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Worsening Workers' Health by Lowering Retirement Age: The Malign Consequences of a Benign Reform

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Center for Research in Economics, Management and the Arts

Worsening Workers' Health by Lowering
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Working Paper No. 2018-02

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Worsening Workers' Health by Lowering Retirement Age: The Malign Consequences of a Benign Reform*

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ABSTRACT: In 2003, the retirement age of Swiss construction workers was lowered from 65 to 60. This reform has been intended to improve their health. Our study shows the opposite outcome. The human capital theory suggests that investments in employees' productivity by the employer and the employees themselves depend on the time remaining until their retirement. Hence, we hypothesize that pension reforms that reduce employees' working horizon decrease investments in work-related human capital, which translates into a higher prevalence of sickness absences, a longer absence duration, and worse health. By econometrically comparing pre- and post-reform cohorts of construction workers with other blue-collar workers, we find that among 56–60-year-old construction workers, their sickness absences increase from 3.2% to 5.6%, their sickness duration increases by 33%, and their probability of having health problems increases from 9% to 12.7% due to the reform.

KEYWORDS: Pension reform, natural experiment, construction worker, sickness absence, sickness duration, poor health

JEL-Code: I12, J14, J26, L74

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1 Introduction

An aging population is one of the most pressing socioeconomic and political issues in developed countries. While most economists agree that it is necessary to increase the effective retirement age, older workers often face problems in the labor market because productivity declines and health deteriorates with age.¹ A standard claim is that many older workers are just unable to work for additional years because they are physically leached out and suffer from poor health. Therefore, it is often argued that employees engaging in heavy physical work should either be exempt from an increase in the statutory retirement age (SRA) or offered early retirement (Smulders *et al.* 2009). The most frequently referenced occupation with severe manual labor conditions is that of construction workers (Boschman *et al.* 2011, Dong *et al.* 2011, Boschman *et al.* 2013). This labor group suffers from massive work hazards. In the US, the construction sector had the highest absolute and relative rates of fatal work injuries in 2015.² In the European Union, more than 21.4% of all fatal accidents in the workplace took place in the construction sector in 2013.³ Furthermore, some empirical studies have examined the probability of occupational disability in the construction sector (Arndt *et al.* 2005, Dyreborg *et al.* 2010) and the relationship between disability and early retirement (Rothenbacher *et al.* 1998, Szubert and Sobala 2005, Alavinia *et al.* 2007). Considering the extremely high rate of disability in the construction sector, Lander *et al.* (2016) find no major improvement in workplace injuries between 1980 and 2010 in Denmark. This sobering result is in line with the findings of other authors who have investigated the effectiveness of safety and injury prevention programs (Kines *et al.* 2007, Hengel *et al.* 2013, Viester *et al.* 2015).

In this context, surprisingly, there is no evidence of how changes in the work horizon (i.e., the time left to retirement) influence the health outcomes among groups engaged in intensive manual work. The human capital theory states that investments increase according to the length of the work horizon. Hence, increasing (decreasing) the work

1 For a summary of the reforms implemented between 2009 and 2013, see the report of the Organisation for Economic Co-operation and Development (2013).

2 US Bureau of Labor Statistics (2016), <https://www.bls.gov/iif/oshwc/cfoi/cfch0014.pdf>

3 Eurostat (2017), <http://ec.europa.eu/eurostat/statistics-explained/>

horizon increases (decreases) the health investment before retirement, which should lead to better (worse) health outcomes.

Consistent with this theoretical argument, there is sparse albeit growing empirical evidence that it is not only an individual's biological age, but also the duration until retirement, that determine employability before the retirement age (Hairault *et al.* 2010, Bauer and Eichenberger 2017). While employability depends on human capital, the latter relies on investments, which in turn depend on the expected time until retirement. From this perspective, we hypothesize that lowering the SRA has a negative side; it hampers human capital investment and results in lower human capital as mirrored in employees' health-related activities and health status.

If a change in the SRA has an impact on health hazards for groups engaged in hard manual labor before retirement, we expect the older workers in this group to be mainly affected for several reasons. First, sickness absences and their duration increase as work life progresses (Brenner and Ahern 2000, Alavinia *et al.* 2009). Second, older construction workers have more health complaints (Hoonakker and van Duivenbooden 2010), are exposed to greater overload in manual work (Jebens *et al.* 2015), and have a reduced ability to work (Liira *et al.* 2000). Third, Chau *et al.* (2004) document an increased level of injuries requiring hospitalization of older workers, whereas Schwatka *et al.* (2012) find no effect of age on the number, only on the severity of injuries. Finally, older workers are disproportionately affected by retirement reforms since their remaining work life is relatively short; thus, its relative change due to an adjustment in the pension age is relatively large.

To our best knowledge, this paper is the first to provide an analysis of how lowering the retirement age jeopardizes the health of older workers by increasing the probability and duration of their sickness absences, as well as their probability of suffering from poor health. We focus on a pension reform in Switzerland in 2003, which reduced the retirement age of Swiss construction workers from 65 to 60. We concentrate on male construction workers who are close to retirement. The reform is of special interest for three reasons. First, almost all empirical investigations that we have reviewed have examined reforms that increase the SRA and the early retirement age (ERA). Such pension reforms are most often accompanied by other policy measures that aim at

increasing employment and thus tend to inflate the effects of the pension reforms. In contrast, in the reform under our investigation, such accompanying measures would attenuate the effect in which we are interested. Thus, our test is conservative. Second, our reform covers only a subgroup of all workers. This focus allows us to compare the treatment group with control groups that have not been subject to the reform. Third, the reform is a highly subsidized early retirement plan, since there is no actuarially fair increase in the contribution or reduction in rents for those who can retire at 60, which attenuates selection problems.

We estimate the causal impact of this reform on health outcomes by using a difference-in-differences model and longitudinal data from the Swiss Labor Force Survey (SLFS). To identify the effects of the policy change in the construction sector, we use two different approaches. First, we compare construction workers with other blue-collar workers with similar characteristics but from branches without such a reform (Approach I). Second, we compare older construction workers (aged between 56 and 60) with a variety of younger cohorts (Approach II). Approach II assumes that younger workers are relatively less affected by the reform. The main results of the empirical estimates are as follows. First, after the reform, the employment rate for construction workers does not only decrease in the 61–65-year-old cohort but also in the 56–60 group. Second, the probability of being absent for at least one day of the previous week increases from 3.2% to 5.6% among 56–60-year-old construction workers. Third, the duration of absences in the 56–60 age group is positive and statistically significant, increasing from 0.132 to 0.176 points (33%). Fourth, the probability of poor health increases from 9% to 12.7% among workers aged 56 to 60. Comparisons among different age cohorts of construction workers (Approach II) yield similar results. Fifth, we explore by how many years construction workers are endogenously aging as a result of the reform. More precisely, in terms of absences and absence duration, 55-year-old workers in the post-reform period are similar to 58-year-old workers in the pre-reform period. Finally, regarding health outcomes, 55-year-old workers in the post-reform period can be compared with their 57-year-old blue-collar workers.

The remainder of this paper is organized as follows. Section 2 integrates our contributions with the relevant literature. Section 3 characterizes the reform program. Section 4 provides information on the data, variables, and treatment groups. Section 5 explains the

empirical strategy and identification. Section 6 presents the results and offers some robustness checks. Section 7 concludes this paper.

2 Relation to the literature

Our paper complements a growing branch of literature investigating how retirement, the transition to retirement, and pension reforms affect health outcomes after retirement.⁴ The scope of our study goes beyond that of existing research in two ways. First, instead of examining the impact on health after retirement, we investigate health outcomes for cohorts who are close to retirement. Second, many of the previous studies suffer from endogeneity concerns because individuals' decision to retire early is often driven by their health status or other personal problems. In contrast, in the reform under our investigation, retirement at the lower age of 60 covers all construction workers and is highly subsidized. Thus, endogeneity problems are reduced because the reform exogenously determines the new retirement age.

A few studies analyze the effect of pension reforms on mortality rates after retirement. In Switzerland, Lalive and Staubli (2014) and in Sweden, Hult *et al.* (2010) and Hagen (2016) find that an increase in the retirement age has no effect on mortality. In contrast, Hallberg *et al.* (2015) show that offering early retirement to the military corps at 55 (down from 60) reduces mortality among those aged 56 to 70. The overall effect of retirement on health is also ambiguous. Some studies find that retirement has a positive effect on health status and reduces the use of healthcare (Coe and Zamarro 2011, Eibich 2015) and that earlier retirement is better for health (Westerlund *et al.* 2009). Other authors conclude that retiring later improves wellbeing (Gall *et al.* 1997), promotes better health (Alavinia and Burdorf 2008, Calvo *et al.* 2013), and reduces mental and physical fatigue (Westerlund *et al.* 2010). Other studies find nuanced effects. For example, Johnston and Lee (2009) show that retirement is beneficial for subjective wellbeing and mental health but has a detrimental effect on objective physical health. In contrast, a few studies illustrate that cognitive functions weaken after retirement (Bonsang *et al.* 2012,

⁴ For a theoretical approach to aging, pension reform, and retirement in a life cycle model, see Grafenhofer *et al.* (2007), Jaag *et al.* (2010), and Keuschnigg *et al.* (2011).

Mazzonna and Peracchi 2012, Celidoni *et al.* 2017). On the other hand, Shai (2018) shows that health outcomes are negatively impacted by an increase in the retirement age, and the effect is especially strong on people with lower educational levels. Finally, several studies report that retirement and its timing have no influence on health outcomes. For instance, van Solinge (2007) shows that only involuntary retirement decreases subjective health, while Butterworth *et al.* (2006) find that later retirement generally has no effect on health. Finally, van Zon *et al.* (2016) estimate that a higher retirement age leads to better functional health before the age of retirement and to decreased functional health after retirement. However, van Zon *et al.* (2016) consider differences in *de facto* individual retirement ages only (i.e., no pension reform) but do not effectively account for endogeneity issues.

