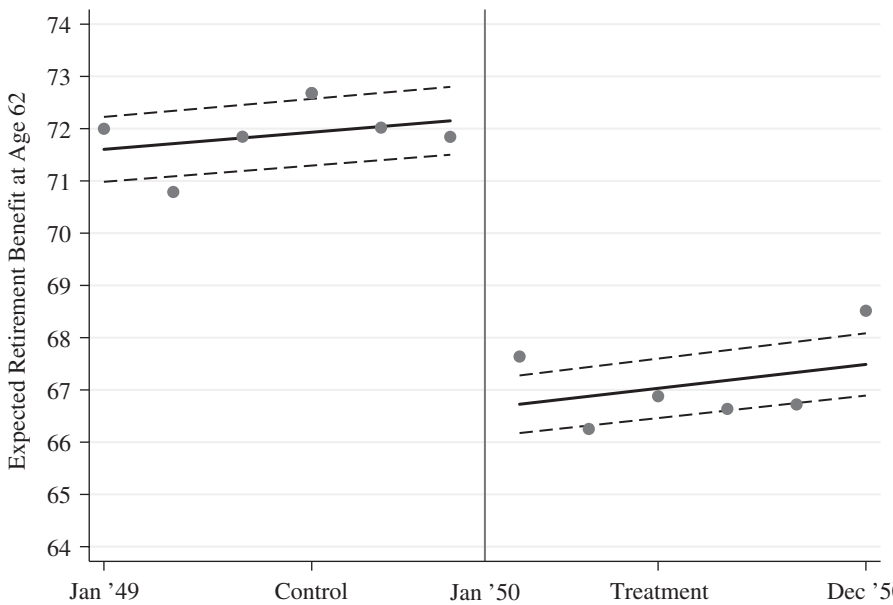

Fig. 2. *Validity of The Natural Experiment: Do People Understand The Reform?*
Notes. This Figure presents the mean expected pension benefit at age 62 as a percentage of present wage income (for two successive birth months). The information is based on the following survey question: 'If you retired at the age of 62, how large would your pension benefit be (in percentage of your net wage income)?' The vertical line marks the threshold dividing the control and treatment groups.

3. Results

3.1. *Main Results*

We operate simple linear probability models for whether an individual is depressed. This is equivalent to a regression discontinuity approach (Van der Klaauw, 2002), given that we have a sharp discontinuity induced by the change in the pension system and the small age difference between the treated and the controls. In addition, Section 2 shows us that those in the treated and control groups are very similar in observed characteristics. The first column of Table 2 presents the estimation results of a base specification of the treatment effect on workers' mental health in which only age, as the number of days after 1 January 1949, is included as control variable. An individual is defined to be depressed if the CES-D8 score equals 4 or higher (see Section 2). The estimation results show that the reform has a sizable impact on workers' mental health: the coefficient of the treatment dummy is equal to 0.028, which means that depression rates are 2.8% points higher among the treatment group compared to the control group. This implies that two years after the policy change depression rates among the 1950 cohort were about 40% higher than among the 1949 cohort. As expected, Column 2 shows that the treatment effect remains unchanged when we add more individual controls that are fixed as of 2006 (marital status, education, sector). In further analyses, we therefore build on the base specification with only age included as control variable.

Table 2

The Effect of the Policy Change on Depression

Variables	OLS depressed 2008	OLS depressed 2008	OLS raw CES-D8 2008	OLS depressed 2009	OLS overall health 2008	OLS doctor visits 2008	OLS number of sick days >14 days 2008	OLS health limits work 2008
Treated (affected by the policy)	0.028**	0.028**	0.103	0.019*	−0.018	0.044	0.016	0.009
	(0.011)	(0.011)	(0.064)	(0.072)	(0.039)	(0.140)	(0.020)	(0.062)
Age (in days divided by 100)	0.004	0.004	0.011	0.002	−0.000	0.014	0.002	−0.004
	(0.003)	(0.003)	(0.015)	(0.017)	(0.009)	(0.033)	(0.005)	(0.014)
Control variables	Excluded	Included	Excluded	Excluded	Excluded	Excluded	Excluded	Excluded
Observations	5,274	5,274	5,274	6,148	5,268	5,214	5,265	5,258
R^2	0.002	0.006	0.001	0.001	0.000	0.000	0.001	0.000

