

Pension rights, human capital development and well-being

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Pension rights, human capital development and well-being

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Raymond Michel Montizaan

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1 Introduction

1.1 Motivation

Population ageing is a major challenge faced by many European countries in the next decades. European countries will experience a steep increase in the share of elderly persons in their population, while the share of workers who are in their prime age is declining. These demographic changes, in combination with retirement patterns, can lead to a considerable rise in the ratio of older, inactive individuals to workers. If labor force participation by age and gender remains unchanged, the Organisation for Economic Co-operation and Development (OECD, 2006) projects that the number of retirees will double by 2050 in most European countries. The OECD states that severe labor market shortages can arise, since the number of retirees will exceed the number of young labor market entrants by the year 2015, and by almost 1 million persons annually around the year 2030. Moreover, the increasing number of retirees will impose high costs on the working population, since future levels of public pension expenditures as a percentage of the gross domestic product in the EU-25 are expected to rise by 2.7%-points to 14.6% in 2050 (Economic Policy Committee, European Commission, 2006). Although European governments have made first steps to solve the pension issue, much disagreement remains as to what kinds of policy measures are necessary and the size of the required retrenchment in pension rights. Moreover, no agreement exists on the design and nature of reforms, particularly because the extent of potentially undesirable side effects is unknown. Therefore, in the last two decades, many economists have examined the determinants and impact of retirement and the effects of proposed pension reforms on a variety of economic variables.

Most studies within the retirement literature analyzed the impact of financial incentives provided by social security and pension systems on labor force participation (e.g., Stock and Wise, 1990; Gruber and Wise, 1998; Samwick, 1998; Bingley and Lanot; 2004; Chan and Stevens, 2004). The common finding is that the provisions and design of social security programs often provide strong incentives to leave the labor force early. In particular, pension systems in the past decades have not been actuarially fair and have imposed a large implicit tax on continued labor force participation. A much smaller body of literature showed that the retirement decision is also strongly related to the human capital development of older workers (e.g., Becker, 1975; De Grip et al., 2002; Lau and Poutvaara, 2006; Dave et al., 2008; Falba et al., 2009). The gains from continued work are critically affected by the productivity of the human capital stock of firm's employees. In this context, deteriorations in the health of older workers or in the economic value of their skills, which can lead to productivity losses and compromised work performance, are relevant determinants

of the retirement decision. Moreover, training investments to maintain productivity are a vital factor that can influence the attitude of employers toward their older employees and the attitude of elderly workers concerning the continuation of their career.

The literature on the human capital development of older workers, however, has difficulties assessing the direction of causality between health or training investments and the retirement decision. The few empirical studies that examined the relation between training investments and retirement generally found that the training participation of older workers is strongly associated with postponed retirement, but cannot establish whether these training investments are designed to maintain productivity, deterring employers from prematurely ending the labor contracts of older workers, or whether retirement expectations influence training decisions (Bassanini et al., 2007; Fouarge and Schils, 2009). Indeed, human capital theory clearly argues that the present value of net returns to human capital investments in older employees is lower due to the shorter period during which they, as well as their employers, can reap the benefits of these investments. Hence, the anticipation to early retirement, which in turn is affected by the incentives provided by the pension systems, can be a relevant determinant of the training participation of older workers (Lau and Poutvaara, 2006).

The same applies to most empirical studies on the relation between health and retirement. Several studies found that early retirement is associated with poor health and suggest that investments in the prevention of illness and disease can induce later retirement (e.g., Tsai et al., 2005; Dave, et al., 2008). Conversely, Charles (2004) found that retirement has a direct exogenous positive effect on mental well-being and Falba et al. (2009) found that mental health is negatively affected by deviations from an expected retirement date. The latter mechanism is especially intriguing, since it implies that pension reforms that have become central to the European social policy agenda are likely to induce later retirement, but may also decrease the productivity of older workers through deteriorating health or lower job effort. It is this notion, until now largely overlooked by both policy makers and economists, that motivates the research in this thesis.

1.2 Aim of the study

The aim of this thesis is to determine the extent to which pension reforms affect training investments and the job effort, well-being and mental health of older work-

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ers. This question is extremely relevant today, since the forecasted labor market shortages due to demographic changes imply that policy makers should not focus solely on increasing the labor force participation of older workers, but also on maintaining their productivity. This thesis contributes to the literature on human capital development in two ways. First, our contribution lies in examining the impact of changing retirement expectations on training investments and health status, both relevant indicators of worker's productivity, while appropriately accounting for the endogenous nature of the retirement decision. The approach here consists of exploiting a natural experiment that generates a drastic change in the Dutch public sector's pension system that affects only a subgroup of workers. Moreover, this pension reform is independent of training investments or worker health status. Our findings are therefore less likely to be confounded than those in previous studies. Second, this thesis contributes to the existing literature by focusing on the potential side effects of the pension reform. We try to unravel behavioral responses to the pension reform in job effort and well-being. Information on how strongly workers react to a perceived 'unfair' retrenchment in their pension rights and whether workers with different personal traits react differently can help facilitate the implementation of future pension reforms. We are particularly interested whether the reactions of workers differ with their reciprocal inclinations. In line with the experimental laboratory results of Ederer and Fehr (2007), who found that subtle forms of deceit undermine the effectiveness of effort provision in a dynamic tournament setting, we expect that negatively reciprocal workers are more likely to retaliate a drop in their pension rights, by, for example, reducing their effort. Moreover, the literature on life satisfaction showed that people predominantly care about their relative position rather than their absolute position (e.g., Ferrer-i-Carbonell, 2005; Luttmer, 2005; Clark et al., 2008b), which could imply that those affected by the pension reform may be sensitive to the extent to which colleagues in their organization suffer from the reform as well. It is worth examining how strongly social comparisons can influence the reactions of workers affected by a drop in their pension rights.

1.3 Outline

The remaining chapters of this thesis are structured as follows. Chapter 2 provides detailed information on the pension reform and the policy context used to identify the causal effects of changing retirement expectations on the human capital development and health status of older workers. Chapter 3 addresses the question of whether or

not an exogenous retrenchment in pension rights that induces later retirement stimulates human capital investments in older workers, that is, whether workers who are forced to retire later participate more in training courses. Chapter 4 subsequently analyzes the impact of the reform on the mental health status of those with retrenched pension rights. Chapters 5 and 6 evaluate how differences in personal traits affect workers' behavioral responses in terms of job effort and well-being. Chapter 5 investigates how and to what extent the job effort of treated workers is influenced by negative reciprocal inclinations with respect to the 'unfair' retrenchment of pension rights. Chapter 6 shows the influence of social comparisons on the well-being of those affected by the reform. Finally, Chapter 7 summarizes the main findings and policy implications of this study, and provides a discussion on unexplored questions and remaining issues that will be part of our future research agenda.

Chapter 2: the natural experiment

Our analyses are based on a reform of the Dutch public sector's pension system that generated a considerable retrenchment of pension rights for a subgroup of workers through the introduction of a severe discontinuous rule. Prior to 2006, public sector workers with 40 years of tenure in the Netherlands could retire at the age of 62 years and three months with a replacement rate of 70% of their average yearly earnings since 2002. As of 2006, those born before January 1, 1950, could continue to retire under the old rules, but for those born on or after January 1, 1950, the replacement rate was lowered to 64%. These workers, of about the same age, now needed to work an additional one year and one month to obtain the 70% replacement rate enjoyed by their counterparts who might be just a few days, weeks, or months older. Since the 2006 reform is part of a series of reforms that begun in the 1980's and cannot be designated as an isolated event, Chapter 2 describes the present Dutch pension system and the policy context and processes that preceded this reform. Furthermore, it explains how the data for this research were collected. Finally, it notes that the exploitation of regression discontinuities has substantial advantages, but also some disadvantages. Hence, it discusses the pros and cons of using a regression discontinuity design and the implications of the disadvantages for the results from the following chapters and it presents some descriptives that justify the experimental approach.

Chapter 3: training participation

Human capital theory predicts that pension systems that encourage early retirement have a negative effect on human capital formation because early retirement reduces the period in which the benefits of training investments can be reaped (see Ben-Porath, 1967; Becker, 1975). Chapter 3 tests this hypothesis by examining the effects of the exogenous change in future pension benefits on worker training participation one year after the reform. While the existing literature (Bassanini et al., 2007; Fouarge and Schils, 2009) provides only indirect evidence, this chapter explicitly analyzes the effects of early retirement incentives and how changing retirement expectations can affect training participation. It compares the training participation of workers who were born in 1949 and remain entitled to generous pension rights under the old system with the training participation of those who were born in 1950 and subject to the new, less generous system that stimulates later retirement. We find that the pension reform indeed postpones expected retirement and increases participation in training courses among older employees. However, the treatment effect is only significant for workers in larger organizations, which suggests that larger organizations are better able to customize their training policies to the new retirement system. This can be explained by the fact that they have more formally developed and sophisticated human resource management (HRM) systems than small organizations (Koch and McGrath, 1996). The treatment effect for workers in large organizations remains robust to the inclusion of age and to the use of alternative specifications. Furthermore, the treatment effect is lower for more highly educated workers. We attribute this result to the higher intrinsic motivation to train of highly educated workers, irrespective to sudden shocks to their expected retirement age. Moreover, the treatment effect is - as might be expected - only significant for participation in longer training courses.

Chapter 4: mental health

Chapter 4 assesses the mental health effects of the 2006 pension reform. Our analyses are most closely related to the study of Falba et al. (2009), which examines the impact of deviations of actual retirement dates from earlier expectations on mental health. This thesis contributes to the literature by providing results that are most likely to be less confounded due to the natural experimental character of our data and by showing that there are *ex ante* effects that are substantial and persistent over time. The data also allow, to some extent, for further analysis of savings decisions and retirement expectations. The mental health of workers born in 1949 (59 years

old in 2008) and 1950 (58 years old in 2008) is compared two years after the policy change. We find strong effects from the exogenous change in the retirement system. Depression rates for the 1950 cohort are about 40% higher than for the 1949 cohort. Moreover, this effect persists over time and is stronger the closer one was born to the threshold date. Furthermore, the effects differ substantially for different types of workers, being stronger for unmarried workers and negligible for workers whose partner has a pension or income. The question remains as to why the depression rate of the affected group is so much higher and why this effect persists over time. We find that later expected retirement is important for mental health, but that other factors are also at work. The next two chapters show that negative reciprocal inclinations and social comparisons strongly affect the treatment effects.

Chapter 5: negative reciprocity and job effort

The discontinuous assignment rule and the strong differential treatment of workers born around January 1, 1950, is likely to be perceived as unfair, especially because it was announced only a couple of years before the expected retirement date of the affected workers. Chapter 5 complements the evidence from laboratory experiments that highlight the behavioral relevance of reciprocity in stylized labor markets by explicitly analyzing how negative reciprocal inclinations to the retrenchment in pension rights affect job effort (Fehr et al., 1993; Brown et al., 2004; Ederer and Fehr, 2007). The measure of negative reciprocity is derived from the responses to three survey questions. Job motivation is used as a proxy for job effort. We find that motivation is lower among highly negatively reciprocal individuals who face an unexpected drop in their pension rights. The negative effect on job motivation is greater for the group of workers that arguably perceive the policy change as being most unfair, namely, negatively reciprocal workers born close to the cut off date of January 1, 1950. Moreover, job motivation is lower among negatively reciprocal workers who can hold their employer directly responsible for the retrenchment of their pension rights, that is, those who work for the national government which initiated the policy change.

Chapter 6: social comparison and subjective well-being

Chapter 6 investigates the effects of the pension reform on well-being and its underlying domains. We find strong and persistent effects on both life and job satisfaction that are sizable compared to income effects on well-being. This chapter shows that a major part of the treatment effect can be explained by social comparisons. In

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particular, consistent with the recent literature on relative concerns in happiness (e.g., Ferrer-i-Carbonell, 2005; Luttmer, 2005; Clark et al., 2008b), we find evidence that suggests that workers compare their own replacement rate with the average replacement rate of similar individuals in their organization, and that spillovers within the household can explain part of the treatment effect. However, the lion's share of the treatment effect is explained by non-monetary incentives associated with the unfairness of the treatment and social comparison. Although we control for monetary incentives by including the individual replacement rate and the average replacement rate of similar individuals in an organization, treated workers are found to be less affected by the reform when the share of the treatment group is larger in the organization where they are employed. Moreover, the treatment effect increases with the share of the control group in the organization where the worker is employed.

2 The Natural Experiment and Research Design

2.1 Introduction

This chapter provides background information on the 2006 reform of the pension system for Dutch public sector workers and on the research design used to identify the effects of changing retirement expectations on the human capital development and health of older workers. Since this reform was part of a series of reforms that started in the 1980's and cannot be designated as an isolated event, Section 2.2 starts with a description of the present Dutch pension system and the policy context and processes that induced the major reforms preceding 2006. Next, this chapter presents a detailed discussion on the main characteristics of the 2006 pension reform.

Section 2.3 explains how the data were collected and discusses the validity of the research design. The main advantage of the discontinuity in the pension rules exploited here is that one can use the exogenous variation provided by policy changes to infer a causal relation between the explanatory variables and the dependent variable of interest. However, the literature on experiments shows that natural experimental approaches can still be subject to problems that can damage the validity of the experimental design. Using the validity check list for natural experiments developed by Campbell (1957, 1969), Campbell and Stanley (1966), Cook and Campbell (1979) and Meyer (1995), Section 2.3 discusses how to deal with five potential caveats that could damage our research design or the generalizability of the results in the following chapters. Moreover, descriptives are provided that seem to justify our experimental approach and that give an indication of the primary behavioral responses of workers to the retrenchment in their pension rights. Finally, Section 2.4 summarizes the chapter and presents conclusions.

2.2 Pension reform

The Dutch pension system consists of three main pillars: 1) at age 65, in principle all residents are entitled to a state old age pension financed by contributions that are levied along with the income tax; 2) most workers are entitled to a supplementary sectoral or firm pension of the defined-benefit type; and 3) individuals can voluntarily build up savings typically taken as annuities through an insurance company. Due to the extensive provisions of supplementary pensions in the second pillar, the third pillar is less well developed in the Netherlands. For almost all workers, early retirement before the age of 65 is possible only through the sectoral pension systems in the second pillar. In general, for individual workers participation in sectoral pension schemes is mandatory. These pension schemes are negotiated between unions

and employer organizations at the sector or firm level and are officially set forth in collective agreements that are in general binding to all firms in the sector by law (Delsen and Poutsma, 2005). The administration of these schemes is delegated to pension funds to which both employers and employees contribute.

2.2.1 Past changes in the pension system

The early retirement schemes, which are part of the pensions in the second pillar, were introduced in the mid-1970's of the past century. Large surpluses on the labor market in combination with a high level of youth unemployment led to the introduction of the so-called VUT-schemes (*Vervroegde Uittreding Regelingen*). The idea was that jobs occupied by elderly employees would become available to the young unemployed. The VUT schemes operated as Pay-As-You-Go (PAYG) systems in which the working population pays for the retirement of those who retired early. The VUT-schemes were mainly characterized by a weak relation between contributions and claims, mild eligibility conditions, and a flat rate, that is, the size of the benefits were independent of the retirement date.¹ The financial conditions of the VUT-schemes were favorable to older workers since yearly gross benefits could equal up to 80% of the last earned gross wage. This implies that these schemes imposed a high implicit tax on continued work since workers who postponed their retirement forfeited the benefits that they otherwise would have received and did not receive a higher benefit during the remaining years of their retirement. Moreover, the VUT-schemes did not incorporate the possibility of retiring part-time and did not allow individuals to continue working when they received a VUT-benefit, or imposed a penalty on the benefit whose size equals the wage. Hence, VUT schemes heavily supported early and full retirement.

In the 1980's and early 1990's, the VUT-schemes became increasingly popular. Early retirement well before the age of 65 became a social norm in the Netherlands, which resulted in a decrease in the labor force participation rate of men in the age group 50 to 64 years to a historically low percentage of 58% in 1995 (CPB, 2005). However, the VUT-schemes increasingly became the subject of social debate. Since the 1990's, the effects of the demographic changes of the Dutch population on the financial sustainability of the VUT-schemes started to become visible, and it was acknowledged that the growth of the number of early-retirees would impose high costs on the working population due to the PAYG nature of the VUT-schemes. Further-

¹Eligibility rules differed between sectors, but in general workers were eligible for a VUT-benefit when they had a tenure of at least 10 to 15 years.

2 The Natural Experiment and Research Design

more, shortages started to arise in specific segments of the labor market (particularly for high-skilled jobs) and the youth unemployment rate decreased drastically.

The second half of the 1990s was marked by a first round of pension reforms aimed at decreasing pension rights and postponing retirement. These pension reforms mainly consisted of the gradual replacement of the VUT-schemes by less generous pre-pension schemes, facilitated by the *Wet Fiscale Behandeling Pensioenen 1999*. The new pre-pension schemes were capital funded, introduced a direct relation between an individual's contribution and pension rights, and introduced actuarial adjustments across different retirement ages, that is, workers who postpone their retirement receive a higher benefit. Moreover, early retirement wealth was considerably lower compared to that of the VUT-schemes which resulted in a drop of 10%-points in the gross replacement rates at a given retirement age (Labour Inspectorate, 2004), although in most sectors transitional arrangements were introduced to smooth the transition from the PAYG-financed VUT-schemes to the actuarially fair and capital-funded pre-pension schemes. Furthermore, workers were allowed to retire part-time, and penalties on continued work after retirement were abolished.

In practice, the transitional arrangements were so effective in smoothing the transition that most older workers were still able to retire early under similar conditions as those who retired under the old schemes. Workers within a certain age range, with a minimum amount of tenure, and who were employed when pre-pension schemes were introduced could, in general, still retire within the old VUT-scheme or could derive pre-pension rights similar to those they had before. An exception was the *Algemeen Burgelijk Pensioenfonds* (ABP), the public sector's pension fund, which introduced a relatively large reform of their retirement schemes in 1997. ABP introduced actuarial adjustments and decreased the pension wealth of workers who retired after April 1, 1997. The retrenchment in pension rights depended on birth date. Workers born before December 1, 1944, could retire against a replacement rate of 70% when they were 60 years old, while workers born after December 30, 1944, and before April 1, 1947, had to postpone their retirement by two months to reach the same replacement rate.² Moreover, the 1997 reform prescribed that all workers born after March 31, 1947, had to work until the age of 62 years to receive a replacement rate of 70%.

²Early retirement at a replacement rate of 70% was considered to be the social norm in the Netherlands in the 1990s.

2.2.2 Natural experiment: 2006 pension reform

We now turn to the pension reform that provides the basis for the natural experiment exploited in this thesis. 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). Before 2006, employees and employers were allowed to deduct their contribution to sectoral early retirement schemes from their current pre-tax income. In line with its policy of stimulating the labor force participation of older workers, the government announced a second round of pension reforms in the second half of 2005 by enacting the *Wet Vut, Prepensioen en Levensloop* (Wet VPL) which became effective on January 1, 2006, replacing the *Wet Fiscale Behandeling Pensioenen 1999*. The new legislation led to the abolishment of the favorable tax treatment of early retirement schemes in the second pillar of the pension system for all workers born after 1949 and the introduction of the so-called *Life Course Savings Program* (Levensloopregeling). Given the expected demographic changes, it was acknowledged by then that a new reform of the pension system would be necessary to cope with the effects of the aging work force on the sustainability of the pension system and the labor market. In that sense, the change in pension rights was not entirely unexpected; however, the timing of the reform, as well as the implementation of a particular discontinuous assignment rule and the strong differential treatment of workers born around January 1, 1950, came as a surprise to all employees when it was announced on July 5, 2005.³

Anticipation of the change in tax rules generated input to the collective bargaining on the introduction of a new pension scheme for the public sector in the Summer of 2005 (*ABP Flexible Pension Scheme*). The new pension scheme for public sector workers was launched on January 1, 2006, and prescribes that workers born before 1950 remain entitled to the old, more generous pre-pension rights if they have worked continuously in the public sector since April 1, 1997. This means that such workers can retire between ages 55 and 65. Retirement at age 62 years and three months yields, in the case of 40 years of tenure, a pension benefit at a replacement rate of 70% of net average yearly earnings since 2002. However, workers born after 1949, and those before 1950 who did not work continuously in the public sector since April 1, 1997, are subject to the new and less generous system. The new flexible pension system is characterized by (i) a drop in pension benefits, (ii) an increase in pension contribution payments to partly account for the drop in pension wealth

³The abolition of the favorable tax treatment was not limited to the public sector but applied also to workers in the private sector.

2 The Natural Experiment and Research Design

resulting from (i), and (iii) stronger incentives to continue working, generated by larger penalties on pension income when retiring before commencement of the state pension at age 65 and by larger supplements for later retirement. Furthermore, the eligibility age for pension benefits was increased to 60 years, and workers can now decide to continue working until their 70th birthday.

Note that the supplements to the pension benefits for later retirement ensure that workers who already had planned before the shock in the pension system to continue working until they are 65 do not experience a drop in pension wealth. However, before the shock in the pension system occurred, the proportion of workers who planned to retire at the age of 65 was extremely small. Approximately 80% of all workers in the Netherlands who retired under the old pre-pension rules in 2004 chose early retirement at the age of 62 years or younger (Statistics Netherlands, 2009). Moreover, the penalties on pension income before the retirement age of 65, together with the increase in the eligibility age ensures that workers in the treatment group are always constrained in their options to retire early. This applies especially to workers who were born just after the treatment threshold.

For younger workers, the increase in pension contributions will partly compensate 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 favorable tax rules and the steeper early retirement scheme, workers born in 1950 face a substantial decrease in their pension benefits if they wish to retire at age 62 and three months. More specifically, the replacement rate drops to 64%, or they must work an additional 13 months to qualify for a pension at a replacement rate of 70%.⁴

Next to the abolishment of the favorable tax treatment, the government introduced the *Life Course Savings* program in 2006. This program allows tax-free savings 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* program, which can be used to finance about two years of early retirement. Special arrangements were made for older workers who were most affected by the pension reform. Those born in the years 1950 through 1954 are allowed to save more than 12% of their annual earnings, so long as the cumulative maximum does not

⁴A small minority of older workers born after 1950 can still retire early without experiencing a substantial drop in income, namely employees with burdensome jobs (firemen, paramedics, and policemen) who are eligible for special arrangements that allow early retirement against a replacement rate of at least 70% between ages 55 and 61. For this reason, these workers are not included in the data.

exceed 210% of annual earnings. Note that workers of the 1950 cohort have to save approximately 14% of their annual earnings for seven years to finance early retirement at age 62 and three months. It is likely that only a very small fraction of these workers will be able to save such a high proportion of their earnings each year before retirement. Moreover, in that case the pension reform affects the current wealth of the workers affected by the reform.

2.3 Data collection and validity of the research design

The natural experiment thus consists of a clear policy discontinuity, i.e., employees born after 1949 and workers born before 1950 who did not work continuously in the public sector since April 1, 1997 experienced a considerable drop in their pension rights while the pension rights of those who were born before 1950 and did not have any work interruptions since 1997 did not change.

2.3.1 Data collection

We exploit this discontinuity in pension rights in our analyses and use matched survey data and administrative data for male full-time employees in the public sector who were born in 1949 or 1950. The administrative data come from the pension fund for public sector employees in the Netherlands (ABP) and cover all male workers in the public sector who were born in 1949 or 1950. The data contain detailed information on annual wage income, birth dates, the number of years of contribution and the establishment size of the workers in our sample

The panel survey data are available for three years. The analyses in Chapter 3 are based on the first data wave, while Chapters 4 and 5 use data from the second wave. Chapter 6 uses data from the second and the third waves. 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 27,871 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.⁵ In the second stage, those who gave permission (11,458 workers sent their e-mail address) received an e-mail in March 2007 with a link to the survey. Potential respondents were not informed about the experimental nature of the study and no references were made to the shock in

⁵This 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.

the pension system. In the first wave, a total of 8,526 individuals answered the questionnaire, of which 7,739 completed it fully. The second and third waves of the panel were based on surveys held in March 2008 and 2009. The second wave includes 6,078 respondents who completed the questionnaire. The number of respondents in the third wave (7,711 respondents) was slightly higher due to an update of the list of e-mail addresses.⁶

2.3.2 **Validity of the research design**

The main advantage of using a natural experimental approach is that a researcher can use exogenous variations provided by policy changes, randomization, or other events to infer a causal relation between the exogenous explanatory variable and the dependent variable of interest. Natural experiments are especially useful when estimates can be biased because of endogenous variation in the explanatory variables or selection. As already established in Chapter 1, this also applies to the literature on the human capital development of older workers, which has great difficulty assessing the direction of causality between health or training investments and the retirement decision. However, natural experimental approaches can also suffer from fundamental flaws. The general problems that can be encountered when making inferences based on natural experiments are documented by Campbell (1957, 1969), Campbell and Stanley (1966), Cook and Campbell (1979), Meyer (1995), and Harrison and List (2004). These studies divide the threats to validity into two classes: internal and external validity. Internal validity refers to whether the difference in the dependent variable can be safely attributed to differences in the values of the relevant explanatory variables, while external validity refers to the extent to which the findings can be generalized to different groups of individuals and contexts. These studies show that the poor research design of a natural experiment undermines the causal interpretation and can lead to invalid estimates. Consequently, we must determine the extent to which our research design and natural experimental approach result in valid estimations of the effect of decreased pension rights on training behavior, job effort, well-being, and mental health. Therefore, the following discusses how to overcome five potential caveats relevant to our design that are described in the validity list developed by Meyer (1995). The first four issues refer to the internal validity of the experiment, and the final issue discusses its external validity.

⁶Panel attrition is predominantly caused by changes in e-mail addresses. Therefore, in 2009 a new call to participate in the study was sent by mail to those individuals whose e-mail addresses no longer existed. Moreover, all individuals who did not respond to the first call received a new request in 2009 to participate in the study and to give their e-mail address.

1) *Focus on male birth cohorts from 1949 and 1950*

A main characteristic of our research design is that we only focus on men born in 1949 or in 1950. Therefore, we only include those who were born just before or after the treatment threshold. We make this selection because of four reasons, all related to the internal validity of the experiment. First, the limited age difference between the treatment and control group in the sample, in combination with the simple and transparent age criterion that determines entitlement to the old or new pension rights, guarantees that the treatment and control groups are similar and therefore easily can be compared. It is unlikely that workers who are just a few days, weeks, or months older have, on average, different observable and unobservable characteristics. Extending the selection to cohorts born farther away from the treatment threshold could lead to the introduction of new biases, since the similarity between the age cohorts would decrease. Second, widening the age difference between the treatment and control groups would also lead to the inclusion of younger workers for whom the increase in pension contributions partly compensates the decrease in their pension benefits over time. The workers in the present sample have hardly an escape route. As already mentioned, the affected workers must save an extremely high proportion of their earnings for seven years to finance early retirement at age 62, and have no incentives to move to the private sector since the pension reform applies to all workers in the Netherlands. Third, related to the previous reason, it is recognized that the outcomes of natural experimental approaches can still be affected by omitted variables. Events or policy changes other than the experimental treatment can be a relevant factor that provides alternative explanations for the results of a natural experiment. Since the age difference between the treatment and control groups is limited in our dataset, it is less likely that other policy rules or events related to age affect the treatment and control groups in different ways. To our knowledge there have been no other institutional changes that differentially affect the 1950 (1949) cohort as opposed to the 1949 (1950) cohort. Fourth, this limited age difference reduces the effects of possible age trends on our outcomes.

Within the cohort from 1949 and 1950, we restrict our analyses further to full-time workers who have worked continuously in the public sector since April 1, 1997. Remember that those who did not work continuously in the public sector also fall under the new pension rules. Hence, in principle a second treatment exists besides those born in 1950.

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However, we do not use this group in the main analyses, since it is very likely that unobserved characteristics that might have caused the career break since 1997 are also correlated with training participation and mental health status.⁷

Furthermore, we exclude employees in the military and a few other specific occupations such as fire fighting or ambulance nursing because they are entitled to other early retirement arrangements, which also changed since January 1, 2006, and therefore might disturb the treatment effect of introducing the new pension system. This selection, in combination with the restrictions to the 1949-1950 cohort and to those who have worked continuously in the public sector since 1997, guarantees that we end up with a sample of workers for which the pension reform is clear, since the small differences in their age are the only factor determining their membership in the treatment group or control group.

Finally, the exclusion of women further increases the homogeneity of the group of workers in our sample. The focus on male employees is due to the fact that in the Netherlands, in general, male employees aged 56 or 57 years are the main wage earner, while female workers in the same birth cohort often have disrupted careers. In this age range, on average, men have contributed 28 years to the pension fund, whereas women have contributed on average for only 16 years. Moreover, only a small, select group of these women is still working. In the public sector, only 30% of all employees born between 1940 and 1950 are women, and 72% of these work in a flexible or part-time job (Statistics Netherlands, 2005). This implies that the working women in this birth cohort who are eligible to the old pension rules belong to a highly selective group.

2) *Mismeasurement*

Mismeasurement can easily arise when surveys do not ask the respondents to answer the questions truthfully, leading to possibly highly inflated answers (see Harrison and List (2004) for a discussion on this topic). In addition, the values of the answers can be affected by the respondents' awareness of the survey's experimental objectives. To avoid these caveats, the invitation letter sent to the respondents bore great similarities with the cover letter advocated by Dillman (1978). The letter conveys general information about the social usefulness of the study, without revealing the survey's experimental character. To put some pressure on the recipients, the letter tries to convince individuals that the survey is important for policy decisions and that their contribution is extremely valuable to the research project. Moreover, the

⁷This results in the exclusion of a small group of 260 individuals. In the next chapters we will, however, perform additional robustness analyses that include those with disrupted careers.

letter explicitly mentions that confidentiality is ensured, so that respondents need not fear repercussions from responding in a socially undesired manner.

Furthermore, references to the natural experimental character of the research project were avoided in the survey itself and explicit questions that could give away the survey's experimental character were not included.⁸

3) Self-selection and similarities between the treatment and control groups

Another relevant issue is whether our outcomes are affected by self-selection. Non-respondents may have different characteristics than those who filled in the questionnaire, and therefore our results may not be perfectly generalizable to the entire male population of public sector workers born in 1949 or 1950. In this respect, the natural experimental approach used here does not differ from the approach of other studies that use non-experimental survey data. However, self-selection could impose a much larger problem when the non-response differs between the treatment and control group. In that situation, the similarity of the two groups is no longer guaranteed, and the natural experiment loses its validity. It is essential for the employees in the treatment and control group to have, on average, the same characteristics except for the deviation in pension rights. Therefore, this section will delve further into the similarity of the treated and untreated respondents and whether they form a representative sample of the total male population of public sector workers in the Netherlands born in 1949 or 1950.