This present study is related to a strand of literature dealing with how a change in the retirement age affects individuals prior to retirement. While the impact on employability has already been mentioned, some studies investigate how a change in the retirement age (ERA or SRA) influences investment in older workers, typically measured as participation in training (Fouarge and Schils 2009, Montizaan *et al.* 2010, Brunello and Comi 2015). Moreover, Bauer and Eichenberger (2017) estimate how an increase in the SRA for females increases not only training for older workers but also their own job involvement, the support they receive from their colleagues, and their participation in leisure activities. Bertoni *et al.* (2016) focus on investment in health following an increase in the ERA. Finally, several studies examine the spillover effects of a higher retirement age on other pension systems (Duggan *et al.* 2007, Staubli and Zweimüller 2013, Atalay and Barrett 2015). Those studies also assess the effects on workers who are close to retirement (i.e., the working population), and they adopt the idea that the time remaining until the expected retirement plays an important role. As they all evaluate instances where SRA or ERA has been increased by a change in the regulation, so far, there is no evidence on the impact of lowering the retirement age on health outcomes for the working population.

Our paper makes three main contributions. First, to our best knowledge, it is the first to focus on this highly informative reform setting that allows investigating how the retirement age affects the employability of older workers. While other authors (e.g., Staubli and Zweimüller 2013) have shown that an increase in the ERA enhances the

employability of older workers, we strengthen their arguments by examining what occurs if the retirement age is lowered. Given Staubli and Zweimüller's (2013) findings, we expect the complementary result that lowering the retirement age decreases the older age groups' probability of working. If this argument applies, employment decreases not only among workers aged 61 to 65, but due to anticipatory effects, also among those aged 56 to 60.

Our second main contribution lies in investigating how lowering the retirement age affects the health and health-related work behavior of older workers before they reach the new retirement age. More precisely, we estimate the effects of the pension reform on sickness absences, the duration of these absences, and health problems. For instance, Bertoni *et al.* (2016) examine how postponing the ERA in Italy increases the behavior related to health investment before retirement, through increased physical activity, reduced smoking and alcohol consumption, and improved dietary habits, but they do not investigate health outcomes. Given this evidence, we expect that reducing the working horizon impacts the health behavior of individuals aged 56 to 60, as well as their employers' investments, translating into more and longer sickness absences and poor health.

Our third contribution is uncovering the quantitative effect of changing the SRA on social aging. Reducing the SRA by five years makes 55-year-old workers exhibit similar health outcomes as 58-year-old workers before the reform. Thus, for individuals whose biological age is 55, the reform inflates their social age to 58.

3 The pension reform in the Swiss construction sector

3.1 The Swiss pension system

Switzerland has a three-pillar pension system.⁵ The first pillar is a public and nationwide pay-as-you-go (PAYG) system. It is mandatory for everyone and highly redistributive. The second pillar consists of employer-specific occupational pension plans, which are capital funded. They are mandatory for all salaried workers. Combined, the first and the second pillars are intended to ensure at least 60% of the worker's last income. The privately funded third pillar complements the pension system.⁶ The SRA defines eligibility for the first and the second pillars. It is set at 65 years of age for men and 64⁷ for women.⁸ Since the 10th revision of the pension insurance scheme (enacted in 1997), early retirement under the public pension scheme has become possible (Luisi 2007). The ERA is set two years before the SRA (i.e., 63 for men and 62 for women) but imposes a lifelong pension cut of 6.8%.

3.2 Reform in the construction sector in 2003

In the early 1990s, a public discourse emerged regarding the improvement of the physical health of construction workers in their old age. The Association of Entrepreneurs in Construction formulated the idea of an old-age fund to facilitate early retirement and bridge the pension gap until the SRA. However, it was only in 2000 when the Geneva Cantonal Office for Work Inspection and Relations published a report that reanimated the discussion. It was estimated that for men born between 1925 and 1927, the general probability of being an invalid upon reaching the SRA was 15.2%. However, for workers in the construction sector, the probability was 40%, the highest level of all branches (Gubéran and Usel 2000). At the end of 2002, the Swiss Association of Entrepreneurs in Construction and the labor unions representing construction workers⁹ agreed on the

5 For further information, see the Swiss Federal Administration, <https://www.bsv.admin.ch/>

6 Under the system, 100% of retirees are covered by the first pillar, 68% by the second pillar, and only 28% by the third pillar (see the Swiss Federal Office of Statistics).

7 The retirement age for men has been 65 since the implementation of the system in 1948. The retirement age for women was 65 from 1948 to 1963, 62 from 1964 to 2000, and 63 from 2001 to 2005.

8 Firms are allowed to provide their employees with better conditions (i.e., grant their employees earlier retirement with full benefits at the cost of the firms).

9 These are the two largest unions in Switzerland: Unia (formerly Gewerkschaft Bau & Industrie [GBI]) and Syna.

Collective Working Convention for Flexible Anticipated Retirement¹⁰ (FAR plan), which was enacted in July 2003.¹¹

The most important change brought about by the FAR plan was a stepwise introduction of the lower retirement eligibility in the construction sector. As shown in Figure 1 and Table A-1, the decrease was gradual; specifically, construction workers retired at the age of 63 in 2003, 62 in 2004, 61 in 2005, and since January 2006, at the age of 60. Hence, the cohort of 1946 was the first one to retire at the age of 60.

Figure 1: Retirement age according to the FAR plan



Source: Authors' own compilation

Figure 2 provides an overview of the groups affected by the reform. Workers in the main construction sector, who have been working there for the last seven years, are subject to the FAR plan and have a reduced retirement age of 60. Hence, the scope of the application covers general construction, civil engineering, underground mining, and roadbuilding. Employees of such firms are subject to the new law, including a) gangers; b) foremen; c) skilled workers, such as masons, carpenters, and road builders; d) construction workers; e) specialists, such as machine operators, chauffeurs, and isolators; and f) security personnel who ensure safety in track work. Managers and commercial personnel in the

¹⁰ In German, Gesamtarbeitsvertrag für den flexiblen Altersrücktritt im Bauhauptgewerbe (GAV FAR)

¹¹ For a detailed juridical review of the reform, see Keller (2008).

construction sector are not covered by the new law. Moreover, all other blue- and white-collar workers remain under the old SRA.

Given that the official SRA for men in Switzerland was not affected by the reform, the construction sector had to develop a plan to finance the general low retirement age of construction workers. The introduction of an old-age fund, a sector-specific PAYG system, facilitates the new retirement scheme. Contributions from employers (4%) and employees (1%)¹² constitute the assets of the new foundation. It provides the following benefits. First, entitled workers receive a pension of 80% of their last gross salary if it is in the normal range; for exceptionally high incomes, the share decreases. Second, until reaching the SRA, entitled workers have their contributions for the first and the second pillars paid to be eligible for public and occupational pensions upon reaching the age of 65. Moreover, construction workers can top up their pensions through work to a maximum of 20%.

Figure 2: Scheme of pension reform in the construction sector

Treatment intended by reform FAR plan (T)		No treatment intended by reform (C)
<p align="center">Construction sector (including construction workers (building and civil construction and underground mining), gangers, and carpenters) <i>In 2003: ~ 260,000 workers</i></p>		<p align="center">All other sectors (including blue-collar and white-collars, leading positions in construction sector). <i>In 2003: ~ 2 million workers (where 500,000 are other blue-collar workers)</i></p>
No FAR plan (T.1.)	FAR plan (T.2.)	
Before 2003: No flexible anticipated retirement scheme. Full retirement age 65, also for construction workers.	<p>2003-2005: Transition phase. Flexible anticipated retirement scheme for workers aged 63 in 2003, 62 in 2004, 61 in 2005. Full pension if 15 years full time in this sector (during the last 20 years). (T.2.1.)</p> <p>After 2006: Flexible anticipated retirement scheme for workers aged 60. Full pension if 15 years full time in this sector (during the last 20 years). (T.2.2.)</p>	
No flexible anticipated retirement	Flexible anticipated retirement	No flexible anticipated retirement

Source: Authors' own compilation

¹² Since July 2002: 2% for employers and 0.5% for employees; since January 1, 2003: 3% for employers and 0.75% for employees; since January 1, 2004: 4% for employers and 1% for employees; since July 1, 2016, 5.5% for employers and 1.5% for employees.

4 Data and variables

The empirical specifications are based on the Swiss Labor Force Survey (SLFS). The SLFS is an individual questionnaire aimed at permanent Swiss residents of all working and non-working statuses. It is a longitudinal survey, which started in 1991. The sample is randomly generated by using information from public registers and is representative of the Swiss population. We restrict our dataset to males working full time (specified as more than a 90% work contract). Women are excluded for two main reasons. First, similar to the case in most of the developed countries, only a few women work in the Swiss construction sector. Thus, we fear that these women comprise a specific selection, to a certain extent. Second, there was a general pension reform specifically for women at the same time, which could lead to confounding interpretations of our results. Moreover, we exclude all self-employed individuals (with or without employees), all apprentices, individuals who own firms, those working in family firms, as well as individuals occupying management positions. For the main specification, we restrict the sample to three pre- and two post-reform waves, that is, the years 2001 ($K-2$) to 2005 ($K+2$). Year K (2003) is treated as a pre-reform period because the interviews were all held before July 2003 and thus prior to the enactment of the reforms. For robustness tests and gauging of the long-term effects, we extend the sample across more periods, covering 1999 to 2007, 1997 to 2009, 1995 to 2011, and 1991 to 2015. Furthermore, we exclude two out of 26 Swiss cantons from our analysis. The cantons Vaud (VD) and Valais (VS) implemented specific early retirement schemes for construction workers, which were only later harmonized with those of the other cantons. Therefore, we exclude them from our sample. Finally, we only include individuals aged between 20 and 65.