Notes. * <0.10. * <0.05. *** <0.01. Columns 1 and 2 show the results of linear probability models that measure the treatment effect on depression rates in 2008. The depression indicator is based on the CES-D8 scale, which consists of eight items to measure depressive symptoms. This depression indicator is constructed by first dichotomising (yes/no) responses and reversing the coding of the positively phrased items to achieve a count variable from 0 to 8, where higher values suggest worsening depressive symptoms. In the next step, we construct a dummy variable that indicates whether workers are considered to be depressed. We used the suggested score of 4 and above, consistent with probable clinical depression. Column 3 shows the results of a OLS model on the raw CES-D8 score (after dichotomising) which ranges from 0 to 8. Column 4 shows the treatment effect on depression rates in 2009. Finally, Columns 5–8 show the treatment effect on other physical health measures. Standard errors are in parentheses.

Until now, we used the cut-off score of 4 to construct the depression variable, which is suggested by several validation studies as the optimal score for determining depression (Blazer *et al.*, 1991; Andresen *et al.*, 1994; Beekman *et al.*, 1995; Steffick, 2000; Reyes-Gibby *et al.*, 2002). Nevertheless, the raw CES-D8 score may contain additional information. Therefore, we also perform a regression on the raw CES-D8 scale (count variable from 0 to 8), where higher values suggest worsening depressive symptoms. Column 3 of Table 2 shows that the coefficient of the treatment dummy is positive, indicating that workers born in 1950 indeed score higher on the raw CES-D8 score. However, the treatment effect is not significant at the standard levels (p-value of 0.108). This may be partly due to the raw CES-D8 score showing substantive heaping at the 0 score.[14]

Although the third data wave in 2009 suffers from a substantial share of respondents from the control group who left the sample in that year because of early retirement, it is useful to check whether the effect of elevated depression rates persists over time. Several related empirical studies on life satisfaction investigate hedonic adaptation processes and find that people recover after sudden shocks in their income, although often only partially (Brickman *et al.*, 1978; Clark *et al.*, 2008*a*; Oswald and Powdthavee, 2008). It is therefore conceivable that the treated workers in our sample also become accustomed over time to the fact that they have reduced pension rights and, therefore, become less depressed. In this context it is important to note, however, that the depression indicator used in our earlier analyses was measured in the spring of 2008, which is already more than two years after the implementation of the new pension system. Column 4 of Table 2 contains the results of a regression analysis on depression rates in 2009 and indicates that the effect of the reform is slightly smaller in magnitude, but still significant at the 10% level. This suggests that the effects of the reform may persist over time.

The survey includes measures of physical health and it would be interesting to see whether the retrenchment in pension rights also affects these measures of health. One would expect the treatment effect to be smaller or maybe even absent as it takes more time for physical health effects to manifest. Results on analyses on overall health, health limitations at work, number of doctor visits and whether workers were sick for more than 14 days are reported in Columns 5–8 of Table 2. It is interesting to see that there are indeed no treatment effects on these health variables.

3.2. *Bandwidth Analyses*

We include age in the specifications of Table 2 as the number of days after 1 January 1949. Clearly, age should be controlled for, since this may be a relevant factor for depression. At the same time, there is little variation in the age variable and it clearly

[14] We deal with this issue by estimating a Poisson regression such as that in Falba *et al.* (2009). The coefficient of the treatment dummy (0.175) is highly significant in the Poisson model (standard error is 0.072). We also estimated a regression analysis on a depression indicator with a cut-off score of 5. Because the number of depressed workers is very small when applying this cut-off, we pooled the data of the 2008 and 2009 surveys to increase the number of observations and found a significant coefficient for the treatment dummy of 0.012 (with standard error 0.006). Finally, we estimated linear probability models on the underlying items of the Center for Epidemiologic Studies Depression Scale 8 (CES-D8) score. We find that treated workers indicate more often that they had a 'restless sleep' and had problems in 'getting going'.