For each survey wave, we checked whether there are deviations in the survey participation rate between the treatment and the control group. For each year, the differences in the participation rates are extremely small. In 2007, 30.5% of all the workers in the treatment group participated in the survey versus 31.0% among the control group. In 2008, the survey participation rates were 21.6% for the treatment group and 22.2% for the control group, while in 2009 they were 27.7% and 27.9%, respectively. Simple t-tests show that these small differences in the participation rates are statistically insignificant (t-stat = 0.97 in 2007, 1.20 in 2008, and 0.32 in 2009).

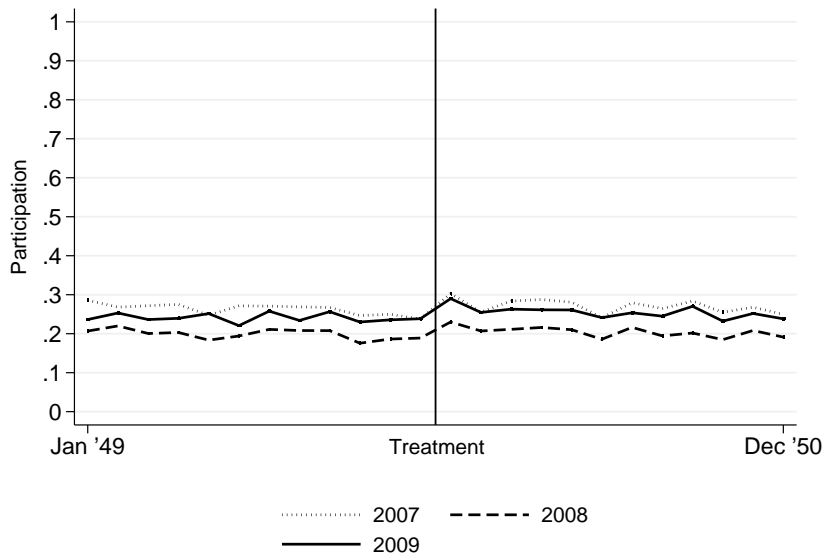
Figure 2.1 further shows that the survey participation rates in the three panel waves do not differ across different birth months, while a simple probit analysis also confirms that selection into the survey in each wave was not related to the treatment

⁸Another kind of mismeasurement arises when the definitions or survey questions change over time. To maintain the panel character of the dataset and to avoid mismeasurement, we did not change the survey questions relevant to our analyses over time. Moreover, this problem does not apply to the analyses in Chapters 3-5 which are based on observations in one particular year. Only the analyses in Chapter 6 are based on survey data collected in two different years.

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(year of birth). These probit analyses included several control variables available in the administrative data, such as the work sector, contractual work hours, birth month and yearly wage (in logs). For the 2007 wave, we observe that the coefficient of the treatment dummy is small and statistically insignificant from zero. (coefficient is -0.035 with a standard error of 0.058). The results for 2008 and 2009 show that the coefficient of the treatment dummy remains small and insignificant (coefficient is 0.009 in 2008 and 0.021 in 2009).⁹

Figure 2.1: Participation into the survey



Although the absolute survey participation rates give no indication of self-selection that could damage the experimental nature of our research, it is still possible that the treated and the untreated respondents differ in their observable and unobservable characteristics. Therefore, Table 2.1. presents some descriptive statistics for the treatment and control group. Table 2.1 shows that most differences between the groups of workers born in 1949 and those born in 1950 are indeed extremely small. Both job and personal characteristics are similar across the two groups, and, with a few exceptions, not significantly different from each other. Concerning the job characteristics, we observe slight but statistically significant differences in the yearly wage and number of years contributed to the pension fund. However, these differences are not larger than what could be expected from the small age difference between the control and the treatment group. Furthermore, the table shows

⁹Moreover, the probit analyses show that the peak in the participation rate in January 2009 is not statistically significant from zero.

that 65% of all the public sector workers of both birth cohorts have mentally demanding work, versus only 8% who claim to have a physically demanding job.¹⁰ Approximately 67% of the respondents of both cohorts have a high level of education, whereas more than 91% are married.¹¹ In addition, the respondents of both birth cohorts visit their doctor two times a year, on average. The fraction of married individuals in the sample is slightly higher among the group that is not affected by the reform, and this difference is significant at the 10% level. This higher fraction is consistent with the continuously declining fraction of married men with year of birth. Municipal register data covering the entire Dutch population confirm that the fraction of married men is slightly higher for the 1949 cohort compared to the 1950 cohort (Statistics Netherlands, 2008). Therefore, it is relevant to control for marital status in our multivariate analyses. Nevertheless, the differences in the observables seem to be sufficiently small to ensure the validity of the natural experiment.

4) Awareness of the policy change

The final question concerning the internal validity of the natural experiment is whether workers born in 1950 are really aware of the consequences of the new pension system on their individual situations. The strong differential treatment of workers born around January 1, 1950, came as a surprise to public sector employees. To make the introduction of the new pension system known 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 organizations and ABP jointly explained the new flexible pension scheme. All 1.2 million ABP participants received a letter about the core characteristics of the new scheme, and a complete electronic service package for public service employers was developed. Therefore, one can assume that on January 1, 2006, most public sector employees born after 1949 and their employers were indeed familiar with the exogenous shock in their pension rights.

¹⁰The questions on physically versus mentally demanding work are based on two survey questions that asked how well the respondent identified with the statements: *I have physically (mentally) demanding work*. Answers ranged from 1 (very good) to 5 (bad). For this table, the answers are dichotomized (1 corresponds to a score of 1 or 2, and 0 corresponds to a score of 3 or higher).

¹¹The public sector employs many highly educated workers. The fraction in our sample is consistent with the Dutch *OSA Labor Supply Panel*, a representative panel survey of the working population in the Netherlands.

Table 2.1: Means of variables for respondents affected and not affected by the policy change

Variables	2007			2008			2009		
	Born in 1950	Born in 1949	T-stat	Born in 1950	Born in 1949	T-stat	Born in 1950	Born in 1949	T-stat
	I	II	III	IV	V	VI	VII	VIII	IX
Job characteristics									
Yearly wage (in logs)	10.78	10.79	1.94	10.83	10.84	0.94	10.83	10.84	1.91
Years contributed to pension	30.29	31.80	9.92	31.45	32.86	8.26	32.41	33.64	7.59
Organization size (in logs)	7.07	7.10	0.70	7.09	7.11	0.41	7.13	7.10	0.71
Physically demanding job	0.08	0.07	0.95	0.09	0.09	0.01	0.08	0.08	0.38
Mentally demanding job	0.65	0.64	0.73	0.65	0.63	1.75	0.66	0.66	0.65
Personal characteristics									
Marital status	0.91	0.92	2.33	0.91	0.92	1.73	0.90	0.92	2.96
Lower secondary education	0.14	0.13	1.09	0.12	0.13	1.62	0.12	0.12	0.04
Higher secondary education	0.05	0.04	0.52	0.04	0.04	0.18	0.04	0.04	1.77
Intermediate vocational education	0.15	0.15	0.06	0.15	0.15	0.58	0.16	0.16	0.41
Higher education	0.67	0.68	1.06	0.69	0.68	0.77	0.67	0.68	1.11
Number of doctor visits in past year	NA	NA	NA	2.13	2.12	0.02	2.20	2.28	1.23
Number of observations	3,686	3,326		2,724	2,471		3,529	3,083	

Table 2.2: Retirement expectations

	1950	1949
Expected retirement benefit at age 62		
2007	66.18	72.02
2008	67.00	72.10
2009	63.38	68.40
Expected retirement age		
2007	63.50	62.70
2008	62.11	61.88
2009	63.45	63.09

Of course this assumption must be verified empirically. Since our data contain information on workers' expectations about the replacement rate at age 62, we can check whether those born after 1949 indeed expect to receive a lower pension benefit if they were to retire early.

Three open survey questions on retirement expectations deal with this issue:

- 1) *At what age do you expect to retire?*
- 2) *At what age did you expect to retire 5 years ago?*
- 3) *Suppose, you were to retire at the age of 62. How large would your pension benefit be as a percentage of your net wage income?*

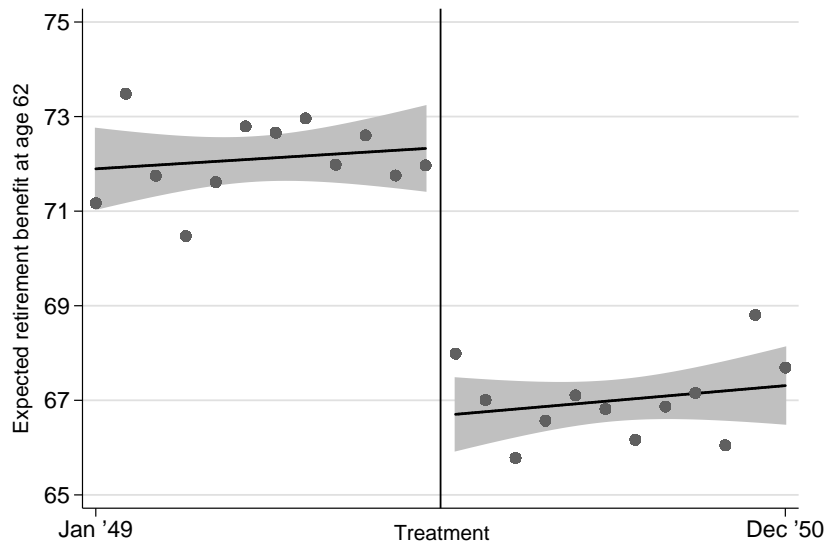
Table 2.2 shows that the actual decrease in pension rights due to the pension system reform has a strong effect on the expected replacement rate treated workers expect to receive if they retire when they reach the age of 62. In 2007, the average expected replacement rate at the age of 62 years is 72.0% for the control group, and only 66.2% for the treatment group. The fact that both replacement rates are only 2%-points above the actual benefit they will receive when they do not increase their pension savings, whereas the mean difference in the expected retirement benefit between the treatment and control group is 5%-points, indicates that workers have fairly accurate retirement expectations. We also observe that the expected replacement rate for both groups dropped considerably in 2009 as a consequence of the financial crisis which caused a dramatic fall in the funds of the public sector's pension fund. The drop in funds induced the pension fund to (temporarily) stop the use of indexation, that is, adjusting pensions to the wage trend or inflation rate. Nevertheless, the size of the difference in the expected retirement benefit between the treatment and control groups remains stable over the years.

Table 2.2 further indicates that the expected retirement ages of the treatment and control groups partially converges after 2007. Employees born in 1949 expected in 2007, on average, to retire at the age of 62 years and six months, whereas employees born in 1950 anticipate retiring at 63 years and four months. This means that treated

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workers probably expect to delay their retirement by eight months. However, in 2008 and 2009 employees born in 1950 expected to retire only four months later than those born in 1949. Table 2.3 reports the results of further analyses on the expected retirement age in 2008. Table 2.3 shows that after two years the reform still has a significant effect on the expected age of retirement, but this effect is significant only at the 10% level when the full set of controls is added to the specification. The coefficient of 0.274 amounts again to about four months' postponement of retirement. We also perform a regression on the probability of late retirement (i.e., later than age 63). This regression indicates that the probability of late retirement is increased by 16 %-points and that this effect is significant at the 1% level.

Figure 2.2: Expected retirement benefit at age 62



The data points presents the mean expected pension benefit at age 62 as a percentage of present wage income for each birth month. Furthermore, fitted lines and confidence intervals are calculated based on individual information. The information is based on the following survey question asked in 2008: *Suppose you were to retire at the age of 62. How large would your pension benefit be (as a percentage of your net wage income)?* The vertical line marks the threshold dividing the control and treatment groups.

Figures 2.2 and 2.3 show expected replacement rates and expected retirement age, respectively, in 2008 for workers born in 1949 and 1950. The dots represent the expected average replacement rates or the expected retirement age for individuals born in a specific month. Both figures show a clear break in expectations around the threshold date (December 31, 1949).¹² It seems reasonable to conclude from these figures that employees are indeed familiar with the consequences of the new pension system with respect to their individual situations.

¹²Figures for the other survey years show the same pattern.

2.3 Data collection and validity of the research design

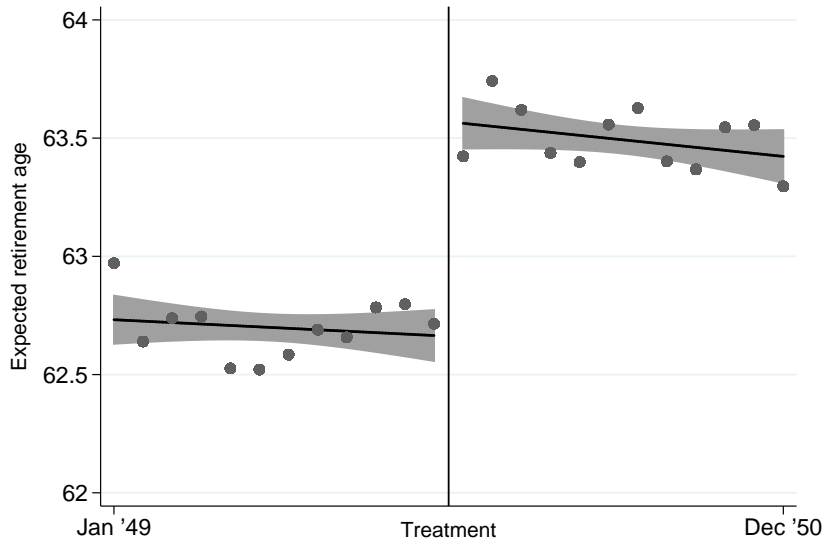
Table 2.3: Do people change their retirement age?

Variables	Expected retirement age		Late retirement (retirement > 63 years)		Expected retirement age 5 years ago	
	I	II	III	IV	V	VI
Treatment dummy	0.221*** (0.071)	0.274* (0.147)	0.171*** (0.012)	0.154*** (0.025)	-0.080 (0.064)	-0.139 (0.130)
Age (in days /100)		0.040 (0.035)		0.001 (0.006)		0.004 (0.031)
Marital status		-0.220 (0.141)		-0.021 (0.024)		-0.445*** (0.124)
Lower secondary education		-0.124 (0.146)		-0.040 (0.025)		-0.259** (0.128)
Higher secondary education		0.081 (0.207)		-0.006 (0.036)		0.106 (0.179)
Higher education		0.030 (0.126)		-0.030 (0.022)		0.236** (0.114)
Government		-0.344 (0.215)		-0.041 (0.037)		-0.483** (0.193)
Education sector		-0.362 (0.221)		-0.027 (0.038)		-0.052 (0.198)
Energy, public transportation		-0.678*** (0.234)		-0.106*** (0.040)		-0.729*** (0.222)
Physically demanding work		0.025 (0.124)		-0.004 (0.021)		-0.052 (0.113)
Mentally demanding work		-0.134 (0.082)		-0.047*** (0.014)		-0.278*** (0.073)
Fraction of non-routine tasks		0.248 (0.184)		0.094*** (0.032)		0.581*** (0.163)
Yearly wage (in logs /100)		-25.755 (16.888)		-0.356 (2.913)		41.410*** (15.501)
Years contributed to pension (/ 100)		-3.888*** (0.634)		-0.891*** (0.109)		-6.821*** (0.559)
Pension rights at other pension funds		-0.168 (0.124)		-0.006 (0.021)		0.015 (0.105)
Partner has pension or income		0.119 (0.089)		-0.002 (0.015)		0.092 (0.075)
Net housing wealth		-0.159* (0.093)		-0.029* (0.016)		-0.083 (0.078)
Inheritance		0.023 (0.113)		0.008 (0.020)		0.099 (0.095)
Missing info on other pension		-0.111 (0.175)		-0.060** (0.030)		-0.091 (0.148)
Partner info missing		0.035 (0.189)		-0.023 (0.033)		0.152 (0.166)
Net housing wealth missing		0.018 (0.181)		0.016 (0.031)		-0.048 (0.158)
Info on inheritance missing		-0.017 (0.149)		0.009 (0.026)		-0.148 (0.126)
Self-reported general health		0.013 (0.064)		-0.027** (0.011)		-0.188*** (0.056)
Self-reported work limitations		-0.309** (0.121)		-0.069*** (0.021)		-0.297*** (0.107)
Number of doctor visits in past year		-0.002 (0.017)		0.003 (0.003)		0.003 (0.015)
Sick for > 14 days in past year		-0.035 (0.115)		-0.003 (0.020)		0.004 (0.103)
Observations	5,319	4,752	5,319	4,752	4,853	4,323
R-squared	0.002	0.019	0.004	0.066	0.000	0.092

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. The dummy variable for intermediate vocational education and a dummy measuring whether one works in a sector other than the government, education, energy and public transportation, are used as reference categories.

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Figure 2.3: Expected retirement age



The data points present the mean of the expected retirement age for each birth month from January 1949 to December 1950. Furthermore, fitted lines and confidence intervals are calculated based on individual information. Our sample consists of two birth year cohorts, where employees born in 1949 are entitled to the old pension rules and those born in 1950 are subject to the new pension rules. The vertical line marks the threshold dividing the control group from the treatment group.

Since the difference in the expected retirement age decreased while the difference in the expected replacement rates did not converge, one should raise the question of whether the reform affected the savings and/or retirement behaviors of those affected, since workers with retrenched pension rights could have saved more to maintain their previously planned retirement date. Delavande and Rohwedder (2008) explored this issue with respect to the United States. The authors used an Internet survey to ask the respondents of the Health and Retirement Survey (HRS) what they would do if their social security benefits were cut by 30%. The authors found that, on average, individuals would postpone their retirement by 1.13 years. About two-thirds of the respondents indicated that they would reduce their spending to compensate for the drop in pension wealth.

The 2008 survey includes two savings questions that clarify such behavioral responses:

- 1) *Did you make additional savings arrangements for your pension in the past year?*
- 2) *Do you participate in the Life Course Savings program?*

In Table 2.4, we report the results from analyses of the savings questions. Columns I and II report the results from the first question and Columns III and IV report

2.3 Data collection and validity of the research design

the results from the second question. The first column of Table 2.4 shows a strong effect from the reform on additional pension savings if no controls are included. The coefficient is positive, indicating that those who are affected by the reform have been engaged in additional savings in the past year to compensate for the loss in pension wealth. However, the effect becomes insignificant when the full set of regressors is included. Columns III and IV also show strong effects from the pension reform on participation in the *Life Course Savings* program. The control group (1949) has a participation rate of around 6%, whereas the participation rate of the affected cohort is about 9%-points higher. Adding controls reduces the magnitude of the reform effect, but the effect is nevertheless substantial and strongly significant. Unfortunately, we cannot observe how much participants save in the *Life Course Savings* program. Workers in the 1950 cohort would have to save 14% of their annual yearly earnings for about seven years to finance early retirement at the age of 62. It is likely that only a small fraction of participants are able and willing to save such a high share of their earnings each year prior to retirement. Hence, participation in the *Life Course Savings* program will most likely only partially compensate the loss in pension wealth. Moreover, such additional savings reduce the current wealth of the treated workers.

5) External validity

Concerning the experiment's external validity, the major drawback of the research design is that the effects of the treatment on workers born after 1950 are not analyzed. It is most likely that the results found in this thesis are not perfectly generalizable to younger cohorts. First, for younger workers, the increase in pension contributions will partly compensate for the decrease in pension benefits over time, and they will have more time to make private arrangements to decrease the gap in their pension rights. Second, younger cohorts, particularly those who recently left school and still have to build up their career in the public sector, are likely to 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 limitations in the generalizability of the results to younger cohorts, the results found in this thesis are highly relevant. The 1949-1950 birth cohort belongs 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. Furthermore, the focus on these two specific birth cohorts enables us to examine in detail the behavioral responses in terms of negative reciprocal behavior and social comparisons of those who just barely fall under the new pension system.

2 The Natural Experiment and Research Design

Table 2.4: Do people supplement their retirement income with extra pension savings?

Variables	Extra pension savings		Life Course Savings	
	I	II	III	IV
Treatment dummy	0.044*** (0.012)	0.011 (0.024)	0.094*** (0.010)	0.052*** (0.019)
Age (in days /100)		-0.006 (0.006)		-0.005 (0.005)
Marital status		0.027 (0.023)		-0.008 (0.018)
Lower secondary education		-0.031 (0.024)		-0.012 (0.020)
Higher secondary education		0.025 (0.034)		-0.020 (0.027)
Higher education		0.035* (0.021)		0.008 (0.017)
Government		-0.015 (0.036)		-0.019 (0.029)
Education sector		0.014 (0.037)		-0.011 (0.029)
Energy, public transportation		-0.013 (0.039)		0.028 (0.033)
Physically demanding work		0.034* (0.021)		0.016 (0.017)
Mentally demanding work		0.026* (0.014)		0.006 (0.011)
Fraction of non-routine tasks		0.020 (0.030)		0.014 (0.024)
Yearly wage (in logs /100)		6.410** (2.795)		6.687*** (2.274)
Years contributed to pension (/100)		-0.563*** (0.103)		-0.098 (0.085)
Pension rights at other pension funds		0.035* (0.020)		0.020 (0.016)
Partner has pension or income		-0.008 (0.015)		0.011 (0.010)
Net housing wealth		0.020 (0.015)		0.020* (0.011)
Inheritance		0.045** (0.019)		0.050*** (0.013)
Missing info on other pension		0.090*** (0.029)		0.361*** (0.040)
Partner info missing		-0.040 (0.031)		-0.005 (0.050)
Net housing wealth missing		0.001 (0.030)		0.233*** (0.046)
Info on inheritance missing		0.070*** (0.025)		0.408*** (0.035)
Self-reported general health		-0.003 (0.010)		0.003 (0.008)
Self-reported work limitations		-0.002 (0.020)		0.025 (0.016)
Number of doctor visits in past year		0.002 (0.003)		-0.002 (0.002)
Sick for > 14 days in past year		0.018 (0.019)		-0.005 (0.015)
Observations	5,360	4,870	3,899	3,489
R-squared	0.003	0.035	0.022	0.197

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. The dummy variable for intermediate vocational education and a dummy measuring whether one works in a sector other than the government, education, energy and public transportation, are used as reference categories.

2.3 Data collection and validity of the research design

The results, however, can still be relevant for younger cohorts, since a new pension reform has been announced recently. The government has submitted a bill to increase the eligibility age for the state's old age pension (pillar 1) from 65 to 67 years. Most interestingly, the increase in the eligibility age is accompanied by a similar transitional arrangement that entails a discontinuity in pension rights across different younger birth cohorts. The eligibility age for the state's old age pension increases to 67 years for those born in 1960 or later, and to 66 years for those born between January 1, 1955 and January 1, 1960, while those born before January 1, 1955 will start receiving this benefit at age 65. The resignation of the government in Spring 2010 means that the precise details of the reform remain uncertain at present. However, in anticipation of the forthcoming policy change, employer organizations and unions in the Netherlands recently signed an agreement that entails a similar discontinuity in pension rights around January 1, 1955.

A second threat to the generalizability of the results follows from the exclusion of women in this research. However, as mentioned before, women born in 1949 or in 1950 and who are still working, are part of a rather small and selective group that predominantly work in a flexible or part-time job. Moreover, it is doubtful whether the results of employed women born in 1949 or in 1950 could be easily generalized to younger female birth cohorts, since the labor force participation rate of women among younger birth cohorts is much higher.

A third threat to the external validity of the natural experiment is that this thesis focuses entirely on the public sector, for whom the pension change was more profound. Although caution is necessary when generalizing the results, these results could also apply to large organizations in the private sector for two reasons: 1) There has been a substantial convergence in the conditions of public and private sector workers in the Netherlands in the last two decades (CAOP, 2006). Since 1993, in the public sector, negotiations between unions and employer organizations on labor conditions have been decentralized into eight different sectors, each with its own collective agreements. 2) Labor market conditions have increasingly induced individual organizations within the public sector to apply more flexible wage schemes and performance-related payments to increase individual productivity (Ministry of the Interior and Kingdom Relations, 2008).

Furthermore, it should also be noted that, in most European countries, the public sector is the largest employer (Pilichowski and Turkisch, 2008) and that in these countries reforms of the retirement schemes of public sector workers are planned to stimulate the labor market participation of older workers (see Palacios and White-

house (2006) for an overview of recent reforms to public sector pension plans in European countries).

2.4 Conclusion

In this chapter we discussed the reform of the pension system for public sector workers in the Netherlands that became effective in 2006 and which provides the basis for our natural experiment. The pension reform involves a simple and clear discontinuity rule that prescribes that those born before 1950 with 40 years of tenure can still retire at age 62 and three months with a replacement rate of 70%, while those born after 1949 will receive a replacement rate of only 64% when they retire at this age. Furthermore, in 2006, the Dutch government introduced the *Life Course Savings* program which allows tax-free savings 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 program, which can be used to finance about two years of early retirement. However, it is unlikely that workers of the 1950 cohort will be capable of saving enough to compensate for the loss of their pension wealth.

Subsequently, we described the dataset we use for the analyses in the next chapters. It consists of matched survey data and administrative data for male full-time employees in the public sector who were born in 1949 or 1950. The panel survey data are available for three years and were gathered in two stages.

This section then examined the internal and external validity of the research design. The focus on the 1949-1950 cohort combined with carefully avoiding mentioning the experimental nature of the research project in the communication process guarantees internal validity (since it increases the similarity of the treatment and control groups). Descriptive analyses show that there is no evidence of any differences in self-selection between the treatment and control groups. The differences in observables between the treatment and control groups are indeed sufficiently small for the research purposes of this thesis. Moreover, both groups have reasonably accurate retirement expectations.

The focus on the 1949-1950 cohort of public sector workers is, however, at a loss in terms of generalizing the results of the following chapters to younger age cohorts. Nevertheless, it is argued that the results found in this thesis are highly relevant because these specific cohorts belong to the baby boom generation and consequently impose the highest costs on the pension system. Moreover, the results can also still

be relevant for younger cohorts, since a new pension reform has been announced that will affect them in the same way as the 1949-1950 cohort was affected by the 2006 reform. Furthermore, our results are also highly relevant because most European countries have proposed reforms of the retirement schemes of public sector workers.

Appendix

Figures 2.4 and 2.5 show histograms of the expected retirement ages in 2008 of the workers affected by the pension reform and the control group, respectively (*At what age do you expect to retire?*), whereas Figures 2.6 and 2.7 show workers' expectations five years ago (*At what age did you expect to retire five years ago?*). From Figures 2.6 and 2.7, one can conclude that the distribution of expected retirement ages five years ago was almost identical for the 1949 and 1950 cohorts. However, this is very different for Figures 2.4 (those affected by the reform) and 2.5 (those not affected by the reform). Compared to the 1949 cohort, a much larger share of those treated expects to retire at age 65. On the other hand, quite surprisingly, fairly equal fractions of both the treated and the control groups expect to retire at the age of 57 or 58. The treated workers may harbor no such expectation, because in the new pension system they cannot receive pension benefits prior to the age of 60. One might wonder whether these workers do not fully understand all the details of the new system. However, Figure 2.2 clearly shows that the cohort affected by the reform on average understands the implications for their replacement rate should they wish to retire at age 62. Therefore, these workers will probably leave the labor force via alternative exit routes, such as disability insurance, *Life Course Savings* or specific retirement arrangements.

Figure 2.4: Expected retirement age: treatment group

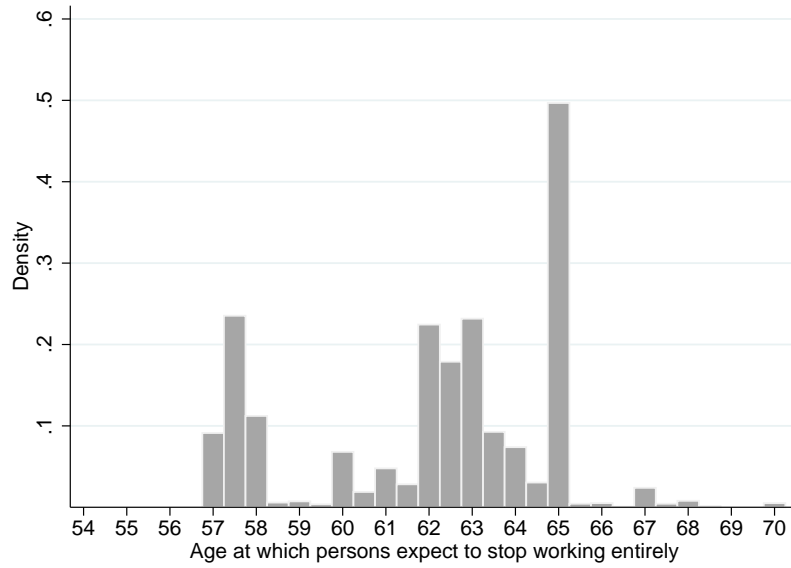


Figure 2.5: Expected retirement age: control group

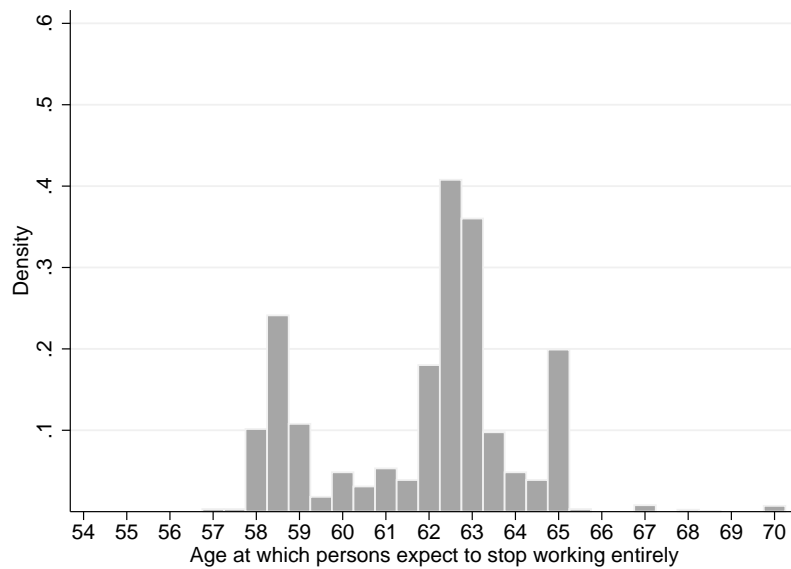
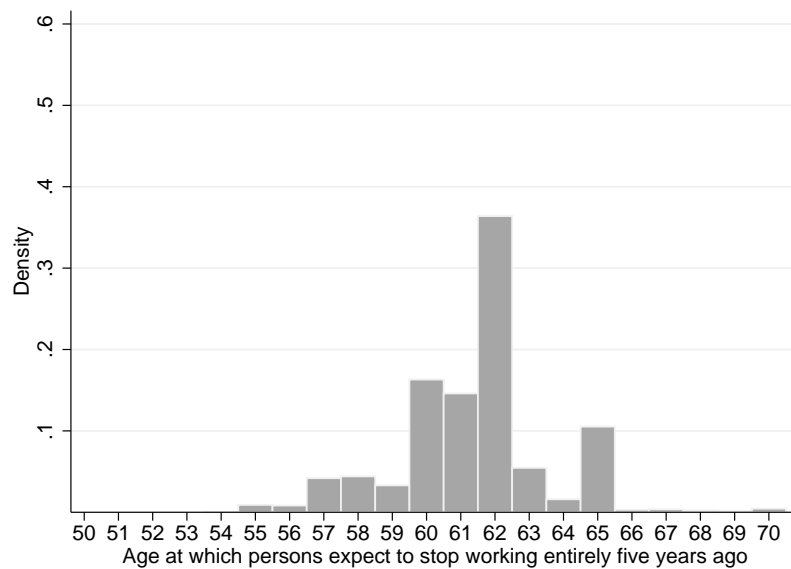
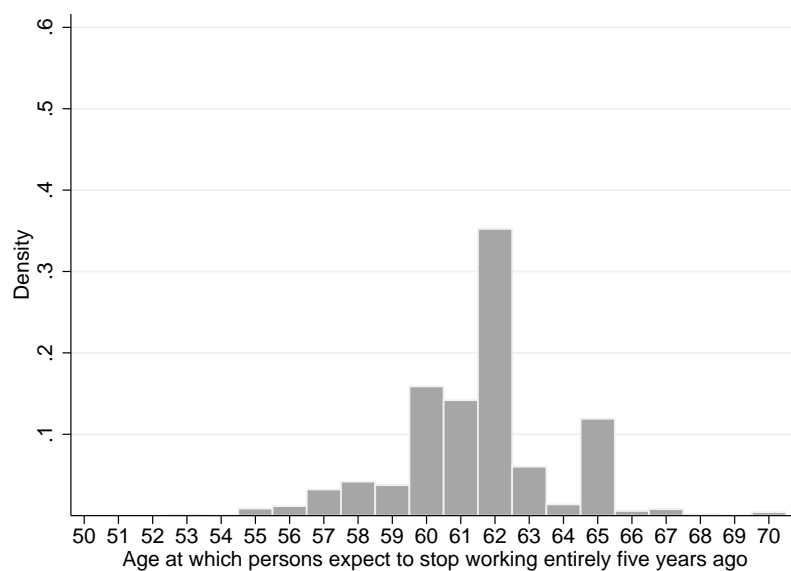


Figure 2.6: Expected retirement age five years ago: treatment group**Figure 2.7:** Expected retirement age five years ago: control group

3 Training Participation

3.1 Introduction

This chapter focuses on the impact of the abolishment of generous early retirement systems on training participation.¹ Human capital theory predicts that early retirement systems have a negative effect on human capital formation. It is argued that the present value of net returns to human capital investments in older workers is lower due to the shorter period during which both employees and employers can reap the benefits of these investments (see Becker, 1975; Ben-Porath, 1967).² Lau and Poutvaara (2006) integrated human capital investments and retirement decisions in a life cycle model and showed that generous social security systems induce early retirement and therefore have an indirect negative effect on human capital investments.