4.1 Treatment and control groups

As shown in Figure 2, the reform covers construction workers from the period after 2003. For our empirical approach, we contrast different groups, always having one treatment (T) and one control group (C). First, we compare construction workers (T) with other blue-collar workers (C) (Approach I). This allows us to compare similar groups, since blue-collar workers are closer to construction workers. Hence, the two groups will less likely differ in their covariates. The descriptive statistics in Table A-2 suggest that this

assumption is valid. The differences between the two groups are quite small. The fulfillment of this comparison is a necessary condition to satisfy the main identification issue in the difference-in-differences technique, i.e., the common-trend assumption. In other words, in the post-treatment period, the expected potential outcome of non-treatment is not related to the treatment status (Lechner 2010). For Approach I, we specify a treatment dummy as follows:

$$TREAT1_{it} = \begin{cases} 1 & \text{if construction} \\ 0 & \text{if other blue collar} \end{cases}$$

Second, we contrast older (T) construction workers to younger (C) ones (Approach II). Although the whole sector is affected by the reform, we expect the effect to be disproportionately high for older workers (i.e., 56–60 years old). There are two reasons for this assumption. First, investments in human capital depreciate over time, and second, returns on these investments have to be discounted. Thus, the present value of the return on investments for an additional year of work exponentially grows when retirement becomes closer. This means that older construction workers clearly differ from younger ones, a phenomenon that we test in Approach II.¹³ Therefore, we construct a new treatment dummy, where we compare older construction workers (56–60 years old) with different groups of younger ones (36–40, 41–45, 46–50, and 51–55 years old), as follows:

$$TREAT2_{it} = \begin{cases} 1 & \text{if old construction worker} \\ 0 & \text{if young construction worker} \end{cases}$$

To pursue the two approaches, it is inevitable that we must identify workers who fall under the new FAR plan. Unfortunately, no direct question addresses this criterion. However, the SLFS provides a four-digit occupational classification, with which we are able to categorize all of the jobs that fall under the new law. Moreover, we can exclude managers, the self-employed, and workers in firms owned by their families, none of whom fall under the FAR plan. For the two approaches, we use the respondents' first report of their occupation in the panel dataset to construct the treatment dummy. This allows us to clearly separate those affected and unaffected.

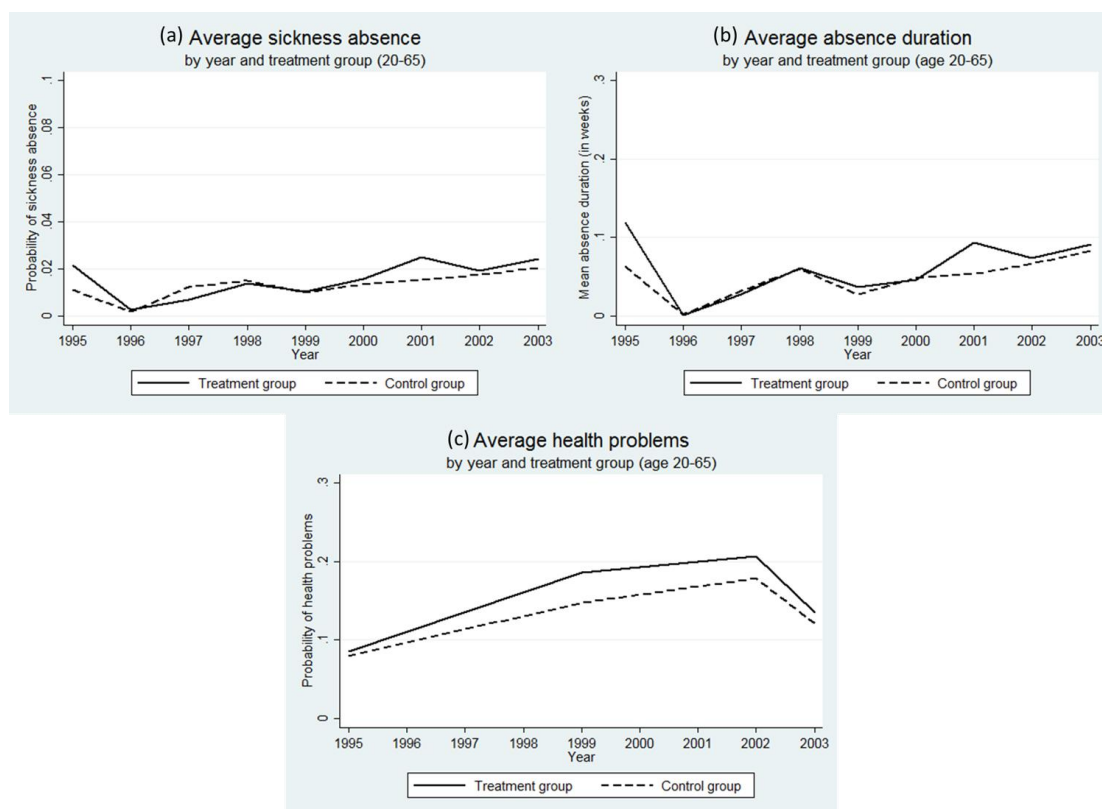
¹³ Moreover, it allows us to control for the so-called grey peril and to account for the fact that older workers might have different preferences about public policies (Mello *et al.* 2017).

4.2 Endogenous and exogenous variables

The SLFS is a rich dataset, which provides encompassing information on job characteristics. However, the data on health status are limited. We use the following questions to construct our three dependent variables. The first question is “Were you absent from work last week because of sickness/an accident/health problems?” This variable is collected over the whole period between 1991 and 2015. It is quite specific because it connects health problems to absence from the workplace. The first dependent variable, *absence due to sickness*, measures the proportion of workers who are absent from work because of health problems. The second question is “How long have you been absent: a) one week, b) two weeks, c) three weeks, d) one to three months, e) four to six months, f) seven to twelve months, or g) more than one year?” This second endogenous variable, *duration of absence*, is available over the whole period and measures the duration under seven categories. It is set zero, if the individual was not absent. The third set comprises four questions, as follows: “Did you receive an invalidity pension last year?” (between 1991 and 1995). “Did you have an accident last year?” (in 1999 and 2002). “Did you have a physical or a psychological problem last year?” (between 2003 and 2009). “Do you have illnesses or health problems?” (between 2010 and 2015). To construct our third dependent variable, *health problems*, we rely on those four questions, since the single questions were not asked over the whole period. We are fully aware that the specification of the variable may have an influence on the measured difference between the treatment and the control groups in Approach I. However, Approach II is unaffected by this problem.

Figure 3 plots the changes in the different outcome variables over time and per treatment group. A main concern to be considered is whether the common-trend assumption holds true. For all variables, there seems to be a similar time trend for general construction workers and other blue-collar workers before the reform. Especially, panels (a) and (c) show the clear-cut parallel trend in the pre-reform period. For panel (b), the curves differ more, where the treatment group has two marked peaks that are not fully mirrored by the control group, but a parallel trend remains. In sum, the graphs support the common-trend assumption. Nevertheless, they only provide a raw picture and do not control for specific determinants that could cause divergence between the two series.

Figure 3: Common-trend assumption: pre-reform average sickness absence (a), absence duration (b), and health problems (c) by year and treatment group



Sources: SLFS (1995-2003) and authors' own compilation

For our empirical analysis, we add several exogenous variables (the full set is found in Table A-2 in the Appendix) as the socioeconomic status might affect how the health status is reported (Angel 2016). Specifically, we control for standard personal characteristics (age, nationality, education, and marital status) and various job-related characteristics (e.g., experience, shift work, permanent contract, income category, work hours, and firm size), as well as year and regional fixed effects (seven central regions of Switzerland).

5 Empirical strategy

5.1 Effectiveness of reform and identification

The intention of the 2003 reform was to improve the health of construction workers by lowering their *de facto* retirement age. While the SRA for men in Switzerland is 65, the reform was designed to subsidize retirement at the age of 60 to such an extent that it became unappealing for construction workers not to retire at that new normal age. Given its aim, the reform provides no actuarially fair compensation to construction workers for full-time work beyond 60. From an economic perspective, such a reform can be expected to translate into a lower mean retirement age. Figure 4 shows the mean retirement ages for construction and non-construction workers between 1999 and 2009. Over time, the retirement ages in both sectors decline. However, the decline in the treatment group is more pronounced. Especially, two points in time are important. First, in mid-2001, the retirement age in the construction sector fell below that of the non-construction sector. This effect was mainly driven by an increase in the mean retirement age of the other blue-collar workers. Second, after the enactment of the reform in 2003, we observe the steepest fall in the retirement age of the construction sector. From this finding, we conclude that the reform mostly changed the employability once the reform had been enacted.

However, three mechanisms might make the effect of the reform on retirement look smaller than it actually is. First, some of the construction workers who formally retired at 60 possibly declared in the SLFS that they were not yet retired because *de jure*, their SRA remained at 65. Second, some of the construction workers who formally retired at 60 but still worked part time at no more than 20% (which is allowed as a pension top-up between the ages of 60 and 65) possibly reported that they were not yet retired. Third, anticipation effects could be factors of concern. While the employers' association and the labor unions surprisingly agreed so quickly on the reform at the end of 2002, we could not exclude the possibility that some workers had anticipated the reform for several months. If this would apply, they would be expected to be less likely to retire early (between the ages of 60 and 63) in 2002. Before the reform, it was only possible to retire early by transitioning to other social security systems (i.e., unemployment pension or invalidity pension) or part-time work (Been and van Vliet 2017). This option would be highly unappealing compared with the early retirement scheme, which has been applied in the construction sector since 2003.