correlates with the reform dummy. This may affect our results. Moreover, it is conceivable that effects from the reform may be stronger for individuals who just missed the old generous pension system by a few days, weeks or months. After all, the threshold of 1 January 1950, is arbitrary, and given that there is an effect from the pension reform on the depression rate, one may expect that this effect will be stronger on those who, by fate, just missed the threshold. By omitting the age variable in the regressions but estimating the regressions for a specified reduced age window so that these age effects are less relevant, we address the issue of the sensitivity of our findings to the inclusion of the linear age variable and to the distance of workers to the treatment threshold. We implement the procedure of Imbens and Kalyanaraman (2010) to derive the optimal bandwidth (age window) for our regression discontinuity design by minimising an expected squared error loss criterion. The essential idea behind the algorithm of Imbens and Kalyanaraman (2010) is that the bandwidth increases with the variance in outcomes at the cut-off, with a diminishing density of the forcing variable (age in our application) or when the shape of the curves on opposite sides of the cut-off becomes increasingly symmetrical. According to this procedure, the optimal age window is equal to 180 days around 1 January 1950 (2,629 observations). Table 3 presents the estimation results of the linear probability model for this age window. Column 1 shows that we find a significant coefficient for the treatment dummy of 0.024, which is remarkably similar to the coefficient found for the whole sample. The fact that the size of the coefficient does not change buttresses our previous findings.[15] Column 2 of Table 3 presents the results of OLS estimations for the optimal age window on the raw CES-D8 score. We find a coefficient of 0.088 (standard error is 0.046) which is somewhat lower than the coefficient found in Column 3 of Table 2 but is significant at the 10% level.[16]

3.3. *Heterogeneous Treatment Effects*

This subsection analyses whether the pension reform differentially affects different types of workers. Table 4 presents the estimation results of separate analyses on subsamples and shows that there are indeed substantial differences in the impacts of the reform for different subgroups. First, we checked whether the pension reform only affects workers with bad health. If this were the case, one could argue that these workers were more likely to become depressed anyway and that the reform only accelerated this process. Therefore, Columns 1 and 2 show estimations of the treatment effect for workers with bad and good health, respectively. The division in good and bad health is made by using the answers to the following question: 'In general, how would you describe your current health?' The response categories ranged from 1 (very bad) to 5 (very good). Workers are considered to be in bad health when they have a score below 4. Reassuringly, we find that treated workers in good health are more depressed than the untreated workers in good health. The treatment effect for workers in bad

[15] Moreover, choosing an even smaller age window (born within three months around 1 January 1950 (1,305 observations) gave virtually identical results (coefficient of 0.025 with a standard error of 0.011).

[16] A Poisson estimation on the raw CES-D8 score yields a coefficient for the treatment dummy of 0.151 with a standard error of 0.051.

Table 3
The Effect of the Policy Change on Depression in 2008: *Optimal Bandwidth*

Variables	OLS depressed 180-day bandwidth	OLS raw CES-D8 180-day bandwidth
Treated (affected by the policy)	0.024***	0.088*
	(0.008)	(0.046)
Age (in days divided by 100)	Excluded	Excluded
Control variables	Excluded	Excluded
Observations	2,629	2,629
R^2	0.003	0.001

Notes. * <0.10. ** <0.05. *** <0.01. Columns 1 and 2 show the results of OLS estimations of the treatment effect on the depression rate (cut-off ≥ 4) or the raw CES-D8 score (ranging from 0 to 8) for an age window of 180 days around the treatment threshold of 1 January 1950. This age window is equal to the optimal bandwidth derived from the Imbens and Kalyanaraman (2010) procedure. Standard errors are in parentheses.

health is somewhat larger, although not statistically significant at the standard levels (p-value of 0.111) due to the smaller sample size.