This chapter exploits the 2007 wave of the panel dataset described in Chapter 2 to examine the impact of an exogenous increase in the expected retirement age on training participation of public sector workers due to the exogenous pension reform in 2006. This major pension reform treated two very similar groups of employees differently. The reform consisted of the abolishment of pre-pension plans for everyone born after December 31, 1949. If workers were born before 1950 and have been working in the public sector continuously since April 1, 1997, they remained entitled to the older, more generous pre-pension rights. This means they can retire at the age of 62 years and three months with a pension at a replacement rate of 70%. Workers born after 1949, however, are subject to the new, less generous pension system, which substantially lowers their pension benefits and creates incentives to postpone retirement. We expect that the increase in the expected retirement age induced training participation of workers in the treatment group.³ We also expect that the expected postponement of retirement has different effects across organizations. Baron et al. (1987), Booth (1993), and Bassanini et al. (2007) showed that training propensity is much greater among larger organizations, since offering training courses to employees incurs fixed costs that are generally paid by the employer. Since large organizations are more likely to employ a larger number of treated workers, they benefit from

¹This chapter is a slightly revised version of Montizaan, Cörvers and De Grip (2010a).

²For employers, the benefits of investing in training of older workers take the form of retained or increased productivity. For older workers, training increases job satisfaction and can result in positive wage returns (Shields and Ward, 2001). An extensive literature estimates the wage returns of training investments. The general finding is that private returns are high, although studies that account for potential endogeneity, found much lower returns (Leuven and Oosterbeek, 2004; Leuven and Oosterbeek, 2008).

³Since the public sector's pension fund calculates the pension benefits on career-average wages rather than on the salary earned before retirement, the drop in the replacement rate hardly influences the pension wealth associated with the value of new human capital investments when workers in the treatment group decide at the end of their career not to postpone retirement.

having economies of scale and are therefore better capable to meet the fixed costs of the training provided. Moreover, it is generally found that larger organizations have more formally developed and sophisticated HRM policies than small organizations, which may increase their ability to adapt their personnel policies to labor market shocks (Koch and McGrath, 1996).

The data we use in this chapter incorporate detailed information on older workers' individual pension rights, expected sources of income after retirement, and training participation. We compare the training participation of workers born just before the treatment threshold (i.e., born in 1949) and who remain entitled to their old, generous pension rights, with that of those employees born just after the break (i.e., born in 1950) and therefore subject to the new pension system. We run separate analyses for individuals employed in small and large organizations.

We provide robust evidence that the postponement of retirement due to the sudden change in pension rights has a positive impact on the training participation of older workers, although exclusively in larger organizations. This indicates that larger organizations are better capable to customize their training policies to the new situation than smaller organizations. Furthermore, workers in the treatment group participate more often in long training courses. Finally, the treatment effect appears to be greater among less-educated workers.

This chapter builds on previous studies that empirically analyzed the relation between early retirement and training investments. Fouarge and Schils (2009) and Bassanini et al. (2007) investigated the relation between the generosity and flexibility of pension systems and the training participation of older workers across different European countries. Both studies found that the training participation of older workers is lower in countries with more generous early pension systems. Since there exists a negative relation between pension plan generosity and early retirement age, both studies indirectly provide evidence of a causal relation. However, neither study rule out reverse causality, that is, the effect of training participation on the retirement age. For example, when older workers participate in a training course, they and their employer might consider postponing retirement in order to further reap the benefits of their investment.

Other empirical studies have focused on related questions. Most of these analyzed the relation between age and training participation and found that training incidence decreases with age (e.g., De Grip en Van Loo, 2002; Bassanini et al., 2007). Several studies also analyzed the effects of shocks to life expectancy on human capital investments (Kalemli-Ozcan et al., 2000; Jayachandran and Lleras-Muney, 2009). These studies found that a drop in mortality rates, which increases the potential pe-

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riod during which investment benefits can be reaped, induces investments in human capital.

We contribute to the existing literature in two ways. First, the limited age difference between the treatment and control groups in our sample and the simple and transparent age criterion guaranties the internal validity of the experiment and substantiates the causal direction. Our findings are therefore less likely to be confounded than the earlier studies by Fouarge and Schils (2009) and Bassanini et al. (2007). Second, this chapter explicitly analyzes the effects of early retirement incentives and how changing retirement expectations can affect training participation, whereas the extant literature on the relation between (expected) life expectancy and human capital development provides only indirect evidence of this relation.

This chapter proceeds as follows: Section 3.2 presents a brief description of the data and our main dependent variables. Section 3.3 introduces our estimation strategy. Section 3.4 presents the results of our analyses and Section 3.5 concludes.

3.2 Data

For the analyses in this chapter, we exploit the 2007 wave of our panel data set. As described in Chapter 2, the data of this wave were collected in two stages one year after the introduction of the new pension system. The survey asked detailed questions on the expected retirement age, training participation and expected sources of income after retirement other than the pension benefits provided by the public sector's pension fund. In total, 8,526 individuals started answering the questionnaire, 7,739 of whom completed it. The survey data are matched to administrative data of the pension fund that provides information on individual pension rights built up at ABP, annual wages, tenure in the public sector, size of the organization where employed, and the number of working hours. See Chapter 2 for more details on the data.

The dependent variables in the analyses on training participation are based on three survey questions, two that measure the incidence of training and one to measure the number of hours spent on training courses in 2006;

Table 3.1: Descriptives

Variables	Mean I	All St. Dev II	Born in 1950 III	Born in 1949 IV	T-stat V
Training					
Training participation in 2006 (1 if trained)	0.56	0.50	0.57	0.54	2.69
Training participation in 2005 (1 if trained)	0.50	0.50	0.50	0.50	0.10
Number of hours trained (if trained)	46.79	36.84	47.27	46.21	2.41
Pension and worker characteristics					
Expected retirement age	63.11	1.75	63.50	62.70	19.26
Expected retirement benefit at age 62	68.94	12.36	66.18	72.02	20.69
Good overview of pension rights	0.59	0.49	0.56	0.64	6.90
Years contributed to pension	31.00	6.40	30.29	31.80	9.92
Extra savings for pension since 2006	0.23	0.42	0.26	0.21	4.50
Yearly wage (in euros)	48,620	18,379	48,419	48,835	1.94
Organization size (in logs)	7.09	1.78	7.07	7.10	0.70
Marital status	0.91	0.29	0.91	0.92	2.33
Lower secondary education	0.13	0.34	0.14	0.13	1.09
Higher secondary education	0.04	0.21	0.05	0.04	0.52
Intermediate vocational education	0.67	0.36	0.15	0.15	0.06
Higher education	0.67	0.47	0.67	0.68	1.06
Income partner (1 if partner has own income)	0.74	0.44	0.75	0.73	1.84
Number of observations	7,012		3,686	3,326	

The number of pension years and the yearly wage income are extracted from administrative data of Dutch public sector's pension fund (ABP). The number of pension years indicates how many years individuals build up pension rights with the ABP.

- 1) *Did you undergo training or take a course in 2006 that was relevant to your job?*
- 2) *Did you undergo training or take a course in 2005 that was relevant to your job?*
- 3) *How many hours did you spend in total on training courses in 2006?*

Table 3.1 shows that the training incidence in 2006 is about 3%-point higher in the treatment group, although they train on average only one hour more than those in the control group. We also note that there was no difference in training participation in 2005 (before the treatment) between the treatment and control group and that training participation increased for both groups in 2006. However, the treatment group's training incidence increased with 7%-points compared to only 4%-points for the control group. This indicates that the exogenous shock to pension rights affected human capital investments. It is worth noting that employers in the public sector strongly drive these human capital investments. In our sample, 97% of all training courses were paid for by the employer. Moreover, in 60% of all cases, the employer, or the employer and the employee together took the initiative for to participate in training. The high incidence of training found for the treatment group as well as the control group corresponds closely to the level of training participation for the public

3 Training Participation

sector in the *OSA Labour Supply Panel*, a representative panel survey of the Dutch working population.⁴

Furthermore, Table 3.1 provides an exploratory analysis of the changes in retirement expectations. The table shows that the decrease in pension rights due to the pension system reform has indeed increased the expected retirement age in 2007. In 2007, workers who were born in 1949 expected, on average, to retire at the age of 62 years and six months whereas workers born in 1950 expected to retire when they are 63 years and four months old. For both groups, these mean expected retirement ages corresponds closely to a replacement rate of 70%.

3.3 Empirical strategy

We make use of a natural experimental approach to identify the causal effect of the pension reform on human capital investments. In our application, assignment of pension rights depends deterministically on the birth date (b) of public worker i . The assignment rule is discontinuous and has the form

$$D_i = \begin{cases} 1 & \text{if } b_i \geq \bar{b}: = 1 \text{ January } 1950, \\ 0 & \text{otherwise.} \end{cases} \quad (3.1)$$

where D is the assignment indicator. The probability of receiving training can be stated as

$$E(T_i) = F(\alpha + \beta D_i), \quad (3.2)$$

where $\alpha = E(T_{0i})$ is the training probability without an exogenous change in pension rights and $\beta = E(T_{1i}) - E(T_{0i})$ is the difference in training probability caused by the treatment effect. It is expected that workers in the treatment group have greater incentives to invest in their human capital compared with those in the control group. Using the *European Community Household Panel*, Bassanini et al. (2007) found that

⁴The *OSA Labour Supply Panel* also provides additional information on the type of training courses taken in the period 2000-2002. Although there is rich variation in the types of training courses attended, many public sector employees participated in information technology courses (19% of all public sector employees), management courses (13%) or administrative courses (6%).

private returns on training are approximately 3.7% in the Netherlands. We use this percentage to calculate returns measured in wages and pension income on a training investment in 2006 for both the treatment and control group. Assuming a discount rate of 3% and a life expectancy of 78 years, compared with the control group, employees born in 1950 have an extra return due to the expected postponement of their retirement of approximately 2,400 euro. For workers born in 1950, the total return on training until their death will be about 21,400 euro, whereas workers born in 1949 have a return of 19,000.⁵

There is no indication that persons close to \bar{b} are subject to discontinuities other than the difference in pension rights. Therefore, comparing the training investment behavior of workers who were born in 1949 and remain entitled to their old, generous pension rights with that for workers born in 1950 and subject to the new pension system will provide unbiased estimates of the treatment effect (see Hahn et al. (2001) for a discussion of regularity conditions at the selection threshold):

$$\beta = T^+ - T^-, \quad (3.3)$$

where $T^+ = \lim_{b \downarrow \bar{b}} E(T|b)$ and $T^- = \lim_{b \uparrow \bar{b}} E(T|b)$. We estimate the treatment effect with regression models of the form

$$T_i = \alpha + \beta D_i + \eta P_i + \theta Y_i + \tau M_i + \zeta X_i + \epsilon_i \quad (3.4)$$

where T represents the training participation of worker i and D is the treatment dummy variable, which is 0 if the employee was born in 1949 or 1 if born in 1950. We control for the number of years employees have built up their pension P . The variable Y is the yearly wage income, M stands for marital status, and ϵ is a random error. We also include some additional control variables (X). Among others, we control for the presence of extra pension savings since 2006, the presence of 11 alternative

⁵The negative effect of the reduction in replacement rates on the return on training investments after retirement in 2006 is negligible because of two reasons: 1) Pension benefits are based on career-average wages rather than the final salary before retirement, implying that returns on additional investments for this group affect primarily wages over the remaining career and only marginally affect pension benefits after retirement; 2) There will only be a drop in replacement rates if workers in the treatment group decide not to postpone retirement. However, most workers delay retirement in order to achieve the expected replacement rate they had before the change.

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pension income sources, level of education attained and employee sub-sector. The alternative pension income sources cover most of the potential retirement income sources in the third tier of the pension system.

As mentioned in Chapter 2, we restrict the analysis to workers who have worked continuously since April 1, 1997, in the public sector because employees born before 1950 who did not work continuously in the public sector during this period are also subject to the new pension system. This restriction was made to avoid complications concerning unobserved characteristics of workers than can simultaneously affect training participation and eligibility for pension rights. Unobserved characteristics that influence the decision to work can affect whether a person is in the treatment or control group and the training decision. Moreover, workers who changed jobs in the last 10 years within the public sector and had a short career break might train more because they are working in a relatively new job. Estimates of the treatment effect on the training behavior of persons with career disruptions can therefore be upward biased. Focusing on those workers without a disrupted career ensures the validity of the experiment.⁶

Furthermore, we exclude workers who are employed in the military and some other specific occupations such as fire fighting or ambulance nursing because they are entitled to other early retirement arrangements, which also changed since January 1, 2006, and therefore might disturb the treatment effect of introducing the new pension system. The final sample consists of 7,019 men, 3,692 of whom were born in 1950 and form the treatment group and 3,327 of whom were born in 1949 and belong to the control group.

3.4 Estimation results

We first estimate the effect of the expected time to retirement on training participation in 2006 without using the exogenous variation of the treatment dummy variable. We calculate the expected time to retirement by subtracting the age of the individual in 2006 from the expected retirement age. Column I of Table 3.2 reports the estimation results. We find a significant positive relation between the expected time to retirement and training participation. This suggests that postponing retirement by one year can lead to 1.3% higher training participation.

⁶However, we perform an additional robustness analysis on a sample of workers born in 1949, including those with a disrupted career. The results show that workers with reduced pension rights train significantly more than those in the same age cohort who are still entitled to the old pension rights (the coefficient of the treatment dummy is 0.133, with a standard error of 0.065).

Table 3.2: Expected time to retirement, treatment and training participation in 2006

Dependent Variable: Training participation in 2006	LPM Total Sample I	LPM Total Sample II	LPM Large Organizations III	LPM Small Organizations IV
Expected time to retirement	0.013*** (0.004)			
Treatment dummy		0.027* (0.015)	0.057*** (0.021)	-0.005 (0.020)
Years contributed to pension	0.001 (0.001)	0.000 (0.001)	-0.001 (0.002)	-0.000 (0.002)
Extra savings for pension since 2006	0.018 (0.020)	0.017 (0.020)	0.039 (0.029)	0.002 (0.027)
Yearly wage (/10,000)	0.006 (0.006)	0.006 (0.006)	0.006 (0.008)	0.012 (0.010)
Contractual work hours	0.398* (0.222)	0.444** (0.218)	0.153 (0.312)	0.680** (0.310)
Marital status	0.058** (0.026)	0.060** (0.026)	0.095** (0.037)	0.029 (0.037)
Organization size (/10,000)	-0.003 (0.011)	-0.002 (0.011)	0.008 (0.012)	-0.680* (0.310)
Constant	-0.249 (0.236)	-0.190 (0.232)	-0.031 (0.337)	-0.180 (0.339)
Observations	4,508	4,587	2,265	2,312

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. The number of years contributed to the pension fund, the yearly wage and the size of the organization where employed are extracted from the administrative data of the Dutch public sector's pension fund (ABP). Other control variables included in the estimations are: educational levels, sub sector fixed effects and dummy variables indicating the presence of 11 other potential income sources after retirement. The most relevant alternative income sources are: another pension fund, a partner's income and savings.

Concerning the control variables, we find that additional pension savings in 2006 are not significantly related to an employee's training participation.⁷ Also, the number of years in which pension contributions have been paid, and annual wages are not significantly related to training participation. Training participation of married workers is approximately 6% higher than that of workers who are not married. Also, workers with a higher level of education and those who work more hours participate more in training courses. Training participation of workers with a higher vocational education or an university background is between 17% and 28% higher than for those with lower levels of education.

Since the estimation in Column I does not exploit the exogenous policy shock, the previous results may be subject to causality problems. The question is whether anticipation of early retirement affects training participation or whether training investments induce employees to postpone retirement. Therefore, we proceed by estimating the effects of the treatment dummy variable on training participation as described in Equation 3.4. The use of the treatment dummy enables us to assess the causal effect of the exogenous shock in the retirement expectations of public

⁷We also estimated the model without control variables. We found that adding control variables does not affect the coefficient of the expected time to retirement.

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sector employees on their training participation. Column II of Table 3.2 shows that there is a positive treatment effect on training participation of about 2.7%.⁸ As the average expected retirement age between the two groups differs by 10 months, this corresponds to an increase in training participation of 3.2% when the working period until retirement is extended by one additional year. The coefficients of the other variables in our analysis do not differ much from those presented in Column I.

The positive treatment effect is only marginally significant for the whole sample, which includes workers in both large and small organizations. As mentioned before, we expect the treatment effect to be stronger for workers in large organizations. Large organizations are more likely to employ a larger number of treated workers, which implies that they can more easily meet the fixed costs of training courses which have to be paid irrespective of the number of workers who train. Also, larger organizations have more formally developed and sophisticated HRM policies than small organizations (Koch and McGrath, 1996). Therefore, we continue by separately estimating the treatment effect for workers in organizations above or below the median organization size in our sample (the median organization has approximately 1,000 employees). The results shown in Columns III and IV of Table 3.2 confirm our expectations. We find a strongly significant treatment effect for workers of large organizations, while no significant effect is found for those who work in small or medium sized organizations. The difference in magnitude of the coefficients of the treatment variable is significant. For workers in large organizations, the training incidence in the treatment group is approximately 5.7% higher than in the control group. On the other hand, the treatment effect for workers in smaller organizations is approximately zero. Since, we only find a significant effect for workers in large organizations, our further analyses will exclusively focus on these workers.⁹

⁸We also test whether our estimations are biased due to selectivity. It can be argued that more motivated workers were more prepared to respond to the questionnaire. In that case, our previous estimations would be biased. However, our results remain robust when estimating probit models accounting for sample selection on training participation following the approach of Van de Ven and Van Praag (1981). Moreover, we did not find any evidence of selection bias.

⁹All further analyses were also performed on the sample of employees working in organizations below the median size. However, we did not find significant effects on the training participation of these workers.

3.4.1 Age and treatment effect

It is well known that the probability of receiving training is a function of age. A relevant question is whether the difference in training propensities between the two birth cohorts goes beyond what would be expected based on the limited age difference alone. We approach this issue in the two estimations shown in Table 3.3.¹⁰ The estimation in Column I contains workers' age as an additional variable. Since we know the exact birth date of all the workers in our sample, this variable measures age in days. With this correction for age, we find that the coefficient of the treatment dummy remains positive and highly significant.

However, some multicollinearity exists between the treatment dummy and the age variable that can confound the treatment effect. Another way to control for age effects is to compare the training incidence in 2005 of workers who were born in 1949 with the training incidence in 2006 of those born in 1950. By comparing the training incidence at the same age for both groups of workers, there is no need to include the age variable and potential biases due to multicollinearity are avoided. Column II of Table 3.3 shows that the treatment effect in this additional robustness analysis remains robust. Again we find a positive coefficient that is significantly different from zero and slightly higher than the coefficient shown in Column III of Table 3.2. The increase in the coefficient is less profound than when we include age directly in our estimations (see Column I of Table 3.3).¹¹ Nevertheless, on the basis of the two previous estimations, we can conclude that the treatment variable indeed measures a treatment effect that goes beyond potential differences in the training propensities expected based on the age difference between the two birth cohorts.

3.4.2 Skill groups and training participation

Several recent empirical studies found that actual training participation depends on worker's motivation to train (e.g., Oosterbeek, 1998; Leuven and Oosterbeek, 1999; Croce and Tancioni, 2007) and that this motivation is highly correlated with the level

¹⁰We also performed additional analyses where we reduced the age window around the treatment threshold. When we reduced the sample to those workers born close to the treatment threshold (three, six and nine months born around January 1, 1950), we found that the coefficient of the treatment dummy variable is slightly higher for workers born closer around the treatment threshold.

¹¹A potential drawback of this analysis is that we are not able to account for potential business cycle effects. However, we expect that the probability that the business cycle confounds our estimation results is low. First, when considering business cycles, we can observe that the years 2005 and 2006 are similar. In both years, the Dutch economy experienced economic growth. Second, the *OSA Labour Supply Panel* shows that the incidence of training in the public sector remained stable between 2004 and 2006.

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Table 3.3: Age effects, expected time to retirement and training in large organizations

Variables	LPM Training 2006 I	LPM Training 2005 / 2006 II
Treatment dummy	0.109** (0.043)	0.062*** (0.021)
Age (in days /10,000)	-1.472 (1.003)	
Years contributed to pension	-0.002 (0.002)	-0.003 (0.002)
Extra savings for pension since 2006	0.029 (0.029)	0.018 (0.029)
Yearly wage (/10,000)	0.008 (0.008)	0.012 (0.008)
Contractual work hours	0.250 (0.312)	0.493 (0.310)
Marital status	0.112*** (0.037)	0.123*** (0.037)
Organization size (/10,000)	0.007 (0.009)	0.010 (0.009)
Constant	-0.763 (0.511)	-0.336 (0.332)
Observations	2,265	2,265

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. In Column I, the dependent variable is training participation in 2006. The dependent variable in Column II measures the training incidence in 2005 of workers born in 1949 and the training incidence in 2006 of those born in 1950. Other control variables are: educational levels, sub sector fixed effects, and dummy variables indicating the presence of 11 other potential income sources after retirement.

of education (Maximiano and Oosterbeek, 2006). In general, more highly educated workers have a high training propensity due to their stronger motivation to invest in their human capital, irrespective of the duration of their remaining work life. On the other hand, we expect that the training behavior of less educated workers to be more affected by the treatment, since these workers who were initially less inclined to train are now forced to participate in training courses to keep up their productivity during their extended work life.

We test this hypothesis by analyzing whether the pension reform has differentially affected the training behavior of workers with different levels of education. Column I of Table 3.4 shows the regression results for employees with a secondary or lesser education, Column II shows the results for those with an intermediate vocational education, and Column III for workers with a higher vocational education or a university degree. Table 4 shows that there are substantial differences in the impact of the retrenchment of pension wealth for the three skill groups. The treatment effect is much larger for workers who are less educated.¹² This greater impact is consistent with the existing literature on training motivation and suggests that highly educated

¹² Approximately 46% of the treated workers with a secondary or lesser education participated in a training course, 56% of those with an intermediate vocational education and 60% with a higher vocational education or a university degree.

Table 3.4: Treatment and training in large organizations: differences among skill groups

Dependent Variable: Training participation in 2006	Primary or Secondary Education I	Intermediate Vocational Education II	Higher Education III
Treatment dummy	0.197** (0.090)	0.191* (0.106)	0.073 (0.054)
Age (in days /10,000)	2.033 (2.155)	3.638 (2.485)	1.089 (1.259)
Years contributed to pension	-0.004 (0.004)	-0.004 (0.005)	0.001 (0.002)
Extra savings for pension since 2006	0.070 (0.073)	0.047 (0.072)	0.026 (0.035)
Yearly wage (/10,000)	0.048* (0.025)	0.046 (0.036)	0.006 (0.008)
Contractual work hours	0.466 (0.956)	1.764 (1.375)	0.166 (0.341)
Marital status	0.073 (0.075)	0.084 (0.088)	0.076 (0.049)
Organization size (/10,000)	-0.018 (0.022)	-0.022 (0.031)	0.030* (0.017)
Constant	-0.956 (1.242)	-2.671 (1.684)	-0.268 (0.594)
Observations	477	387	1,411

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. Column I presents the results for workers with a secondary education or less, Column II shows the results for workers with an intermediate vocational education, and Column III shows the results for workers with a higher vocational education or a university degree. Other included control variables are: educational levels, sub sector fixed effects and dummy variables indicating the presence of 11 other potential income sources after retirement.

workers are indeed intrinsically more motivated to train, despite the sudden shock to their expected retirement age (Maximiano and Oosterbeek, 2006).

3.4.3 Training participation: long or short courses

One might expect the positive effects of a short training course on productivity to wear out in a short time period. If short courses only have short-term effects on productivity and the workers in our sample still have to work for approximately six or seven years until they retire, it is possible that participation in short courses has not yet been affected by the change in pension rights and expected time to retirement. Since about 50% of the workers who participated in training spent less than a full-time workweek of 40 hours in training courses, we perform additional analyzes for participation in long training courses and participation in short training courses. We construct a dummy variable for participation in long training courses that is coded as 1 if workers trained more than 48 hours in 2006. The indicator for short training courses is coded as 1 if workers spent less than 48 hours on training in 2006. Table 3.5 shows that there exists a significant treatment effect on participation in long training courses while the impact of the pension reform on short training courses is insignificant. Workers in the treatment group participate approximately 7.3% more in long training courses than workers who were born in 1949.

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Table 3.5: Treatment effect in large organizations: short and long training courses

Variables	LPM Long Training Courses I	LPM Short Training Courses II
Treatment dummy	0.073** (0.034)	0.047 (0.040)
Age (in days /10,000)	0.561 (0.792)	1.046 (0.928)
Years contributed to pension	0.001 (0.002)	-0.002 (0.002)
Extra savings for pension since 2006	0.023 (0.023)	0.014 (0.027)
Yearly wage (/10,000)	0.007 (0.007)	-0.002 (0.008)
Contractual work hours	0.600** (0.249)	-0.445 (0.292)
Marital status	0.023 (0.030)	0.069** (0.035)
Organization size (/10,000)	0.029*** (0.010)	-0.022* (0.011)
Constant	-0.938** (0.407)	0.254 (0.477)
Observations	2,265	2,265

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. The dependent variable in Columns I is coded 1 if employees spent more than 48 hours in training courses in 2006. The dependent variable in Columns II is coded 1 when workers trained 48 hours or less. Other control variables are: educational levels, sub sector fixed effects and dummy variables indicating the presence of 11 other potential income sources after retirement

3.5 Conclusion

Human capital theory predicts that early retirement systems have a negative effect on human capital formation because early retirement reduces the period in which the benefits of training investments can be reaped. In this study, a natural experiment was used to identify the effects of an exogenous change in early pension rights on older workers' training participation. For this purpose, we used matched survey and administrative data for male employees in the Dutch public sector who were subject to a substantial pension reform in 2006. We compared employees who were born in 1949 and remain entitled to their old, generous pension rights with those born in 1950 and subject to the new system.

Using simple LPM regressions, we found that postponement of retirement due to the shock to pension rights has a positive impact on the training participation of older men. However, the treatment effect is only significant for workers in larger organizations. This suggests that larger organizations are better able to customize their training policies to the new retirement system with as possible explanation that they have more formally developed and sophisticated HRM policies than small organizations (Koch and McGrath, 1996). We found that the treatment effect for workers in large organizations remains robust when we control for age and if we use

alterative specifications. Furthermore, we found that the treatment effect is larger for lower educated workers. Moreover, as expected, the treatment effect was only significant for participation in longer training courses.

This chapter focused on those employed in the public sector for whom the pension change was more profound, which is in most European countries the largest employer (Pilichowski and Turkisch, 2008). Although caution is necessary when generalizing the results, we think that our results could also apply to large organizations in the private sector. There has been substantial convergence in the employee conditions of public and private sector workers in the Netherlands in the last two decades (CAOP, 2006). Since 1993, in the public sector, negotiations between unions and employer organizations on labor conditions are decentralized into eight different sectors with tailor-made collective agreements. Furthermore, labor market conditions have increasingly induced individual employers within the public sector to apply more flexible wage schemes and performance-related payments in order to increase individual productivity (Ministry of the Interior and Kingdom Relations, 2008).

Moreover, most European countries face an ageing working population and are adapting retirement systems to stimulate the labor market participation of older workers by postponing retirement, either by increasing the retirement age directly or by reducing the generosity of pension benefits. These reforms are not restricted to national retirement plans but do also apply to the retirement schemes of public sector workers (see Palacios and Whitehouse (2006) for an overview of recent reforms to public sector pension plans in European countries).

In this context, our results are highly relevant since success in prolonging employment will highly depend on policies preventing skills obsolescence to maintain the productivity of older workers (see also De Grip and Van Loo, 2002). Consistent with human capital theory, our estimates indicate that workers employed in the public sector who are forced to work longer increase their training participation and hence will increase their productivity and remain attractive to their employers. However, this only holds for those employed in large organizations in this specific sector. Older workers employed in small organizations seem to be more restrained in their human capital investments when public policy restricts the opportunities of early retirement and therefore might have more problems remaining productively employed for a longer period.