Thus, we would expect more construction workers to delay retirement. Moreover, construction workers who retired early before the reform tended to be those who suffered from severe health problems, which prevented them from waiting for the reform. If this would apply, the observed retirement age in 2002 would be increased due to the reform, but those who continued to work would be in a poorer state of health. Thus, our tests tend to underestimate the effects of the reform on health; in other words, the tests are conservative. Of course, this picture only provides a raw indication. Thus, we validate it with an empirical test.

Figure 4: Mean self-reported retirement age over time in construction and non-construction sectors



Sources: SLFS (1999-2009) and authors' own compilation

Table 1 shows the linear probability estimations about whether the reform changed the probability of working from the age of 56 to 60, as well as from 61 to 65, in the interaction term TREAT*POST. While the fact that construction workers reduced their mean working activity between the ages of 61 and 65 is straightforward to explain, the effect of the reform on the work activity between the ages of 56 and 60 is more interesting. According to the theory of endogenous aging, the probability that an individual would still be working is expected to decline when the employee is approaching the statutory retirement age. Countries with a retirement age of 60 have the lowest employment rate

between the ages of 55 and 60 (Hairault *et al.* 2010). Thus, lowering the retirement age from 65 to 60 is also expected to have an effect on the probability of working between the ages of 56 and 60, as confirmed by the empirical results presented in Table 1. While the probability of still working between the ages of 61 and 65 decreases by 2.2 percentage points, the probability of working between the ages of 56 and 60 decreases by 3.2 percentage points.

Table 1: Probability of working, 1995–2011

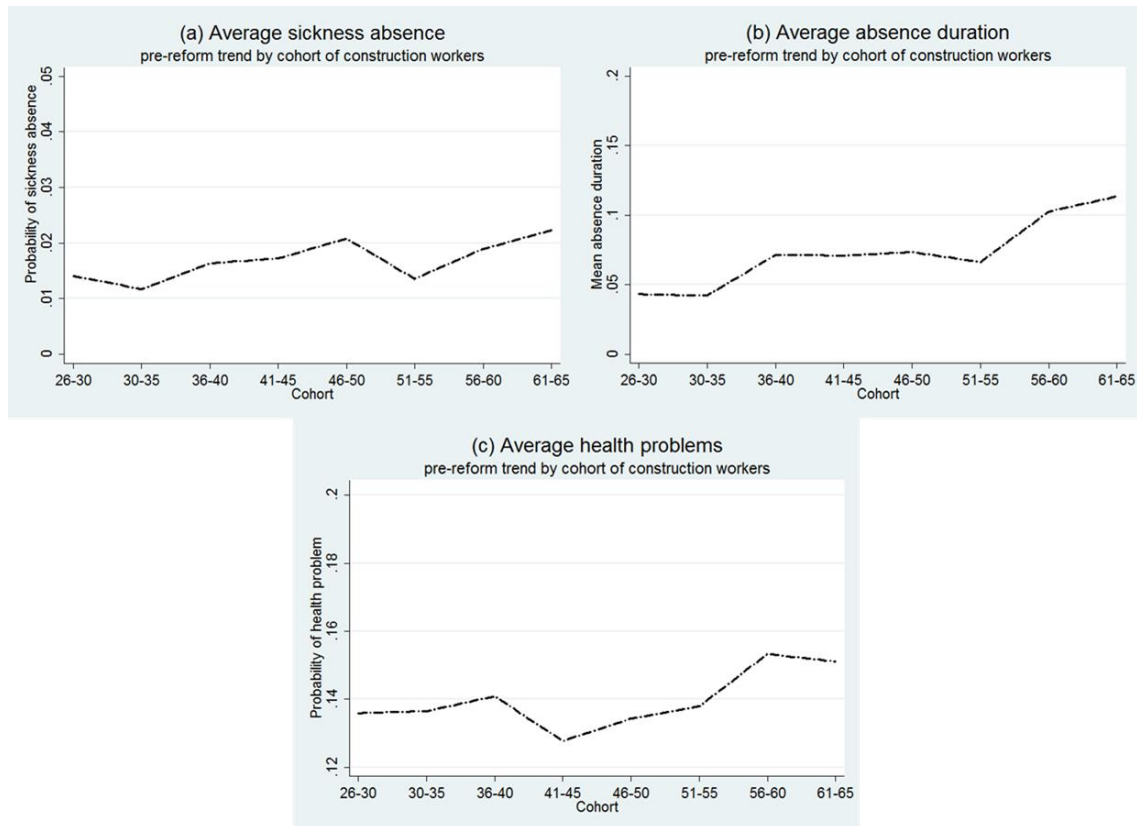
	Ages 56–60		Ages 61–65	
	(1) Probability of working	(2)	(3) Probability of working	(4)
TREAT	0.0448* (0.0236)	0.0416* (0.0232)	0.0409 (0.0316)	0.0367 (0.0315)
POST	0.0471** (0.0198)	0.00539 (0.0268)	0.0601** (0.0251)	0.00218 (0.0337)
TREAT * POST	-0.0630** (0.0280)	-0.0617** (0.0278)	-0.0754** (0.0371)	-0.0742** (0.0367)
Personal characteristics	yes	yes	yes	yes
Year trend	no	yes	no	yes
Regional dummies	no	yes	no	yes
R2	0.022	0.031	0.118	0.130
Observations	4629	4629	4199	4199
Marginal effects				
TREAT = 1; POST = 0	0.856***	0.854***	0.524***	0.522***
TREAT = 1; POST = 1	0.823***	0.822***	0.502***	0.500***

Notes: Estimations show linear probability regressions. We include blue-collar workers in the years 1995–2011. The treatment group includes construction workers, whereas the control group comprises the other blue-collar workers. POST stands for the post-reform period after July 2003. Personal characteristics consist of the variables age, nationality, education, and marital status. We add a year trend and cantonal dummies to control for canton-specific differences. Standard errors are clustered on the individual level and shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We expect this decreased employability to affect health outcomes as follows. On one hand, the lower working horizon and declining employability of older workers reduce the net present value of the returns on investment in the human capital for this group. On the other hand, based on Figure 5, we know that average sickness absence, sickness duration, and health problems rise with increasing age. For panels (a) to (c), we observe a steady increase over age. Applying the theory of endogenous aging, according to which many important variables adapt to the new retirement age, we also expect higher rates and duration of absence, as well as increased health problems, for the 56–60-year-old group. The underlying idea follows from Figure 6. A steady increase leads to the highest rates and duration of absence and health problems just before reaching the retirement age. The pension reform in the construction sector shifts the curve to the left, where the outcomes

are highest at the new (lower) retirement age. As a result, we expect more absences and increased health problems at each age point (e.g., 58). Since the effect of a lower retirement age is disproportionate and hence highest for the ages immediately preceding the new retirement age, we suppose that the greatest effects will be on the 56–60-year-old group.

Figure 5: Pre-reform average (a) sickness absence, (b) absence duration, and (c) health problems by cohort in the construction sector

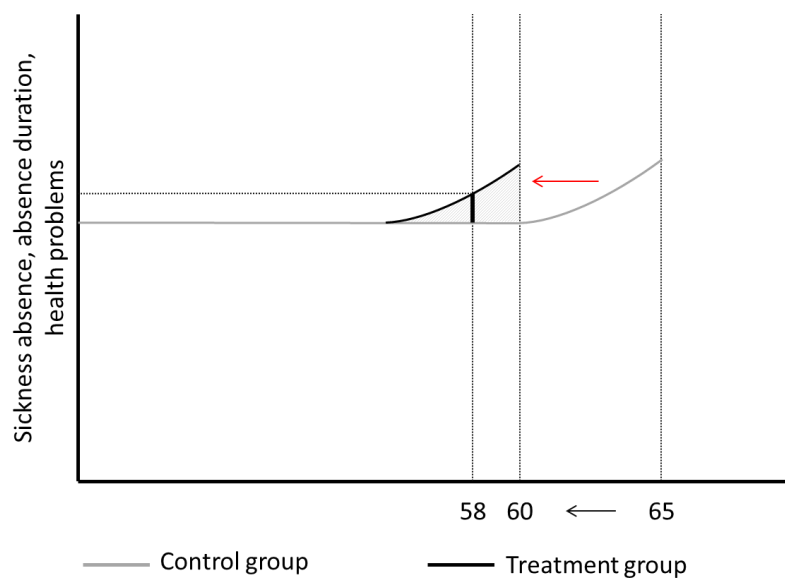


Sources: SLFS (1995-2011) and authors' own compilation

Several arguments can be presented to support this assumption. First, from a firm's perspective, the reduced net present value of the productive potential of this cohort makes it less valuable to preserve the health of its members. Thus, firms tend to use their productive potential to the fullest extent. This translates into more sickness absences and health problems. Second, for similar reasons, firms have an incentive to reduce safety regulations for older workers. Again, this leads to health complications. Third, from the workers' perspective, the shorter working horizon causes the 56–60-year-old cohort to behave less cautiously and more carelessly, leading to increased risks of accidents and

illness.¹⁴ Fourth, the fact that workers know that they will soon be leaving the labor market encourages them to engage in morally hazardous behavior. They try to exploit the situation by being absent from work as frequently as possible. The opportunity cost of being fired is lower than before the reform because of the lower working horizon. In sum, we are unable to identify the exact channel. However, we expect a clear-cut effect. The shorter working horizon after the reform increases sickness absences and their duration, as well as health problems, for the 56–60-year-old cohort compared with the same cohort before the pension reform.

