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Worsening workers' health by lowering retirement age: The malign consequences of a benign reform[☆]

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ABSTRACT

We analyze how a reduction in retirement age affects pre-retirement sickness absences and health problems. We examine a policy change in pension eligibility in the Swiss construction sector, which lowered retirement age from 65 to 60. While the reform was intended to improve workers' health, it resulted in the opposite outcome. We find that sickness absences for 56–60-year-old construction workers increase by 33% when working until 60 instead of 65, and their probability of self-reported health problems increases by 54%. We also find a negative but less pronounced effect for the 61–65 age group. Our results imply that lowering the retirement age has not only material costs but also unintended immaterial costs. If the effect is symmetric, it implies that increasing retirement age has immaterial benefits by improving pre-retirement health of older workers.

Introduction

Population aging is one of the most pressing societal issues. While most economists recommend increasing the effective retirement age, older workers often suffer from declining productivity and health.[1] In this context, a common claim is that many older workers, especially those who engage in heavy physical work, are unable to work for a longer span of years. Therefore, they should be exempt from an increase in the statutory retirement age (SRA) and, rather, be offered early retirement (Smulders et al., 2009). The most frequently referenced strenuous manual labor occupation is that of construction workers (Boschman et al., 2011, 2013; Dong et al., 2011), as they face massive work hazards. For example, in 2017, more than one fifth of all fatal workplace accidents in the European Union occurred in the construction sector.[2]

Surprisingly, there is almost no evidence on how lowering retirement age and, thus, decreasing the work horizon impacts pre-retirement health outcomes of individuals engaged in intensive manual work. While the literature on post-retirement health outcomes offers mixed evidence on the relationship between retirement and health, there may also be several opposing mechanisms before retirement. Decreasing the working horizon reduces the time in which workplace injury can occur. But it also reduces the incentives for employers and employees to invest in the health of their workers and colleagues, respectively (Bauer and Eichenberger, 2017; Bertoni et al., 2018), and this unplanned event may even be stressful for individuals (Falba et al., 2009). In a similar vein, a lower working horizon may induce individuals to engage in morally hazardous behavior when they, for instance, are absent from work more often than necessary and justify their increased absences by reporting more health problems.

Such unintended costs of a lower working horizon have important policy implications. Sickness absences are costly for firms, employees,

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[1] For a summary of the reforms implemented between 2009 and 2013, see the report of the OECD (2013).

[2] Eurostat (2020), https://ec.europa.eu/eurostat/statistics-explained/index.php

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and health-care systems. To gain new insights into the pre-retirement health effects of a lower retirement age, we investigate a pension reform enacted in Switzerland in 2003 that reduced the retirement age of Swiss construction workers from 65 to 60. The reform offers a unique setting. First, it covers only a subgroup of workers. This allows us to compare the treated group with control groups that have not been subject to the reform. Second, the reform is a highly subsidized early retirement plan without an actuarially fair increase in contributions or reductions in pensions for those who retire at 60. Therefore, all treated construction workers were strongly incentivized to retire at age 60, which attenuates selection problems.

We estimate the causal impact of this reform on self-reported health measures (i.e., sickness absences and health problems) 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 on the construction sector, we use two different approaches. First, we compare construction workers (treatment group) with other blue-collar workers (control group) with similar characteristics (Approach I). Second, we compare older construction workers (aged between 56 and 60) with a variety of younger cohorts of construction workers (Approach II), assuming that younger workers are less affected by the reform.

To our knowledge, this is the first paper studying the effects of a lower working horizon on pre-retirement sickness absences from work and self-reported health problems. It offers two main contributions. First, the recent literature on the effects of pension reforms focuses mainly on reforms increasing the retirement age, and very little is known about the effects of a shorter working horizon (see also Hallberg et al., 2015). This paper contributes to our understanding of these effects, particularly for certain blue-collar workers who often suffer from bad health due to their heavy workload and accidents. The second contribution is the focus on pre-retirement health. We investigate the effects of the reform on employees who are still active in the labor market (i.e., not yet retired). While previous research on the relationship between retirement and health concentrates on post-retirement health outcomes (e.g., Shai, 2018), the effects on pre-retirement health are not yet well understood (de Grip et al., 2012).

Although we are not able to fully disentangle the different mechanisms through which retirement affects (self-reported) pre-retirement health, we provide evidence, that our results at least partly reflect actual health outcomes and not only morally hazardous absenteeism and justifications. In particular, we also take post-retirement health into consideration, as individuals have no incentive to claim bad health in order to justify absences from work after retirement.

Our results show that the reform had an effect on the working behavior of older workers. The employment rate for construction workers decreased not only in the 61–65-year-old cohort but also among 56–60-year-olds. There were marked effects on pre-retirement health. Specifically, we find that the probability of having been absent for at least one day of the week prior to the interview increased by 33 percent (i.e., from 3.49% to 4.64%) among 56–60-year-old construction workers. Moreover, the probability of reporting poor health increased by 54 percent (i.e., from 7.6% to 11.7%) among construction workers aged 56 to 60. Comparisons between different age cohorts of construction workers (Approach II) yield similar results. Finally, we document that the reform has also affected post-retirement health. We find evidence for a higher prevalence of back problems, impediments to daily life and chronic diseases in the age groups 66–70 and 66–75.

The remainder of this paper is organized as follows: Section 2 locates our contribution in the relevant literature and develops a theoretical framework. Section 3 characterizes the reform in the construction sector. Section 4 provides information on the data, variables, and treatment groups. Section 5 explains the empirical strategy and the results. Section 6 presents some robustness checks and refines the analysis and discussion. Section 7 concludes.