4 Shattered Dreams and Mental Health

4.1 Introduction

This chapter assesses the mental health effects of the change in the Dutch pension system.¹ 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 2002.² As of 2006, those born before January 1, 1950, could continue to retire under the old rules, but for those born on or after January 1, 1950, the replacement rate is lowered to 64%. 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 *ex ante* mental health effects from a change in a retirement system, which occur well before people 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 United States, the United Kingdom, 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, health care expenditures of depressed individuals are about four times higher than those of non-depressed individuals. In addition to these direct effects on health care costs, indirect costs from 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 share of disability insurance expenditure in the western world is 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 in order to mitigate the adverse effects of an aging population. Increasing labor force participation rates among older workers improves the fiscal stability of pension systems. However, a natural question which 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 health care costs at ages after retirement. Our finding of persistent *ex ante* health

¹This chapter is based on joint research with Andries de Grip and Maarten Lindeboom.

²Until January 1, 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 since 2002.

effects 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 analyzes usually found that those who retire early have worse post-retirement health. Tsai et al. (2005) compared mortality rates at later ages and found that post-65 mortality rates are higher for those who retire early. Dave et al. (2008) found that earlier retirement is associated with poor physical and mental health after retirement. It has been hypothesized 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 the retirement age would lead to better individual health and well-being and may reduce the burden on (public) health care 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 retirees with later retirees. Indeed, health may affect work and vice versa. Moreover, unobserved factors may confound the relationship between health and work.

Recent papers in this area tried to circumvent this endogeneity problem by using an Instrumental Variable approach. Charles (2004) and Neuman (2007) used age-specific retirement incentives provided by the US social security system to capture changes in labor force participation that are unrelated to health. Similarly, Bound and Waidmann (2008) employed age-specific retirement incentives of the UK social security system to gauge the effect of retirement on health. Coe and Lindeboom (2008) used the availability of retirement windows as an instrument. All these studies confirm that the cross-sectional association between health and retirement is positive; i.e., 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) found no negative effect from early retirement on male health; if anything, these authors report a temporary increase in self-reported health and improvements in health of highly educated workers. Bound and Waidmann (2008) found no evidence of negative health effects from retirement and some evidence that there may be a positive effect for males. Neuman (2007) found, for subjective health measures, that retirement maintains health, but found no effect on objective health variables. Charles (2004) focused on mental health and found that the direct effect

of retirement on mental well being is positive. Our findings of strong *ex ante* mental health effects are consistent with the Charles (2004) findings.

Finally, the finding of *ex ante* effects of retirement on mental health has 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 behavior, with health included as an exogenous regressor (see e.g., 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. In the presence of *ex ante* health effects, changes in the retirement system will not only have an impact on the budget constraint, but will also influence health prior to retirement. This will confound both the health effects and the effect of financial incentives in retirement models.³

Our contribution is most closely related to the recent paper by Falba et al. (2009), which examined the impact on depression of deviations of actual retirement dates from their preceding expectations. The paper found significant effects on depression at age 62 from those working more than expected and from those working less than expected. Our study differs from this paper 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 health and that affects only a subgroup of workers. Our analyses build on the survey conducted in 2008 and consists of approximately 6,078 observations of full-time working males born in 1949 and 1950. 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 guarantees the internal validity of the experiment. Our findings are therefore less likely to be confounded. Furthermore, there have been no other institutional changes that differentially affected the 1950 (1949) cohort as opposed to the 1949 (1950) cohort. Second, our study shows that there are *ex ante* effects, that these are substantial and that they persist over time. Third, our data allow, to some extent, for further analysis of savings decisions and retirement expectations. This gives more insight into the mechanisms underlying our findings.

This chapter proceeds as follows: Section 4.2 describes the data and the variables used in our analyses. Section 4.3 presents the results of the empirical analyses. In Section 4.4, we further explore mechanisms that may explain our finding of higher

³Part of the effect of the financial incentives will be absorbed by the health effect if health changes prior to actual retirement. This suggests, moreover, that there are feedback effects of work 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.

depression rates among workers affected by the reform. Section 4.5 closes with a discussion of our conclusions.

4.2 Data

In this chapter, we rely on data from the second wave of the survey, which was held in March 2008 and includes 6,078 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. We only use the second wave because the first wave does not have information on mental and physical health and the third wave does not include important control variables such as personal wealth, savings behavior, and several job characteristics.

As described in Chapter 2, the analysis is restricted to full-time employees who have worked continuously in the public sector since 1997.⁴ For these workers, the pension reform is clear and simple, as age is the only criterion that determines whether a worker is eligible for the restricted or the more generous retirement scheme. The final sample consists of 5,195 men, of which 2,724 were born in 1950 (the treatment group) and 2,471 were born in 1949 (the control group).

Our primary 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 the *Center for Epidemiologic Studies Depression Scale* (CES-D, Radloff 1977). The CES-D8 is a well validated instrument for measuring emotional function and depressive symptoms (see Blazer et al., 1991; Hays et al., 1993; Adams et al., 2003; Falba et al. 2009) and is used in many large sample-population based studies such as the *US Health and Retirement Survey* (HRS). The CES-D8 consists of eight items, of which six 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). To create the variable used in our analyses, we first dichotomize (yes/no) responses 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. 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

⁴This resulted in the exclusion of 260 observations on employees born in 1949 who were not eligible for the old pension rights. This group is not large enough to serve as an additional control group.

4 Shattered Dreams and Mental Health

and above, consistent with probable clinical depression (see Andresen et al., 1994; and Blazer et al., 1991).

In addition to mental health, we collected information about physical health, using both objective and subjective measures.

- 1) *In general, how would you describe your current health?*
- 2) *How would you describe your health compared to others with the same age?*
- 3) *How often did you visit your doctor in 2007?*
- 4) *Do you agree with the following proposition? My health limits me in the kind and amount of work I am able to perform.*

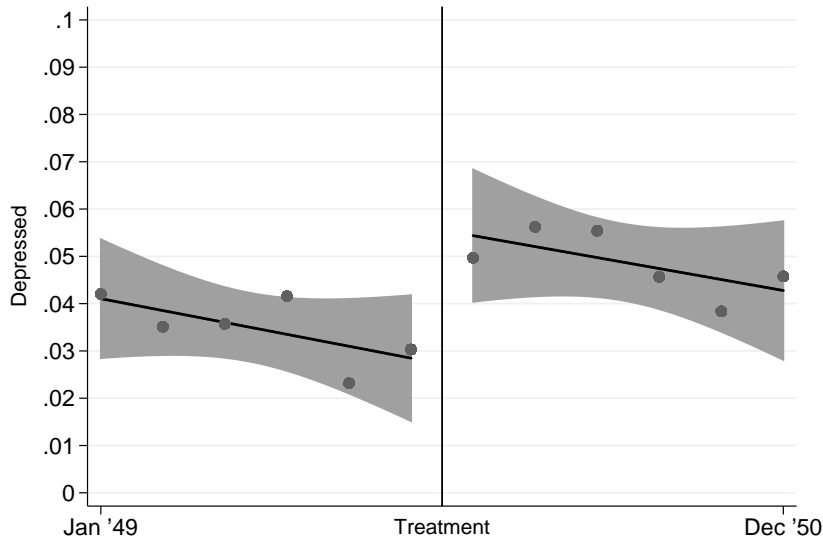
Response categories of Questions 1 and 2 ranged from 1 (very good) to 5 (very bad). Response categories of Question 4 ranged from 1 (totally disagree) to 5 (totally agree).

Table 4.1 presents descriptive statistics for the entire sample, the treatment group and the control group. The last column gives the results of a simple t-test for equality of the means of a variable for both groups. The table shows that on average 4.3% of all public sector workers are depressed as measured by our depression indicator.⁵ Workers with retrenched pension rights are relatively more depressed (5.0%) than those who are not affected by the policy change (3.5%). A simple t-test shows that the difference between the two groups is significant at the 5% level (t-stat = 2.63). A similar result is found for the raw CES-D8 score (t-stat = 2.03). This preliminary analysis indicates that there is a negative *ex ante* effect from the shock in the pension system on the mental health of workers nearing retirement. Figure 1 presents a scatter plot of the mean depression rate (mean for two birth months) and confirms that the deviations in the depression rate between the treatment and control group emerge precisely around January 1, 1950, threshold.

⁵Since we only have working men in our sample, this percentage is lower than the depression rate for the total population in the Netherlands which also includes unemployed and disabled individuals.

Table 4.1: Descriptives

Variables	All			T-stat	
	Mean I	St. Dev II	Born in 1950 III	Born in 1949 IV	V
Health					
Depressed	0.04	0.20	0.05	0.04	2.63
CES-D8 score	0.59	1.17	0.62	0.55	2.03
Number of doctor visits in past year	2.13	2.53	2.13	2.12	0.02
Sick for > 14 days in past year	0.17	0.37	0.16	0.17	0.83
Health limits work (yes=1)	0.18	0.39	0.19	0.19	0.28
Retirement expectations					
Expected retirement benefit at age 62	69.41	11.75	67.00	72.10	16.07
Expected retirement age	62.00	2.58	62.11	61.88	3.12
Expected retirement age 5 years ago	61.46	2.24	61.42	61.50	1.24
Job Characteristics					
Fraction of non-routine tasks	0.29	0.21	0.30	0.29	0.99
Physically demanding work	0.09	0.29	0.09	0.09	0.01
Mentally demanding work	0.64	0.48	0.65	0.63	1.75
Yearly wage (in logs)	10.83	0.28	10.83	10.84	0.94
Other personal characteristics					
Lower secondary education	0.12	0.33	0.12	0.13	1.62
Higher secondary education	0.04	0.20	0.04	0.04	0.18
Intermediate vocational education	0.15	0.36	0.15	0.15	0.58
Higher education	0.69	0.46	0.69	0.68	0.77
Marital status	0.92	0.28	0.91	0.92	1.75
Sectors					
Government	0.43	0.50	0.44	0.41	2.26
Education sector	0.39	0.49	0.38	0.41	2.57
Energy, public transportation	0.14	0.35	0.14	0.14	0.05
Other (Judicial sector, Utilities)	0.03	0.17	0.03	0.03	0.67
Wealth and Income sources after retirement					
Years contributed to pension	32.15	6.32	31.45	32.86	8.26
Pension rights at other pension funds	0.13	0.34	0.13	0.13	0.16
Partner has pension or income	0.47	0.50	0.47	0.48	0.28
Net housing wealth	0.56	0.50	0.57	0.56	0.73
Inheritance	0.14	0.34	0.14	0.14	0.61
Annuity insurance	0.52	0.50	0.52	0.52	0.12
Life insurance	0.26	0.44	0.26	0.27	0.23
Savings account > 15,000 Euros	0.61	0.49	0.62	0.60	0.71
Investment	0.35	0.48	0.35	0.35	0.28
Life Course Savings program	0.12	0.32	0.16	0.06	9.31
Other assets or pension savings	0.10	0.30	0.10	0.09	1.28
Extra savings for pension since 2006	0.25	0.43	0.27	0.22	3.83
Number of observations	5,195		2,724	2,471	

Figure 4.1: Depression

The data points presents the mean depression rate (for two birth months) from January 1949 to December 1950. Fitted lines and confidence intervals are calculated based on individual information. The vertical line marks the threshold dividing the control group from the treatment group.

Table 4.1 also shows that there are no significant differences in the averages of the physical health measures between the two groups. In 2007, on average, workers visited their doctor twice, 17% of workers were sick more than 14 days and 18% indicated that their health limits them in their job. The t-statistics for the differences between the controls and the treated for number of doctor visits, self-reported health and the limitations question are 0.05, 0.83 and 0.28, respectively.⁶

⁶Furthermore, Table 4.1 shows that most differences between the group of workers who are affected by the new pension system and the group that falls under the old system are extremely small (see also Table 2.1 in Chapter 2). Job characteristics, personal characteristics and wealth components are similar across both groups, and with a few exceptions, are not significantly different from each other. We observe, however, some very small yet significant differences between the two groups in the government and education sector. Among the set of wealth variables, three variables are significantly different between the treatment and control groups: the number of years individuals built up pension rights in the public sector pension fund; the response to a question on whether respondents undertook extra savings arrangements for their retirement in the past year; and the response to a question on whether individuals participated in the *Life Course Savings* program. The difference in the pension rights variable is due to the small age difference between the control and the treatment groups. The extra pension savings and participation in the *Life Course Savings* program are likely to be responses to the policy reform.

4.3 Estimation results

We operate simple linear probability models for whether an individual is depressed. The results are presented in Table 4.2. An individual is defined to be depressed if the CES-D8 score equals 4 or higher (see Section 4.2). The table includes a base specification, where, apart from the indicator for the reform, no other variables are included. The remaining columns refer to specifications where, subsequently, additional controls are included. In specification II, we add a set of individual controls, including age, marital status, education, wage income, how many years the worker has contributed to the pension fund, and job characteristics (sector of work). This specification excludes an indicator for whether individuals characterize their work as mentally or physically demanding, and the proportion of routine tasks. It is conceivable that depression status may have a direct effect on these variables. In specification III, we add a set of controls for personal wealth. Although our data set contains indicators for wealth aside from those included here, such as whether individuals have an annuity or a life insurance policy, whether they have more than 15,000 Euros in their bank account, and participate in a *Life Course Savings* program, we decide not to include these variables in the regressions because some of these variables may themselves be influenced by the reform and may therefore absorb some of the effect of the reform on depression.

For wealth variables that we do include, it is less likely that they are affected by the pension reform. However, there were some missing observations: the sample size is reduced to 3,314 observations if we omit those observations where at least one of the wealth variables is missing. We therefore include indicators for whether there was item non-response on each of the included wealth variables.⁷ In the last specification (IV) we include the three potentially endogenous job characteristics variables (whether the work was mentally or physically demanding and the fraction of routine tasks) along with other health indicators and a health care utilization variable. One could argue that these health variables are potentially endogenous in the sense that they may relate to unobservable factors that also influence depression, or that causality may run from depression status to the health variable. Note however, that we find no differences between both groups for these health and job characteristics variables (see Table 4.1 and the discussion in the data section). Inclusion of these variables controls for individual heterogeneity and it is therefore expected that such

⁷We also run a regression where we include only these 3,314 observations. The coefficient of the reform was even higher (0.032 with standard error of 0.013) and was significant at the 1 percent level.

inclusion will have little impact on the estimate of the reform indicator. A comparison of the reform dummy in specifications III and IV will be informative on this issue.

4.3.1 Basic results

The results displayed in Table 4.2 are very clear: in all specifications the reform has significant effects on mental health and the magnitude of these effects increases when we add controls for individual heterogeneity. These effects are sizeable. For instance, the coefficient in the last specification (specification IV) is 0.028, which implies that depression rates increase with 2.8 %-points as a result of the shock in the pension system. The other variables in Table 4.2 show the expected signs. For example those who are married have a lower probability of being depressed; the same holds for those whose partner has a pension or an income. The health variables are all strongly significant, although these effects may be biased as discussed above. We see however, no large changes in the reform indicator across the different specifications in columns II, III and IV. Next, we perform analyses on the total CES-D8 score. This variable ranges from 0 to 7 and shows substantive heaping at the 0 score. Following Falba et al. (2009), we estimate a Poisson model;⁸ the results are reported in the appendix and they indicate that the reform also has a significant impact on the raw CES-D8 score.

4.3.2 Age trends and persistence of the impact of the reform

Of relevance is whether this effect of elevated depression rates persists over time. It is important to note that the depression indicator used in the analyses above is measured in Spring 2008, already more than two years after the implementation of the new pension system. To further check for the persistence of the pension reform effect, we also use available data from the 2009 wave. This wave does not include important savings, and wealth variables. We therefore run regressions on a limited set of controls, comparable to specifications I, II and III of Table 4.2. The results are reported in the Appendix of this chapter. The results of these additional analyses indicate that the effect of the reform is slightly smaller in magnitude, but still significant at the 5%-level. This suggests that the effects of the reform persist over time.

We also perform regressions on sub-samples of the 2008 wave for different age

⁸A regression of $\log(\text{CES-D8} + 1)$ gave similar results.

Table 4.2: Treatment effect on mental health (depression indicator; CES-D8 ≥ 4)

Dependent Variable: Depression	I	II	III	IV
Treatment dummy	0.015*** (0.006)	0.026** (0.012)	0.026** (0.012)	0.028** (0.011)
Age (in days /100)		0.003 (0.003)	0.003 (0.003)	0.003 (0.003)
Marital status		-0.044*** (0.011)	-0.037*** (0.011)	-0.035*** (0.011)
Lower secondary education		0.003 (0.011)	0.005 (0.011)	0.001 (0.011)
Higher secondary education		-0.009 (0.016)	-0.003 (0.016)	-0.008 (0.016)
Higher education		0.005 (0.009)	0.011 (0.010)	0.008 (0.009)
Government		-0.002 (0.017)	-0.002 (0.017)	-0.012 (0.016)
Education sector		0.002 (0.017)	0.003 (0.017)	-0.017 (0.017)
Energy, public transportation		-0.006 (0.018)	-0.009 (0.018)	-0.025 (0.018)
Physically demanding work				-0.002 (0.009)
Mentally demanding work				0.024*** (0.006)
Fraction of non-routine tasks				-0.026* (0.014)
Yearly wage (in logs /100)		-3.743*** (1.264)	-3.712*** (1.270)	-1.484 (1.264)
Years contributed to pension (/100)		0.073 (0.046)	0.094* (0.049)	0.094** (0.047)
Pension rights at other pension funds			0.016* (0.010)	0.011 (0.009)
Partner has pension or income			-0.022*** (0.007)	-0.018*** (0.007)
Net housing wealth			0.003 (0.007)	0.009 (0.007)
Inheritance			-0.016* (0.009)	-0.012 (0.008)
Missing info on other pension			0.010 (0.014)	0.002 (0.013)
Partner info missing			-0.024 (0.015)	-0.015 (0.014)
Net housing wealth missing			0.017 (0.014)	0.020 (0.014)
Info on inheritance missing			-0.002 (0.012)	-0.002 (0.011)
Self-reported general health				0.034*** (0.005)
Self-reported work limitations				0.066*** (0.009)
Number of doctor visits in past year				0.007*** (0.001)
Sick for > 14 days in past year				0.028*** (0.009)
Observations	5,195	4,854	4,854	4,765
R-squared	0.001	0.009	0.012	0.101

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. The dummy variable for intermediate vocational education and a dummy measuring whether one works in a sector other than the government, education, energy and public transportation, are used as reference categories.

Table 4.3: Treatment effect on mental health: results for different birth cohorts

Variables	Born within 3 months of 1-1-1950 I	Born within 6 months of 1-1-1950 II	Born within 9 months of 1-1-1950 III	Born within 12 months of 1-1-1950 IV
Treatment dummy	0.024** (0.011)	0.022*** (0.008)	0.020*** (0.006)	0.016*** (0.006)
Observations	1,168	2,407	3,557	4,765
R-squared	0.126	0.116	0.100	0.101

Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The regressions include the same set of regressors (without the age variable) as in specification IV of Table 2 (a set of health variables, individual and job characteristics and selected wealth variables).

windows around the treatment threshold. We include these regressions for two reasons. First, it is conceivable that effects from the reform may be particularly strong for individuals who barely missed the old generous pension system by a few days, weeks, or months. After all, the deadline of December 31, 1949, 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. A second reason is that we include age in the specifications of Table 4.2 as the number of days after January 1, 1949. Clearly age should be controlled for as this may be a relevant factor for depression. At the same time, there is little variation in the age variable and it clearly correlates with the reform dummy; this may affect our results. By omitting the age variable in the regressions, but nevertheless estimating the regressions for different age windows, we address the issue of the sensitivity of our findings to the inclusion of the linear age variable.

The results are presented in Table 4.3. The table reports only the coefficients of the reform indicator, but all regressions include the full set of controls (only age is excluded), as in specification IV of Table 4.2. Table 4.3 shows that the effect of the reform is stronger for those born near the threshold date and that the effect gradually decreases with a wider window around the threshold date. We think that the age range for the smallest window is too small for age effects to be relevant. Therefore, the coefficient of 0.024 for the treatment effect can be expected to be a reliable estimate of the causal impact of the reform. This coefficient is only slightly smaller than the 0.028 that we obtained from specification IV in Table 4.2, where age is included as a linear trend. This adds confidence to the estimates presented in Table 4.2.

The pattern in the coefficients of Table 4.3 suggests that those who were born just after the threshold date (January-March 1950) are more depressed than those who were born later in the year (April-December 1950). The average depression rate for those born in the January-March 1950 period is 0.053, compared to 0.048 for their

younger counterparts born in the April-December 1950 period. However, a simple t-test shows that this difference is not significant ($t\text{-stat} = 1.0$). Alternatively, there could be a ‘relief effect’, meaning that particularly those who just qualified (i.e., those born October - December 1949) have lower depression rates. We therefore check the depression rates for several groups. The average depression rate for those born October - December 1949 is 0.027, whereas the depression rate for their older counterparts (January - September 1949) who also qualified for the old generous pension system is 0.036. This suggests a relief effect. A t-test, however, revealed that the differences between these two groups (approximately 550 observations) is not significant ($t\text{-stat} = 0.50$). Hence, we can conclude that the previous results are most likely not driven by a relief effect.

4.3.3 Heterogenous effects

One might wonder whether the pension reform differentially affects different types of workers. Table 4.4 presents the results of separate analyses on subsamples. The regressions include the same set of explanatory variables as in specification IV of Table 4.2, but again we here report only the coefficient for the reform indicator. The table shows that there can be substantial differences in the impacts of the reform for different subgroups. The effects are in the expected direction. For instance, those who are not married experience a substantially greater effect from the reform, although the effect is significant only at the 10% level, which may be due to the low number of unmarried males in our sample. The reform has the greatest financial consequences for those with a higher income and those with nearly full pension rights (measured by number of years contributing to the pension fund). Indeed, the effect of the reform on mental health is also highest for these workers. Similarly, workers whose partners have a pension or an income are likely to be less affected by the income shock due to the pension reform. We also note that for this group the reform has no significant impact on the depression rate.

Table 4.4: Treatment effect on mental health: heterogenous effects

Variables	Married		Below average income		Partner has pension or income		Number of years contributed (≥ 32)	
	Yes I	No II	Yes III	No IV	Yes V	No VI	Yes VII	No VIII
Treatment dummy	0.023** (0.011)	0.099* (0.055)	0.021 (0.019)	0.029** (0.013)	0.008 (0.014)	0.037** (0.017)	0.032** (0.016)	0.022 (0.015)
Observations	4,376	389	1,972	3,009	2,265	2,150	2,544	2,221
R-squared	0.091	0.214	0.131	0.080	0.092	0.111	0.105	0.106

Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The regressions include the same set of regressors as in specification IV of Table 2 (a set of health variables, individual and job characteristics and selected wealth variables).

The estimation results presented in Tables 4.2, 4.3 and 4.4 are all consistent with strong *ex ante* effects from the pension reform on mental health. Is it likely that our findings are due to some artifact? 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. Further, 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, but that they will entirely lose their rights to retire before age 65. 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 a firm (Euwals et al., 2006)

Those born in 1949 still have the option to retire between age 55 and 65. We therefore also examined whether some workers stopped working between the introduction of the reform in 2006 and the second wave of the survey (2008). This may influence our results if this effect is sizeable. We find that only 64 workers of the 1949 cohort who participated in the first wave (7,739 workers) retired during 2006 and 2008. This number is too small to substantially change the results.

We drop individuals who did not work continuously in the public sector since 1997. 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. Therefore, in principle, a drop in mental health caused by the pension reform might lead to that workers whose mental health worsened the most are not selected into our sample. However, the descriptive statistics provided in Chapter 2 give no reason to expect this. Moreover, when this is the case, one would expect to see this reflected in other observable factors that are correlated with depression (such as marital status) or with gaps in employment (income for instance). We see no differences in the observed variables between the two groups. We run additional regressions in which we add this group to the sample (260 full-time workers) and include two additional dummy variables measuring whether workers worked continuously in the public sector and whether those with career interruptions were born in 1950. 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 controls).

Are there other factors that differ between the two groups, such as the level of

education? The averages in Table 4.1 reveal no important differences in this respect. Furthermore, there have not been changes in the system that may have differentially affected the 1950 (1949) cohort as opposed to the 1949 (1950) cohort.

4.3.4 Why is the affected cohort so depressed?

An important question is why the reform has such a strong impact on mental health? One possibility is that (the prospect of) longer working in itself causes decreased mental health. We explore this by examining the effect of expectations about the retirement age on depression. For this, we use an Instrumental Variable (IV) approach in which we instrument expected retirement age with the reform dummy.⁹ This measures a Local Average Treatment Effect (LATE). Table 4.5 shows that the coefficient of the expected retirement age is equal to 0.116 and is not significant at the standard levels (s.e. = 0.088). However, the F-test of the first-stage regression suggests a weak instruments problem (the F-statistic equals 2.53, which is well below the value of 10 suggested by Staiger and Stock, 1997). We also examine the effect of expected late retirement (later than age 63) on mental health. The IV regression reveals a positive and strongly significant coefficient (0.179, s.e. 0.0078). The F-test of this first stage regression equals 35.90. This finding thus suggests a strong causal effect of expected late retirement on depression.¹⁰

We also performed a regression in which we regressed the depression indicator simultaneously on the treatment dummy, savings, expected late retirement, and a full set of controls. The idea is that if (forced) savings and late expected retirement are the main factors responsible for the relatively high depression rate among those affected by the reform, one would expect that the effect of the reform indicator (treatment dummy) diminishes when we include these variables. However, the results show that the reform indicator remains large (coefficient is 0.031) and strongly significant (s.e. 0.013) after inclusion of savings and late expected retirement. One can conclude from this that apparently also factors other than (forced) savings and late expected retirement are responsible for the relatively high depression rate among those affected by the reform.

⁹It is likely that retirement age expectations are endogenously related to the depression rate. Either because there are feedback effects from depression on retirement expectations, or because there are unobservables that correlate both to depression and individual expectations. When using the reform dummy as an instrument we assume that any effect of the reform on mental health must run via retirement expectations

¹⁰We also performed IV-regressions in which we instrumented extra pension savings and investments in the *Life Course Savings* program. In both cases the F-test of the first-stage regression suggests a weak instruments problem.

Table 4.5: Effects of late retirement on mental health (IV-regressions)

Dependent Variable:		
Depression		
	I	II
Expected retirement age	0.116 (0.088)	
Late retirement (retirement > 63 years)		0.179** (0.078)
Age (in days /100)	-0.001 (0.003)	0.003 (0.003)
Marital status	-0.006 (0.029)	-0.031*** (0.012)
Lower secondary education	0.018 (0.023)	0.010 (0.012)
Higher secondary education	-0.014 (0.030)	-0.006 (0.017)
Higher education	0.006 (0.018)	0.012 (0.011)
Government	0.028 (0.043)	-0.005 (0.018)
Education sector	0.024 (0.044)	-0.013 (0.018)
Energy, public transportation	0.051 (0.068)	-0.009 (0.021)
Physically demanding work	-0.001 (0.018)	0.003 (0.010)
Mentally demanding work	0.037** (0.016)	0.030*** (0.008)
Fraction of non-routine tasks	-0.053 (0.034)	-0.042** (0.017)
Yearly wage (in logs /100)	1.919 (3.375)	-1.129 (1.379)
Years contributed to pension (/100)	0.513 (0.330)	0.251*** (0.087)
Pension rights at other pension funds	0.028 (0.023)	0.010 (0.010)
Partner has pension or income	-0.031* (0.016)	-0.017** (0.007)
Net housing wealth	0.027 (0.019)	0.013* (0.008)
Inheritance	-0.012 (0.016)	-0.012 (0.009)
Missing info on other pension	0.008 (0.026)	0.010 (0.015)
Partner info missing	-0.006 (0.027)	-0.004 (0.016)
Net housing wealth missing	0.024 (0.026)	0.019 (0.015)
Info on inheritance missing	-0.006 (0.021)	-0.007 (0.012)
Self-reported general health	0.034*** (0.009)	0.041*** (0.006)
Self-reported work limitations	0.104*** (0.033)	0.080*** (0.011)
Number of doctor visits in past year	0.007*** (0.002)	0.006*** (0.001)
Sick for > 14 days in past year	0.036** (0.017)	0.031*** (0.009)
F-statistic weak identification test	2.53	35.90
Observations	4,643	4,643

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. Expected retirement age and late retirement are instrumented with the treatment dummy. The dummy variable for intermediate vocational education and a dummy measuring whether one works in a sector other than the government, education, energy and public transportation, are used as reference categories.

Another potential factor 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 workers is adversely affected by their employer who reneges on pre-existing arrangements (violates an implicit contract) in ways that it is difficult to adjust to once one has taken those rules into account in one's plans. Workers born in 1950 face this new situation with relatively short notice, too short to completely offset the change in the system with additional savings. 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 January 1, 1950, came as a surprise when announced in July 2005. The question is to what extent the treated workers in our sample perceived the pension reform as unfair and whether they respond to the shock in their pension rights in a negative reciprocal way.

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, but also by the difference between one's own income and the income of a reference group (see for an overview, Clark et al., 2008b). Ferrer-i-Carbonell (2005) found that the income of the reference group is about as important as own income for individual happiness. Moreover Calvo et al. (2009) examined the factors that affect individual happiness in the transition to retirement and showed that it is relevant whether people perceive the transition from work to retirement as chosen or forced. These authors suggest that it is the sense of control over their own retirements that influences the happiness of older workers. It appears 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 as the longer period before retirement allows them to better compensate for their loss of pension wealth.

4.4 Conclusion

The pension reform that was implemented in 2006 induced a sharp discontinuous treatment of pensions rights of cohorts born around January 1, 1950. We exploited this discontinuity to measure the effect of changes in pension rights on mental health and found that the reform has a strong impact on the mental health of workers who

are affected by the reform. Our analysis reveals that those who by chance are exposed to a pension reform that confronts them with substantially lower pension wealth have higher depression rates. This effect persists over time and grows stronger the closer one is born to the threshold date. Furthermore, we find differing effects for different types of workers. For instance, the effects are stronger for unmarried workers and negligible for workers whose partner has a pension or an income. Our data do not allow us to give a definitive answer to the questions of why the depression rate of the affected group is so much higher and why this effect persists over time. We find that later expected retirement is important for mental health, but other factors are also at work. The discontinuous assignment rule and the strong differential treatment of workers born around January 1, 1950, is most likely perceived to be unfair, especially because it was announced only a few years before the retirement date of the affected workers. 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; this 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 toward extending working life and to 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 either to financial crises or to 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 it is 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 it will also engender costs associated with depression and worse physical health. As mentioned, health care expenditures of depressed persons are about four times higher than those of non-depressed individuals. Moreover, there are high indirect costs due to loss of productivity, flawed decision making, and workplace accidents (OECD, 2008). Governments should take these effects and costs into account when redesigning pension policies.