Figure 6: Expected adjustment behavior after reform



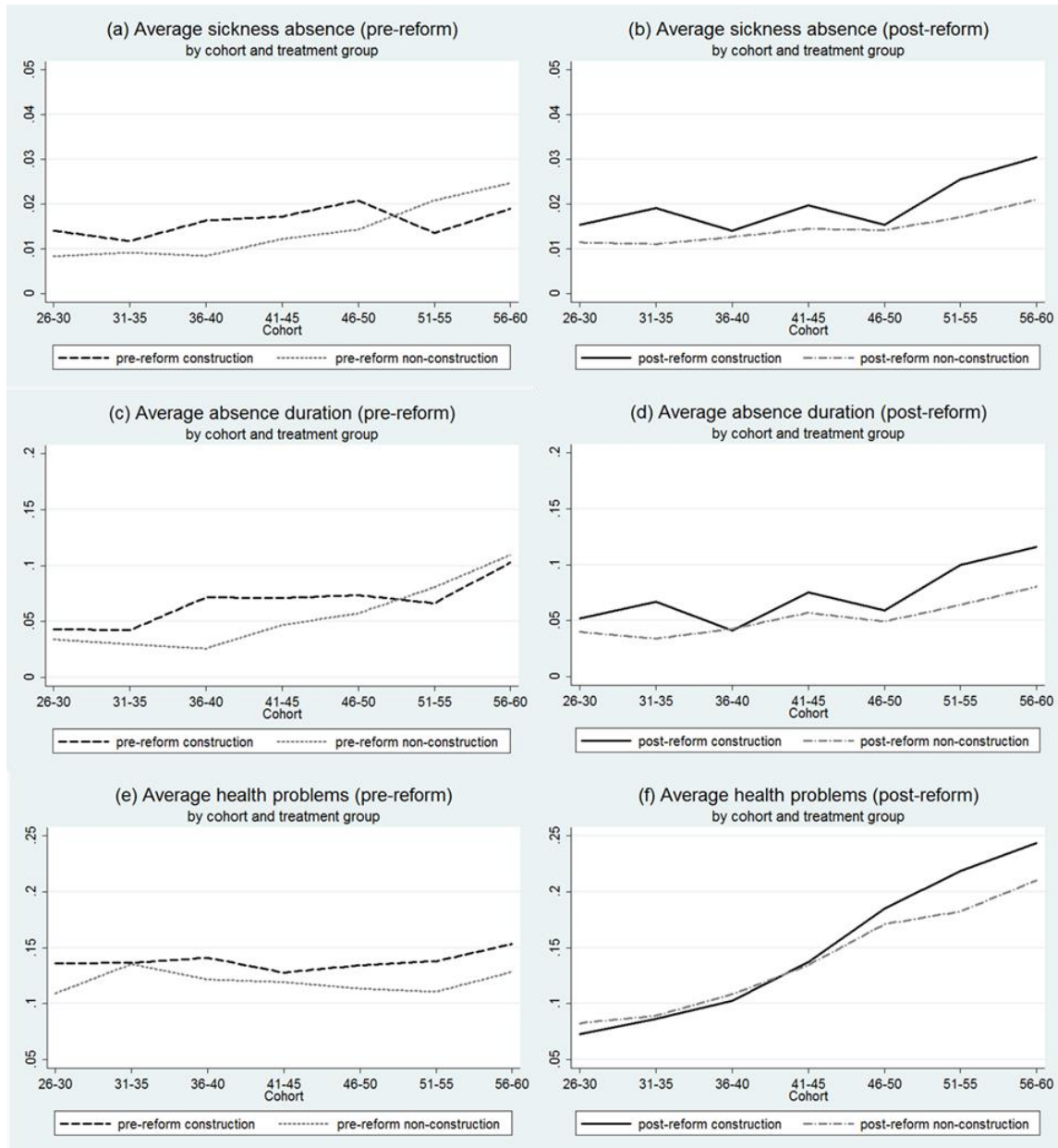
Source: Authors' own compilation

Figure 7 provides the first illustrative evidence for the theoretically expected shift illustrated in Figure 6. Panels (a), (c), and (e) show the outcomes in the pre-reform periods, while (b), (d), and (f) depict the results in the post-reform periods. Two aspects in favor of our hypothesis are important to note. First, we observe different patterns for the pre- and the post-reform graphs. While the panels in the pre-reform periods show similar paths for the construction and the other blue-collar workers, the post-reform path is systematically different for the two groups. From the 51–55-year-old cohort and up, there is an increasing disparity between the construction and the non-construction

¹⁴ It could also be argued that as individuals have to bear their healthcare costs alone after retirement and as retirement approaches faster, they could also behave more cautiously. Moreover, physical pain seems to be worse for workers who are still in the labor market compared with retirees.

workers. Second, the start of the steady increase in health outcomes (sickness absence and absence duration) among older workers shifts by five years in the post-reform period compared with the pre-reform period.

Figure 7: Average sickness absence (a) and (b), absence duration (c) and (d), and health problems (e) and (f) by cohort in the treatment and the control groups during pre- and post-reform periods



Sources: SLFS (1995-2011) and authors' own compilation

This effect is evident when comparing construction workers in the pre- and the post-reform periods (i.e., the black dashed curve in (a) and (c) and the black solid curve in (b) and (d), respectively). While for the pre-reform cohorts, the pre-retirement increase in health outcomes is only clearly visible for the 56–60 age group, it occurs five years earlier

for the post-reform construction workers (i.e., 51–55). Finally, note that the paths for pre- and post-reform average health problems in panels (e) and (f) look quite different at first sight. This discrepancy comes from the above-discussed fact that this variable has been redefined in the questionnaire. Nevertheless, the paths reflect the theoretically expected effect. While the difference between the two lines representing health problems for construction and non-construction workers looks quite constant as long as the two groups have the same retirement age in panel (e), the lower retirement age for construction workers seems to make the difference, increasing for the 51–55 and the 56–60 age cohorts. In sum, the descriptive statistics already show the reform’s effects on the three health variables.

5.2 Difference-in-difference

Based on the preceding discussion, we estimate the following regression model:

$$y_{it} = \alpha POST03_t + \tau TREAT_{it} + \rho POST03_t * TREAT_{it} + \beta X_{it} + \tau + \phi_s + \varepsilon_{it}, (1)$$

where the endogenous variable y_{it} denotes *sickness absence* (dummy), *duration of absence* (under seven categories), or *health problems* (dummy) for individuals i in year t . While we estimate linear probability models for the dummy variables, we apply Ordinary Least Squared (OLS) to the duration of absence. The variable $POST03_t$ is the post-reform dummy. The variable $TREAT_{it}$ is the treatment group indicator, which stands for $TREAT1_{it}$ (in Approach I) and $TREAT2_{it}$ (in Approach II). The variable of interest is the difference-in-differences regressor, i.e., the interaction term $POST03_t * TREAT_{it}$. The variable τ represents a general year trend, and the regional dummy ϕ_s stands for time-invariant differences across the seven regions of Switzerland (Region Lake Geneva, Espace Mittelland, Northwestern, Zurich, Eastern Switzerland, Central Switzerland, and Ticino). The variable X_{it} embodies individual personal and job characteristics to control for other confounding effects. Equation (1) is estimated separately for the different age groups 20–40, 40–60, 51–55, 56–60, and 59–60, using the years 2001 to 2005 for the main estimates. Similar to the method used by Bertrand *et al.* (2004), we use five periods to exclude the serial correlation of the outcome and the intervention variables. We use clustered standard errors at the individual level for all estimates. Although the reform has

been phased in gradually, we do not distinguish among birth cohorts. One concern could be that other interventions might have influenced our results. However, to our best knowledge, no other reforms for men in the blue-collar sectors have been introduced at the same time.

6 Empirical results

6.1 Main effects (Approaches I and II)

Table 2 summarizes our main set of results for Approach I (which compares construction with non-construction workers) to estimate the impact of the policy reform on the prevalence of sickness absences and health problems between 2001 and 2005.¹⁵ Table 2 shows the interaction effects of TREAT*POST, as well as the marginal effects, which are the predicted probabilities for construction workers before and after the reform. Columns (1) to (5) provide the same model, along with a full set of control variables. The columns only differ by the specific age group being considered. Columns (1) to (3) cover large age groups, especially younger workers. While they are affected by the reform, the impact should be small if not negligible. Another picture is expected for columns (4) and (5). The 56–60 and the 59–60 age cohorts are directly and disproportionately affected by the lower retirement age. Table 2 confirms these expectations. Column (1) shows the whole group of 20–40-year-old workers. We do not find the reform to have any effect for the three outcome variables. This is unsurprising, since the reform is mostly relevant to older workers. For similar reasons, there is no effect on the 40–60 and the 51–55 age cohorts.

Column (4) of panel A shows that the policy change increases the probability of sickness absences, holding all of the other factors at the means, from 3.2% to 5.6% for the 56–60 age group between 2001 and 2005. The closer it approaches the new lower retirement age, the larger the effect becomes. Hence, the result is even more pronounced for the 59–

¹⁵ Table A-3 shows the analogous results for a wider period. We extend the sample to the years between 1999 and 2007, 1997 and 2009, 1995 and 2011, and 1991 and 2015. Generally, all of the results are also confirmed when more years are included.

60 age group (column 5), where the probability of being absent from work for health reasons increases to 5.9%. Comparing columns (4) and (5) enables us to conclude that it concerns the whole 56–60 age group, since including the 59–60 age group reduces the effect in terms of significance level. This is also important for interpreting the results. Obviously, they are not only driven by the last term effects occurring in the very last year before retirement.