Furthermore, it is conceivable that workers who contributed the longest to the pension fund are more disappointed and frustrated by the sudden 'unfair' retrenchment of their pension rights. For these workers the loss in pension wealth is highest. Columns 3 and 4 of Table 4 show that treated workers whose number of contribution years are above the median are indeed significantly more depressed (coefficient of 0.038) than those who contributed fewer years (coefficient of 0.015). We further expect that workers with higher incomes are more strongly affected, since the reform has the largest financial consequences for these workers. The supplementary sectoral pension benefits (the second pillar of the pension system) that are affected by the pension reform comprise a smaller share of the total pension benefits for workers with lower wages. On the other hand, the share of the old age pension (pillar one) that provides a minimum retirement benefit at a flat rate is comparatively large for workers with low wages, so the reform has a lower impact on their pension wealth. Columns 5 and 6 show, however, that the difference in the size of the coefficients of the treatment dummy for workers with an income below or above the sample median is not statistically significant, possibly due to the higher share of private savings of high-income workers. One would also expect that the treatment effect differs with marital status and with the partner's income. It is likely that the financial impact of the pension reform is larger for workers who have to provide for a family and whose partner does not contribute to the household income. Columns 7 and 8 of Table 4 show that married and unmarried men are not differently affected by the reform but this may in part be due to the low number of unmarried males in our sample. Conversely, as can be expected, Columns 9 and 10 show that workers whose partners have a pension income are less affected by the income shock due to the pension reform than those who solely have to provide for their family.[17] This suggests that income still may play a role.

It is possible that the pension income of the partner has also decreased due to the pension reform, with the consequence that the partner's treatment further increases the

[17] The information on partner's pension income is based on a survey question that asked the workers in our sample whether their partner will have their own pension income after retirement.

Table 4

The Effect of the Policy Change on Depression in 2008: *Heterogeneous Effects*

	Bad health		Number of years contributed above median		Income above median		Married		Partner has pension income	
Variables	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Treated (affected by policy change)	0.066	0.020**	0.038**	0.017	0.030**	0.028	0.025**	0.062	0.017	0.056**
	(0.041)	(0.008)	(0.014)	(0.015)	(0.011)	(0.017)	(0.016)	(0.052)	(0.014)	(0.023)
Age (in days divided by 100)	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Control variables	Excluded	Excluded	Excluded	Excluded	Excluded	Excluded	Excluded	Excluded	Excluded	Excluded
Observations	1,113	4,155	2,640	2,634	2,625	2,641	4,820	454	2,456	1,527
R^2	0.003	0.002	0.003	0.001	0.002	0.002	0.001	0.004	0.001	0.004

Notes. * <0.10. ** <0.05. *** <0.01. This Table shows the heterogeneous treatment effects on depression rates in 2008 (cut-off ≥ 4). Columns 1 and 2 show estimations of the treatment effect of workers with bad or good health. The division in good and bad health is made by using the answers on the following question 'In general, how would you describe your current health?'. Response categories ranged from 1 (very bad) to 5 (very good). Workers are considered to be in bad health when they have a score below 4. Columns 3 and 4 show the results for workers who contributed more or less than the median number of contribution years, while Columns 5 and 6 present the results for samples of workers with an income above or below the median income within the total sample. Columns 7–10 show heterogeneous effects based on marital status and whether partners have their own income or receive a pension. All estimations are performed using a linear probability model. Standard errors are in parentheses.

Table 5
Heterogeneous Treatment Effects in 2008: *Treatment of Spouses*

Variables	Partner born in 1950 or later	Partner born in 1950	Partner born in 1950 or later with own pension income	Partner born in 1950 with own pension income	Partner born in 1950 or later without own pension income	Partner born in 1950 without own pension income
Treated (affected by the policy)	0.022*	0.036*	0.011	0.039	0.054**	0.059*
	(0.013)	(0.021)	(0.016)	(0.028)	(0.028)	(0.034)
Age (in days divided by 100)	Included	Included	Included	Included	Included	Included
Control variables	Excluded	Excluded	Excluded	Excluded	Excluded	Excluded
Observations	3,696	1,122	1,853	602	1,191	336
R^2	0.001	0.003	0.002	0.004	0.003	0.010