Appendix

Table 4.6: Treatment effect on mental health (Poisson regressions on CES-D8 score)

Dependent Variable: CESD-8 score	I	II
Treatment dummy	0.066*** (0.021)	0.093*** (0.034)
Age (in days /100)		0.012 (0.008)
Marital status		-0.182*** (0.036)
Lower secondary education		0.017 (0.034)
Higher secondary education		0.016 (0.051)
Higher education		0.040 (0.029)
Government		-0.013 (0.055)
Education sector		-0.007 (0.056)
Energy, public transportation		-0.005 (0.059)
Physically demanding work		0.020 (0.025)
Mentally demanding work		0.206*** (0.018)
Fraction of non-routine tasks		-0.099** (0.044)
Yearly wage (in logs /100)		-15.650*** (4.102)
Years contributed to pension (/100)		0.169 (0.139)
Pension rights at other pension funds		0.015 (0.029)
Partner has pension or income		-0.092*** (0.020)
Excess housing wealth		-0.009 (0.021)
Inheritance		-0.042 (0.026)
Missing info on other pension		0.096** (0.046)
Partner info missing		-0.123*** (0.035)
Excess housing wealth missing		0.000 (0.042)
Info on inheritance missing		-0.001 (0.036)
Self-reported general health		0.214*** (0.013)
Self-reported work limitations		0.285*** (0.033)
Number of doctor visits in past year		0.013*** (0.003)
Sick for > 14 days in past year		0.142*** (0.026)
Observations	5,195	4,765

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The table shows marginal effects. Standard errors in parentheses. The dummy variable for intermediate vocational education and a dummy measuring whether one works in a sector other than the government, education, energy and public transportation, are used as reference categories.

Table 4.7: Persistency of the treatment effect on mental health (depression indicator; CES-D8 ≥ 4): results based on the 2009 wave

Dependent Variable: Depression			
	I	II	III
Treatment dummy)	0.012** (0.005)	0.022** (0.011)	0.021** (0.010)
Age (in days /100)		0.003 (0.003)	0.003 (0.002)
Marital status		-0.038*** (0.010)	-0.038*** (0.010)
Lower secondary education		-0.014 (0.010)	-0.014 (0.010)
Higher secondary education		-0.001 (0.015)	0.003 (0.015)
Higher education		0.001 (0.009)	-0.001 (0.009)
Government		-0.018 (0.015)	-0.020 (0.014)
Education sector		-0.003 (0.015)	-0.013 (0.014)
Energy, public transportation		-0.022 (0.016)	-0.028* (0.016)
Physically demanding work			0.019*** (0.006)
Mentally demanding work			-0.011 (0.009)
Yearly wage (in logs /100)		-0.039*** (0.012)	-0.018 (0.012)
Years contributed to pension (/100)		-0.000 (0.000)	-0.000 (0.000)
Self-reported general health			0.043*** (0.004)
Self-reported work limitations			0.070*** (0.008)
Number of doctor visits in past year			0.004*** (0.001)
Sick for > 14 days in past year			0.021*** (0.008)
Observations	6,021	5,419	5,337
R-squared	0.001	0.008	0.095

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. The dummy variable for intermediate vocational education and a dummy measuring whether one works in a sector other than the government, education, energy and public transportation, are used as reference categories.

5 De-motivating Workers: The Role of Negative Reciprocity

5.1 Introduction

This chapter analyzes how negative reciprocal inclinations to the retrenchment in pension rights affect job effort.¹ Experimental economists and psychologists have provided ample evidence showing that reciprocity influences decision making in controlled environments (Schmidt and Fehr, 2000).² Reciprocity is an in-kind response to friendly actions or hostile acts. It has been proven to have a significant impact on human behavior in stylized labor markets (e.g., Fehr et al., 1993; and Brown et al., 2004) but also in laboratory experimental settings (e.g., Falk and Zehnder, 2007; and Falk, 2007). A distinction can be made between negative reciprocity, which indicates retaliatory tendencies, and positive reciprocity, which relates to cooperative reciprocal behavior. Theory predicts that positively reciprocal employees increase their effort above the required level when being treated generously by their employer (see e.g., Akerlof, 1982; and Rabin, 1993). Negative reciprocity is hypothesized to lead workers to retaliate, for example, by reducing their effort, when treated unfairly.³ Despite strong laboratory evidence showing that reciprocity is a key driver of human motivation in stylized labor markets, it is important to evaluate the importance of reciprocity in a real labor market setting, in which confounding factors and constraints can undermine the behavioral relevance of reciprocal inclinations.

Field studies that empirically assess the economic relevance of reciprocity in a specific labor market context are scarce. A field study by Krueger and Mas (2004) showed that a labor strife at a U.S. tire production site coincides with the production of substantially lower quality tires, arguably a result of workers reducing effort during the strife, suggesting that the strife induced workers to respond in a negatively reciprocal way.

The key advantage of our study is that we have data from a natural experiment that enables us to compare the response of more negative reciprocal workers who are treated unfairly to treated workers who are less reciprocal and to a control group that is not treated. In particular, we observe how reciprocity affects a workers' job motivation, a key determinant of work effort (see Bowles et al., 2001). We

¹This chapter is based on joint research with Frank Cörvers, Andries de Grip and Thomas Dohmen.

²Numerous studies have shown that individuals reciprocate trust in trust games (Berg et al., 1995). Moreover, it has been documented that individuals (in bargaining games) are willing to reject unfair offers even at a personal cost (e.g., Güth et al., 1982; Camerer and Thaler, 1995), and that individuals who participate in public good games are prepared to punish others who violated certain norms of cooperation or fairness (e.g., Fehr and Gächter, 2000; Carpenter and Seki, 2010).

³Ederer and Fehr (2007), for example, found that subtle forms of deceit undermine the effectiveness of effort provision in a dynamic tournament setting.

exploit exogenous variation in pension rights of Dutch public sector employees that is brought about by the major 2006 reform of the pension system which treats two very similar groups of employees differently. The legislative change curtailed the pensions rights of those born in 1950 (and later), but did not change the pension benefits of employees born in 1949 (and earlier). When workers born in 1950 compare their pension rights to the rights of those who are only slightly older and still enjoy the more generous pension rights of the old regime, or compare their pension rights to their rights before the policy change, they are likely to perceive the policy change as an unfair treatment.

Since the government initiated the change, public sector employees born in 1950 may perceive their employer to be directly responsible for the retrenchment of their pension rights, which entails both a breach of an informal agreement and unfair treatment relative to their slightly older colleagues. The abolishment of the favorable tax treatment was not limited to the public sector but did also apply to workers in the private sector. However, the major difference with the private sector is, that in the public sector the national government is not only the employer in the sectoral bargaining process, but also the initiator of the pension reform. Moreover, it is relevant to notice that since the second half of the 1970s, new entrants in the public sector received the prospect of early retirement and high pension benefits. The pension provision was used as one of the means to attract new employees in the public sector. Hence, the drop in pension rights might be perceived as being more unfair by the public sector workers compared to those employed in the private sector.

We expect that workers' reactions to the 'unfair' treatment depends on their negative reciprocal inclinations and may therefore be strongly heterogenous. In particular, we expect that the treated employees (i.e., those who were born just after December 31, 1949) with strongly negatively reciprocal inclinations are less motivated for their job than treated workers with weakly negatively reciprocal inclinations or workers in the control group, who are slightly older (i.e., those born just before January 1, 1950) but otherwise similar.

We will test this hypothesis using matched survey and administrative pension fund data on male employees in the Dutch public sector who were born in 1949 or 1950. More specifically, we use the 2008 wave of the panel that includes information on employees' job motivation, retirement expectations and reciprocal inclinations. The latter are elicited from answers to six questions that measure positive or negative reciprocity. Perugini et al. (2003) have confirmed the behavioral validity of these questions in controlled laboratory experiments. We compare the level of job motivation of employees in the treatment group with job motivation of employees in

the control group, and assess whether the treatment effect depends on employees' degree of negative reciprocity. We find that the exogenous decrease in pension benefits is associated with a significant reduction in job motivation among negatively reciprocal employees. Job motivation is the lowest for treated individuals with strongly negatively reciprocal inclinations. Furthermore, we find that negatively reciprocal employees who were born in the first quarter of 1950 have the lowest motivation, indicating that these workers perceive the policy change as particularly unfair. Moreover, job motivation is lower among negatively reciprocal persons who work at the central government that developed the policy change and therefore can directly hold their employer responsible for the retrenchment of their pension rights.

The chapter is organized as follows. In section 5.2, we provide more information on the data, sample selection and descriptive statistics. Results are presented in Section 5.3, and Section 5.4 concludes.

5.2 Data

In this chapter, we rely on data from the second wave of the survey, which was held in March 2008 and includes 6,078 employees of the public sector. In this wave, we asked detailed questions on reciprocal behavior, job motivation and retirement expectations.

As mentioned in Chapter 2, we excluded workers who are employed in some specific burdensome occupations (such as fire fighting, ambulance nursing and policemen) where other early-retirement schemes are still in place that allow them to retire early without experiencing a substantial drop in income. In our main analysis, we restrict the sample to those employees who had continuously worked in the public sector since 1997 (thereby excluding 260 employees, who are not eligible for the pre-reform early retirement option even if they were born before 1950). In a robustness analysis, however, we will include the workers who did not work continuously in the public sector since 1997. Due to item-non-response on the variables of interest, the estimation sample is further reduced. It contains 4,397 men of whom 2,311 were born in 1950 from the treatment group and 2,086 who were born in 1949 belong to the control group.

The dependent variable in our econometric specifications is a measure for job motivation, which is based on a 5-level Likert item that asks respondents to indicate how well the following statement applies to them personally:

- 1) *How well does the following statement apply to you? At times, I have difficulties to motivate myself for my job.*

Answer categories ranged from 1 ('does perfectly apply to me') to 5 ('does not apply to me at all'). Our reciprocity indicators are based on a reciprocity scale which was developed and validated by Perugini et al. (2003). They performed comprehensive validation tests in order to investigate the predictive power of their reciprocity scale for behavior in ultimatum and dictator games and considered in detail the relationship of their scale with other personality measures. We use the six items that have the highest loadings on the principal components that measure positive and negative reciprocity in their validating studies for the UK and Italy (Perugini et al., 2003).⁴ Respondents had to indicate on a 5-point scale (1 means 'does not apply to me at all' and 5 means 'does perfectly apply to me') how well they identified themselves with each of the following six statements:

- 1) *If someone does me a favor, I am prepared to return it.*
- 2) *If I suffer a serious wrong, I will take revenge as soon as possible, no matter what the costs.*
- 3) *If somebody puts me in a difficult position, I will do the same to him/her.*
- 4) *I go out of my way to help somebody who has been kind to me before.*
- 5) *If somebody offends me, I will offend him/her back.*
- 6) *I am ready to undergo personal costs to help somebody who helped me before.*

Statements (2), (3) and (5) refer to negative reciprocity. Statements (1), (4) and (6) refer to positive reciprocity. We construct our measures of positive and negative reciprocity by taking the arithmetic average of a respondent's answers to questions (2), (3), (5) and (1), (4), (6), respectively.

A relevant concern is how well these survey questions measure reciprocal inclinations of the individuals in our sample. Various factors such as strategic motives, self-serving biases or lack of attention could induce respondents to distort or unintentionally miss-report their true reciprocal behavior (Camerer and Hogarth, 1999). We are confident, however, that our measures are valid indicators of reciprocity, albeit measured with error, for the following reasons. First, our reciprocity measures are experimentally validated. Second, previous research has demonstrated the validity of survey questions on related social preferences, e.g., 'trust' (see Fehr et al., 2003; Bellemare and Kröger, 2007; and Falk and Zehnder, 2007). Third, Dohmen et al.

⁴These 6 items were also included in the 2005 wave of the German *Socio-Economic Panel Study* (SOEP), and were used in the study by Dohmen et al. (2009).

Table 5.1: Descriptives

Variables	All		Born	Born	T-stat
	Mean I	St. Dev II	in 1950 III	in 1949 IV	
Take revenge for a serious wrong	3.06	1.04	3.06	3.06	0.27
Retaliate for being put in a difficult position	2.54	0.85	2.54	2.54	0.14
Reciprocate insult with an insult	2.60	0.91	2.62	2.60	0.19
Reciprocate a favor	4.29	0.64	4.27	4.31	1.76
Exert effort to help somebody who is kind	4.11	0.62	4.11	4.11	0.25
Undergo personal costs to help someone who was helpful	3.73	0.70	3.72	3.73	1.43
Negative reciprocity (averaged)	2.73	0.79	2.73	2.74	0.05
Positive reciprocity (averaged)	4.04	0.51	4.04	4.05	1.53
Expected retirement benefit at age 62	69.41	11.75	67.00	72.10	16.07
Extra savings for pension since 2006	0.25	0.43	0.27	0.22	3.83
Yearly wage (in euros)	53,132	16,420	53,131	53,132	0.94
Organization size (in logs)	7.10	1.77	7.09	7.11	0.41
Marital status	0.92	0.28	0.91	0.92	1.75
Self-reported general health	2.06	0.72	2.05	2.07	0.80
Number of observations	4,397		2,311	2,086	

(2009) show that the survey measures of reciprocity that we employ in this chapter are correlated with behavioral outcomes in a way that is consistent with theoretical predictions.⁵

Table 5.1 presents descriptive statistics for the entire sample (Column I), and separately for the treatment group (Column III) and the control group (Column IV). We do not observe significant differences in the average responses to each of the six different reciprocity measures between the treatment and control groups, indicating that the change in pension rights did not affect social preferences. The sample averages for the three items that measure negative reciprocity range from 2.6 to 3.1 and are smaller than the averages for the items measuring positive reciprocity (4.3 to 3.7). A substantial number of respondents reports that the statements on positive reciprocity apply to them perfectly, while respondents identify themselves on average less with the statements on negative reciprocity. The variance within the negative reciprocity measures is larger than within the positive reciprocity measures.⁶ In Table 5.1, we also report summary statistics for our two reciprocity measures that are constructed by averaging agreement with the three statements concerning positive and negative reciprocity respectively. Again, there are no differences in reciprocal

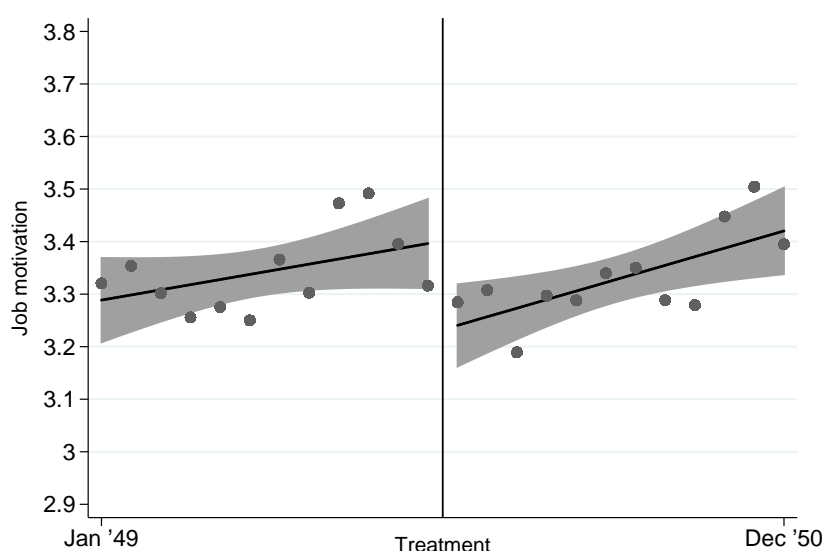
⁵Dohmen et al. (2009) relate the six measures of reciprocal inclinations to actual labor market outcomes of respondents in the *German Socio-Economic Panel Study* (SOEP), and find that positive reciprocity is significantly correlated with higher wages and working harder while negative reciprocity is correlated with a higher chance of being unemployed. While their evidence is suggestive of a causal relation they can not rule out reverse causality, that is, a relation running from outcomes to self-assessed reciprocal inclinations.

⁶Reassuringly, these patterns of the distribution of answers to the six reciprocity questions for the entire sample show similar patterns as for the respondents in the SOEP (see Dohmen et al., 2009).

behavior between the treatment and the control group, according to these measures. There are also no large between-group differences in other attributes that will be used in our analyses below, such as annual wage income, extra pension savings in the past year, number of years in which workers have built up their pension, marital status, self-reported health status, educational attainment, and the sub-sector of employment.

Figure 5.1 plots predicted values from linear regressions for job motivation across birth months along with a 95%-confidence interval. The figure clearly shows a negative relationship between the age of workers and their job motivation. But more importantly, the figure shows that there is a drop in the mean level of job motivation for workers who were born just after 1949, suggesting a causal impact of the retrenchment of pension rights on the level of job motivation.

Figure 5.1: Job motivation



The data points present the mean of the job motivation for each birth month from January 1949 to December 1950. Job motivation is based on the following 5-level Likert item: *At times, I have difficulties to motivate myself for my job*. Answers categories ranged from 1 ('does perfectly apply to me') to 5 ('does not apply to me at all'). Furthermore, fitted lines and confidence intervals are calculated based on individual information. Our sample consists of two birth year cohorts, where employees born in 1949 are entitled to the old pension rules and those born in 1950 are subject to the new pension rules. The vertical line marks the threshold dividing the control group from the treatment group.

5.3 Estimation results

To investigate whether the retrenchment in pension rights may trigger systematically stronger reductions in job motivation among more negatively reciprocal employees, we start our analysis by estimating the impact of unfair treatment on job motivation

for the different quartiles of the distribution of negative reciprocity. To this end, we compare the job motivation of employees in the control group (i.e., those born in 1949) to the job motivation among employees in the treatment group (i.e., those born in 1950) for each of the four quartiles of the distribution of negative reciprocity. We estimate ordered probit models in which we relate job motivation to a set of control variables (including our measures of negative and positive reciprocity, annual wage income, extra pension savings in the past year, number of years in which workers have built up their pension, marital status, self reported health status, educational attainment, and the employment sub-sector) and our variable of interest: a treatment dummy that takes the value 1 if the worker was affected by the retrenchment in pension rights (i.e., born in 1950).

The four columns of Table 5.2 summarize the results of these four estimations. Job motivation is always lower for employees in the treatment group. We also find that negative reciprocity generally reduces job motivation of all workers significantly, while positive reciprocity has no significant impact. Moreover, employees with bad health and those who increased their pension savings in the past year are less motivated while job motivation is positively correlated with wage income and being married.

The most important insight of Table 5.2 comes, however, from a comparison of the coefficient of the treatment dummy across columns. This comparison shows that the treatment effect is heterogeneous with respect to reciprocal behavior: The drop in job motivation is the greatest and statistically significant among treated workers in the upper quartile of the negative reciprocity distribution (see Column I), and lowest among the least negatively reciprocal treated workers (see Column IV). These results indicate that the lower job motivation of those who were born in 1950 as shown in Figure 5.1 is primarily due to the response of the most negatively reciprocal workers.

5.3.1 Basic results

Building on the result that the effect of the retrenchment of pension rights is heterogeneous with respect to negative reciprocal inclinations, we next pool the data and estimate an ordered probit model that includes interaction terms between the measures of reciprocity and the treatment dummy. In this model, we include the same controls as in the ordered probit models presented in Table 5.2.

Table 5.2: Treatment effect on job motivation across quartiles of the distribution of negative reciprocity

Dependent Variable: Job motivation	75-100% I	50-75% II	25-50% III	0-25% IV
Treatment dummy	-0.350*** (0.102)	-0.058 (0.084)	-0.131 (0.087)	-0.029 (0.091)
Negative reciprocity	-0.106*** (0.030)	-0.099*** (0.031)	-0.095*** (0.031)	-0.101*** (0.030)
Positive reciprocity	0.020 (0.044)	0.038 (0.042)	-0.010 (0.043)	0.036 (0.040)
Extra pension savings since 2006	-0.080 (0.051)	-0.086* (0.048)	-0.050 (0.049)	-0.041 (0.049)
Years contributed to pension	-0.002 (0.004)	-0.002 (0.004)	0.000 (0.004)	-0.001 (0.004)
Yearly wage (in logs)	0.324*** (0.117)	0.273** (0.107)	0.264** (0.110)	0.382*** (0.110)
Organization size (in logs)	-0.007 (0.019)	-0.012 (0.019)	-0.018 (0.019)	-0.014 (0.019)
Marital status	0.009 (0.082)	0.062 (0.074)	0.012 (0.078)	0.112 (0.075)
Self-reported general health	-0.366*** (0.030)	-0.421*** (0.029)	-0.398*** (0.029)	-0.381*** (0.029)
Age (in days / 100)	-0.038* (0.020)	-0.013 (0.019)	-0.041** (0.019)	-0.025 (0.019)
Observations	2,444	2,806	2,685	2,720

*** < 0.01, ** < 0.05, * < 0.10. Standard errors are in parentheses. Ordered probit estimates. The treatment effect on job motivation is estimated for four different quartiles of the distribution of negative reciprocity. The measure for negative reciprocity is constructed using the average of three underlying questions on negative reciprocity. Columns I to IV contain estimates for workers in descending order of negative reciprocity. Thus the workers that have been selected for the regression analysis presented in Column I exhibit strongly negative reciprocal behavior. Additional control variables are: educational levels; sub sector fixed effects.

Our coefficient of interest is the coefficient on the interaction between negative reciprocity and the treatment dummy. This coefficient captures differential responses in job motivation by treated workers depending on their negatively reciprocal inclinations. Table 5.3 shows that the coefficient of the interaction term between the treatment variable and our indicator for negative reciprocity is negative and significantly different from zero, indicating that the negative treatment effect is significantly stronger for the negatively reciprocal workers. The interaction between positive reciprocity and the treatment variable has no effect on the level of job motivation, while coefficients of the other control variables are similar to those in Table 2. Column II of Table 5.3 shows that our key result, namely that job motivation is most reduced among treated workers who exhibit strongly negatively reciprocal behavior, is robust to the estimation technique. OLS estimates lead to the same conclusion.⁷

The results of further robustness checks shown in Table 5.4 buttress our findings. In this analysis, we include workers with career breaks after April, 1997. Column I contains the estimation results for the full 1949 sample. The treatment dummy equals 1 for workers who were born in 1949 and who are not entitled to the old pension rights, and is coded 0 for all other workers born in 1949. The estimation results show that the interaction between the treatment dummy variable and negative reciprocity is negative and significantly different from zero. Column II contains the estimation results for the full 1949 and 1950 sample, and includes two treatment dummy variables. The first treatment dummy equals 1 for workers born in 1949 and who are not entitled to the old pension rights, and is coded 0 otherwise. The second treatment dummy equals 1 if workers were born in 1950 and equals 0 for those born in 1949. The estimation results show that both interactions between the treatment dummy variables and negative reciprocity are negative and significantly different from zero. The negatively reciprocal workers of the 1949 treatment group appear to be slightly more de-motivated than those who are in the 1950 treatment group. However, the difference in size of the two coefficients is insignificant at the 5%-level.

⁷In additional robustness checks we investigated whether our results are sensitive to the construction of our reciprocity measures. We estimated ordered probit models including alternative measures of negative and positive reciprocity which are constructed based on principal component analysis on the six underlying items, and find that the interaction effect between negative reciprocity and the treatment remains highly significant.

Table 5.3: Negative reciprocity, treatment and job motivation

Dependent Variable: Job motivation	Ordered probit I	OLS II
Interaction treatment and negative reciprocity	-0.108*** (0.041)	-0.109** (0.043)
Interaction treatment and positive reciprocity	-0.043 (0.064)	-0.035 (0.066)
Negative reciprocity	-0.102*** (0.031)	-0.099*** (0.031)
Positive reciprocity	0.033 (0.047)	0.018 (0.049)
Treatment dummy	0.333 (0.276)	0.294 (0.283)
Extra pension savings in previous year	-0.136*** (0.038)	-0.138*** (0.039)
Years contributed to pension	-0.004 (0.003)	-0.004 (0.003)
Yearly wage (in logs)	0.242*** (0.085)	0.215** (0.087)
Organization size (in logs)	0.002 (0.014)	0.005 (0.015)
Marital status	0.100* (0.058)	0.109* (0.060)
Self-reported general health	-0.394*** (0.023)	-0.403*** (0.023)
Age (in days /100)	-0.032** (0.015)	-0.033*** (0.016)
Constant		3.316*** (1.150)
Observations	4,397	4,397

*** < 0.01, ** < 0.05, * < 0.10. Standard errors are in parentheses. The measures of negative and positive reciprocity used in the estimations are constructed by taking the average of the three underlying items. Column I presents results which are based on ordered probit estimates. Column II presents OLS coefficient estimates. Additional control variables in the estimations are: educational levels; sector fixed effects.

Table 5.4: Treatment effect on job motivation: workers with career breaks

Dependent Variable: Job motivation	1949 and 1950	
	I	II
Interaction treatment 1949 and negative reciprocity	-0.269** (0.115)	-0.244** (0.114)
Interaction treatment 1949 and positive reciprocity	0.064 (0.152)	0.061 (0.151)
Interaction treatment 1950 and negative reciprocity		-0.104** (0.041)
Interaction treatment 1950 and positive reciprocity		-0.040 (0.064)
Negative reciprocity	-0.095*** (0.031)	-0.102*** (0.031)
Positive reciprocity	0.020 (0.048)	0.033 (0.047)
Treatment dummy 1949	0.622 (0.656)	0.520 (0.653)
Treatment dummy 1950		0.325 (0.275)
Extra pension savings in previous year	-0.041 (0.054)	-0.145*** (0.037)
Years contributed to pension	-0.002 (0.004)	-0.004* (0.002)
Yearly wage (in logs)	0.290** (0.118)	0.211** (0.082)
Organization size (in logs)	-0.028 (0.021)	-0.002 (0.014)
Marital status	0.031 (0.085)	0.111* (0.057)
Self-reported general health	-0.390*** (0.032)	-0.393*** (0.023)
Age (in days /100)	-0.015 (0.021)	-0.027* (0.015)
Observations	2,240	4,593

*** < 0.01, ** < 0.05, * < 0.10. Ordered probit estimates. Column I presents estimation results for workers who are born in 1949. The treatment dummy equals 1 for workers who are not entitled to the old pension rights, since they did not work in the public sector continuously since April, 1997, and equals 0 for workers who remain entitled to the old pre-pension rights. Column II contains estimation results for the 1949 as well as the 1950 cohort. The model includes two treatment dummy variables. The first treatment dummy equals 1 for workers who were born in 1949 and who are not entitled to the old pension rights and is coded 0 otherwise. The second treatment dummy equals 1 if workers were born in 1950 and equals 0 for those born in 1949. Additional control variables in estimations are: educational levels; sub sector fixed effects. Standard errors are in parentheses.

5.3.2 Perceived unfairness of the policy change

Until now, we have implicitly presupposed that the perceived unfairness brought about by the retrenchment in pension rights is the same among all treated workers. However, there might be differences, and if we could identify these differences in perceptions, we would expect that those who feel to be treated most unfairly among the negative reciprocal react stronger to the policy change. It is plausible to assume that workers who were born only shortly after the treatment threshold perceive the retrenchment of their pension rights as more unfair. They compare their pension rights to the rights of those who are born just a few days earlier but still enjoy the more generous pension rights of the old regime. Accordingly, we expect that strongly negatively reciprocal workers in this specific group will be more de-motivated than workers who were born later in 1950.

We test this conjecture by comparing job motivation of workers born in different quarters in 1950.⁸ The treatment group in Column I of Table 5.5 consists of workers who were born in the first quarter of 1950 while the group in Column II consists of workers born in the second, third or fourth quarter of 1950. The table confirms our expectation that negatively reciprocal workers who were born on or just after January 1, 1950, are stronger de-motivated than workers who were born later in the year. The coefficient of the interaction term between negative reciprocity and the treatment variable is substantial and significant in Column I, while the coefficient is smaller and only marginally significant in the next column, indicating that workers who were born shortly after the treatment threshold perceive the treatment as more unfair. Observing that the drop in job motivation is larger the more negative reciprocal a worker is and the more unfair the perceived treatment is, is suggestive of a causal impact of reciprocity on the size of the treatment effect. A plausible mechanism that entails this result is that workers who are negatively affected retaliate against their employer by providing less effort.

5.3.3 The employer-employee relation

Clearly, we should expect this response to be stronger, the more workers hold their employer responsible for their adverse treatment. One of the reasons of our focus on

⁸We also checked, whether the effect of the interaction between the treatment dummy and negative reciprocity can be attributed to the seasonality of birth, by performing additional estimations on a sample of workers born in the first quarter of 1949 or in the first quarter of 1950. We find that the interaction effect between negative reciprocity and the treatment dummy remains strongly significant and therefore seasonality of birth is not likely to be the main determinant of the significant interaction effect.

Table 5.5: Treatment effect on job motivation: results for different birth cohorts

Dependent Variable: Job motivation	I 1950 vs IV 1949 I	II-IV 1950 vs IV 1949 II
Interaction treatment and negative reciprocity	-0.165** (0.082)	-0.114* (0.069)
Interaction treatment and positive reciprocity	-0.134 (0.131)	0.033 (0.110)
Negative reciprocity	-0.081 (0.061)	-0.086 (0.061)
Positive reciprocity	-0.005 (0.098)	-0.005 (0.098)
Treatment dummy	0.861 (0.550)	0.136 (0.465)
Extra pension savings in previous year	-0.108 (0.077)	-0.230*** (0.052)
Years contributed to pension	-0.003 (0.005)	-0.006 (0.004)
Yearly wage (in logs)	0.175 (0.172)	0.204* (0.123)
Organization size (in logs)	0.002 (0.030)	0.022 (0.020)
Marital status	-0.084 (0.133)	0.147* (0.079)
Self-reported general health	-0.479*** (0.048)	-0.388*** (0.033)
Observations	1,092	2,143

*** < 0.01, ** < 0.05, * < 0.10. Standard errors are in parentheses. Ordered probit estimates. In Column I, workers born in the first quarter of 1950 are compared to workers in the control group who were born in the fourth quarter of 1949. Column II compares workers born in the second, third or fourth quarter of 1950 with those born in the fourth quarter of 1949. Additional control variables in both estimations are: educational levels; sub sector fixed effects.

the public sector is that employees in this sector are likely to hold their employer responsible for their deteriorating pensions, since the government, which is regarded as the corporate management of the public sector, initiated the pension reform by abolishing the favorable tax treatment. Yet, the extent to which employees hold their employer responsible might differ across the different sub-sectors in the Dutch public sector.⁹ Civil servants who work for the national government arguably most closely link the unfair treatment directly to their own employer, since it was the national government that initiated the policy reform. Consequently, we expect that the treatment effect is larger among negatively reciprocal workers in the government departments.

Therefore we estimate the impact separately for workers employed in the national government departments and workers in the remaining sub-sectors of the public sector. Columns I and II of Table 5.6 show the results of ordered probit estimates.

⁹The 15 sub-sectors are: national government departments, defence (only civilian personnel), provinces, municipalities, judiciary, primary and secondary education, intermediate vocational education, higher vocational education, universities, research and scientific policy sector, teaching hospitals, district water boards, water, energy and public utilities, voluntary members (including ABP and public transport) and a remaining category.