Theoretical considerations and relevant literature

Evaluating the relationship between retirement and health outcomes is not trivial as individuals' decisions to retire early are often endogenously driven by their health status or other personal characteristics. While there are methods of addressing these endogeneity problems, the literature mainly focuses on post-retirement health. However, the effect of retirement on various post-retirement health outcomes still remains ambiguous. Some studies find retirement to have a positive effect on health (Westerlund et al., 2009), to reduce the use of healthcare (Coe and Zamarro, 2011, Eibich, 2015), and to reduce mortality rates (Hallberg et al., 2015). Similarly, a higher statutory retirement age is found to negatively impact health outcomes in general (Shai, 2018) and, more

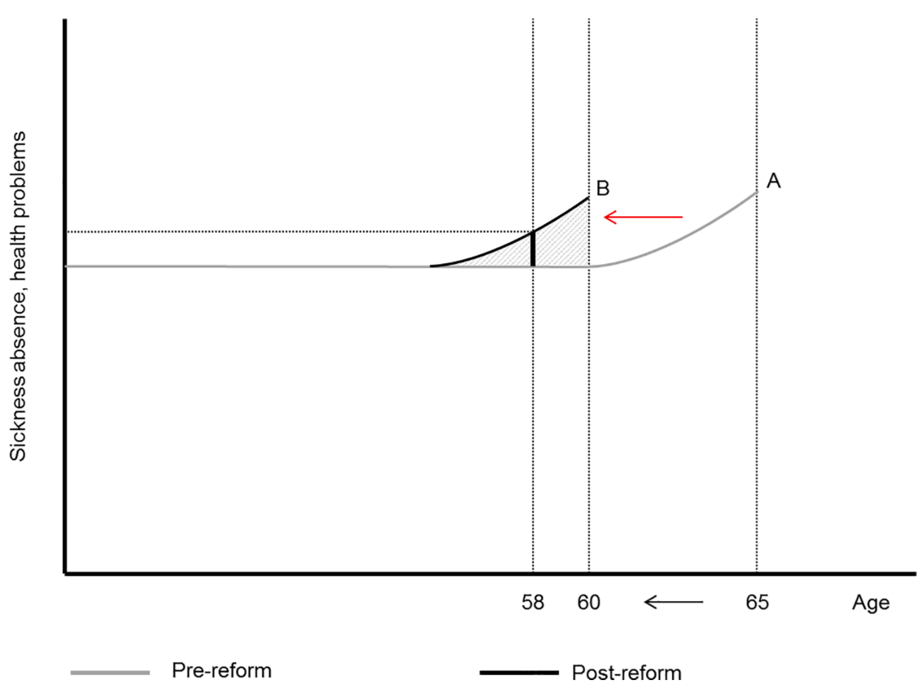

Fig. 1. Health outcomes before and after the reform. Source: Authors' own compilation.

specifically, functional health (van Zon et al., 2016) after retirement.

Other authors have found retirement to have a negative effect on health. A few studies illustrate that cognitive functions weaken after retirement (Bonsang et al., 2012, Celidoni et al., 2017, Mazzonna and Peracchi, 2012). Retiring at a higher age improves well-being (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), whereas early retirement increases mortality (Kuhn et al., 2018).

Finally, several studies report that retirement and its timing exhibit no or an unclear and complex influence on health outcomes. For instance, van Solinge (2007) shows that only involuntary retirement decreases subjective health. Johnston and Lee (2009) find that retirement is beneficial for subjective well-being and mental health, but has a detrimental effect on objective physical health. In contrast, Butterworth et al. (2006) find that later retirement generally has no effect on health in Australia, which is supported by more recent studies with Swiss (Lalive and Staubli, 2014) and Swedish data (Hult et al., 2010, Hagen, 2018), that find that an increase in the statutory retirement age has no effect on mortality.

While these studies address effects on health outcomes, other studies focus on channels driving these health outcomes. Eibich (2015) was probably the first to investigate how retirement changes health-relevant individual behavior. It seems to increase physical exercise and decrease drinking and smoking (Celidoni and Rebba, 2017, Motegi et al., 2020).

In contrast to the broad literature on post-retirement health and health-relevant behavior, the literature on pre-retirement health is very sparse. There are three plausible channels through which statutory retirement age and expected individual retirement age can affect pre-retirement health outcomes. The first is related to human capital theory (Becker, 1964). The length of the working horizon affects the return on, and thus the extent of, investments in work related human capital. Building on this theory, we expect a similar mechanism in our research setting, that is, a lower working horizon reduces the net present value of the returns on investments in human capital (e.g., safety investments, attentive behavior to reduce accidents) for workers who are subject to the new reform. The argument is sketched in Fig. 1, which shows the outcomes of behavioral adjustments due to a pension reform. Health problems stay rather constant during active years, but increase in the years before retirement (curve A). According to human capital theory (Becker, 1964), a pension reform that lowers retirement age induces health measures to adapt to the new (lower) retirement age. Thus, we presume a shift of the health-outcome curve to the left (curve B) which induces age-controlled health outcomes to decrease. Importantly, older workers are disproportionately affected by the lower retirement age, as their remaining working horizon is relatively short.

The investment channel can originate from three different groups: employers, co-workers and workers themselves. First, from an employer's perspective, reducing the work horizon of a cohort of employees reduces the net present value of the productive potential of that cohort (Bauer and Eichenberger, 2017), which makes it less valuable for the employer to preserve the health of their employees. This translates into more sickness absences and health problems. Moreover, for similar reasons, employers have an incentive to reduce their investment in safety[3] (Feng, 2013) and in professional training (Fouarge and Schils, 2009, Montizaan et al., 2010) for older workers. Second, from the co-workers' perspective, the incentives to support their colleagues at work shrink when the latter approach retirement age (Bauer and Eichenberger, 2017). Third, from the workers' perspective, the long-term costs of accidents, which reduce their capacity to work, decrease when the working horizon is shortened due to the reform. This in turn weakens their own job involvement and their health-related behavior, such as behaving cautiously (Moore and Viscusi, 1988, Viscusi, 1993). This is in line with Bertoni et al. (2018), who examine a pension reform that increases the working horizon and translates into more health-promoting investments by individuals before retirement, such as physical exercise, no smoking, and a strict diet. Hence, for a reform lowering the working horizon, we expect less health-promoting behavior by employees and their employers, which in turn can be expected to negatively affect the actual health of workers.