Notes. * <0.10. ** <0.05. *** <0.01. This Table shows treatment effects on depression rates in 2008 (cut-off ≥ 4) for workers with partners (with and without having their own pension income) who were born before or after the treatment threshold. All estimations are performed using a linear probability model. Standard errors are in parentheses.

depression rates of the men in our sample.[18] To address this question, we made use of the partner's birth year. Unfortunately, the second wave of our sample does not include information about the labour market status of the partner. Age may therefore only serve as a crude proxy for the effect of the pension reform on the (working) partner. We used this variable to perform separate analyses on subsamples of workers with partners who were born before 1950 or after 1949, with or without their own pension income. Columns 1 and 2 of Table 5 show that treated workers with treated partners are not significantly more depressed than those with untreated workers. As mentioned above, age may be a crude proxy for exposure of the partner. Moreover, female workers in this birth cohort often have disrupted careers and contributed, on average, for only 16 years, limiting the eligibility to the old pension rules and the possibility of drawing early retirement benefits before the age of 65. Columns 3–6 of Table 5 further confirm that the mental health of the men in our sample is more affected by the fact that their partner has no own pension income than by their partner's treatment status.

3.4. *Robustness*

The estimation results presented in Tables 2–5 show strong effects from the pension reform on mental health of the workers in our sample. Could it be that the difference in mental health between the two birth cohorts is due to another cause besides the pension reform? We are not aware of studies demonstrating differences in the mental health of the two cohorts prior to the pension reform. We are not able to check this with our data, since we only have post-2006 data. However, if such effects existed, one

[18] A recent and growing body of literature emphasises the incidence of joint retirement among dual-career families and the increasingly importance of spillover effects between the financial incentives of men and their spouses. Among others, Blau (1997, 1998), Baker (2002) and Coile (2004) find strong financial spillovers that affect individual retirement behaviour.

would also expect to see some differences in other health variables. Table 1 shows that this is not the case, and it also shows that no other factors systematically differed between the two groups that may have caused the differences in mental health. Furthermore, there have been no other changes that may have differentially affected the 1950 cohort as opposed to the 1949 cohort.

We pursue this issue further by performing a falsification test on the first wave of the 'Survey of Health, Ageing and Retirement in Europe' (SHARE), which is a representative survey that was held in 2004 and contains information on the mental health and socioeconomic status of a large group of individuals aged 50 and over in the Netherlands. The SHARE data enable us to perform similar analyses on mental health outcomes between the treatment and control groups prior to the reform. SHARE incorporates the EURO-D scale, which is an instrument for detecting probable cases of depression. The EURO-D scale was introduced by the 'EURODEP Concerted Action Programme', a consortium of 14 research groups, that studies the epidemiology of late-life depression. The scale consists of 12 items, of which 11 are present in the original CES-D scale (consisting of 20 items). The EURO-D scale ranges from 0 (not depressed) to 12 (very depressed). Prince *et al.* (1999) show in a validation study that the EURO-D scale is internally consistent and provides a good assessment of depressive symptoms. Moreover, they compare the performance of the EURO-D scale with that of the CES-D scale and conclude that both scales are highly correlated. Since several optimal cut-off points are suggested in the literature, we perform analyses on the EURO-D scale using different common cut-off points. Table 6 shows the results of this falsification test (cut-off is 4 or higher), in which we operate simple linear probability models for whether an individual is depressed in a base specification that only includes age (in months) as an additional control variable.[19] We find that two years before the pension reform there were no significant differences in depression rates between the treatment and control groups. This result holds when we increase the sample size by including workers who were born further away in time from the treatment threshold, or by including individuals who work in the

Table 6

Depression in the SHARE Dataset Before the Policy Change

Variables	Born within one year around 1 January 1950 All employed 2004	Born within one year around 1 January 1950 Public sector 2004	Born within five years around 1 January 1950 All employed 2004	Born within five years around 1 January 1950 Public sector 2004
Treated (affected by the policy)	0.076	−0.222	0.006	−0.039
	(0.101)	(0.164)	(0.051)	(0.080)
Age (in months)	Included	Included	Included	Included
Observations	139	68	719	303
R^2	0.023	0.034	0.003	0.014

Notes. * <0.10. ** <0.05. *** <0.01. This Table shows the results of linear probability models that measure the effect of the treatment dummy on depression rates two years prior to the policy reform (cut-off ≥ 4 on the EURO-D scale). Standard errors are in parentheses.