Table 5.6: Treatment effect on job motivation: heterogenous sector effects

Dependent Variable: Job motivation	National Government I	Other Sectors (II)
Interaction treatment and negative reciprocity	-0.249** (0.101)	-0.077* (0.046)
Interaction treatment and positive reciprocity	-0.054 (0.151)	-0.030 (0.071)
Negative reciprocity	-0.030 (0.075)	-0.115*** (0.034)
Positive reciprocity	0.078 (0.111)	0.011 (0.052)
Treatment dummy	0.674 (0.639)	0.221 (0.307)
Extra pension savings in previous year	-0.292*** (0.096)	-0.109*** (0.041)
Years contributed to pension	0.002 (0.007)	-0.005* (0.003)
Yearly wage (in logs)	0.292 (0.196)	0.286*** (0.089)
Organization size (in logs)	0.010 (0.029)	-0.007 (0.012)
Marital status	0.027 (0.131)	0.118* (0.065)
Self-reported general health	-0.348*** (0.055)	-0.405*** (0.025)
Age (in days /100)	0.070** (0.036)	0.024 (0.017)
Observations	811	3,586

Standard errors are in parentheses. *** < 0.01, ** < 0.05, * < 0.10. ordered probit estimates. Educational levels are included as control variables.

As expected, we find that the size of the coefficient of the interaction between the treatment term and our indicator for negative reciprocity is much larger for employees in the government departments. The coefficient in the analysis for those in the national government is approximately twice as large as the coefficient found in Column I of Table 5.3. Moreover, we find that the coefficient is strongly significant for the employees of the national government departments, while the coefficient is only marginal significant for workers in the other sub-sectors. Hence, negatively reciprocal workers who can directly associate the unfair treatment to the behavior of their own employer, retaliate stronger through a reduction of their job motivation.

5.3.4 Alternative indicator of productivity

As it is difficult to measure work effort directly, we have looked at job motivation as a proxy of work effort. Alternatively one could argue that reduced work effort should be reflected in reduced productivity and therefore pledge for an direct assessment of the input of negatively reciprocal individuals among the treated on productivity. It is difficult, however, to measure individual productivity in the public sector.

Table 5.7: Negative reciprocity, treatment and alternative productivity indicator

Dependent Variable: Depression	Probit I
Interaction treatment and negative reciprocity	0.011** (0.005)
Interaction treatment and positive reciprocity	0.004 (0.008)
Negative reciprocity	-0.005 (0.004)
Positive reciprocity	0.009 (0.006)
Treatment dummy	-0.027 (0.042)
Extra pension savings in previous year	0.007 (0.005)
Years contributed to pension	0.001 (0.000)
Yearly wage (in logs)	-0.008 (0.011)
Organization size (in logs)	0.001 (0.002)
Marital status	-0.031*** (0.011)
Self-reported general health	0.036*** (0.003)
Age (in days /100)	0.002 (0.002)
Constant	-3.970 (2.764)
Observations	4,287

*** < 0.01, ** < 0.05, * < 0.10. Standard errors are in parentheses. Educational levels and sub sector fixed effects are included as control variables.

In Chapter 4 we found that the unexpected drop in pension rights has a negative impact on workers' mental health. Workers in the treatment group are significantly more depressed than those who are born in 1949. Since depression is known to be a relevant determinant of a lower productivity, mistakes at work, faulty products as well as increased sickness absence (see OECD, 2008), the mental health variable may serve as a useful alternative indicator for a worker's productivity.¹⁰ Table 5.7 reports the estimation results of a probit regression on the depression indicator. We find that workers in the treatment group who are strongly negatively reciprocal are indeed more depressed (the coefficient of the interaction dummy equals 0.011 with a standard error of 0.005) than less negatively reciprocal workers. The fact that this

¹⁰In Chapter 4, we used the CES-D8 indicator of depression that consists of eight items of which six negatively phrased statements that reflect the presence of depressive symptoms and two positively phrased statements that reflect the absence of depressive symptoms. We dichotomized (yes/no) responses and reversed 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 constructed a dummy variable which indicates whether workers are depressed (score of 4 and above is coded 1).

effect is also found using this alternative indicator of productivity gives confidence to our estimation results on job motivation.

5.4 Conclusion

In this chapter, we have shown that negative reciprocity is an important determinant of job motivation. Using the exogenous shock in pension rights, we find that a decrease in pension rights is associated with lower job motivation among negatively reciprocal employees. Moreover, negatively reciprocal workers who were born in the first three months of 1950 are more de-motivated than those who were born later in 1950, plausibly because the former perceive the differential tax treatment as more unfair because their age hardly differs from the age of those who are unaffected by the reform. We also find that negatively reciprocal workers who are employed by the national government and therefore directly can associate the unfair treatment to the behavior of their own employer, have a lower job motivation than those who are employed in other sub sectors of the public sector. The results are robust to the use of alternative estimation methods or different indicators of productivity.

Our findings complement earlier experimental evidence. In accordance with an ultimatum game, the drop in motivation can be interpreted as the sanctioning of unkind or hostile actions (e.g., Güth et al., 1982; Camerer and Thaler, 1995). Our evidence shows that negatively reciprocal individuals do not only sanction actions which they perceive as unkind or hostile in laboratory settings, but behave in the same manner when they feel that they are treated unfairly by their employer. Consequently, the intended effects of pension reforms that aim to increase labor force participation can be distorted by the decreasing job motivation of negatively reciprocal workers who feel that they are treated unfairly. Therefore, it is crucial to consider reform designs that provide less scope for being perceived as unfair by particular groups. In the specific example of tax legislation that affects pension rights, an alternative design that entails less discontinuous differences in pension rights would arguably cause less disruption in terms of negatively reciprocal responses.

6 Subjective Well-being and Social Comparison

6.1 Introduction

In this chapter, we investigate the effects of the 2006 pension reform on workers' well-being and its underlying domains.¹ In particular, we analyze the extent to which social comparison of the treated workers to those who are not treated by the pension reform affects their reaction to the drop in their pension rights. We exploit both the 2008 and the 2009 waves of the panel dataset as described in Chapter 2. These two waves contain various indicators of well-being and administrative data of the public sector's pension fund. Comparing individual well-being of male workers born in 1949 or 1950 two and three years after the shock in the pension system, we find strong and persistent effects of the drop in pension rights on well-being and job satisfaction. The impact of the shock on the well-being of the workers in our sample is equivalent to a difference in the annual wage of about 40%. Both the size and the persistence of this impact can be explained as the result of social comparisons, since workers appear to compare their own replacement rate with the average replacement rate of similar individuals in their organization (c.f., Bender, 2004; DiTella and McCulloch, 2008; Montesoglu and Vendrik, 2009). However, our results show that a major part of the treatment effect is non-monetary. When we control for comparisons of replacement rates, we still find that treated workers suffer less from the reform when the share of the treatment group is larger in the organization where they are employed, and suffer more when the share of the control group is larger. Furthermore, we find that the satisfaction of the men in our sample is not only negatively affected by their own treatment, but also by the treatment of their spouse.

We contribute to the existing well-being literature in several ways. First, this study contributes to the recent literature on relative concerns in happiness (e.g., Ferrer-i-Carbonell, 2005; Luttmer, 2005; Clark et al., 2008b). In particular, we find significant and sizable effects on the job satisfaction of treated workers of both the percentage of treated employees and the percentage of untreated employees in the organization in which they work. Such social comparisons and the ensuing strong feelings of being unfairly treated can explain both the strikingly large difference in satisfaction that we find and the absence of indications of adaptation over time between the treatment and control group. The key advantage of our study is that unlike previous studies, we are able to avoid the problems of spurious correlations and reverse causality between the dependent variable and the regressors by exploiting the natural experiment on the drastic change in the retirement system.

¹This paper is based on joint research with Maarten Vendrik.

Second, this study contributes to the small number of studies on the effects of bad life shocks on well-being. Among others, Clark and Oswald (1994), Winkelmann and Winkelmann (1998), Clark et al. (2008a) have shown that unemployment and lay-offs significantly reduce well-being and found that there are significant lag and lead effects. Other studies focused on the relation between disability shocks and well-being (Oswald and Powdthavee, 2008), environmental shocks (Luechinger, 2009), divorce effects (Lucas, 2005; Gardner and Oswald, 2006) and the effects of winning the lottery (Gardner and Oswald, 2007). To our knowledge, this is the first study to document strong and persistent effects on individual well-being of altered future pension income caused by an exogenous pension reform.

Third, our results are relevant to the rather small literature that empirically analyzed the relation between retirement and well-being. Studies in this field have found conflicting results, partially because they did not correct for time-invariant individual heterogeneity which can give rise to spurious correlations between retirement and well-being and could not assess the direction of causality between retirement and well-being over time (e.g., Cavan et al., 1949; Barron et al., 1952; Thompson, 1973; Beck, 1982; Kim and Moen, 2002; Börsch-Supan and Jürges, 2006; Dave et al., 2006). An exception is the study of Charles (2004) who controlled for this by instrumenting retirement on the basis of discontinuous age-specific retirement incentives in the US social security system and changes in laws affecting mandatory retirement and social security benefits. He found compelling evidence that there is a direct exogenous positive effect of retirement on mental well-being after taking into account that retirement and well being are simultaneously determined. Opposite to Charles (2004) who focused on the actual transition into retirement, we show that there are strongly negative effects on individual well-being when workers have to adapt their expectations about retirement due to a negative exogenous shock in the pension system. Furthermore, the shock substantially compromises job satisfaction which can result in a decline in effort provision and individual productivity among the treated workers. Moreover, while Charles (2004) used the one-sided measures of depression and loneliness, this study uses answers to self-assessed questions on well-being and its underlying domains.

Fourth, our study builds on a recent and growing body of literature emphasizing the incidence of joint retirement among dual-career families and the increasingly importance of financial spillover effects between men and their spouses (Blau, 1997; Blau, 1998; Baker, 2002; Kim and Moen, 2002; Coile, 2004). It does so by examining whether a shock in the pension benefits of spouses negatively affects the well-being of our respondents.

This chapter proceeds as follows. Section 6.2 describes the data and Section 6.3 discusses our empirical strategy. Section 6.4 presents the results of the empirical analyses. Section 6.5 concludes the chapter.

6.2 Data

The survey data as described in Chapter 2 are available for three years. In this study, we rely on data from the second and the third wave of the panel, which were held in March 2008 and 2009, respectively. The second wave includes 6,078 respondents who completed the questionnaire. The number of respondents to the third wave (7,711 respondents) is higher due to the refreshment of the list of e-mail addresses.² We only use the second and the third wave since only these two waves contain information on subjective well-being. As mentioned in Chapter 2, the analysis is further restricted to full-time workers who have worked continuously in the public sector since 1997. For these workers, the pension reform is clear, since age is the only criterion that determines whether a worker belongs to the treatment group or the control group. Remember that those who did not work continuously are also no longer entitled to the old more generous pension rules. Hence, in principle, we have a second treatment group next to those who were born in 1950. However, we do not use this group in our analyses since it is very likely that unobserved characteristics that can have caused the career break since 1997 are also correlated with our indicators of subjective well-being.³ The final sample consists of 5,195 men in 2008 and 6,612 men in 2009.

Our empirical analysis is based on subjective, self-reported measures of well-being that were extracted from individual answers to life satisfaction questions of the survey. Psychologists and economists have made ample use of such questions in the past three decades. In particular, for measuring well-being, we make use of the following life satisfaction question which was asked in the surveys of 2008 and 2009:

²Panel attrition is predominantly caused by changes in e-mail-addresses. Therefore, in 2009 a new call to participate in the study was sent by mail to individuals whose e-mail address did not exist anymore. Moreover, in 2009 all individuals that did not react to the first call again received a request to participate in the survey and to give their e-mail address. Probit analyses that measure whether there are differences in the selection into the survey by the treatment or the control group for the years 2008 and 2009 show that the coefficient of the treatment dummy remains small and insignificant (coefficient is 0.0087 in 2008 and 0.0208 in 2009).

³This resulted in the exclusion of 260 observations from the 2008 wave of employees born in 1949 who were not eligible for the old pension rights. In 2009, 239 employees were excluded. These groups are not large enough to serve as an additional control group.

We would like to ask you about your satisfaction with your life. Please answer by using the following scale, in which 0 means totally unsatisfied and 10 means totally satisfied.

How satisfied are you at present with your life as a whole?

The reliability of this subjective well-being measure is lower than that typically found for education, income and many other microeconomic variables due to its discreteness and non-random measurement errors. However, previous studies have shown that the reliability of this measure remains sufficiently high to support much of the research that is currently being undertaken on subjective well-being, particularly in studies where group means are compared, that is, across activities or demographic groups (Frey and Stutzer, 2002a,b; Ferrer-i-Carbonell and Frijters, 2004; Luttmer, 2005; Krueger and Schkade, 2008).

Furthermore, we asked questions on separate domains of well-being, which gives us the advantage to observe how the shock in the pension system affects different aspects of well-being:

We would like to ask you about your satisfaction with your life. Please answer by using the following scale, in which 0 means totally unsatisfied and 10 means totally satisfied.

- 1) *How satisfied are you with your job?*
- 2) *How satisfied are you with your wage?*
- 3) *How satisfied are you with the number of hours you work per week?*
- 4) *How satisfied are you with the amount of leisure time you have?*
- 5) *How satisfied are you with your household income?*
- 6) *How satisfied are you with your pension wealth?*
- 7) *How satisfied are you with your health?*

Answers to the first three questions are available in both waves. Answers to the other four questions are only available in the 2009 wave.

In addition to the questions on well-being, we gathered detailed information in the 2009 wave on the spouses of the male public sector employees in our sample. Among others, we asked questions on the education level and own income of the spouse, actual work hours, and the birth year of the spouse. The last question introduces the possibility to explore not only how the well-being of the male workers in our sample is affected by the shock in their own pension wealth, but also to analyze whether the treatment of someone's partner reduces their well-being. We will later come back to this issue.

6 Subjective Well-being and Social Comparison

Table 6.1 presents descriptive statistics of the 2008 and 2009 wave for the entire sample, the treatment group and the control group. Columns V and X give the results of a simple t-test for equality of the means of a variable for those born in 1949 or 1950. The table shows that, on average, workers in the public sector are reasonably satisfied with their life. The mean score on the life satisfaction question in 2008 was 7.66, with a standard deviation of 1.26. In 2009, the score was somewhat higher (7.82 with a standard deviation of 1.18). Concerning the individual domains of well-being, all scores on these aspects are lower than the score on the life satisfaction question. Furthermore, we can observe that workers are, in general, less satisfied with their wage and their pension wealth, while they are more satisfied with their household income and health.

The table shows that there are small differences in the scores on our measure for well-being between the group of workers who can still retire under the old rules and those with reduced pension rights. In both 2008 and 2009, the score of the life satisfaction variable is, on average, 0.10 lower for those born in 1950. The t-tests show that this difference between the two groups is significant at the 1%-level. Hence, this exploratory analysis provides some preliminary evidence of a treatment effect. In first instance, the difference may seem marginal, but when we compare the size of the treatment effect in the next section with the effects of having a higher income on well-being, it will turn out that the effect is sizable. When we look at the underlying domains of well-being, we observe that all seven scores are lower for those who were born in 1950. T-tests, however, show that the differences between the control and the treatment group are only significant for four of the seven domains: job satisfaction (in 2009), satisfaction with hours worked and spare time, and satisfaction with one's pension wealth.

These differences in average satisfaction between the treatment and the control group are arguably the result of shifts in the underlying distributions of the individual satisfaction scores towards lower scores for the 1950 group. In particular, in the sample of 2009, the percentage of poor well-being scores (i.e., score below 6) is 4.7 for birth year 1950, versus 3.3 for birth year 1949, while the percentage of high well-being scores (i.e., score of 7 or higher) is 68.8 for those born in 1950, versus 71.8 for birth year 1949. Thus, among those born in 1950, there are more unhappy and fewer very happy men.

Table 6.1: Descriptives

Variables	2008				2009			
	Mean	St. Dev	Born in 1950	Born in 1949	Mean	St. Dev	Born in 1950	Born in 1949
	I	II	III	IV	VI	VII	VIII	IX
Well-being								
Well-being	7.66	1.26	7.61	7.71	7.82	1.18	7.77	7.87
Job satisfaction	6.72	1.83	6.69	6.75	7.02	1.66	6.96	7.09
Satisfaction with wage	6.45	1.90	6.45	6.45	6.88	1.69	6.84	6.93
Satisfaction with hours worked	6.81	1.88	6.73	6.90	3.26	1.80	6.83	6.99
Satisfaction with leisure time	NA	NA	NA	NA	NA	1.81	7.02	7.24
Satisfaction with household income	NA	NA	NA	NA	7.31	1.58	7.29	7.34
Satisfaction with pension wealth	NA	NA	NA	NA	6.66	1.70	6.47	6.87
Satisfaction with health	NA	NA	NA	NA	7.22	1.83	7.19	7.25
Retirement expectations								
Expected retirement benefit at age 62	69.41	11.75	67.00	72.10	65.72	12.49	63.38	68.40
Expected retirement benefit at age 63	NA	NA	NA	NA	73.19	12.90	69.92	76.92
Expected retirement benefit at age 65	NA	NA	NA	NA	81.42	13.69	81.10	81.80
Expected retirement age	62.00	2.58	62.11	61.88	63.27	17.00	63.45	63.06
Job characteristics and wealth								
Yearly wage (in logs)	10.83	0.28	10.83	10.84	10.83	0.29	10.83	10.84
Years contributed to pension	32.15	6.32	31.45	32.86	32.98	6.59	32.41	33.64
Investments in Life Course Savings program	NA	NA	NA	NA	0.10	0.30	0.14	0.05
Contractual working hours	0.99	0.05	0.99	0.99	0.99	0.06	0.99	0.99
Other personal characteristics								
Marital status	0.92	0.28	0.91	0.92	0.91	0.28	0.90	0.92
Lower secondary education	0.12	0.31	0.12	0.13	0.12	0.32	0.12	0.12
Higher secondary education	0.04	0.19	0.04	0.04	0.04	0.20	0.04	0.04
Intermediate vocational education	0.15	0.34	0.15	0.15	0.16	0.36	0.16	0.16
Higher education	0.69	0.49	0.69	0.68	0.77	0.48	0.67	0.68
Number of doctor visits in past year	2.12	2.53	2.13	2.12	2.24	2.75	2.20	2.28
Number of observations	5,195		2,724	2,471	6,612		3,529	3,083

6.3 Empirical strategy

We make use of a natural experiment to identify the causal effect of the shock in pension rights on well-being. To measure the treatment effect, we estimate random effects models of the following form:⁴

$$LS_{it} = \alpha_i + \beta D_{it} + \zeta X_{it} + a_i + u_{it} \quad (6.1)$$

Here, LS stands for well-being and D for the treatment dummy which is 0 if the employee was born in 1949 and 1 if born in 1950. X is a vector of control variables, a refers to random effects and u is the error term.

The choice for using random effect models is given by the necessity to account for unobserved heterogeneity by including random effects.⁵ We can safely assume that the coefficient of the treatment variable is not biased by unobserved individual heterogeneity, such as individual reference levels for well-being, because of the experimental set up of this study. After all, the coefficient of the treatment dummy and the assignment to the treatment or control groups is exogenous, and as good as random. However, this does not hold for relevant control variables, such as marital status, the number of years contributed to the pension fund, and the yearly wage. Although we are primarily interested in the coefficient of the treatment variable, it is relevant for our understanding of the impact of the pension shock on well-being, and its underlying domains, to compare the size of the coefficient of the treatment dummy more closely to unbiased correlations for these other variables.

Furthermore, the random effects model treats well-being as a cardinal construct. There are two reasons why we use this type of model: i) Panel estimation uses the

⁴We can only perform random effects estimations on the variable measuring well-being and the first three domains of well-being since these variables are available in both waves. Because the other four domains are only available in the 2009 wave, we perform simple OLS regressions on these domains.

⁵The random effects model includes the strong assumption that the error terms are random and not correlated with the observable variables. To address this issue, various studies have included so called Mundlak terms (Mundlak, 1978). The benefit of Mundlak terms is that the correlation between the unobservables and the observables acts only through the time averages of the observable variables, while the remaining part of the unobserved effect is assumed to be independent of the observables (see Buligescu et al. (2009) for more details on this issue). We have also estimated models including these Mundlak terms (results are included in the Appendix). However, since the coefficients of the control variables are difficult to interpret in these specifications and the coefficient of the treatment dummy is not affected by the inclusion of the Mundlak terms, we proceed by showing the results from standard random effect models.

whole sample, rather than a reduced sample in models that deals explicitly with the ordinal character of the dependent variable. ii) The results of linear models are more intuitive and easier to interpret. Also, cardinal and ordinal analyses of well-being yield, in general, similar results (Ferrer-i-Carbonell and Frijters, 2004). Consistent with the existing literature, we find that the estimates of ordered probit models including random effects do not differ much from those in our main analyses (see Table 6.11 of the Appendix).⁶

6.4 Estimation results

Table 6.2 shows the results of random effect models of well-being on the shock in pension rights. The first column of Table 2 contains the results of a base specification, where, apart from the indicator for the reform, no other variables are included. The remaining two columns refer to specifications where, subsequently, controls are included. Column II includes the birth date and the interaction between the birth date and the treatment dummy as additional variables to account for potential age trends in the dependent variable or shifts in age trends due to the shock in the pension system that may confound our analyses. It is well known that well-being is a function of age. Several cross-sectional studies found that well-being increases with age (e.g., Campbell et al., 1976; Diener et al., 1984; Prenda and Lachman, 2001) while other studies found that this relationship is curvilinear and that well-being declines again after the age of 65 (Lang and Heckhausen, 2001). This empirical evidence implies that it is relevant to control for this potential age pattern, and to investigate whether a difference in life satisfaction between the two birth cohorts goes beyond what would be expected from the limited age difference alone. Column III includes the following additional control variables: marital status, yearly wage (in logs), years contributed to the pension fund, contractual working hours, number of doctor visits, year of measurement, educational levels, and sub-sector fixed effects.

The results of the three specifications presented in Table 6.2 show that the reform has a significant negative effect on well-being and that the magnitude of this effect slightly increases (although this increase is not statistically significant) when we add controls for individual heterogeneity. The reform reduces well-being with approximately -0.136. In order to get an idea of the size of the impact of the reform in the pension system, it is instructive to compare this coefficient with that of the log wage, although the coefficient of the log wage can be upward biased since the direction

⁶Table 6.11 also shows that we find similar results if we use OLS.

Table 6.2: Treatment effect on well-being

Dependent Variable: Well-being			
	I	II	III
Treatment dummy	-0.107*** (0.026)	-0.128** (0.052)	-0.136** (0.054)
Birth date (/365)		0.065 (0.064)	0.052 (0.067)
Treatment dummy *(Birth date -1)		-0.097 (0.090)	-0.063 (0.094)
Marital Status			0.579*** (0.061)
Yearly wage (in logs)			0.262*** (0.070)
Years contributed to pension			-0.002 (0.002)
Contractual working hours			0.179 (0.231)
Number of doctor visits in past year			-0.062*** (0.006)
Year of measurement		0.144*** (0.016)	0.153*** (0.018)
Constant	7.805*** (0.019)	7.694*** (0.038)	4.606*** (0.759)
Observations	11,428	11,428	9,988
Number of individuals	7,773	7,773	6,996

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. The estimations in Column III include the following other control variables: educational levels, sub sector fixed effects. The term ‘Treatment dummy*(Birth date -1)’ measures whether there is a break in the slope of well-being as a function of the birth date. We subtract 1 from the birth date since the the main effect should be captured by the treatment dummy and therefore the interaction term should be zero at the treatment threshold.

in causality between a worker’s wage and well-being is not clear. Nevertheless, the comparison shows that the impact of the pension reform is sizable. If we assume that causality runs from the log wage to well-being, a one-standard deviation (0.278) difference in the log wage in our dataset is associated to only a 0.073 difference in well-being. More intuitively, the impact of the reform is equivalent to a difference in the average annual wage of approximately 20,000 euro (an annual wage which is 39% lower). While, in first instance, this impact seems to be too large, it is very plausible. One has to bear in mind that these workers have to work for only a few additional years in which they will receive a wage. Oppositely, the drop in their replacement rate, in case they decide to retire early, will affect their yearly pension income until their death. Furthermore, if workers wish to fully restore the gap in their pension rights, they have to continue working for one year and one month (see Chapter 2).

Table 6.3: Treatment effect on separate domains of well-being

Variables	Job satisfaction I	Satisfaction with wage II	Satisfaction with hours worked III	Satisfaction with leisure IV	Satisfaction with household income V	Satisfaction with pension wealth VI	Satisfaction with health VII
Treatment dummy	-0.222*** (0.077)	-0.060 (0.076)	-0.133 (0.081)	-0.092 (0.096)	0.011 (0.081)	-0.323*** (0.089)	-0.107 (0.088)
Birth date (/365)	0.035 (0.097)	0.059 (0.096)	0.068 (0.103)	-0.186 (0.122)	0.013 (0.103)	-0.102 (0.113)	-0.014 (0.112)
(Treatment dummy *(Birth date -1)	0.143 (0.133)	-0.125 (0.132)	-0.231 (0.141)	0.086 (0.167)	-0.137 (0.141)	0.149 (0.154)	0.125 (0.153)
Marital Status	0.199*** (0.068)	-0.147*** (0.067)	0.146** (0.072)	0.042 (0.088)	0.098 (0.074)	-0.212*** (0.081)	0.254*** (0.080)
Yearly wage (in logs)	0.566*** (0.098)	2.456*** (0.096)	-0.612*** (0.104)	-0.276** (0.126)	1.463*** (0.106)	0.913*** (0.117)	0.113 (0.116)
Years contributed to pension	-0.008*** (0.003)	0.007** (0.003)	0.004 (0.003)	0.000 (0.004)	0.009** (0.003)	0.036*** (0.004)	-0.000 (0.004)
Contractual working hours	1.323*** (0.346)	0.171 (0.342)	-1.669*** (0.368)	-1.059** (0.414)	-0.672* (0.350)	-0.265 (0.383)	0.483 (0.383)
Number of doctor visits in past year	-0.106*** (0.006)	-0.037*** (0.006)	-0.071*** (0.007)	-0.092*** (0.009)	-0.077*** (0.007)	-0.068*** (0.008)	-0.300*** (0.008)
Year of Measurement	0.262*** (0.024)	0.382*** (0.024)	0.054* (0.028)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Constant	-0.150 (1.066)	-20.575*** (1.053)	14.912*** (1.138)	11.737*** (1.368)	-8.036*** (1.157)	-3.329*** (1.265)	6.320*** (1.256)
Observations	9,982	9,976	9,981				
Number of individuals	6,993	6,994	6,993	5,437	5,422	5,419	5,437

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. The results in Columns I-III are estimated using random effects models. The results shown in Columns IV-VII are based on OLS estimations. Other control variables included are: educational levels, sub sector fixed effects.

6 Subjective Well-being and Social Comparison

Table 6.3 shows the results of random effect models and OLS models for the seven domains of well-being previously mentioned. The table shows that the coefficient of the treatment dummy is negative for almost every domain. The coefficient of the treatment dummy is, however, only significantly different from zero for two domains: job satisfaction and the satisfaction with pension wealth. Note here, that the significant difference in the satisfaction with leisure shown in Table 6.1 is not due to the impact of the pension reform but due to an age trend. In general, younger workers are more dissatisfied with the amount of leisure time they have. The coefficient of the treatment dummy in the specification with job satisfaction as dependent variable implies that job satisfaction among the treated workers is approximately 0.222 lower, which is approximately 13% of the standard deviation of the job satisfaction variable. If we again relate this coefficient to that of the log wage, we observe that the difference between the treatment and control groups in job satisfaction is similar to that of well-being. The impact of the pension reform on job satisfaction is equivalent to a 33% difference in the annual wage. Not surprisingly, we find that the size of the coefficient of the treatment dummy is the largest in the specification on satisfaction with pension wealth. Satisfaction with pension wealth is 0.323 lower among the treated. This implies a large shift due to the pension reform of 19.4% in the standard deviation of the satisfaction with pension wealth.

Tables 6.2 and 6.3 made it clear that the shock in pension rights, mainly through a substantially lower level of job satisfaction and satisfaction with pension wealth, caused a considerable drop in well-being.⁷ Job satisfaction is a policy relevant domain of well-being that might affect productivity and job effort of workers (Jones et al., 2009). Oppositely, the lower level of satisfaction with pension wealth of the treatment group is hardly surprising. Therefore our further analyses will exclusively focus on well-being and job satisfaction.⁸ Figures for these two satisfaction indicators, in which dots represent the average satisfaction for all individuals born in a specific month, further confirm our estimation results by showing that there is a clear break in satisfaction around the threshold date of December 31, 1949 (see the Appendix).

⁷In a control analysis, we constructed an alternative measure for well-being by performing a principal components analysis on the seven domains available in the 2009 wave. We interpret the first component, which explains 47% of the variance, as an aggregate concept of well-being. Table 6.10 shows that our previous results are confirmed. These results are in line with Van Praag et al. (2003) who showed that well-being can be seen as an aggregate concept based on separate domains.

⁸We also performed all further analyses on the other domains of well-being. However, we do not find robust and significant effects of the pension reform on these domains, except satisfaction with pension wealth.

6.4.1 Age trends and persistence of the impact of the reform

It is conceivable that due to the arbitrary treatment threshold, effects from the reform may be particularly strong for individuals who barely missed the old generous pension system by a few days, weeks, or months and therefore can have stronger incentives to compare themselves with the control group (c.f. Chapters 4 and 5). Several studies have shown that individuals especially care about their relative position to others (see Frey and Stutzer (2002a,b) for a review on this literature). Moreover, although we controlled for birth date in the previous analyses, it is necessary to perform additional regressions on subsamples for a smaller age window around the treatment threshold. The multicollinearity between the treatment dummy and the age variable might confound the measured treatment effect. By omitting the age variable in the regressions, but nevertheless estimating the regressions for smaller windows, we address the issue of the sensitivity of our findings to the inclusion of the linear age variable.⁹ The results are presented in Table 6.4 in which regressions are shown on samples of workers born three, six or nine months around the treatment threshold.

The table shows that the coefficient of the treatment dummy is significant in all specifications. As the age range for the smallest age window is sufficiently small for age effects to be irrelevant, while keeping enough observations to identify the treatment effect, the coefficients of -0.154 for the treatment dummy on well-being, and -0.184 on job satisfaction, can be expected to be reliable estimates of the causal impact of the reform. If we compare the coefficients of the treatment dummy for this specific sample with those found in estimations on the whole sample in which we exclude employee age, it becomes clear that the coefficients of our measures for well-being and job satisfaction are slightly higher among this specific group. For the whole sample, we find that the impact of the pension reform on well-being is -0.114, while the impact on job satisfaction is -0.119 (see Column IV of Table 6.4). This suggests that those who were born just after the threshold date (i.e., in January-March 1950) are indeed more frustrated about the pension reform than those born later in the year. However, the difference in the coefficients is not significant at the 5%-level.

⁹We also estimated models in which we included the squared term of the birth date variable, allowed for breaks in the slope of birth date trends or introduced a third and fourth polynomial. These additional analyses confirm the robustness of the coefficient of the treatment dummy.