Table 2: Approach I: Construction versus non-construction workers, main estimation, 2001–2005

	Ages 20–40	Ages 40–60	Ages 51–55	Ages 56–60	Ages 59–60
	(1)	(2)	(3)	(4)	(5)
A. Sickness absence					
TREAT*POST	-0.00850 (0.00711)	0.00974 (0.00933)	0.0106 (0.0237)	0.0681*** (0.0232)	0.0521* (0.0311)
R2	0.005	0.013	0.026	0.033	0.067
Observations	5687	5303	1138	950	360
<i>Marginal effects</i>					
TREAT = 1; POST = 0				0.032***	0.019
TREAT = 1; POST = 1				0.056***	0.059**
B. Absence duration					
TREAT*POST	-0.0135 (0.0261)	0.0482 (0.0403)	0.0154 (0.110)	0.227** (0.0959)	0.166 (0.119)
R2	0.007	0.014	0.024	0.029	0.073
Observations	5687	5303	1138	950	360
<i>Marginal effects</i>					
TREAT = 1; POST = 0				0.132***	0.086
TREAT = 1; POST = 1				0.176***	0.199**
C. Health problems					
TREAT*POST	-0.0192 (0.0148)	-0.0237 (0.0165)	-0.0572 (0.0385)	0.112*** (0.0397)	0.180*** (0.0690)
R2	0.045	0.026	0.035	0.060	0.089
Observations	5222	4926	1055	869	330
<i>Marginal effects</i>					
TREAT = 1; POST = 0				0.090***	0.069**
TREAT = 1; POST = 1				0.127***	0.145***
Job characteristics	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes
Year trend	yes	yes	yes	yes	yes
Regional dummy	yes	yes	yes	yes	yes

Notes: Estimations in panel A and C show linear probability regressions; panel B shows OLS regressions using equation (1). We include blue-collar workers in 2001–2005 (2002–2005 for panel C). The treatment group comprises construction workers, whereas the control group consists of the other blue-collar workers. POST stands for the post-reform period after July 2003. Personal characteristics include the variables age, nationality, education, and marital status. Job characteristics comprise experience, shift work, permanent contract, income category, work hours, and firm size. We add a year trend and a regional dummy to control for region-specific differences. Standard errors are clustered on the ID level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Panel B shows a similar picture for the duration of absence. We do not find an effect for younger cohorts (columns 1–3). However, turning to the 56–60 age group, the results are statistically significant at the 5% level. Column (4) specifies that the reform provokes an increase in the duration of absence among construction workers from 0.132 to 0.176

points on this 7-point scale, implying a 33% increase. As demonstrated by column (5), there is no effect on the 59–60 age group. Again, in panel B, the results are not driven by the last term effect.

Finally, panel C reports how the new reform has a different effect on the health of the treatment group compared with the control group. Again, we observe no effect on the younger cohorts (columns 1–3). However, the probability of having health problems between the ages of 56 and 60 increases from 9% to 12.7% between 2002 and 2005. Column (5) shows that the probability of having health problems between the ages of 59 and 60 increases from 6.9% to 14.5%. When we include the 59–60 age group (again, only those individuals who are still working full time), the effect is larger than when considering only the 56–60 age cohort.

Table 3 summarizes the results for Approach II, where we compare older construction workers with younger ones. This approach has two aims. First, we tackle the potential concern about non-parallel trends between construction workers and other blue-collar workers by focusing on differently affected construction workers. As mentioned, a statistically significant difference between older and younger construction workers before and after the reform implies that older workers are mostly affected by the pension reform. Second, we try to eliminate the concern that the effects we find for health problems in Approach I could be driven by the redefinition of the respective variables (of course, our effects for the probability and duration of absences are not influenced by this measurement problem). This could be the case if the redefinition of *health problems* from 2003 onwards affects construction workers differently than other blue-collar workers (e.g., both have similar amounts of accidents (question in 2002), while construction workers tend to indicate more physical problems (question between 2003 and 2005)). Since the redefinition occurs alongside the reform, we have to address this concern.

Table 3 sheds light on this assumption. In panel A, specifications (a) and (c) confirm that the effect on older workers is statistically different from the effect on younger workers. While the sickness absence of younger workers tends to decline as time goes by (not shown), it strongly increases for older workers due to the reform. The reform has a greater effect on the 56–60 age group than all the younger cohorts. With the reform, the probability of being absent increases by 3.8 (4.9) percentage points for the 56–60 age

group compared with the 36–40 (46–50) age group. For panel B, in the absence duration, we find similar effects. The effect of the reform is again greater on the 56–60 age group. Absence duration increases significantly more for the older cohort compared with the younger ones (i.e., between 0.69 and 0.7 points on the 7-point scale).

Table 3: Approach II main results, 2001–2005, old construction workers compared with young ones

	36–40 vs. 56–60	41–45 vs. 56–60	46–50 vs. 56–60	51–55 vs. 56–60
	(1)	(2)	(3)	(4)
A. Sickness absence				
Age 56–60*POST	0.0388* (0.019)	0.028 (0.022)	0.049** (0.024)	0.015 (0.026)
R2	0.032	0.025	0.031	0.023
Observations	1374	1300	1017	979
B. Absence duration				
Age 56–60*POST	0.687** (0.039)	0.335 (0.266)	0.701** (0.318)	0.178 (0.301)
R2	0.089	0.061	0.077	0.041
Observations	1374	1300	1017	979
C. Health problems				
Age 56–60*POST	0.108*** (0.035)	0.099*** (0.036)	0.092** (0.040)	0.114*** (0.114)
R2	0.042	0.039	0.049	0.044
Observations	1269	1209	956	913
Job characteristics	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes
Year trend	yes	yes	yes	yes
Regional dummy	yes	yes	yes	yes
Base age range	Base = 36–40	Base = 41–45	Base = 46–50	Base = 51–55
Years included	2001–2005	2001–2005	2001–2005	2001–2005

Notes: Estimations in panels A and C show linear probability regressions; panel B shows ordered linear probability regressions using equation (1). We include construction workers only in 2001–2005 (2002–2005 for panel C). The treatment group consists of 56–60-year-old construction workers, whereas the control group comprises different younger cohorts. POST stands for the post-reform period after July 2003. Personal characteristics include the variables age, nationality, education, and marital status. Job characteristics consist of experience, shift work, permanent contract, income category, work hours, and firm size. We add a year trend and a regional dummy to control for region-specific differences. Standard errors are clustered on the ID level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

In panel C, we scrutinize the concern that the definition of the variable *health problems* might play a role. However, this concern can be excluded. We find that the negative effect of the reform on the health of older construction workers statistically differs from that on younger workers. It is important to emphasize that while the health problems among younger age groups decrease (not shown), the probability of having health issues between

the ages of 56 and 60 increases. Hence, the reform affects the 56–60 age group by 9.2–11.4 percentage points more than all the younger cohorts.

6.2 Robustness checks and placebo test

In the next step, we present the sensitivity analysis and the placebo tests. We conduct four different tests. First, we account for the panel structure of our dataset and include individual and year fixed effects. Second, we provide placebo tests where we incorrectly assume that the reform occurred in 1997 and 2001, respectively. Third, we test for the heterogeneity effect and include groups that have been excluded from our main estimates. Fourth, we examine the 56–58 age group, that is, those affected by the full five years of the reform.

Table 4: Fixed-effect estimation of main effects

	Ages 20–40	Ages 40–60	Ages 51–55	Ages 56–60
	(1)	(2)	(3)	(4)
A. Sickness absence				
TREAT*POST	-0.00813 (0.0124)	0.0112 (0.0128)	0.0271 (0.0405)	0.0552** (0.0278)
R2	0.471	0.412	0.424	0.438
Observations	3582	3569	695	695
B. Absence duration				
TREAT*POST	-0.00268 (0.0412)	0.0609 (0.0495)	0.170 (0.150)	0.220** (0.110)
R2	0.537	0.425	0.474	0.427
Observations	3582	3569	695	695
C. Health problems				
TREAT*POST	-0.00162 (0.0187)	-0.0152 (0.0203)	-0.0241 (0.0513)	0.103** (0.0495)
R2	0.589	0.552	0.534	0.540
Observations	3235	3270	641	632
Year FE	yes	yes	yes	yes
ID FE	yes	yes	yes	yes

Notes: Estimations in panels A–C show fixed-effect regressions using equation (1). We include blue-collar workers in 2001–2005 (2002–2005 for panel C). The treatment group comprises construction workers, whereas the control group consists of the other blue-collar workers. POST stands for the post-reform period after July 2003. In a fixed-effect model, the single effects of TREAT and POST are omitted. However, the interaction term can still be interpreted. Personal characteristics include the variables age, nationality, education, and marital status. Job characteristics comprise experience, shift work, permanent contract, income category, work hours, and firm size. Standard errors are clustered on the ID level and shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 shows the fixed-effect estimation for our main effect between 2001 and 2005. Since both of the single effects, TREAT and POST, are time-invariant, they are omitted in the regression model. Nonetheless, the interaction effect is still valid and interpretable. In panels A and C, we estimate linear probability models to account for the dummy structure of the outcome variable. In these panels, the effects can be directly interpreted. When controlling for fixed effects, the decrease in the retirement age for the construction sector increases the probability of absence due to illness in the 56–60 age group by 5.52% compared with non-construction blue-collar workers. The duration of the absence increases by 0.22 on the 7-point scale (panel B). Finally, the probability of having health problems between the ages of 56s and 60 increases by 10.3% after being affected by the reform for the construction workers compared with other blue-collar workers. This effect is considerable in terms of size.

Turning to the results of the fake treatment, Table 5 reports two sets of incorrect treatment. First, we take exactly five periods before the real treatment period between 1999 and 2003 and define a fake treatment in 2001. For each of the three outcome variables, all of the specifications are statistically insignificant. In the second row, we include the period between 1991 and 2003 and add a fake treatment in 1997. Again, all of the specifications show no statistically significant results for the fake treatments. As previously discussed, we include or exclude certain groups from our main estimates. One concern could be that this method affects our results. Table A-4 addresses this concern and tests the effect of heterogeneity. The first two columns exclude the job changers. Columns (3) and (4) produce evidence that the effects do not result from dropping the part-time workers. Columns (5) and (6) show donut estimations, where the year of the reform is excluded (2003). Finally, column (7) shows the effect on the 56–58 age cohort, which is treated by the full five years of the reform and has a retirement age of 60 after the reform. Taken together, the size of the effects changes only slightly. In this context, almost all of the specifications are robust and confirm our main results.