[19] Applying other cut-off points does not lead to different results.

private sector. The fact that there was no sign of elevated depression rates prior to the reform gives further confidence to our findings.[20]

Could it be that the reform caused some workers who are less prone to depression to leave the public sector and that this effect is stronger for the 1950 cohort? This effect is not likely. Public sector pensions are relatively generous and, along with the pension reform in the public sector, all other sectors changed their pension plans because the preferential tax treatment of pension premiums was abolished for both public and private sector workers. Furthermore, job mobility rates out of the public sector into the private sector are extremely low for older workers. When moving to another sector, it is likely that these workers will not only incur costs associated with a change in pension fund, since they will usually entirely lose their rights to retire before age 65 because the majority of pensions in the Netherlands impose not only an age criterion but also a minimum number of tenure years within a sector or sometimes even within a firm (Euwals *et al.*, 2006).

Those born in 1949 still have the option to retire between ages 55 and 65. We, therefore, also examine whether some workers stopped working between the introduction of the reform in 2006 and the second wave of the survey (2008), since this can influence our results if this effect is sizeable. We find that only a very small group of workers of the 1949 cohort who participated in the first wave retired between 2006 and 2008. From 2009 onward, however, we observe for the first time that a substantial share of respondents from the control group left the sample because of early retirement (at age 60, see also Section 1).

In our analyses, we drop individuals who did not work continuously in the public sector between 1997 and 2006. It is conceivable that those with mental health problems are more likely to have gaps in their employment history and therefore do not pass this selection criterion. This could affect our results only if this affects the treated cohort differently than the 1949 cohort. Table A1 shows that there is no reason to expect this. Moreover, if this were the case, one would expect to see this reflected in other factors that are correlated with depression (such as marital status) or with income, for instance. As discussed in Section 2, we see no differences in the observed variables between the two birth cohorts (see Table 1). We run additional regressions in which we add this group to the sample (and include a dummy variable for this group in addition to whether workers of this group were born in 1950). We find that both coefficients are insignificant, indicating that the mental health status of this group does not differ from workers born in 1949 and those who worked continuously since 1997 (the control group).

4. Why is the Affected Cohort so Depressed?

A important question that remains is why does the reform have such a strong impact on mental health? The results in Tables 4 and 5 of Section 3.3 indicate that the future income effects of the pension reform are an important mechanism. Treated workers

[20] A similar falsification test on the 'Permanent Survey of Living Conditions: Health and Labour'(POLS) of Statistics Netherlands confirms that there were no differences between the treatment and control groups. This dataset contains the Mental Health Inventory (MHI-5) which is a well-validated scale for depressive symptoms (Kelly *et al.*, 2008) but has the drawback of being substantially smaller than the SHARE.

whose loss of pension rights is high and those who have a partner who does not have a pension income are more often depressed.[21] Another potential mechanism is the way in which the pension system reform was set up. The 2006 reform of the pension system represented a major change that added to previous reforms in the retirement system. In particular, the 1949 cohort was the last cohort that was allowed to retire at relatively young ages against relatively generous replacement rates. The 1950 cohort is the first cohort that must work longer against substantially lower replacement rates. Furthermore, this group of public sector workers is actually adversely affected by their own employers, who renege on pre-existing arrangements (violate an implicit contract) in a way that makes it hard for them to cope with their new, reduced pension rights. Workers born in 1950 face this new situation with relatively short notice, too short to offset the change in the system with additional savings completely. The change in the pension system was not entirely unexpected but the particular type of discontinuous assignment rule and the strong differential treatment of workers born around 1 January 1950, came as a surprise when announced in July 2005. Anecdotal evidence indirectly suggests that it was perceived to be unfair and therefore may have led to severe disappointment. For more on this issue, see Montizaan *et al.* (2011), who examine the relation between job motivation and negative reciprocity.[22] It is likely that these sentiments are less relevant for younger cohorts, and therefore their mental health is likely to be less affected. Unfortunately, we cannot check this with our data.