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Table 6.4: Treatment effect on well-being: results for different birth cohorts

Variables	Born within 3 months I	Born within 6 months II	Born within 9 months III	Born within 12 months IV
Well-being				
Treatment dummy	-0.154*** (0.057)	-0.110*** (0.039)	-0.123*** (0.032)	-0.114*** (0.028)
Marital Status	0.741*** (0.106)	0.583*** (0.068)	0.595*** (0.057)	0.579*** (0.048)
Yearly wage (in logs)	0.269* (0.145)	0.122 (0.099)	0.212*** (0.080)	0.262*** (0.069)
Years contributed to the pension fund	0.002 (0.004)	0.000 (0.003)	-0.001 (0.003)	-0.002 (0.002)
Contractual working hours	0.947* (0.562)	0.679* (0.353)	0.435 (0.302)	0.181 (0.246)
Number of doctor visits in past year	-0.059*** (0.010)	-0.071*** (0.006)	-0.061*** (0.005)	-0.063*** (0.004)
Year of measurement	0.091** (0.036)	0.137*** (0.025)	0.134*** (0.021)	0.153*** (0.018)
Constant	3.918** (1.579)	5.542*** (1.081)	4.778*** (0.880)	4.630*** (0.758)
Observations	2,507	5,081	7,51	9,988
Number of individuals	1,751	3,539	5,253	6,996
	Born within 3 months	Born within 6 months	Born within 9 months	Born within 12 months
Job satisfaction				
Treatment dummy	-0.184** (0.079)	-0.183*** (0.055)	-0.149*** (0.045)	-0.119*** (0.039)
Marital Status	0.203 (0.145)	0.206** (0.095)	0.213*** (0.079)	0.196*** (0.068)
Yearly wage (in logs)	0.646*** (0.199)	0.498*** (0.138)	0.459*** (0.112)	0.565*** (0.098)
Years contributed to pension	0.002 (0.006)	-0.002 (0.004)	-0.003 (0.004)	-0.008*** (0.003)
Contractual working hours	3.132*** (0.773)	1.934*** (0.495)	1.563*** (0.424)	1.326*** (0.346)
Number of doctor visits in past year	-0.087*** (0.013)	-0.115*** (0.009)	-0.110*** (0.007)	-0.106*** (0.006)
Year of measurement	0.199*** (0.046)	0.244*** (0.033)	0.240*** (0.027)	0.262*** (0.024)
Constant	-2.442 (2.165)	-0.140 (1.508)	0.460 (1.233)	-0.112 (1.065)
Observations	2,504	5,083	7,512	9,982
Number of individuals	1,751	3,542	5,255	6,993

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. Other control variables included are: educational levels, sub sector fixed effects. Employee age is omitted.

The results found in Table 6.4 are confirmed by estimations in which we include an interaction dummy for being born in the first three months of 1950 and the treatment dummy. The coefficient of the interaction dummy is not significant in these regressions. We also checked whether a ‘relief effect’ is driving our results, that is, that particularly those who just qualified for the more generous pension system (those born in October-December 1949) have a higher satisfaction level. We restricted our analyses to those born in 1949 and included a dummy for whether someone was born in the last three months of 1949 (results shown in the Appendix). We find that the coefficient of this dummy is small and insignificant in our estimations on well-being and job satisfaction. Hence there is no convincing evidence of a relief effect.

Table 6.5: Development well-being over time

Variables	Well-being (I)	Job satisfaction (II)
Treatment dummy	-0.131** (0.058)	-0.200** (0.081)
Interaction treatment dummy with measurement year	-0.009 (0.035)	-0.038 (0.046)
Birth date (/365)	0.052 (0.069)	0.034 (0.097)
Treatment dummy *(Birth date -1)	-0.063 (0.094)	0.144 (0.133)
Marital Status	0.579*** (0.048)	0.199*** (0.068)
Yearly wage (in logs)	0.262*** (0.070)	0.565*** (0.098)
Years contributed to pension	-0.002 (0.002)	-0.008*** (0.003)
Contractual working hours	0.179 (0.246)	1.320*** (0.346)
Number of doctor visits in past year	-0.062*** (0.005)	-0.106*** (0.006)
Year of measurement	0.158*** (0.026)	0.282*** (0.034)
Constant	4.607*** (0.759)	-0.145 (1.066)
Observations	9,988	9,982
Number of individuals	6,996	6,993

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. Other control variables included are: educational levels, sub sector fixed effects.

The previous analyses show that the lower level of well-being due to the shock in pension rights is prevalent over the whole birth date distribution in our sample.¹⁰ The results suggest that the impact of the pension reform only slowly fades away if we would include workers who were born a few years after 1950. The impact of the pension reform for our specific birth cohort, however, might also disappear over time. Several empirical studies investigated hedonic adaptation processes and found that people indeed do recover after a negative experience, although often they only adapt partially (Brickman, 1978; Oswald and Powdthavee, 2008; Clark et al., 2008a).

It is conceivable that, after some time, the treated workers in our sample become accustomed to the fact that they have reduced pension rights and adapt their assessment of their well-being. However, it should be noted that the satisfaction variables are based on survey waves from Spring 2008 and 2009, already more than two or three years after the implementation of the new pension system. The fact that we still find significant effects suggests the persistence of the impact of the reform.

¹⁰There is also a large body of research on the effects of an individual's birth month on outcomes such as well-being, health and earnings (see Buckles and Hungerman (2008) for a detailed description of this literature). Since we compare two birth years, seasonality effects should not affect our results. Nevertheless, we estimated the treatment effect on restricted samples including only those born in Summer (June-August) or in Winter (January-March). We find that the treatment effect is robust to the birth season of employees in our sample.

To further check this issue, we estimate random effect models on our indicators for well-being and job satisfaction in which we include an interaction term between the treatment dummy and the year of measurement. In case of an adaptation effect, we would expect a significant positive coefficient of this interaction effect. Table 6.5 shows, however, that the sign of the coefficient of the interaction variable in both specifications does not differ significantly from zero. The results of these additional analyses therefore suggest that the effect of the reform remains more or less stable over time. A possible explanation for not finding signs of hedonic adaptation is that the men in our treatment group still have to retire. The fact that they are nearing the age of 62 might make them actually increasingly aware of the drop in their pension rights.

Another explanation of the absence of indications of hedonic adaptation is that the treatment effects on well-being and job satisfaction are mainly caused by comparisons with a social-reference group of untreated people born before 1950. Recent studies on relative concerns in happiness (DiTella and McCulloch, 2008; Montesoglu and Vendrik, 2010) showed that people do not, or much less, adapt to changes in their relative position than they do to absolute alterations in their conditions of life. Hence, the effects of a change in an individual's relative position should be much more persistent than those of a change in the absolute position. Our analyses on different age windows indicate that relative concerns may affect our results. Indeed, the result that those who were born just after the threshold date (January-March 1950) are slightly more frustrated about the pension reform than those who were born later in the year (coefficient of -0.154 for those born in the first three months compared to a coefficient -0.110 for those born between April and June), indicates that workers with retrenched pension rights care about their relative position. In the next section, we pursue this issue further by examining in more detail the importance of social comparison in the treatment effects.

6.4.2 Social comparison and unfairness

People compare themselves with others. This is a deeply ingrained human inclination that is likely to play an important role in the present context as well (e.g., Ferrer-i-Carbonell, 2005; Luttmer, 2005; Clark et al., 2008b). The frustration of those born in 1950 about the tax reform may not so much stem from disappointment about the fall in their expected replacement rate at age 62 per se, but rather from the fact that they are hit by the reform, whereas people who are slightly older are not. In

particular, those born in 1950 will compare their situation with that of people of similar age, sex, and socio-economic characteristics in their social environment.

In the first place, men born in 1950 may suffer less when in their organization the share of similar workers who suffer from the reform as well is larger. Conversely, they may suffer more when the share of similar colleagues who are not covered by the reform is larger. To construct proxies for this social influence in the organization we use administrative data on the total number of workers in the organization in which each employee is working. Since workers are likely to compare themselves primarily with their colleagues in similar positions, and hence similar levels of income, we divided the total male population within each organization into four income quartiles. Next, for each male in our sample, we determined the employment share of male workers born in 1950, respectively 1949, in the same income quartile and organization, as the percentage of the total number of employees in his organization.¹¹ We model the effects of the shares of treated and untreated employees in the income quartiles by organization on a person's well-being and job satisfaction in the following way:

$$Ls_{it} = \alpha_i + \beta_1 D_{it} + \beta_2 PD_{it} + \beta_3 PND_{it} + \beta_4 D_{it} * PD_{it} + \beta_5 D_{it} * PND_{it} + \zeta X_{it} + a_i + u_{it} \quad (6.2)$$

We include interaction terms between the treatment dummy and the percentages of treated ($D * PD$) and untreated workers ($D * PND$) who are working in the same organization and income quartile. We expect a positive coefficient for the interaction term with the share of treated employees, since treated workers may feel less frustrated when more of their colleagues suffer from the treatment as well. Conversely, the interaction with the share of untreated employees will attract a negative coefficient if treated workers feel more frustrated when more of their colleagues with similar characteristics are not covered by the reform. We also include the percentages of treated (PD) and untreated (PND) employees as additive main effects into the regressions.

¹¹These percentages tend to be low with mean 0.82% and standard deviation 1.29 for the share of treated employees born in 1950, and mean 0.78% and standard deviation 1.52 for the share of untreated employees born in 1949. This means that treated workers can compare themselves with approximately six (median) other treated workers in their organization who are in the same income quartile. We also performed robustness analyses in which we calculated the percentages of treated and untreated employees per organization (thus omitting the distinction into income quartiles). These shares are somewhat higher: mean 1.99% with standard deviation 1.55 for the treated workers, and mean 1.86% with standard deviation 1.79 for the untreated workers. We find, however, that the results of our analyses are robust to the use of these different percentages.

Table 6.6: Reference based effects: the effects of the percentage of treated and untreated workers within income quartiles and organizations

Variables	Well-being		Job satisfaction	
	I		II	
Treatment dummy	-0.163*** (0.063)		-0.238*** (0.088)	
Interaction percentage treated and treatment dummy	0.076** (0.038)		0.136*** (0.053)	
Interaction percentage untreated and treatment dummy	-0.029 (0.036)		-0.101** (0.051)	
Percentage treated (by organization and income quartile)	-0.054 (0.034)		-0.139*** (0.048)	
Percentage untreated (by organization and income quartile)	0.023 (0.015)		0.015 (0.022)	
Birth date (/365)	0.050 (0.069)		0.031 (0.097)	
Treatment dummy *(Birth date -1)	-0.061 (0.094)		0.143 (0.133)	
Marital Status	0.579*** (0.048)		0.204*** (0.068)	
Yearly wage (in logs)	0.260*** (0.070)		0.555*** (0.098)	
Years contributed to pension	-0.002 (0.002)		-0.008*** (0.003)	
Contractual working hours	0.182 (0.246)		1.324*** (0.346)	
Number of doctor visits in past year	-0.062*** (0.005)		-0.106*** (0.006)	
Year of measurement	0.153*** (0.018)		0.260*** (0.024)	
Constant	4.643*** (0.760)		0.052 (1.068)	
Observations	9,988		9,982	
Number of individuals	6,996		6,993	

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. Other control variables included are: educational levels, sub sector fixed effects.

The estimation results in Table 6.6 are largely in agreement with our expectations for the coefficients of the interaction terms. The interactions with the share of treated employees have significant coefficients with the expected positive signs for well-being as well as job satisfaction, and the interactions with the share of untreated employees have coefficients with the expected negative signs, although only significant for job satisfaction. Thus, treated employees feel indeed less bad, in terms of both well-being and job satisfaction, when more of their colleagues with matching characteristics suffer from the treatment as well, and they feel worse when more of their colleagues are not covered by the reform (c.f., Clark, 2003). However, the main additive effect of the share of treated employees in the organization is significantly negative for job satisfaction. Thus, there is a negative external effect of the percentage of treated employees on the job satisfaction of each worker in the organization, including the untreated employees. This suggests that frustrated employees who were born in 1950 have a negative impact on the general atmosphere in the organization.

Since relative deprivation effects on well-being and job satisfaction have been found in the happiness literature to be both larger and more persistent over time than absolute effects of worsening conditions, the social comparison results above partly explain why the treatment effect is remarkably strong and stable between 2008 and 2009. However, the large size of the treatment effect compared to the wage effect can also suggest that the treatment effect may contain an important non-monetary component that originates from feelings of being treated in an unfair manner by being treated differently than people slightly older, in addition to the direct monetary effect, that runs via the fall in the average expected replacement rate at age 62 from 70 to 64% (see Chapter 2).

We test for the existence of such non-monetary treatment effects by adding the logarithms of the expected individual replacement rate at age 62 and the average expected replacement rate of all individuals within the same income quartile and organization where respondents are employed as control variables. These added variables should capture the direct monetary effect, while then the coefficient of the interaction between the treatment dummy and the percentage of treated and untreated employees should capture the remaining non-monetary effect.¹² We expect that well-being is higher when a worker's own expected replacement rate is higher, while a higher average replacement rate in their organization might reduce their personal well-being.

¹²We also included interaction terms between the individual and average replacement rate. Estimations revealed highly insignificant coefficients of the interaction terms of individual and average replacement rate. Hence, we dropped them in the regressions.

Table 6.7: Reference based effects: the effects of the percentage of treated and untreated workers and comparison of replacement rates within income quartiles and organizations

Variables	Well-being		Job satisfaction	
	I		I	II
Treatment dummy	-0.131** (0.064)		-0.224** (0.090)	
Interaction percentage treated and treatment dummy	0.067* (0.038)		0.128** (0.054)	
Interaction percentage untreated and treatment dummy	-0.026 (0.038)		-0.092* (0.053)	
Percentage treated (by organization and income quartile)	-0.047 (0.035)		-0.134** (0.049)	
Percentage untreated (by organization and income quartile)	0.021 (0.016)		0.016 (0.022)	
Expected retirement benefit at age 62 (in logs)	0.271*** (0.082)		0.409*** (0.113)	
Average expected retirement benefit (by organization and income quartile, in logs)	-0.144 (0.128)		-0.578** (0.176)	
Birth date (/365)	0.049 (0.070)		0.044 (0.099)	
Treatment dummy *(Birth date -1)	-0.073 (0.096)		0.123 (0.135)	
Marital Status	0.574*** (0.049)		0.196*** (0.069)	
Yearly wage (in logs)	0.283*** (0.071)		0.583*** (0.100)	
Years contributed to pension	-0.002 (0.002)		-0.008*** (0.003)	
Contractual working hours	0.168 (0.252)		1.360*** (0.355)	
Number of doctor visits in past year	-0.064*** (0.005)		-0.107*** (0.006)	
Year of Measurement	0.163*** (0.019)		0.248*** (0.026)	
Constant	3.886*** (0.918)		0.529 (1.284)	
Observations	9,702		9,696	
Number of individuals	6,818		6,814	

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. Other control variables included are: educational levels, sub sector fixed effects.

The resulting coefficient estimates are shown in Table 6.7. The table shows that the coefficient estimates for the treatment dummy and the interaction terms that were significant in Table 6.6 remain significant and have only somewhat fallen in size. This shows that the treatment effects on well-being and job satisfaction have indeed a strong non-monetary component and that relative concerns also play a role in the non-monetary component of the treatment effect. Concerning the monetary-component, as expected, we find that the individual replacement rate has strongly significant positive effects on both well-being and job satisfaction. The average replacement rate has a significant negative effect on job satisfaction, that is even larger in size than the positive effect of the individual replacement rate. This implies that the job satisfaction of workers is negatively affected if the replacement rate of their counterparts is higher. Subsequently, if we denote the individual and average replacement rates of individual i in year t by r_{it} and ra_{it} , respectively, and write the terms $0.409*r_{it} - 0.578*ra_{it}$ as

$$(0.409 - 0.578)*r_{it} + 0.578*(r_{it} - ra_{it}) = -0.169*r_{it} + 0.578*(r_{it} - ra_{it})$$

where coefficient -0.169 is insignificant, we see that the monetary treatment effect on job satisfaction that runs via the fall in the expected replacement rate at age 62 is also mainly relative. Assuming that the individual replacement rates are exogenous, we can also make an assessment of the relative sizes of the monetary and non-monetary component, by noting that the effects of the decline in the individual replacement rates by 6%-points on well-being and job satisfaction ($= 8.6\%$ of the original replacement rate of 70%-points) are approximately given by $0.271*-0.086 = -0.023$ and $0.409*-0.086 = -0.035$, respectively, which constitute only 15% and 14%, respectively, of the total treatment effect on well-being and job satisfaction. This implies that approximately 85% of the total treatment effects on well-being and job satisfaction has a non-monetary nature. As suggested above, these strong non-monetary effects may be attributed to feelings of unfair treatment among men born in 1950.

6.4.3 Spill-over effects of the treatment of spouses

The preceding subsection showed that a major part of the treatment effect can be explained by social comparison between colleagues. Additionally, the reaction of the treated workers in our sample can be affected by their household situation. More specifically, we could expect that a shock in pension benefits of a men's spouse may negatively affects his satisfaction level, since it may give rise to even stronger feelings of unfair treatment. Furthermore, the treatment of one's spouse by the

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pension reform entails a reduction in future household income which may affect his personal situation.¹³

To address this question, we make use of the survey questions on the characteristics of spouses which have been asked in the 2009 wave. As previously mentioned, questions were asked on the education level of the spouse, the gross monthly income of the spouse, and actual working hours. We use the birth year of the spouse to construct an additional treatment dummy that indicates whether spouses were born before 1950 or after 1949. Unfortunately, we do not have reliable information on the size of the reduction in the pension rights of the spouse. Spouses do not necessarily work in the public sector and will be covered by several different sectoral retirement schemes with deviating rules. Nevertheless, as mentioned before, the abolishment of the favorable tax treatment of early retirement schemes in the second pillar of the pension system applied to all sectors, which implies that spouses born in 1950 or later, and who covered by other pension schemes, should also have entrenched pension rights.

The regression in Column I of Table 6.8 shows that, although the coefficient of the treatment dummy is negative, the treatment of the spouse has only a marginally significant impact on the well-being of the men in the sample. Furthermore, the coefficient of the dummy indicating whether the respondents themselves are treated becomes insignificant. This is most likely caused by multicollinearity between one's own treatment dummy and the treatment dummy of one's spouse. In the estimations on job satisfaction, the coefficient of the treatment dummy of the spouse is negative and significantly different from zero. Thus, consistent with the literature on joint retirement among dual-career families, we find suggestive evidence that a reduction of one's spouse's pension rights reduces the well-being of the men in our sample. As can be expected, the treatment of the spouse has a smaller negative effect on the level of job satisfaction than one's own treatment. The job satisfaction of men with a treated spouse is on average 0.107 lower while their job satisfaction is 0.229 lower if they are treated themselves. This corresponds to a shift of 6% of the standard deviation in job satisfaction.

¹³Recent studies emphasized the incidence of joint retirement among dual-career families (Blau, 1997; Blau, 1998; Baker, 2002; and Coile, 2004) and found strong financial spillovers between spouses that affect individual retirement behavior.

Table 6.8: Treatment spouse and well-being

Variables	Well-being	Job satisfaction
	I	II
Treatment dummy	-0.090 (0.060)	-0.229*** (0.086)
Treatment dummy partner	-0.058* (0.035)	-0.107** (0.050)
Birth date (/365)	0.036 (0.076)	0.058 (0.107)
Treatment dummy *(Birth date -1)	-0.091 (0.107)	0.084 (0.151)
Yearly wage (in logs)	0.293*** (0.079)	0.561*** (0.110)
Gross monthly income partner (in logs)	0.004 (0.007)	0.006 (0.010)
Years contributed to pension	-0.003 (0.003)	-0.013*** (0.003)
Contractual working hours	0.314 (0.306)	1.703*** (0.488)
Actual working hours partner	0.003 (0.002)	0.001 (0.003)
Number of doctor visits in past year	-0.059*** (0.007)	-0.104*** (0.009)
Year of measurement	0.136*** (0.020)	0.245*** (0.027)
Constant	4.789*** (0.874)	0.066 (1.238)
Observations	7,521	7,517
Number of individuals	4,921	4,919

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. Other control variables included are: educational levels, sub sector fixed effects.

6.4.4 Treatment and the expected duration of retirement

Finally, one might wonder whether the length of the expected duration of benefit receipts is a factor that can affect the reaction of workers to the shock in their pension rights. It is conceivable that persons with a higher life expectancy put less weight on working an additional year and therefore experience a smaller drop in their satisfaction levels. We explore this hypothesis by introducing an interaction variable between the treatment dummy and the expected likelihood to reach the age of 80 years. The latter variable is based on a survey question asking respondents to assess the likelihood (in percentages) that they will reach the age of 80 years.¹⁴ Naturally, one has to acknowledge that self-assessed life expectancy is endogenously related to well-being, either because there are feedback effects from well-being on life expectancy, or because there are unobservables that correlate with both variables. On the other hand, an unreported regression shows that the self assessed life expectancy is not affected by the exogenous pension reform. The interaction may thus still give a

¹⁴We deliberately use the expected likelihood to reach the age of 80 years because this is the average life expectancy for this male birth cohort in the Netherlands. We also asked the respondents to assess the likelihood that they will reach the age of 65, 75, 85, 90 and 95 years or older. Similar regressions that used these variables instead show that the results shown in Table 6.9 remain robust.

Table 6.9: Treatment and the expected duration of benefit receipt

Variables	Well-being I	Job satisfaction II
Treatment dummy	-0.208** (0.100)	-0.442*** (0.144)
Interaction likelihood to reach the age of 80 years and treatment	0.002* (0.001)	0.004** (0.002)
Likelihood to reach the age of 80 years	0.009*** (0.001)	0.008*** (0.001)
Birth date (/365)	-0.023 (0.075)	-0.046 (0.108)
Treatment dummy *(Birth date -1)	-0.048 (0.102)	0.247* (0.147)
Marital Status	0.557*** (0.053)	0.241*** (0.075)
Yearly wage (in logs)	0.297*** (0.076)	0.616*** (0.108)
Years contributed to pension	-0.000 (0.002)	-0.008** (0.003)
Contractual working hours	0.164 (0.269)	1.529*** (0.385)
Number of doctor visits in past year	-0.052*** (0.005)	-0.085*** (0.007)
Year of measurement	0.150*** (0.020)	0.253*** (0.026)
Constant	3.569*** (0.831)	-1.215 (1.191)
Observations	8,028	8,023
Number of individuals	5,235	5,232

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. Other control variables included are: educational levels, sub sector fixed effects.

useful indication of how the pension reform affects satisfaction levels of workers with different self assessments of their life expectancy, although cautiousness is necessary in the interpretation of the coefficient of this variable.

The results presented in Table 6.9 confirm our hypothesis that persons who assign a lower likelihood to reach the age of 80 years are also more affected by the pension reform. The coefficients of the interaction term have a reasonable size and are significant in both specifications.

6.5 Conclusion

This study uses an exogenous shock in pension rights of Dutch public sector workers to determine the extent to which a drop in generosity of pension systems affects the well-being of workers nearing their retirement. The results provide strong evidence that shocks in pension systems have substantial effects on individual well-being. The two domains of well-being which are most affected by the reform are job satisfaction and satisfaction with pension wealth. We examined whether the lower level of well-being due to the shock in pension rights is prevalent over the whole birth date

distribution of the treated workers in our sample and whether we can observe adaptation effects. The results of these additional analyses suggest that the effect of the reform persists over time and most likely does not quickly fade away if we include workers who were born a few years after 1950. We find that the drop in satisfaction is strongly affected by social comparison with colleagues within the organization where someone is employed. Treated workers are less affected by the reform when the share of the treatment group is larger in the organization where they are employed, and they compare their own replacement rate with the average replacement of comparable individuals in their organization. Moreover, we provide suggestive evidence that among dual-career families, a reduction of one's spouse's pension rights reduces the well-being of the men in our sample. Finally, treated workers with a lower life expectancy have a significantly lower level of well-being than those who retain their old generous pension rights.

The results found are highly relevant for policy makers who aim to revise the pension system in their countries. Changes in workers' pension claims, due to government pension policy changes, will have severe consequences on individual well-being for most workers nearing retirement. Since, the main objective of social policy is to promote the well-being of the population as a whole, policy makers should take the negative side effect of the revisions in their pension systems into account. Moreover, the results show that policymakers as well as employers should be aware of the potential distortive effects of social comparison induced by pension reforms.

Appendix

Table 6.10: Constructed well-being in 2009 (based on seven domains)

Variables	Constructed well-being I
Treatment dummy	-0.197** (0.094)
Birth date (/365)	-0.091 (0.119)
Treatment dummy *(Birth date -1)	0.092 (0.163)
Marital Status	0.057 (0.086)
Yearly wage (in logs)	1.123*** (0.123)
Years contributed to the pension fund	0.012*** (0.004)
Contractual working hours	-0.405 (0.405)
Number of doctor visits in past year	-0.162*** (0.009)
Constant	-11.388*** (1.336)
Observations	5,335

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. Other control variables included are: educational levels, sub sector fixed effects.

Table 6.11: Alternative models and specifications

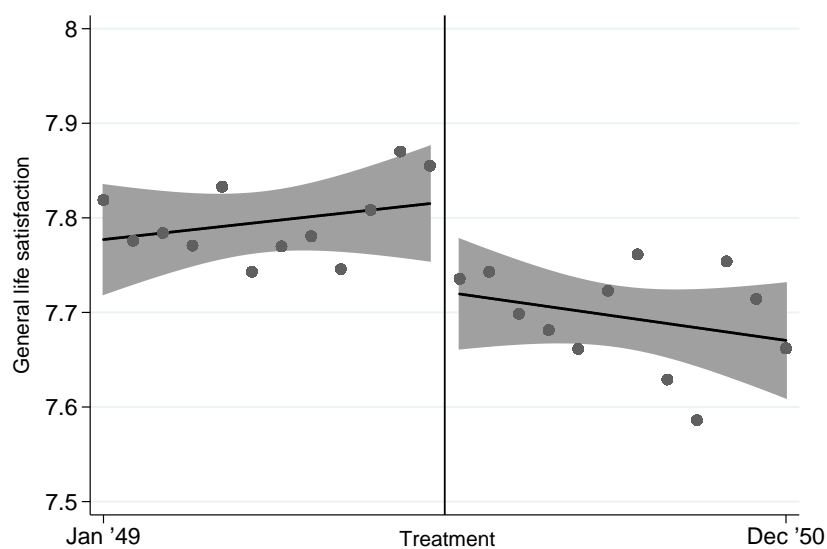
Variables	OLS		RE models including Mundlak terms		Order probit estimates	
	Well-being	Job satisfaction	Well-being	Job satisfaction	Well-being	Job satisfaction
	I	II	III	IV	V	VI
Treatment dummy	-0.117** (0.054)	-0.214*** (0.079)	-0.134** (0.055)	-0.210*** (0.077)	-0.097** (0.042)	-0.116*** (0.041)
Birth date (/365)	0.040 (0.068)	0.023 (0.099)	0.050 (0.069)	0.025 (0.097)	0.034 (0.053)	0.002 (0.052)
Treatment dummy *(Birth date -1)	-0.072 (0.096)	0.138 (0.138)	-0.059 (0.094)	0.150 (0.133)	-0.068 (0.073)	0.087 (0.071)
Marital Status	0.591*** (0.060)	0.202** (0.080)	0.938*** (0.205)	0.380 (0.273)	0.482*** (0.038)	0.112*** (0.037)
Yearly wage (in logs)	0.268*** (0.072)	0.597*** (0.098)	0.770 (15.395)	0.144 (20.852)	0.264*** (0.055)	0.403*** (0.054)
Years contributed to pension	-0.001 (0.002)	-0.009** (0.003)	-0.020*** (0.007)	-0.040*** (0.010)	-0.002 (0.002)	-0.006*** (0.002)
Contractual working hours	0.205 (0.242)	1.479*** (0.414)	-5.033 (-4.516)	0.233 (-6.013)	0.294 (0.197)	1.028*** (0.193)
Number of doctor visits in past year	-0.073*** (0.007)	-0.124*** (0.009)	-0.031*** (0.008)	-0.051*** (0.011)	-0.058*** (0.004)	-0.068*** (0.004)
Year of Measurement	0.164*** (0.021)	0.306*** (0.028)	0.187*** (0.023)	0.317*** (0.031)	0.149*** (0.022)	0.191*** (0.021)
Observations			9,988	9,982	9,988	9,982
Number of individuals	9,988	9,982	6,996	6,993	6,996	6,993

*** p < 0.01, ** p < 0.05, * p < 0.10. Standard errors in parentheses. OLS estimates are corrected for clustered standard errors (clustering among individuals). Other control variables included are: educational levels, sub sector fixed effects.

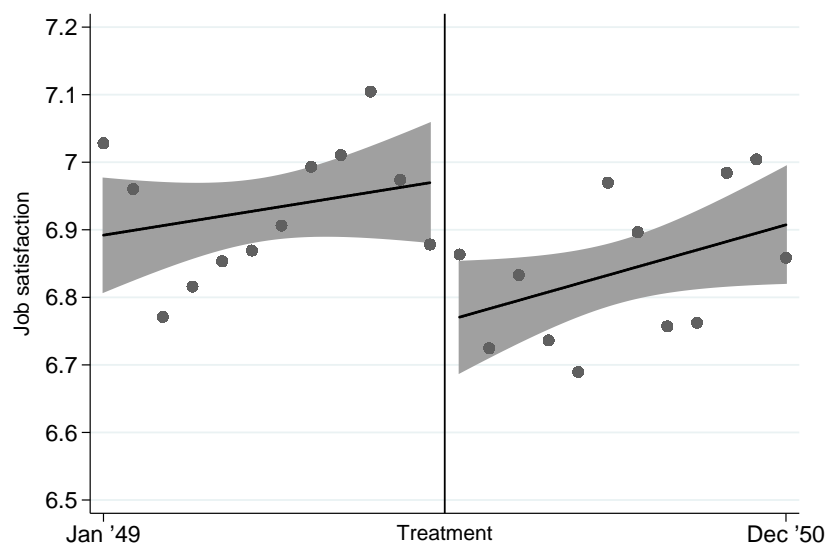
Table 6.12: Well-being of those who just qualify for the old pension system

Variables	Well-being I	Job satisfaction II
Relief dummy	0.055 (0.067)	0.019 (0.096)
Birth date (/365)	-0.008 (0.098)	0.015 (0.139)
Marital Status	0.584*** (0.071)	0.139 (0.101)
Yearly wage (in logs)	0.324*** (0.100)	0.782*** (0.142)
Years contributed to pension	-0.004 (0.004)	-0.005 (0.005)
Contractual working hours	0.368 (0.344)	1.652*** (0.489)
Number of doctor visits	-0.066*** (0.006)	-0.095*** (0.009)
Year of measurement	0.161*** (0.025)	0.274*** (0.034)
	(0.223)	(0.315)
Constant	3.982*** (1.090)	-2.617* (1.547)
Observations	4,705	4,705
Number of individuals	3,302	3,301

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parentheses. The relief dummy is coded 1 for workers who were born in October-December 1949 and 0 for all other workers who were born in 1949. Other control variables included are: educational levels, sub sector fixed effects.