The second potential channel of a lower expected individual retirement age consists of a psychological effect. An unexpected change in the working horizon may be stressful for individuals and affect their mental health before retirement. A study by de Grip et al. (2012) finds that increasing the retirement age in the Netherlands had a negative effect on workers' mental health (by increasing the rate of depression). Falba et al. (2009) show that working at an age when one is expected not to work, as well as not working at an age when one is expected to work, increases depression rates. However, according to the authors, these results depend on the events that trigger early retirement. Early retirement may also bring relief to individuals. Thus, based on these two studies, the effect of retirement age on pre-retirement health outcomes is ambiguous. However, as we investigate the effects of a highly subsidized early retirement plan, the aforementioned mental effects, which may be driven by aspiration levels and financial stress (Falba et al., 2009), can be assumed to play a limited role.

The third potential mechanism via which a lower working horizon may affect the stated health of older workers is morally hazardous behavior. Workers who know that they will soon be leaving the labor market are induced to be absent from work more often than necessary, as the cost of losing the reputation of being a devoted and reliable worker decreases when the working horizon shortens due to the pension reform. Later on, such behavior could induce individuals to report more health problems in order to justify their absence from work. Such justification biases are found in the literature on unemployment (Johansson et al., 2020), disability pensions (Black et al., 2017, Gannon, 2009), and individual early retirement (Gupta and Larsen, 2010, Lindeboom and Kerkhofs, 2009, McGarry, 2004, Mortelmans and Vannieuwenhuyze, 2013).

While our data does not allow us to fully disentangle these three mechanisms, we look at them in more detail in section 6.2. Specifically, we argue that morally hazardous behavior and justification bias (the third mechanism) do not seem to be the only driving factors behind increased absenteeism, but that actual health is negatively impacted, at least to some extent.

Pension reform in the Swiss construction sector

The Swiss pension system

Switzerland has a three-pillar pension system.[4] 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 second pillars are intended to ensure at least 60% of the workers' last income. The privately funded third pillar complements the pension system.[5] The SRA defines eligibility for the first and the second pillars. It

[3] Safety investments are for example, safety equipment costs (including personal protective equipment [PPE]), safety training costs, and safety inspections (Feng 2013).

[4] For further information, see the Swiss Federal Administration, https://www.bsv.admin.ch/

[5] 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).

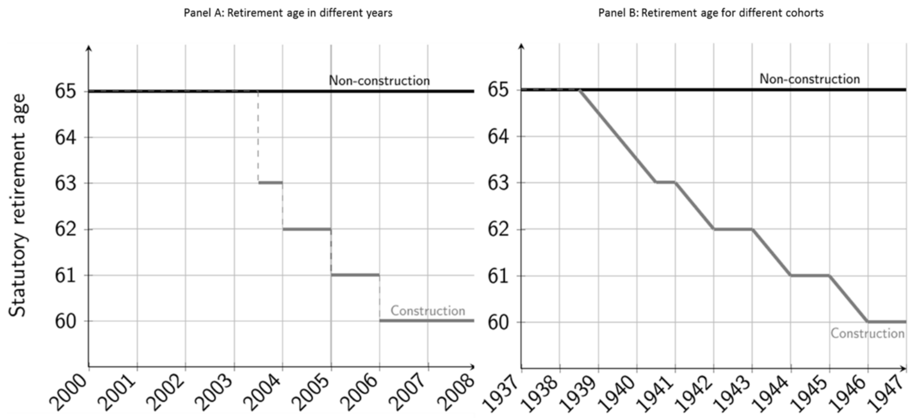

Fig. 2. Retirement age according to the FAR plan. Source: Authors' own compilation.

is set at 65 years of age for men and 64[6] for women.[7] The 10th revision of the pension insurance scheme (enacted in 1997) made early retirement under the public pension scheme possible (Luisi, 2007). The early retirement age (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% per year.

Reform in the construction sector in 2003

In the early 1990s, a public discourse emerged on the improvement of the physical health of construction workers in their old age. *The Association of Entrepreneurs in Construction* formulated the idea of establishing an old-age fund to facilitate early retirement and to bridge the pension gap until the SRA. However, it was only in 2000 that the *Geneva Cantonal Office for Work Inspection and Relations* published a report that reopened the discussion. Based on data for males born between 1925 and 1927, it was estimated that 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 of work (Gubéran and Usel, 2000). At the end of 2002, the *Association of Entrepreneurs in Construction* and labor unions representing construction workers[8] agreed on the *Collective Working Convention for Flexible Anticipated Retirement*[9] (henceforth referred to as the FAR plan) which was enacted in July 2003.[10] Retiring earlier than the ERA is generally not possible. However, it is possible to transition to retirement through other social security systems, such as disability insurance, unemployment insurance, or social assistance. The replacement rate in those systems is usually lower and incentivizes individuals at eligibility for ERA to anticipate the old-age pension with a cut of 13.6% for the two years.

The most important change brought about by the FAR plan has been a stepwise introduction of a lower retirement eligibility in the construction sector. As shown in Fig. 2 and Table A1, 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 to retire at the age of 60. For the cohorts between 1938 and 1945, the retirement age decreased gradually, with some flat phases.[11]

Workers in the main construction sector who have been working there for the past seven years, are subject to the FAR plan and have a reduced retirement age of 60. The scope of application covers general construction, civil engineering, underground mining, and road building. Subject to the new regulations are employees of such firms, 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 construction sector are not covered by the FAR plan. 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 numerous 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 to the first and second pillars paid so that they are eligible for public and occupational pensions upon reaching the age of 65. Moreover, construction workers can top up pensions with a (maximally) 20% job.

[6] 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.

[7] 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).

[8] The two largest unions in Switzerland are Unia (formerly Gewerkschaft Bau and Industrie GBI) and Syna.

[9] In German, this is known as Gesamtarbeitsvertrag für den flexiblen Altersrücktritt im Bauhauptgewerbe (GAV FAR).

[10] For a detailed judicial review of the reform, see Keller (2008).