Table 5: Placebo estimates

	A. Sickness absence		B. Absence duration		C. Health problems	
	Ages 56–60	Ages 59–60	Ages 56–60	Ages 59–60	Ages 56–60	Ages 59–60
TREAT*POST01	-0.021 (0.028)	0.013 (0.042)	-0.129 (0.115)	-0.059 (0.125)	-0.012 (0.093)	0.084 (0.128)
	626 1999–2003	245 1999–2003	626 1999–2003	245 1999–2003	484 1999–2003	191 1999–2003
TREAT*POST97	0.009 (0.023)	-0.001 (0.0419)	0.107 (0.125)	0.026 (0.225)	-0.054 (0.042)	-0.118 (0.0779)
	1077 1991–2003	398 1991–2003	1077 1991–2003	398 1991–2003	840 1991–2003	323 1991–2003
Job characteristics	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes
Year trend	yes	yes	yes	yes	yes	yes
Regional dummy	yes	yes	yes	yes	yes	yes

Notes: Estimations in panels A and C show linear probability regressions; panel B shows OLS regressions using equation (1). We include blue-collar workers in 1991–2003. The treatment group comprises construction workers, whereas the control group includes the other blue-collar workers. POST stands for two fake post-reform periods (POST01 after 2001 and POST97 after 1997). Personal characteristics consist of the variables age, nationality, education, and marital status. Job characteristics include experience, shift work, permanent contract, income category, work hours, and firm size. We add a year trend and a regional dummy to control for region-specific differences. Standard errors are clustered on the ID level and shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

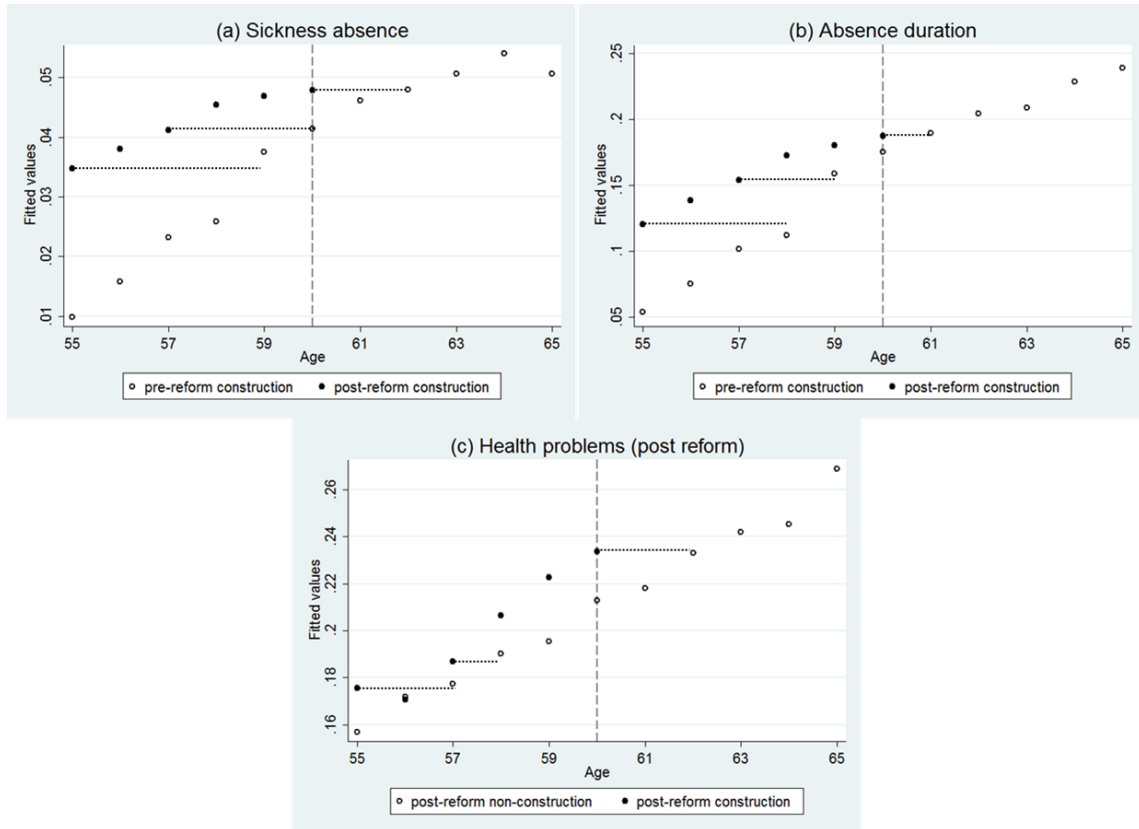
6.3 Distance to retirement and endogenous aging

The main objective of the construction sector reform was to reduce health limitations among older workers. However, our result shows that health problems worsen in the 56–60 age group. Figure 8 illustrates the extent of endogenous aging due to the five-year decrease in the statutory retirement age.

We plot the fitted values for construction workers aged 55–65 before and after the reform. For all age points, the construction workers in the post-reform period occupy a higher level than those in the pre-reform period. More precisely, in Figure 8, we observe that the sickness absence of 60-year-old (55, respectively) construction workers treated by the reform (i.e., post-reform) is at the same level as that of 62-year-old individuals (58.5, respectively) who are not treated by the reform (i.e., pre-reform). Hence, the first group has endogenously aged by 2 to 3.5 years due to the reform. We observe a similar picture for the absence duration in panel (b). The absence duration of 60-year-old (55, respectively) construction workers treated by the reform (i.e., post-reform) is at a similar level as that of 61-year-old (58, respectively) individuals who are not treated by the

reform (i.e., pre-reform). The effect shifts by 1 to 3 years. For panel (c), the effect is similar. Since 55-year-old individuals treated by the reform are on a similar level as 57-year-old non-construction workers, the first group has grown older by 2 years due to the reform. Based on our estimations, we conclude that the reform has increased the social age of the construction workers by 1–3 years.

Figure 8: Fitted values for pre- and post-reform construction workers



Sources: SLFS and authors' own compilation

Notes: For panel (c), the control group differs from those for panels (a) and (b). As the definition of the variable *health problems* changes over time, the value we obtain for the comparison with pre- and post-reform construction workers is largely overestimated (see Figure A-1).

7 Conclusion

In this paper, we have analyzed whether lowering the retirement age for construction workers has a negative effect on their health outcomes before they reach the new retirement age. The human capital theory predicts that a lower working horizon reduces the net present value of older workers. On the other hand, this situation translates into less investment in the health of the cohorts close to retirement and thus affects the prevalence of sickness absences, the duration of these absences, and health problems. Several mechanisms might be at play. First, because of the lower net present value of the return on investment in the workers' human capital, firms tend to use older workers to full capacity. Second, for similar reasons, the firm invests less in safety regulations, leading to more health problems. Third, aware that they have less time left in their employment, older workers tend to be less cautious and place themselves at risk of more accidents. Finally, moral dilemmas could play a role. Older workers exploit the fact that they will soon be leaving the labor market and are absent from work as frequently as possible, even if they are not sick. Taken together, we cannot fully identify which factor is the determinant one. We suppose the existence of an interplay among all of them. However, the evidence for retired workers suggests that moral aspects is not the only driver since we also find evidence for the probability of having health problems.

The overall empirical findings imply that lowering the retirement age negatively affects the health of older workers. The 2003 reform in the Swiss construction sector has offered an informative setting to test our hypothesis. Using the SLFS, we have contrasted pre- and post-reform cohorts of construction workers and have compared them with other blue-collar workers (Approach I), as well as younger construction workers (Approach II). The decrease in the retirement age from 65 to 60 was phased in gradually between 2003 and 2006.

Specifically, our first estimation suggests that while the reform has been effective in reducing employment in the 60–65 age cohort, it has also decreased employment in the 56–60 age cohort. This study complements the literature on the effect of pension reforms on the *de facto* retirement age (e.g., Staubli and Zweimüller 2013) as it is the first to focus on a reform that has lowered the retirement age. Even more interestingly, we observe a higher prevalence of sickness absences, a longer absence duration, and a greater

probability of health problems among 56–60-year-old construction workers after the policy reform. The outcomes are considerable in terms of size. For example, the mean sickness absence in the 56–60 cohort increases from 3.2% to 5.6% in the post-reform period. Furthermore, the duration of absence increases by 0.044 points on the 7-point scale (i.e., 33%) for the 56–60 age group. Likewise, the mean probability of having health problems increases from 9% to 12.7%. Finally, based on our estimates, we conclude that 55-year-old construction workers after the reform are now comparable to their 58-year-old colleagues prior to the reform with respect to their probability of absence and absence duration, as well as to 57-year-olds non-construction workers in terms of the probability of poor health outcomes. Thus, we cannot exclude the fact that this reform has endogenously aged older workers by about three years. Our results have withstood various robustness checks. Given these findings, our paper contributes to the theory of endogenous aging (e.g., Fouarge and Schils 2009, Montizaan *et al.* 2010, Brunello and Comi 2015) in a new setting that offers a lower retirement age for a specific subgroup.