Our findings also relate to the literature on individual well-being and happiness. This literature finds that individual well-being may be affected by income, as well as by the difference between one's own income and that of a reference group (for an overview, see Clark *et al.*, 2008*b*). Ferrer-i-Carbonell (2005) finds that the income of the reference group is about as important as one's own income for individual happiness. Calvo *et al.* (2009) examine the factors that affect individual happiness in the transition to retirement. Their results suggest that what really matters is whether people perceive the transition from work to retirement as chosen or forced. These authors suggest that it is the sense of control over one's own retirement that influences the happiness of older workers. It is clear that control over one's own retirement is a problem for Dutch workers born in 1950. For cohorts born in later years, this is presumably less of a problem, since the longer period before retirement allows them to compensate better for their loss of pension wealth.

5. Conclusion

The pension reform that was implemented in 2006 induced a sharp discontinuous treatment of pensions rights for cohorts born around 1 January 1950. We exploit this discontinuity to measure the effect of changes in pension rights on the mental health

[21] However, analyses in which we regress the depression indicator simultaneously on the treatment dummy, age and the expected replacement rate at age 62, show that the treatment effect remains large (coefficient is 0.028) and strongly significant (standard error is 0.012). It is difficult to interpret the individual coefficients in such a regression as the expected replacement rate can in itself also be affected by the pension reform. This aside, the regression hints that factors other than the pension income effect are responsible for the high depression rate among those affected by the reform.

[22] Negative reciprocity is an in-kind response to hostile acts that indicates retaliatory tendencies to unfair treatment.

of workers approaching retirement and find that the reform had a strong impact on the mental health of workers affected by the reform. Our analysis reveals that those who are exposed to a pension reform that confronts them with substantially lower pension wealth are more often depressed. This effect persists over time and is robust to applying smaller bandwidths around the treatment threshold. Furthermore, we find differing effects for different types of workers. The effects are stronger for workers who experience a larger income loss, and for married men whose partner has no pension income. This suggest that income effects play a role.

We find that the reduction in pension rights is important for mental health but that other factors are also at work. The discontinuous assignment rule and the strong differential treatment of workers born around 1 January 1950 is likely perceived to be unfair and may have led to severe disappointment. Moreover, the pension reform was announced only a few years before the retirement date of the affected workers and too little time remained to allow these workers to fully offset the loss in pension wealth. Workers were suddenly forced into a new situation, with little control over their retirement decision which may have affected their mental health.

Our findings have great relevance for public policy. Currently, most countries in the developed world are revising their pension systems to cope with population aging. The reforms are geared towards extending working life and a smaller role for defined benefit pensions. Furthermore, a substantial part of the pension wealth of workers has recently evaporated due to the current financial crises. Changes in worker pension claims, due to either financial crises or government pension policy changes, will have severe consequences for most workers nearing retirement. Workers either have to accept a substantial drop in pension wealth, increase pension contributions or work substantially longer than they expected before the current crisis. The results of this study show that a sudden irreversible deterioration of future prospects can have serious consequences for the mental health of workers nearing retirement, especially when their own employer reneges on pre-existing arrangements (violates an implicit contract) in ways that are difficult to adjust to once one has taken those rules into account in one's plans. The period before the planned retirement is too short to compensate for losses in pension wealth. In the longer run, these mental health effects may translate into somatic diseases. This will not only affect individual well-being but will also engender costs associated with depression and worse physical health. As mentioned, the healthcare expenditures of depressed persons are about four times higher than those of non-depressed individuals. Moreover, there are high indirect costs due to the loss of productivity, flawed decision making and workplace accidents. Governments should take these effects and costs into account when re-designing pension policies.