[11] A closer examination of the cohorts of 1941 and 1942 explains this decrease in the retirement age in greater detail. The 1941 cohort turned 62 in 2003 and, hence, was not eligible for retirement in 2003 (as the retirement age at that time was 63). However, in 2004, the retirement age had decreased to 62, when the 1941 cohort had already turned 63. Therefore, the entire 1941 cohort was eligible to retire on January 1st, 2004. Thus, an individual born in December 1941 retired in January 2004 at 62 years and some days, whereas an individual born in January 1941 retired also in January 2004 but was then already 63. A similar mechanism applied for individuals born between July 1938 and June 1940 as well as those born in 1943 and 1945. The cohort of 1942, on the other hand, turned 62 in 2004 and was eligible to retire at age 62 during the year of 2004 (in individual months of birth). This mechanism also applied to individuals born after July in 1940 and those born in 1944.

[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, and since July 1, 2016, 5.5% for employers and 1.5% for employees.

Table 1
Descriptive statistics.

	Pre-reform			Post-reform			
	Non-Construction	Construction	Diff (T-C)	Non-Construction	Construction	Diff (T-C)	Diff-in-Diff
Sickness absence	0.050	0.032	−0.018*	0.037	0.042	0.005	0.0233*
Health problems	0.100	0.086	−0.014	0.066	0.091	0.025**	0.039**
Covariates							
Age (in months)	710.91	713.823	2.913	711.992	710.865	−1.126	−4.040
Experience (in days)	7804.188	7561.895	−242.293	7533.747	7572.349	38.603	280.896
Swiss	0.687	0.627	−0.060***	0.461	0.367	−0.094***	−0.034
Shift work	0.324	0.308	−0.016	0.211	0.177	−0.035*	−0.018
Temporary contract	0.292	0.290	−0.001	0.037	0.027	−0.010	−0.009
Permanent contract	0.708	0.710	0.001	0.963	0.973	0.010	0.009
Educational level							
Compulsory school	0.257	0.257	0.000	0.331	0.380	0.049**	0.048*
Apprenticeship	0.064	0.039	−0.025**	0.046	0.055	0.010	0.035**
Higher apprenticeship	0.554	0.595	0.041*	0.495	0.471	−0.024	−0.064**
Vocational school	0.020	0.025	0.006	0.026	0.032	0.006	0.001
Vocational high school	0.015	0.012	−0.003	0.007	0.013	0.006	0.009
Master craftsman	0.057	0.044	−0.013	0.045	0.035	−0.009	0.004
Technician	0.010	0.022	0.012**	0.021	0.008	−0.013**	−0.03***
Polytechnic/University	0.023	0.006	−0.017***	0.030	0.005	−0.025***	−0.008
Income category	5.529	5.501	−0.028	5.926	5.839	−0.087	−0.059
Income (net, in 1000)	58.522	57.494	−1.028	60.982	60.313	−0.669	0.360
Work hours	41.768	41.705	−0.063	41.736	41.483	−0.253*	−0.190
Size of firm	12.054	12.002	−0.052	11.980	11.927	−0.054	−0.002
Married	0.713	0.773	0.060***	0.760	0.765	0.005	−0.055**
Mean retirement age	62.570	61.803	−0.767	61.667	59.812	−1.855***	−1.088
Years	1991–2009						
Ages	56–65						

Notes: We include the years 1991–2009 in our analysis. The treatment group comprises construction workers (T), whereas the control group includes other blue-collar workers (C). The pre-reform period is before July 2003, the post-reform period after July 2003. In the table, we calculate raw mean differences. *p < 0.1, **p < 0.05, ***p < 0.01.

Methods and data

Empirical framework: Identifying health effects of a reform

We are interested in estimating the impact of the reform in the Swiss construction sector on older construction workers affected by the lower retirement age. As health measures, we include subjective self-reported measures of sickness absences and health problems.

In our empirical approaches, we contrast treatment (T) and control (C) groups (Lechner, 2010). Approach I compares construction workers (T) with other blue-collar workers (C). This allows us to compare similar groups, since blue-collar workers are closer to construction workers than average employees. Hence, the two groups will be less likely to differ in their covariates. The descriptive statistics in Table 1 suggest that this assumption is valid as the differences between the two groups are quite small. For Approach I, we specify a treatment dummy as follows:

$$TREATI_i = \begin{cases} 1 & \textit{if construction worker} \\ 0 & \textit{if other blue-collar worker} \end{cases}$$

Approach II contrasts older (T) construction workers to younger (C) ones. Although the whole sector is affected by the reform, we expect the effect to be disproportionately stronger for older workers (i.e., 56–60 years of age) for several reasons. First, sickness absences increase as work life progresses (Alavinia et al., 2009, Brenner and Ahern, 2000). 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 possess a reduced ability to work (Liira et al., 2000). Third, Chau et al. (2004) document an increased level of injuries requiring hospitalization among older workers, though Schwatka et al. (2012) find no effect of age on the number of injuries, but only on the severity. Fourth, older workers are disproportionately affected by retirement reforms since their remaining work life is relatively short; thus, the relative change due to an adjustment in the pension age is relatively large (French and Jones, 2012). Finally, investments in human capital depreciate over time and returns on these investments have to be discounted. Therefore, the present value of the return on investments for an additional year of work exponentially grows when the age of retirement comes closer. Taken together, this indicates that older construction workers clearly differ from younger ones, a phenomenon that we test in Approach II.[13] For this reason, we construct a new treatment dummy in order to compare older construction workers (56–60 years of age) with different groups of younger ones (36–40, 41–45, 46–50, and 51–55 years of age), as follows:

$$TREATII_{i,t} = \begin{cases} 1 & \textit{if old construction worker} \\ 0 & \textit{if young construction worker} \end{cases}$$

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

$$Y_{i,t} = \alpha POST03_t + \theta TREATx_i + \rho POST03_t \times TREATx_i + \beta X_{i,t} + \tau + \phi_s + \varepsilon_{i,t} \tag{1}$$

where the endogenous variable $Y_{i,t}$ denotes subjective health measures for individuals i in year t. The variable $POST03_t$ is the post-reform dummy. The variable $TREATx_i$ is the treatment group indicator, which stands for $TREATI_i$ (in Approach I) and $TREATII_i$ (in Approach II). The variable of interest is the difference-in-differences regressor, that is, the interaction term $POST03_t \times TREATx_i$. 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. The variable $X_{i,t}$ stands for individuals' personal and job characteristics to control for other confounding effects. Eq. (1) is estimated separately for the different age groups 20–40, 40–60, 51–55, 56–60, and 61–65, using the years 2000 to 2006 for the main estimates. We use clustered standard errors at the individual level for all estimates. A concern that should be noted is that other interventions may have influenced the variables.