Figure 6.1: Well-being

The data points present the mean level of well-being for each birth month from January 1949 to December 1950. Fitted lines and confidence intervals are calculated based on individual information.

Figure 6.2: Job satisfaction

The data points present the mean level of job satisfaction for each birth month from January 1949 to December 1950. Fitted lines and confidence intervals are calculated based on individual information.

7 Conclusions

7.1 Summary

The aim of this thesis was to determine the extent to which pension reforms affect training investments and, the job effort, well-being and mental health of older workers. With this goal, we exploited a natural experiment generated through the introduction of a strong discontinuous rule involving a considerable retrenchment of pension rights for a subgroup of workers. Prior to 2006, Dutch public sector workers with 40 years of tenure could retire at the age of 62 years and three months with a replacement rate of 70% of their average yearly earnings since 2002. As of 2006, those born before January 1, 1950, can continue to retire under the old rules, but for those born on or after January 1, 1950, the replacement rate is lowered to 64%. We used matched survey and administrative panel data for male full-time employees in the public sector who were born in 1949 or 1950. The panel survey data are available for three years (2007-2009) and were gathered in two stages. In the first stage, all 27,871 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. In the second stage, those who gave their permission (11,458 workers sent their e-mail address) received an e-mail with a link to the survey.

The main advantage of using a natural experiment is that one can use exogenous variations provided by the policy change to infer a causal relation between the exogenous explanatory variable and the dependent variable of interest. However, natural experimental approaches also have their weaknesses. Therefore, we closely examined the internal and external validity of the research design in Chapter 2. Concerning internal validity, the limited age difference between the treatment and control groups in the sample, in combination with the simple and transparent age criterion that determines entitlement to the old or new pension rights, guarantees that the treatment and control groups are similar enough to each other to compare. Furthermore, the respondents were not aware of the experimental nature of the survey. Descriptive analyses showed that there is no evidence of any differences in self-selection in the survey between the treatment and control groups. The differences in observables between the treatment and control groups are indeed sufficiently small for our research purposes. Moreover, both groups have reasonably accurate retirement expectations. We have to acknowledge that the focus on those born in 1949 and 1950 reduces to some extent the external validity of the results in this thesis, since we cannot provide evidence of the extent to which the effects found are less relevant to younger cohorts. Nevertheless, the results found in this thesis are highly relevant, especially because these specific cohorts belong to the large baby boom generation. Moreover, a new

pension reform has been announced that will likely affect younger cohorts in a similar way as the 1949-1950 cohort was affected by the 2006 reform with different pension rights for two adjacent birth cohorts. Furthermore, many European countries have recently proposed reforms of the retirement schemes of their public sector workers.

Chapter 3 examined the effects of the exogenous change in future pension benefits on workers' training participation one year after the reform. The pension reform indeed postpones the expected retirement date and increases participation in training courses among the treated workers. However, this only holds for those who work in larger organizations. Since the bulk of the training courses are provided and paid for by the employer, this result suggests that larger organizations are better able to customize their training practices to the new retirement system, since they generally have more formally developed and sophisticated HRM systems than smaller organizations. We found that the treatment effect for workers in large organizations remains robust to the inclusion of age and the use of alternative specifications and is lower for more highly educated workers. The latter might be attributed to the higher intrinsic training motivation of highly educated workers. Finally, the major part of the treatment effect is found to be due to higher participation in longer training courses.

Chapter 4 compared the mental health of workers in the treatment and control groups two years after the policy reform. The well validated CES-D8 indicator of depression, derived from the *Center for Epidemiologic Studies Depression Scale* (CES-D, Radloff 1977), is used to measure mental health. We found substantial effects from the exogenous change in the retirement system on the mental health of those treated by the pension reform. Depression rates among the 1950 cohort are about 40% higher than among the 1949 cohort. This effect persists over time and grows stronger the closer one was born to the threshold date. In addition, the effects differ substantially for different types of workers. The effects are stronger for unmarried workers and weaker for workers whose partner has a pension or an income.

Chapter 5 analyzed the extent to which the negative reciprocal inclinations of treated workers to the pension reform affect job effort. The discontinuous assignment rule and the strong differential treatment of workers born around January 1, 1950, is likely to be perceived as unfair, especially because it was announced only a couple of years before the expected retirement date of the affected workers. We used the 2008 wave that includes information on job motivation and reciprocal inclinations. The latter are elicited from answers to six questions that measure positive as well as negative reciprocity and which have been validated by Perugini et al. (2003) in

controlled laboratory experiments. We used job motivation as a proxy for job effort and found that motivation is lower among highly negatively reciprocal individuals who face an unexpected drop in their pension rights. The negative effect on job motivation is larger for the group of workers that arguably perceives the policy reform as being most unfair, namely, negatively reciprocal workers born close to the cut-off date of January 1, 1950. Moreover, job motivation is lower among negatively reciprocal workers who work for the national government which initiated the policy reform, and can therefore hold their employer directly responsible for the retrenchment of their pension rights.

Chapter 6 analyzed the effects of the pension reform on well-being and its underlying domains in the years 2008 and 2009. We found strong and persistent effects on both well-being and job satisfaction that are sizable compared to income effects on well-being. We showed that a major part of the treatment effect can be explained by social comparisons. Workers compare their own replacement rate with the average replacement rate of similar individuals in their organization. However, the major part of the treatment effect is explained by non-monetary incentives associated with the perceived unfairness of the treatment and social comparison. Controlling for monetary incentives by including the individual replacement rate and the average replacement of comparable individuals in their organization, it is found that treated workers are less affected by the reform when the share of the treatment group is larger in the organization where they are employed. Moreover, the treatment effect increases with the share of the control group in the worker's organization. Hence, the results in Chapter 5 and 6 indicate that both personal traits and concerns about unfairness are vital factors that determine individual behavioral responses to the retrenchment of pension rights.

7.2 Contribution to the literature

This thesis first contributes to the human capital literature by explicitly analyzing the effects of an exogenous pension reform on training investments and mental health. In general, existing studies on the human capital development of older workers have difficulty assessing the direction of causality between health or training investments and the retirement decision. Several studies that do not account for the endogenous character of training investments and health status found that the training participation and good health of older workers are strongly associated with postponed retirement (Bassanini et al., 2007; Tsai et al., 2005; Dave, et al., 2008;

Fouarge and Schils, 2009). However, these studies cannot identify whether it is the workers' training investments and good health that postpones retirement or whether the postponement of retirement induces training and health investments. We show that when exploiting a natural experiment that identifies the causal direction, a sudden shock in pension rights leads to a slightly higher training participation of older workers in larger organizations but also strongly negatively influences the mental health of those affected by the reform.

Second, this study contributes to the literature that measured the effects of financial incentives provided by social security and pension systems on labor market participation (e.g., Stock and Wise, 1990; Gruber and Wise, 1998; Samwick, 1998; Bingley and Lanot, 2002; Chan and Stevens, 2004). The major part of this vast literature focused on the role of financial incentives on retirement behavior, with health included as an exogenous regressor (see the review study by Lumsdaine and Mitchell, 1999). To identify the causal effect of financial incentives on retirement, it is preferable to rely on exogenous changes in the retirement system. The results indicate, however, that in the presence of *ex ante* health effects, retrenchment of pension rights not only will have an impact on the labor force participation of older birth cohorts, but also will influence health prior to retirement. This will confound both the health effects and the effect of financial incentives in retirement models, with the consequence that health should be treated as an endogenous regressor in retirement models. Part of the effects of the financial incentives will be absorbed by the health effects prior to actual retirement.

Third, this thesis contributes to the studies that emphasized that reciprocity influences decision making in controlled environments and in stylized labor markets (e.g., Fehr et al., 1993; Schmidt and Fehr, 2000; Falk, 2007; Falk and Zehnder, 2007; Ederer and Fehr, 2007). Although laboratory evidence showed that reciprocity is a key driver of human motivation in stylized labor markets, evaluating the importance of reciprocity in a natural labor market setting, in which confounding factors and constraints can undermine the behavioral relevance of reciprocal inclinations is relevant. Here we can build on the key advantage of our study, that one has data from a natural experiment. This enables a comparison of the response of more negative reciprocal workers who are treated unfairly in a natural setting with that of treated workers who are less reciprocal and of a control group of more negatively reciprocal workers who have not been treated by the pension reform.

Fourth, this study contributes to the well-being literature (e.g., Ferrer-i-Carbonell, 2005; Luttmer, 2005; Clark et al., 2008b) by explicitly analyzing social comparisons and spill-over effects within the household while appropriately accounting for spu-

rious correlations and reverse causality. Furthermore, this thesis complements the literature on the effects of negative life shocks on well-being. To our knowledge, this is the first study to document the strong and persistent effects on individual well-being of altered expectations about future pension income caused by an exogenous ‘bad life shock’ consisting of a pension reform.

7.3 Policy implications

European policy makers generally base their decisions to reform pension systems on policy-oriented studies that provide cost-benefit analyses of the various policy options. These studies used models that include financial parameters but which do not take into account the negative distortive effects on mental health and work effort. Recently, an influential study by the Netherlands Bureau for Economic Policy Analysis (2009) performed simulations to assess the costs and benefits of an increase of the eligibility age for the state’s old-age pension from 65 to 67 years. The study found that raising the eligibility age by two years will reduce future levels of annual public pension expenditures by 0.7% of the gross domestic product. This result is being used by the government to legitimize a bill submitted to the parliament that aims to increase the eligibility age. However, the results in this thesis suggest that the gains of the proposed pension reform may be substantially smaller than projected, since the proposed increase in the eligibility age can adversely affect the mental health of older workers. This decrease in mental health can influence the labor productivity and work effort of older workers and can, furthermore, directly affect health care costs and the inflow of older workers into disability insurance schemes.

Our results also provide new insights that are relevant to the debate on the optimal nature and design of pension reforms. Policy makers and pension funds have advocated transparent transitional arrangements that entail a clear discontinuity in pension rights across different birth cohorts. A simple design of the transitional arrangements can reduce the costs that inevitably arise when reforming a pension system. On the contrary, this thesis indicates that a hard discontinuity rule induces a strong negative treatment effect that, for the main part, can be explained by social comparisons between those born just before and just after the treatment threshold and the ensuing strong feelings of being unfairly treated and negative reciprocal reactions. In the specific example of tax legislation affecting pension rights, an alternative design that entails less discontinuous and transparent differences in pension rights would arguably cause less disruption in terms of negatively reciprocal

responses, and would reduce the possibility of workers with similar characteristics but different pension rights comparing themselves with each other. However, we should note that we cannot provide a definite answer on the debate on the optimal design of pension reforms since we did not have access to data on the adaptation costs and could not assess how younger cohorts would be affected by a more gradual discontinuity rule. Nevertheless, our findings imply that it is relevant to include the distortive effects of a hard discontinuity in the cost-benefit analyses of the various policy options.

Large organizations partially accommodate the new situation by providing more training to the affected workers. These additional training investments are extremely relevant, since they not only aim to maintain the productivity of those who have to work longer, but also can partly counter the de-motivational effects of the pension reform. The results in this thesis, however, indicate that there could be scope to further promote training investments among treated workers in small organizations that appear to be less capable to customize their training policies to the new retirement system.

7.4 Future research

The analyses conducted in this thesis reveal several remaining open questions that identify the scope of future research. For example, the long-term consequences of the pension reform should be analyzed in more depth when future survey waves become available and when those treated by the reform and the control group actually retire. Although the present analyses suggest that the effect of the reform persists over time, one should acknowledge that to conduct a cost-benefit analysis, it is relevant to continue following the development of the well-being and mental health of the workers in our sample in the coming years. Of particular interest in this context is assessing whether the worsened mental health of the treated workers affects the inflow of workers in disability insurance schemes. Moreover, whether the well-being and mental health of treated workers recover after retirement is also a highly relevant question. In addition, further empirical work on actual retirement behavior is warranted. Since most individuals in the sample will retire in the next few years, continuation of the survey will enable an analysis of the effects of the reform on actual retirement decisions and an assessment of the extent to which the retirement expectations as measured in the past three survey waves were accurate.

7 *Conclusions*

The external validity of future research can be increased by gathering additional data for younger cohorts and employees in the private sector. For this research project, data were available only for male public sector employees born in 1949 or in 1950. It is worth investigating how quickly the results fade away when including workers who were born farther away from the treatment threshold, and whether the behavioral responses of both workers and employers in the private sector are similar to those in the public sector.

In addition, more generally, it is of crucial importance to develop a good understanding of employers' reactions to pension reforms, that actually induce their older employees to retire later but also substantially decreases their job efforts. The analyses in Chapter 3 provide a first attempt in this direction by showing that only large organizations accommodate the policy change. An additional employer survey should address this issue further by providing more information on the types and effectiveness of HRM practices employed by organizations to motivate workers with retrenched pension rights. Finally, more information is needed on whether employers are developing alternative escape routes through which they can still send their older, de-motivated employees with retrenched pension rights into an early labor market exit.

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Summary in Dutch

Het doel van deze studie is te bepalen in welke mate hervormingen van pensioenstelsels een effect hebben op trainingsparticipatie, de geleverde inspanning op het werk, het welzijn en de mentale gezondheid van oudere werknemers. Deze onderzoeksvraag is momenteel uitermate relevant vanwege het toenemende aantal gepensioneerden in de Europese Unie. Verwacht wordt dat in vrijwel alle Europese landen de publieke pensioenuitgaven als percentage van het bruto binnenlands product met 2,7%-punt zullen stijgen tot 14,6% in 2050 (Economisch Beleidscomité, Europese Commissie, 2006). Bovendien moet op basis van prognoses van de Organisatie voor Economische Samenwerking en Ontwikkeling (OESO) op de middellange termijn rekening worden gehouden met ernstige arbeidsmarkttekortingen, omdat verwacht wordt dat vanaf 2015 in Europa het aantal gepensioneerden het aantal jonge arbeidsmarkttoetreders zal overtreffen. Voor beleidsmakers is het derhalve van belang dat zij niet alleen aandacht schenken aan het verhogen van de arbeidsmarktparticipatie van ouderen, maar ook dat deze ouderen actief en productief zullen doorwerken.

Het grootste deel van de studies die het pensioengedrag bestudeerden, hebben zich beperkt tot het analyseren van de invloed van financiële prikkels in de pensioensystemen op de arbeidsmarktparticipatie (e.g., Stock en Wise, 1990; Gruber en Wise, 1997; Samwick, 1998; Bingley en Lanot; 2002; Chan en Stevens, 2004). Een veel kleiner aantal studies laat zien dat het pensioengedrag sterk gerelateerd is aan de menselijke kapitaalontwikkeling (trainingsinvesteringen en gezondheid) van oudere werknemers (e.g., Becker, 1975; De Grip et al., 2002; Lau en Poutvaara, 2006; Dave et al., 2008; Falba et al., 2009). Echter, het merendeel van deze studies is niet in staat om de causaliteit tussen de gezondheid van werknemers of trainingsinvesteringen en hun pensioengedrag te bepalen. Een uitzondering is de studie van Charles (2004) die vond dat pensionering een direct exogeen positief effect heeft op de men-

tale gezondheid. Daarnaast vonden Falba et al. (2009) dat de mentale gezondheid van oudere werknemers negatief wordt beïnvloed wanneer wordt afgeweken van een eerder verwachte pensioendatum. Dit laatste mechanisme impliceert dat de pensioenhervormingen die momenteel voorgesteld of geïmplementeerd worden in diverse Europese landen weliswaar mogelijk de pensionering uitstellen, maar ook de productiviteit van oudere werknemers negatief kunnen beïnvloeden. Het is deze notie, waaraan door zowel in de beleidswereld als in economisch onderzoek tot nu toe grotendeels is voorbijgegaan, die als motivatie dient voor het onderzoek in deze thesis.

In deze studie wordt een zogenaamd natuurlijk experiment gebruikt om het effect van hervormingen van het pensioenstelsel op trainingsinvesteringen, de geleverde inspanning op het werk, het welzijn en de mentale gezondheid van oudere werknemers te identificeren. De contributie van deze studie is tweeledig. Allereerst draagt die bij aan de bestaande literatuur van het menselijk kapitaal door de invloed van veranderende pensioenverwachtingen op trainingsinvesteringen en de mentale gezondheid van oudere werknemers te analyseren. Beide zijn relevante indicatoren van de individuele productiviteit van werknemers. Door het hanteren van een natuurlijk experiment wordt omgekeerde causaliteit uitgesloten. Ten tweede gaat deze studie in detail in op de potentiële neveneffecten van de hervorming van het pensioenstelsel door gedragsreacties nader te analyseren. Meer kennis over hoe werknemers reageren op een ‘oneerlijke’ versobering in hun pensioenrechten en over hoe persoonlijke karaktertrekken hun reactie beïnvloeden kan helpen om toekomstige hervormingen van het pensioenstelsel te faciliteren. Daarbij wordt specifiek aandacht besteed aan hoe reciproque gevoelens en sociale vergelijkingen tussen werknemers hun gedrag beïnvloeden.

Het experiment is gebaseerd op een verandering in het pensioensysteem van de publieke sector met als implicatie dat een subgroep van werknemers in de publieke sector wordt geconfronteerd met een substantiële verlaging van hun pensioenrechten. De *Wet Vut, Prepensioen en Levensloop* (Wet VPL) luidde vanaf 2006 voor alle werknemers die geboren zijn na 1949 de afschaffing in van de fiscale aftrekbaarheid van de pensioenpremies die worden afgedragen aan de sectorale pensioenfondsen. In de publieke sector werd deze verandering in de belastingregels gebruikt als input voor de collectieve onderhandelingen tussen de sociale partners voor de introductie van een nieuw pensioensysteem. Het nieuwe pensioensysteem werd geïntroduceerd op 1 januari 2006 en schrijft voor dat werknemers in de publieke sector die geboren zijn voor 1950 hun oude meer genereuze prepensioenrechten (*FPU-regeling*) behouden indien zij sinds 1 april 1997 ononderbroken hebben gewerkt in de publieke sector, terwijl diegenen die geboren zijn na 1949, en degenen die voor 1950 geboren zijn

maar een onderbreking hadden in hun loopbaan, onder het minder genereuze *ABP Keuzepensioenen* vallen. De verandering in het pensioensysteem houdt in dat een werknemer die geboren is in 1949 met 40 dienstjaren kan uittreden op een leeftijd van 62 jaar en drie maanden tegen een vervangingsratio van 70%, terwijl eenzelfde werknemer die geboren is in 1950 op dezelfde uittredeleeftijd een pensioen ontvangt dat equivalent is aan een vervangingsratio van 64%.

Voor de analyses is gebruik gemaakt van gekoppelde survey en administratieve paneldata voor mannelijke full-time werknemers in de publieke sector die geboren zijn in 1949 of 1950. De administratieve gegevens komen van het ABP. De survey data zijn beschikbaar voor drie jaren (2007-2009) en zijn verzameld in twee fasen. In de eerste fase zijn alle 27,871 mannelijke werknemers in de publieke sector die zijn geboren in 1949 of 1950 door het ABP per brief aangeschreven met het verzoek om deel te nemen aan het onderzoek en om hun e-mailadres af te staan. In de tweede fase kregen degenen die hebben aangegeven dat zij wilden meewerken (11,458 werknemers stonden hun e-mailadres af) een e-mail met daarin de link naar de vragenlijst. Dit leverde een unieke dataset op voor respectievelijk 7.739 (2007), 6.078 (2008), en 7.711 (2009) mannen die zijn geboren in 1949 of 1950.

In Hoofdstuk 2 wordt de interne en externe validiteit van onze onderzoeksofzet onderzocht. Een groot voordeel van het exploiteren van een natuurlijk experiment is dat men de exogene variatie die wordt gegenereerd door een beleidsverandering kan gebruiken om causale verbanden vast te stellen. Echter, hoewel een natuurlijk experiment substantiële voordelen met zich meebrengt, is het relevant om ook te kijken naar de potentiële beperkingen van een experimentele onderzoeksofzet. Uit dit hoofdstuk blijkt dat de interne validiteit van het onderzoek grotendeels wordt gegarandeerd door het beperkte leeftijdsverschil tussen de twee groepen medewerkers in onze steekproef. Door het beperkte leeftijdsverschil hebben de respondenten die geboren zijn in 1949 of 1950 vergelijkbare kenmerken, met als uitzondering dat zij verschillende pensioenrechten hebben als gevolg van het simpele transparante leeftijds criterium dat bepaalt of men onder het oude of het nieuwe systeem valt. De respondenten waren ook niet op de hoogte van het experimentele karakter van het onderzoek. Nadere analyses tonen bovendien aan dat er geen significante verschillen vanwege zelfselectie zijn tussen de respondenten die geboren zijn in 1949 of 1950, ondanks dat zij wel accurate pensioenverwachtingen hebben. De focus op mannen die geboren zijn in 1949 of 1950 en werken in de publieke sector impliceert wel dat de generaliseerbaarheid van de resultaten uit de vervolghoofdstukken mogelijk beperkt is. In Hoofdstuk 2 beargumenteren wij echter dat de resultaten uit deze studie zeer relevant blijven, aangezien deze twee cohorten behoren tot de babyboomgeneratie.

Het is juist deze generatie die beleidsmakers wenst te stimuleren om langer door te werken. De resultaten zijn daarnaast eveneens relevant voor jongere cohorten doordat de voorgestelde plannen om de AOW-leeftijd te verhogen eenzelfde discontinuïteit in pensioenrechten introduceert voor jongere cohorten. Bovendien worden er in andere Europese landen eveneens voorstellen gedaan om de pensioensystemen voor de werknemers van de publieke sector te hervormen. Wij beargumenteren ook dat de resultaten van belang zijn voor werknemers in de private sector. In de laatste twee decennia heeft er een substantiële convergentie plaatsgevonden in de arbeidscondities van publieke en private werknemers en binnen de publieke sector worden in toenemende mate beloningssystemen geïntroduceerd die meer ruimte bieden voor flexibele beloningsvormen en prestatiebeloning.

In Hoofdstuk 3 wordt één jaar na de invoering van het nieuwe pensioensysteem het effect onderzocht van de veranderde pensioenaanspraken op de trainingsparticipatie van oudere werknemers. De trainingsparticipatie van werknemers die geboren zijn in 1949 en daarom nog recht hebben op het oude genereuze prepensioen wordt vergeleken met die van degenen die zijn geboren in 1950 en vallen onder het nieuwe systeem dat uitstel van pensionering bevordert. We vinden dat de pensioenhervorming leidt tot een verhoging van de verwachte pensioenleeftijd en dat degenen die geboren zijn in 1950 meer trainen dan de werknemers die geboren zijn in 1949. Dit resultaat is consistent met de theorie van het menselijk kapitaal die voorspelt dat pensioensystemen die prikkels geven tot vervroegde uittreding een negatief effect hebben op de ontwikkeling van menselijk kapitaal, aangezien dergelijke prikkels de periode waarin mensen hun trainingsinvestering kunnen terugverdienen verkort. Echter, de invloed van de hervorming is alleen statistisch significant voor werknemers in grotere organisaties. Dit suggereert dat grotere organisaties beter in staat zijn om hun trainingsbeleid aan te passen aan het nieuwe pensioensysteem. Een mogelijke verklaring hiervoor is dat zij beter ontwikkelde human resource management (HRM) systemen hebben dan kleinere organisaties (Koch en McGrath, 1996). De invloed van de pensioenhervorming blijkt robuust te zijn als leeftijd als onafhankelijke variabele wordt toegevoegd of als alternatieve specificaties worden gehanteerd. Daarnaast vinden wij dat het effect van de versoberde pensioenrechten kleiner is voor hoger opgeleiden, waarschijnlijk omdat zij een hogere intrinsieke motivatie hebben om te trainen, ongeacht de schok in hun pensioenrechten. De invloed van de pensioenhervorming is verder alleen statistisch significant voor de participatie in langere trainingscursussen. Dit zijn waarschijnlijk inderdaad de duurdere cursussen die pas rendabel worden wanneer iemand langer doorwerkt.

In Hoofdstuk 4 worden de effecten van de hervorming van het pensioensysteem

op de mentale gezondheid van degenen met versoerde pensioenrechten onderzocht. Een vergelijking van de mentale gezondheid tussen werknemers geboren in 1949 of 1950 twee jaar na de hervorming van het pensioensysteem laat zien dat de afschaffing van de FPU een sterk negatief effect heeft op de mentale gezondheid. Het effect blijkt persistent te zijn over de tijd en wij vinden dat mensen die geboren zijn vlak na 1 januari 1950 vaker depressief zijn dan degenen die later in dat jaar zijn geboren. Wij vinden overigens dat het uitstel van de verwachte pensioenleeftijd maar gedeeltelijk verantwoordelijk is voor de lagere mentale gezondheid onder degenen die geboren zijn in 1950. Het effect verschilt sterk voor verschillende typen werknemers. Werknemers die ongetrouwd zijn en die geen recht meer hebben op FPU zijn significant vaker depressief, terwijl de invloed van de pensioenhervorming veel kleiner is voor werknemers met een partner die zelf een pensioen heeft opgebouwd of een eigen inkomen heeft na pensionering.

In Hoofdstuk 5 wordt geanalyseerd in welke mate negatief reciproque gedrag ten opzichte van de hervorming van het pensioenstelsel een effect heeft op de inzet op het werk van werknemers met versoerde pensioenrechten. Het is aannemelijk dat de sterke discontinuïteit in de pensioenrechten tussen degenen die geboren zijn in 1949 of 1950 door de laatstgenoemde groep als oneerlijk wordt beschouwd, vooral ook omdat de hervorming van het pensioenstelsel maar enkele jaren voor hun verwachte pensioenleeftijd werd aangekondigd. Dit hoofdstuk vult eerdere resultaten van laboratoriumexperimenten aan door de effecten van een versoering in pensioenrechten te analyseren in de feitelijke arbeidsmarkt. Onze maatstaf voor reciproque gedrag is ontleend aan drie surveyvragen, terwijl werkmotivatie wordt gebruikt als proxy voor de inzet die werknemers leveren op het werk. Wij vinden dat de werkinzet significant lager is bij werknemers die sterk negatief reciproque zijn en versoerde pensioenrechten hebben. Het negatieve effect op de werkmotivatie is het sterkst voor negatief reciproque werknemers die in de eerste drie maanden van 1950 zijn geboren. Het is aannemelijk dat juist deze werknemers de pensioenhervorming als het meest oneerlijk beschouwen. Daarnaast blijkt dat de werkmotivatie substantieel lager is voor negatief reciproque werknemers die direct hun eigen werkgever verantwoordelijk kunnen houden voor de versoering van hun pensioenrechten, namelijk degenen die werken in de sector Rijk dat de aanzet gaf tot de hervorming.

In Hoofdstuk 6 worden de effecten van de hervorming van het pensioensysteem op het welzijn geanalyseerd. We vinden sterke en persistente effecten op het welzijn en de baantevredenheid van de werknemers met versoerde pensioenrechten die relatief groot zijn ten opzichte van inkomenseffecten op welzijn. Dit hoofdstuk laat zien dat een groot gedeelte van de invloed van de afschaffing van de FPU kan worden ver-

klaard door sociale vergelijkingen tussen degenen die wel of niet recht hebben op de oude genereuze pensioenrechten. Wij vinden dat werknemers hun eigen verwachte pensioeninkomen vergelijken met het verwachte gemiddelde pensioeninkomen van werknemers met dezelfde kenmerken binnen hun organisatie, en dat hun welzijn ook negatief wordt beïnvloed door een schok in pensioenrechten van hun partner. Echter, het leeuwendeel van het effect van de hervorming van het pensioensysteem op het welzijn kan worden verklaard door non-monetaire prikkels die gerelateerd zijn aan de sterke discontinuïteit van de hervorming. Ondanks het feit dat wij rekening houden met monetaire prikkels door het individuele pensioeninkomen en het gemiddeld pensioeninkomen van vergelijkbare individuen in dezelfde organisatie als controlevariabelen in de analyse op te nemen, vinden wij dat het welzijn van werknemers met versoerde pensioenrechten minder sterk wordt beïnvloed als het percentage werknemers dat geboren is in 1950 relatief groot is in hun organisatie. Anderzijds heeft het percentage werknemers dat geboren is in 1949 een negatief effect op het welzijn van de werknemers met versoerde pensioenrechten.

De onderzoeksresultaten van deze studie hebben belangrijke beleidsimplicaties. Allereerst houden de meeste modellen die gebruikt worden voor kosten-baten analyses van de verschillende beleidsopties om pensioensystemen te hervormen geen rekening met de negatieve versturende effecten die deze hervormingen kunnen hebben op de mentale gezondheid en de inzet van werknemers op het werk. Een goed voorbeeld hiervan is een recente simulatiestudie van het Centraal Planbureau (2009) waarin de effecten worden geanalyseerd van de verhoging van de AOW-leeftijd van 65 naar 67 jaar op de overheidsuitgaven (geprognosticeerde daling van de uitgaven van 0,7% van het bruto binnenlands product). De onderzoeksresultaten van dit proefschrift suggereren dat de voordelen van deze pensioenhervorming substantieel lager kunnen uitvallen. Ten tweede zijn de gevonden resultaten van belang voor het debat over de wijze waarop pensioenhervormingen vorm moeten worden gegeven. Een eenvoudige en transparante vormgeving van transitieregelingen beperkt de administratieve kosten. Daarentegen, vinden wij in deze studie dat een sterke discontinuïteit een sterk demotiverend effect kan hebben door sociale vergelijkingen en bovendien negatief reciproque gedrag kan oproepen. Dergelijke versturende effecten moeten worden meegenomen in de kosten-baten analyses van de verschillende beleidsopties. Ten derde blijkt dat voornamelijk grote organisaties zich aanpassen aan de nieuwe situatie door meer trainingen aan te bieden aan degenen die langer door moeten werken. Dit roept de vraag op in hoeverre beleidsmakers de trainingsparticipatie kunnen stimuleren van oudere werknemers die langer in kleine organisaties moeten doorwerken.

Biography

Raymond Michel Montizaan was born in Apeldoorn, the Netherlands, on April 27, 1980. He graduated from high school at Veluws College Walterbosch in 1999. He studied Domain Oriëntated Economics at Radboud University from 1999-2004 and Economics at Tilburg University from 2004-2005.

In 2004, he worked as a researcher at the Netherlands Bureau for Economic Policy Analysis (CPB) where he participated in a project on the welfare gains of multilateral trade liberalization.

In 2005, he started working as a researcher at the Research Centre for Education and the Labour Market (ROA) at Maastricht University. He participated in research projects on replacement demand and active aging. In September 2005, he obtained a METEOR grant to write his Ph.D thesis at ROA. Since 2009, he is affiliated to IZA and NSI.

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