Appendix

Table A1
Number of Individuals After Selection

	Number of individuals		Percentage of population	
	Affected by the reform	Not affected by the reform	Affected by the reform	Not affected by the reform
Population				
2007	14,251	13,468		
2008	14,251	13,468		
2009	14,251	13,468		
Sample before selection				
2007	4,341	4,175	30	31
2008	3,079	2,991	22	22
2009	3,952	3,759	28	28
Sample after selection employed in the public sector				
2007	3,950	3,780	28	28
2008	3,041	2,907	21	22
2009	3,856	3,630	27	27
Sample after selection employed in the public sector and individuals not employed in burdensome jobs				
2007	3,753	3,591	26	27
2008	2,910	2,781	20	21
2009	3,601	3,320	25	25
Sample after selection employed in the public sector, individuals not employed in burdensome jobs, and without career breaks since 1997				
2007	3,655	3,507	26	26
2008	2,840	2,729	20	20
2009	3,501	3,227	25	24
Sample after selection employed in the public sector, individuals not employed in burdensome jobs, without career breaks since 1997 and depression indicator is not missing				
2007	NA	NA	NA	NA
2008	2,688	2,586	19	19
2009	3,192	2,956	22	22

Notes. This Table documents the effect of the selections made in our analyses on sample sizes for all waves. Three selections were made to guarantee the internal validity of the research design. First, since we are primarily interested in the mental health effects of the pension reform on workers, we excluded individuals who did not work in the public sector when they filled in the questionnaire. Second, we excluded workers with burdensome jobs. This small minority of workers is eligible to special arrangements that allow early retirement against a replacement rate of at least 70% between ages 55 and 61 and therefore does not experience a substantial drop in income. Third, we excluded workers who did not work continuously between 1997 and 2006. This resulted in 122 observations of employees who were born in 1949 or 1950 who were not eligible to the old pension rights. For the remaining workers, the pension reform is clear and simple, since age is the only criterion determining whether a worker is in the treatment or control group.

Table A2
Survey Participation

Variables	2007	2008	2009
Treated (affected by the policy)	−0.005	−0.006	−0.002
	(0.006)	(0.005)	(0.005)
Constant	0.310***	0.222***	0.279***
	(0.004)	(0.004)	(0.004)
Number of observations	27,719	27,719	27,719
R^2	0.000	0.000	0.000

Notes. * <0.10. ** <0.05. *** <0.01. This Table shows the results of linear probability models that measure the effect of the treatment dummy on participation into the survey for different waves. Standard errors are in parentheses.

Table A3
Depression in the POLS Dataset Before the Policy Change

Variables	Born within five years around 1 January 1950			
	All employed (2001–2005)	Public sector (2001–2005)	All employed (2005)	Public sector (2005)
Treated (affected by the policy)	−0.030	0.039	−0.012	−0.016
	(0.022)	(0.053)	(0.046)	(0.094)
Observations	2,333	372	465	72
R^2	0.004	0.016	0.000	0.003

Notes. * <0.10. ** <0.05. *** <0.01. This Table shows the results of linear probability models that measure the effect of the treatment dummy on depression rates prior to the policy reform. Depression is measured by the Mental Health Inventory (MHI-5), a well-validated scale for depressive symptoms. The MHI-5 comprises five questions on which respondents could answer on a six-point scale (scores between 1 and 6) were higher score indicated better health. The total score of an individual ranges between 5 and 30 which is subsequently transformed through a standard linear transformation into a variable ranging from 0 to 100. To indentify depressive symptoms, we construct a dummy variable in a similar way as we have done with the CES-D8. Since there is an ongoing debate on the optimal cut-off point for the MHI-5 scale, we have used several cut-off points which have been suggested in the literature. In this Table, we report the results for a cut-off at 60. Columns 1 and 2 contain the results for the period 2001–2005 (pooled), while Columns 3 and 4 contain results for 2005. Standard errors are in parentheses.

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