[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 (de Mello et al. 2017).

However, to the best of our knowledge, no other reforms for men in the blue-collar sectors have been implemented during the period under observation.

Data

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 that started in 1991. In this survey, a sample is randomly generated by using information from public registers and is representative of the Swiss population. To pursue our two approaches, we must identify workers who fall under the new FAR plan. The SLFS provides a four-digit occupational classification, with which we are able to categorize all the jobs that fall under the new law. Moreover, we can exclude the managers, 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 eligible for a lower retirement age from those who are not. The SLFS is a rich dataset that provides all encompassing information on job characteristics but only limited information on the health status of employees.

We use several questions to construct our two dependent variables on health measures. The first dependent variable, *absence due to sickness*, measures the proportion of workers who are absent from work because of health problems. It relies on the question, "Were you absent from work last week because of sickness or an accident?" The information collected for this variable spans the entire period between 1991 and 2015. This question is quite specific because it connects health problems to absences from the workplace. The second variable, *health problems* relies on a set of four questions which we draw from specific periods, namely "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), and "Do you have illnesses or health problems?" (between 2010 and 2015). We rely on these four questions, since none of these questions are asked with regard to the whole period. We are aware that the way in which the variable is specified may influence the measured differences between the treatment and the control groups in Approach I. However, by estimating the effect between the years 2003 and 2006, we provide an estimate of Approach I that is unaffected by the redefinition of the variable. As the 2003 interviews took place between April and June, that is, before the reform was implemented in July 2003 is set as a (single) pre-reform period. Thus, it is important to note that Approach II is unaffected by the problem of redefinition.

We apply five restrictions to our treatment and control groups to get our final sample. First, we restrict our dataset to males working full time (specified as more than a 90% work contract).[14] Second, we exclude all self-employed individuals (with or without employees), apprentices, and individuals who own firms, work at family-owned firms, as well as occupy management positions. Third, for the main specification, we restrict the sample to four pre- and three post-reform waves, that is, the years 2000 ($K-3$) to 2006 ($K+3$). 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.[15] Fourth, we exclude two out of the 26 Swiss cantons from our analysis. The cantons Vaud (VD) and Valais (VS) implemented specific early retirement schemes for construction workers that were harmonized with those of the other cantons only later. Therefore, they are excluded from our sample. Finally, we only include individuals aged between 20 and 65.

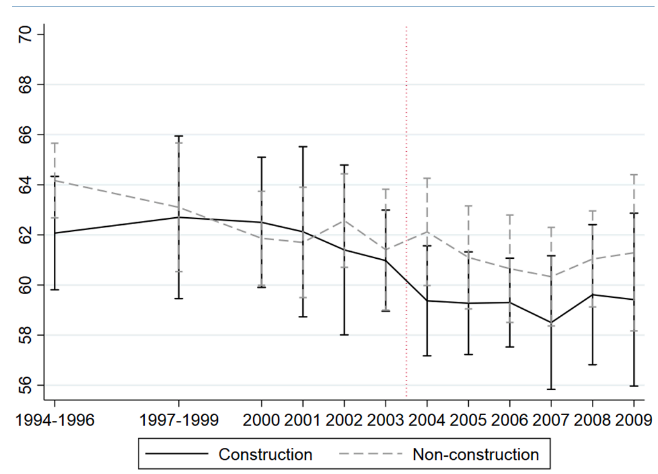

Fig. 3. Mean retirement age over the years in construction and non-construction sectors. Source: SLFS (1994–2011) and authors' own compilation.

We control for several exogenous variables (the full set is given in Table 1) because socio-economic status might affect how health status is reported (Angel, 2016). Specifically, we control for standard personal characteristics (e.g., age, nationality, education and marital status), 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 (for the seven central regions of Switzerland).

In the final part of the paper, we also look at the long-term effects of the reform, that is, how post-retirement health is affected by the lower retirement age. As the SLFS mainly covers job-specific health problems, it is not suitable for analyzing post-retirement effects. Therefore, we take advantage of the Swiss Household Panel (SHP), which contains more information regarding the self-reported health status of retirees (even though the SHP started in 1999, there have only been questions on the health-specific variables since 2004, therefore, the SHP is not suitable for analyzing the effects of the pension reform on pre-retirement health). We use five different self-reported health variables to analyze the health effects among the 65–80 years of age group and its sub-cohorts. These are a) subjective health status, on a scale from 1, "very well", to 5, "not well at all"; b) reported back problems during the previous four weeks, on a scale from 1, "not at all", to 3, "very much"; c) a dummy variable to determine whether the person has a chronic illness or long-term health problems; d) a 10-point scale continuous variable to verify whether the person has impediments in daily life; and e) a continuous variable for the number of days affected by health problems in the previous year. Descriptive staitistics of the SHP are shown in Table A2.

Descriptive statistics

Table 1 presents the descriptive pre- and post-reform differences of

[14] Women are excluded for two main reasons. First, as is standard in developed countries, in Switzerland only a small number women work in the construction sector. Thus, we fear that these women tend to represent a specific selection. Second, there was a general pension reform specifically for women at the same time, which could lead to confounding interpretations of our results.

[15] For robustness tests and the gauging of long-term effects, we vary the sample period, covering as recommended by Bertrand et al. (2004), five periods to exclude the serial correlation of the outcome and the intervention variables, (i.e., 2001 to 2005), but also 1997 to 2009, 1995 to 2011, and 1991 to 2015.

Table 2
Probability of working.