We believe that this paper is of general interest for all countries dealing with an aging population. While the promotion of early retirement in the construction sector is intended to improve the health of construction workers, we find that the contrary is true. Before they reach the new (lower) retirement age, their health problems increase. Thus, they start retirement with a similar amount of health problems. We think that clearly evaluating the effects of early retirement is important for developed countries, and introducing it should be handled carefully. However, to make a full assessment, we have also examined longer-term health effects (Bauer and Eichenberger 2018).¹⁶ Specifically, the preliminary results indicate that construction workers who have benefited from the lower SRA do not necessarily have better health outcomes between the ages of 65 and 80. This is another piece of evidence that our results reflect real health problems and are not only driven by claimed health problems. Finally, our findings also complement those of the literature investigating the effects of increasing the statutory retirement age. It is likely that the effects are at least to some extent symmetric; the workers' health improves when their time left until retirement is increased due to pension reforms.

¹⁶ Preliminary results and the manuscript are available from the authors upon request.

8 References

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Appendix

Table A-1: Overview of reform

Birth year of cohort	Retirement age of cohort	Year of retirement	Statutory retirement age
1935	65	2000	65
1936	65	2001	65
1937	65	2002	65
1938	65	2003	until June: 65; from July: 63
1939	64	2003	until June: 65; from July: 63
1940	63	2003	until June: 65; from July: 63
1941	63	2004	62
1942	62	2004	62
1943	62	2005	61
1944	61	2005	61
1945	61	2006	60
1946	60	2006	60
1947	60	2007	60
1948	60	2008	60
1949	60	2009	60
1950	60	2010	60

Table A-2: Descriptive statistics

	Construction (T)			Non-Construction (C)			$\Delta(T-C)$
	mean	min	max	mean	min	max	
<i>Outcome variables</i>							
Sickness absence	0.02082	0	1	0.01640	0	1	0.00442
Absence duration	0.18295	0	7	0.15989	0	7	0.02306
Health issues	0.11061	0	1	0.11354	0	1	-0.00293
<i>Covariates</i>							
Age (in months)	506.1012	240	780	514.0252	240	780	-7.924
Age	42.175	20	65	42.835	20	65	-0.66
Experience (in days)	4136.025	1	17732	4294.274	1	18379	-158.249
Swiss	0.465093	0	1	0.597003	0	1	-0.13191
Shift work	0.213165	0	1	0.287036	0	1	-0.07387
Temporary contract	0.09215	0	1	0.103417	0	1	-0.01127
Permanent contract	0.90785	0	1	0.896583	0	1	0.01126
<i>Educational level</i>							
Compulsory school	0.213036	0	1	0.167669	0	1	0.04546
Apprenticeship	0.047634	0	1	0.039561	0	1	0.00807
Higher apprenticeship	0.567883	0	1	0.592121	0	1	-0.02424
Vocational school	0.033491	0	1	0.037121	0	1	-0.00363
Vocational high school	0.033813	0	1	0.035361	0	1	-0.00155
Master craftsman	0.042427	0	1	0.059087	0	1	-0.01666
Technician	0.012149	0	1	0.019468	0	1	-0.00732
Polytechnic/University	0.014078	0	1	0.023612 2	0	1	-0.00953
Income category	5.77590	0	10	5.868827	0	10	-0.09293
Work hours	41.8983	7	72	42.03865	3	84	-0.14035
Size of firm	11.39985	1	14	11.82166	1	14	-0.42181
Married	0.674145	0	1	0.64	0	1	-0.02881
Years	1991–2015						
Ages	20–65						

Notes: We include the years 1991–2015. The treatment group comprises construction workers (T), whereas the control group consists of the other blue-collar workers (C).

Table A-3: Approach I. Main estimation, more years

	Ages 20–40	Ages 40–60	Ages 51–55	Ages 56–60	Ages 59–60
	(1)	(2)	(3)	(4)	(5)
I. Years 1999–2007					
I.A. Sickness absence					
TREAT*POST	-0.00806 (0.00564)	0.00733 (0.00757)	0.0243 (0.0191)	0.0351* (0.0189)	0.0464** (0.0186)
R2	0.004	0.011	0.021	0.023	0.022
Observations	9127	8402	1752	1502	556
I.B. Absence duration					
TREAT*POST	-0.0154 (0.0205)	0.0369 (0.0329)	0.0617 (0.0885)	0.0999 (0.0800)	0.129 (0.0915)
R2	0.005	0.011	0.020	0.020	0.065
Observations	9127	8402	1752	1502	556
I.C. Health problems					
TREAT*POST	-0.0194 (0.0135)	-0.0174 (0.0146)	-0.0385 (0.0336)	0.0758** (0.0337)	0.0438* (0.0258)
R2	0.035	0.018	0.026	0.049	0.056
Observations	8219	7660	1594	1359	502
II. Years 1997–2009					
II.A. Sickness absence					
TREAT*POST	-0.00548 (0.00516)	0.00670 (0.00690)	0.0232 (0.0177)	0.0238 (0.0178)	0.0380 (0.0232)
R2	0.005	0.009	0.015	0.014	0.019
Observations	11716	10808	2209	1946	704
II.B. Absence duration					
TREAT*POST	-0.00712 (0.0182)	0.0357 (0.0299)	0.0676 (0.0809)	0.0731 (0.0759)	0.0101 (0.0783)
R2	0.005	0.010	0.014	0.013	0.049
Observations	11716	10808	2209	1946	704
II.C. Health problems					
TREAT*POST	-0.0177 (0.0131)	-0.0211 (0.0141)	-0.0576* (0.0325)	0.0725** (0.0321)	0.117** (0.0520)
R2	0.032	0.016	0.025	0.040	0.055
Observations	10465	9852	2012	1756	643
III. Years 1995–2011					
III.A. Sickness absence					
TREAT*POST	-0.00225 (0.00421)	-0.000750 (0.00593)	0.0176 (0.0146)	0.0112 (0.0153)	0.00380 (0.0230)
R2	0.004	0.008	0.008	0.014	0.020
Observations	16202	15193	3159	2762	1012
III.B. Absence duration					
TREAT*POST	-0.00235 (0.0154)	0.000949 (0.0260)	0.0472 (0.0669)	0.0307 (0.0690)	-0.0482 (0.0973)
R2	0.004	0.008	0.008	0.014	0.044
Observations	16202	15193	3159	2762	1012
III.C. Health problems					
TREAT*POST	-0.0216** (0.0108)	-0.0269** (0.0126)	-0.0733** (0.0290)	0.0665** (0.0286)	0.0937** (0.0469)
R2	0.024	0.026	0.035	0.047	0.041
Observations	14601	14002	2922	2519	937

Table A-3: Approach I. Main estimation, more years (cont.)

IV. Years 1991–2015					
IV.A. Sickness absence					
TREAT*POST	-0.000656 (0.00345)	0.00159 (0.00498)	0.0174 (0.0118)	0.0272** (0.0131)	0.0266 (0.0208)
R2	0.004	0.005	0.006	0.012	0.015
Observations	23172	22738	4930	4176	1520
IV.B. Absence duration					
TREAT*POST	-0.00120 (0.0138)	0.0140 (0.0226)	0.0526 (0.0556)	0.124** (0.0621)	0.0835 (0.0961)
R2	0.003	0.006	0.007	0.011	0.026
Observations	23172	22738	4930	4176	1520
IV.C. Health problems					
TREAT*POST	-0.0148* (0.00871)	-0.0205* (0.0115)	-0.0461* (0.0251)	0.0541** (0.0266)	0.0687 (0.0456)
R2	0.027	0.036	0.042	0.053	0.041
Observations	21557	21530	4686	3928	1443
Job characteristics	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes
Year trend	yes	yes	yes	yes	yes
Regional dummy	yes	yes	yes	yes	yes

Notes: Estimations in panels A and C show linear probability regressions; panel B shows OLS regressions using equation (1). We include blue-collar workers in 2001–2005 (2002–2005 for panel C). The treatment group consists of construction workers, whereas the control group comprises the other blue-collar workers. POST stands for the post-reform period after July 2003. Personal characteristics include the variables age, nationality, education, and marital status. Job characteristics consist of experience, shift work, permanent contract, income category, work hours, and firm size. We add a year trend and a regional dummy to control for region-specific differences. Standard errors are clustered on the ID level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Table A-4: Effect of heterogeneity and robustness

	Ages 56–60	Ages 59–60	Ages 56–60	Ages 59–60	Ages 56–60	Ages 59–60	Ages 56–58
	No job changer		With part-time job		Donut: Exclude 2003		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A.Sickness absence							
TREAT*	0.067***	0.054	0.0647***	0.054*	0.111***	0.0656**	0.0751**
POST	(0.0239)	(0.0341)	(0.0227)	(0.031)	(0.0296)	(0.0319)	(0.0317)
Observations	926	346	1003	378	695	246	594
B.Absence duration							
TREAT*	0.222**	0.158	0.203**	0.166	0.396***	0.209*	0.272**
POST	(0.0988)	(0.125)	(0.0968)	(0.116)	(0.129)	(0.115)	(0.134)
Observations	926	346	1003	380	695	246	594
C.Health problems							
TREAT*	0.118***	0.183***	0.106**	0.170**	0.0919	0.173*	0.0941*
POST	(0.0406)	(0.0705)	(0.0412)	(0.0688)	(0.0583)	(0.0981)	(0.0504)
Observations	850	317	918	346	615	218	548
Job characteristics	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes
Year trend	yes	yes	yes	yes	yes	yes	yes
Regional dummy	yes	yes	yes	yes	yes	yes	yes

Notes: Estimations in panels A and C show linear probability regressions; panel B shows OLS regressions using equation (1). We include blue-collar workers in 2001–2005 (2002–2005 for panel C). The treatment group comprises construction workers, whereas the control group consists of the other blue-collar workers. POST stands for the post-reform period after July 2003. Personal characteristics include the variables age, nationality, education, and marital status. Job characteristics comprise experience, shift work, permanent contract, income category, work hours, and firm size. We add a year trend and a regional dummy to control for region-specific differences. Standard errors are clustered on the ID level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Figure A-1. Fitted values of pre- and post-reform construction workers (health problems)