	Ages 56–60		Ages 61–65	
	(1)	(2)	(3)	(4)
Probability of working 1995–2011				
TREAT × POST	−0.0630**	−0.0617**	−0.0754**	−0.0742**
	(0.0280)	(0.0278)	(0.0371)	(0.0367)
R2	0.022	0.031	0.118	0.130
Observations	4629	4629	4199	4199
Probability of working 2000–2006				
TREAT × POST	−0.0609	−0.0577	−0.126**	−0.130***
	(0.0392)	(0.0394)	(0.0497)	(0.0495)
R2	0.015	0.023	0.127	0.133
Observations	1958	1958	1729	1729
Personal characteristics	yes	yes	yes	yes
Year trend	no	yes	no	yes
Regional dummies	no	yes	no	yes

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. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

the treatment and control groups (of workers aged between 56 and 65). The absences due to sickness before 2003 are higher in the control group (5% for non-construction versus 3.2% for construction workers).[16] In the post-reform period, two things are noticed. While the rate of sickness absence for non-construction workers decreases, it increases for construction workers. A similar pattern is observed for health problems. The two raw difference-in-differences are positive and statistically significant for both health measures, which gives us a strong indication that the lower retirement age increases not only sickness absences but also self-reported health problems.

Construction workers and other blue-collar workers have similar outcomes for many background job-specific characteristics. For example, experience, number of workers in permanent contracts, firm size, and working hours are not statistically different in the treatment and control groups. Furthermore, disposable income increases over time in both groups. However, for some personal characteristics, significant compositional differences are observed. For instance, in the construction sector, fewer workers are of Swiss nationality. Moreover, in the pre-reform period, the probability of being married was higher for construction workers than non-construction workers, though there was no difference in the post-reform period. There are also differences in the various educational levels, and we observe a shift towards less-educated workers in the post-reform period. Taken together, we cannot easily infer the health effects of the pension reform from these raw descriptive statistics. Therefore, an econometric analysis is needed.

Empirical analyses

In this section, we first show that the reform really affected the probability of working. In section 5.2, we test the parallel trends assumption. The main results demonstrating the effects of the reform on health measures are provided in section 5.3 for Approach I and section 5.4 for Approach II.

The impact of the reform on labor market participation

The FAR plan lowers the incentives of older workers to participate on the labor market and lowers their expected mean retirement age. Given that the aim of the reform is to improve the health of construction workers by lowering their de facto retirement age, it provides no actuarially fair compensation to construction workers who work full-time beyond the age of 60. From an economic perspective, such a reform can be expected to translate into a lower mean de facto retirement age. Fig. 3 shows the descriptive statistics of the mean retirement ages for construction and non-construction workers between 1994 and 2009. Over time, the retirement ages in both sectors decline. Even though the raw differences are not statistically significant, the decline of the retirement age in the treatment group is more pronounced.

After the enactment of the reform in 2003, the steepest fall in the retirement age of the construction sector is observed. There are even some plausible explanations for why we may underestimate the effect of lowering the retirement age. First, some of the construction workers who actually retire at 60, according to the FAR plan, possibly self-declare that they are not yet retired because de jure, their SRA remains 65. Second, some of the construction workers who actually retire under the FAR plan top-up their pensions by working part time at no more than 20% and hence do not report themselves as retired. Finally, even though the reform was agreed upon surprisingly quickly at the end of 2002, we cannot ignore that some construction workers postponed their planned retirement in 2002 or at the beginning of 2003 by several months (if physically possible). In this case, the observed retirement age in 2002 is higher due to anticipatory behavior prior to the reform.

The effect of the reform on working behavior before retirement is also subjected to a formal test. Table 2 shows linear probability estimations on whether the reform changes the probability of working from the age of 56 to 60 as well as from 61 to 65, in the interaction term $POST03_t \times TREATx_i$. While the fact that construction workers reduced their mean working activity between the ages 61 and 65 is straightforward to explain, the effect of the reform on work activity between the ages 56 and 60 is more interesting. According to human capital theory, the probability that an individual is working is expected to decline when the individual is approaching statutory retirement age. As shown by Hairault et al. (2010), countries with a statutory retirement age of 60 have the lowest employment rate between the ages 55 and 60. Thus, lowering the retirement age from 65 to 60 can be expected negatively to affect the probability of working between the ages 56 and 60, as is confirmed by the empirical results presented in Table 2. One explanation for this observation is that some construction workers bridge the time

[16] In general, employers have the obligation to continue salary payments in case of sickness. Sickness absences are compensated with 100% of the wage of the respective employee for at least three weeks (for most employers, and the longer someone has been employed, the longer compensation may last). After these first weeks, compensation depends on the reason for the absence. If the reason is classified as an "accident," the worker is compensated with 80% for 720 days (at least). If it is classified as "sickness," it depends whether the employer has "daily allowance insurance." If this is the case, the compensation is also 80% for 720 days, if not, it is less.

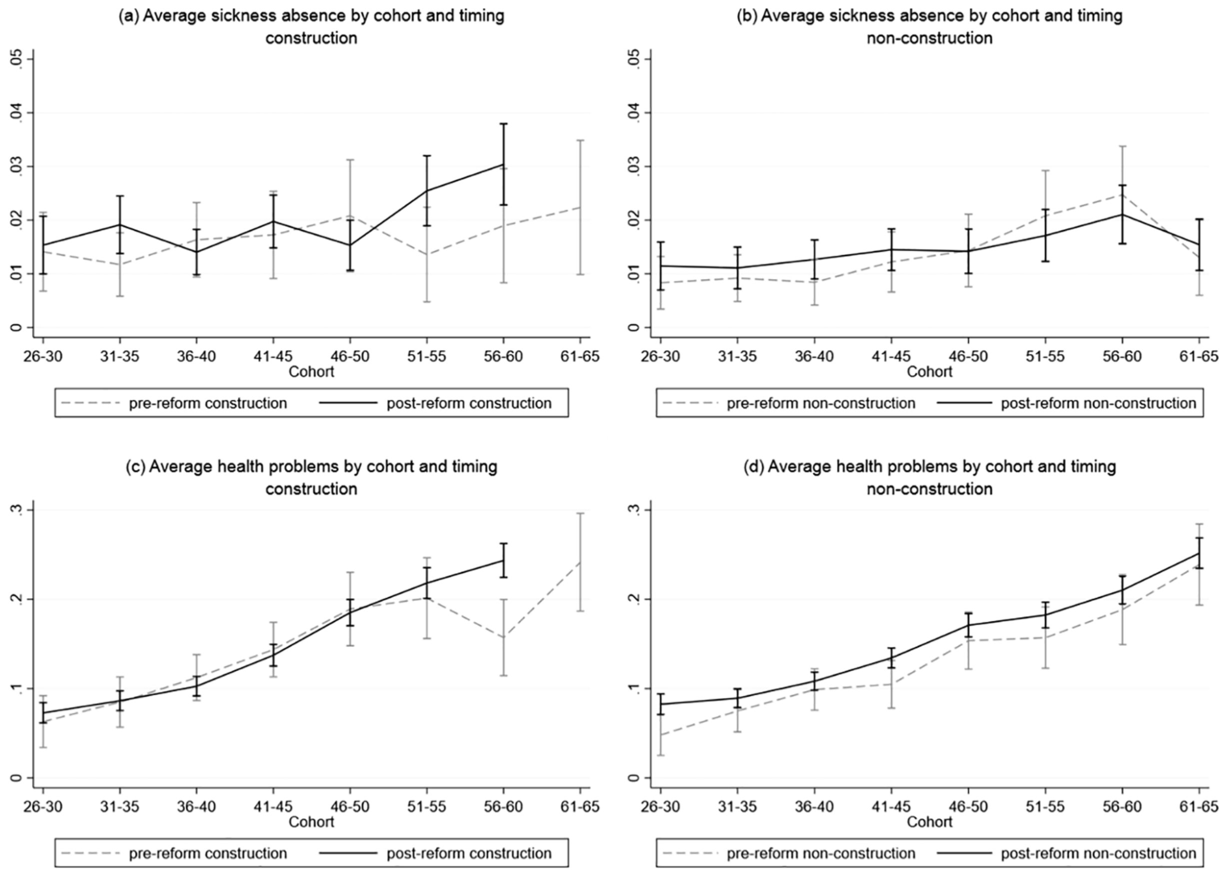

Fig. 4. Average sickness absence (a) and (b), and health problems (c) and (d) by cohort in the treatment and control groups during pre- and post-reform periods. Source: SLFS, 1995–2011 for panels (a) and (b), 2003–2011 for panels (c) and (d) and authors' own compilation.

between their retreat from work and official retirement at the age of 60 with other social security systems, such as unemployment insurance (UI).[17] While over the long term (1995–2011), the reform decreases the employment rate for those aged 61–65 by 7.42 percentage points for construction workers as compared to other blue-collar workers, the same measure decreases for those aged 56–60 by 6.17 percentage points, only slightly less. Estimations for a shorter time period (2000–2006) show related results. While the effect on the 56–60 year-olds is similar in terms of size, it is now only borderline statistically significant. In contrast, the effect for the 61–65 year olds is now larger. The reform decreased the employment rate of construction workers by 13 percentage points as compared to non-construction workers. However, as shown in section B.1, disposable income is not affected by the reform. Taken together, the results show that the reform incentivizes construction worker to lower their labor market participation.

Identifying the common trend assumption of health measures

The main concern that needs to be addressed is whether the common trend assumption holds true. In this subsection, we test descriptively and formally whether the health measures in the treatment group evolve in parallel to the control group in the pre-reform period.

The descriptive graphs in Fig. 4 offer some initial insights for the two empirical approaches. First, while the panels for the non-construction sector (b and d) show similar paths for the pre- and post-reform periods, the path for the construction sector (a and c) is systematically different for the pre- and post-reform periods (Approach I). Starting from the 51–55-year-old cohort, there is an increasing disparity between the pre- and post-reform paths for the construction sector. Second, the increase of the two health variables with age and the confirmation of the theoretically expected shift when lowering the retirement age (see Fig. 1) shows that older workers are disproportionally affected by pension reforms as their working horizon is relatively short. We will take advantage of this effect when comparing older and younger construction workers in Approach II. In sum, the descriptive statistics show that the reform has an effect on the two health measures of older construction workers, which we will test formally in the following.

Fig. 5 shows the plotted coefficients of the construction workers as a percentage point deviation from the base level $TREAT = 0 \times YEAR = 2000$ in panel (a) and $TREAT = 0 \times YEAR = 2003$ in panel (b) for Approach I. Panel (a) shows the interaction coefficient plot for sickness absences for the years 2000–2006 in the age group 56–60. The parallel trend assumption seems to hold for the 56–60 age group, as construction workers in 2001 and 2002 are at almost the same level as other blue-collar workers in 2000. In 2003, the effect jumps by almost 10 percentage points and increases significantly afterwards in the years 2005–2006. After 2003, construction workers have a higher level of sickness absences than other blue-collar workers in 2000.

Panel (b) shows the plotted coefficients of the interaction term $TREAT \times YEAR$ for health problems. The comparison group is other blue-collar workers in 2003, as we have no observations for 2000–2001, and in 2002, the question asked is different. In the years 2004–2006, construction workers have significantly more health problems than blue-collar workers in 2003. However, we cannot draw any further information about the parallel trends from panel (b) as we do not observe more than one pre-reform period with the same definition of the outcome variable. In both panels, we observe a strong adaptation of both

[17] Workers who were unemployed for several month (a maximum of 24 month in the last 7 years) remain eligible for the FAR plan. However, the benefits of the FAR pension are shortened by 1/180 per month receiving the UI benefits.

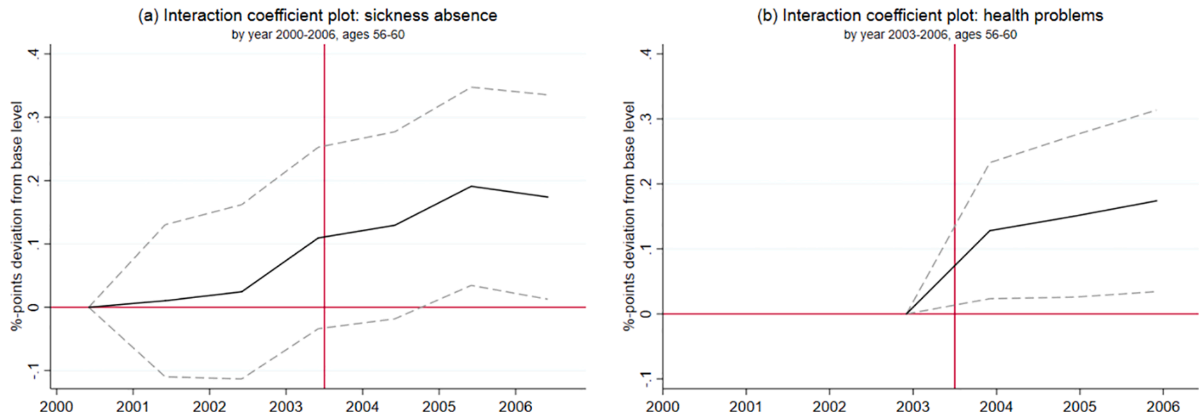

Fig. 5. Average %-point deviation from base level (control groups in 2000 and 2003 respectively) of sickness absence (a) and health problems (b) by year. Notes: SLFS (2000–2006) and authors' own compilation. The two panels show the interaction effect *TREAT* × *YEAR* between 2000 and 2006. The base level is set at 0 and the deviations from 0 are interpreted as the %-points deviation from the base level (control groups in the years 2000 and 2003, respectively). Sickness absences (panel a) are observed in the *last week* before the survey; health problems (panel b) are observed in the *last year* before the survey. As the survey is executed between April and June we adjust the curve in panel (a) accordingly, that is the observations in the mid of the respective year.

outcome variables after the reform. So far, these effects could be driven by both, actual and claimed health. As the effects grow larger over time, it could be a first indication that the effects are at least partly driven by lower investments in health (by the employer or the employee) and hence reflect actual health. However, the discussion on whether actual or claimed health is affected, will take place in more detail in section 6.2.

We proceed in the same way for Approach II (Fig. A1), where we test if the two health measures develop similarly for older and younger construction workers in the pre-reform period. As mentioned previously, we are not able to observe more than one pre-reform period (2003) for the health variable in Approach I, as the question asked in 2002 was changed in 2003. While this might be concerning for comparisons of construction and non-construction workers as the redefinition of health problems from 2003 onward may affect the measured outcomes for construction workers and other blue-collar workers differently,[18] we expect older and younger construction workers to be similarly impacted by this redefinition. The interaction coefficient for sickness absences (panels a–d of Fig. A1) is not statistically significant in the pre- or post-reform years for older and younger cohorts. On the one hand, this supports the parallel trend assumption (not for panels c and d), but on the other hand, older construction workers do not differ from younger ones after the reform. The results are slightly different for our second health measure *health problems* in panels e–h. For this measure, we observe the year 2002, even though its definition changes thereafter. In the two pre-reform periods (2002 and 2003), the interaction coefficients are insignificant, which is a strong indication of the parallel trend assumption. Moreover, for most ages, there is a jump after the reform in the years 2004 and 2005, but it diminishes in 2006.

Approach I: Effects on health, control other blue-collars

Table 3 summarizes the main results for Approach I, which estimates the impact of the policy reform on the prevalence of sickness absences (A) and health problems (B) between 2000 and 2006.[19,20] In Table 3, we estimate Eq. (1), given the relevant model (linear probability model with and without fixed effects) in the respective age groups indicated in the heading. The columns only differ according to the specific age groups under consideration. The impact of the reform is insignificant for younger construction workers in columns (1) to (3) when compared to non-construction workers. In contrast, columns (4) to (7) reveal that health of the 56–60 and 61–65 age cohorts is systematically affected by the lower retirement age. The interaction effect of *TREAT* × *POST* is statistically significantly positive (i.e. the effect on sickness absence and health problems is larger) for these groups.

Column (4) of panel A shows that holding all the other factors at their means, the policy change increases the probability of sickness absences by 1.15 percentage points (=-0.0387 + 0.0502) in the construction sector for the 56–60 age group between 2000 and 2006. Thus, the probability of sickness absences in the construction sector increases by 33 percent from 3.49% in the pre-reform period to 4.64% in the post-reform period. This result is even more pronounced for the 61–65 age group (column 5, note that it refers only to those working still at least 90 percent), where the probability of being absent from work due to health reasons increases by 1.3 percentage points (=-0.0626 + 0.0756). Hence, for this age group, sickness absences increase by 36 percent (from 3.65% to 4.95%).

Panel B shows that the new reform has a different effect on the self-reported health of the treatment group compared to that of the control group. Again, we observe no effect on the younger cohorts (columns 1–3) but a statistically significant effect for the cohorts 56 to 60 and 61 to 65. The probability of having health problems between the ages 56 to 60 increases by 54 percent, or 4.1 percentage points (=-0.061 + 0.102) from 7.59% to 11.69% in the post-reform period. Column (5) shows the probability of having health problems between the ages 61 and 65 increases by 8.67 (=-0.0773 + 0.164) percentage points. This is an increase from almost zero to 8.9%. The effect on the 61–65 age group (again, only those individuals who are still working full time), is larger than when only the 56–60 age cohort is considered.[21]

It is important to note a possible selection bias in the analysis of individuals aged 56–60. If the reform increases the attractiveness of working up to age 60, our sample could consist of relatively more workers experiencing worsening health. Specifically, individuals who

[18] E.g., both have similar amounts of accidents (question in 2002), while construction workers tend to indicate more physical problems (question between 2003 and 2006).

[19] Table A-3 shows the analogous results for other time periods. We shorten and extend the sample to the years between 2001 and 2005, 1997 and 2009, 1995 and 2011, and 1991 and 2015. Generally, all the results are also confirmed when more years are included.

[20] In section B.3, we provide a further analysis on the intensive margin, i.e., on the absence duration.

[21] In section B.2, we explore the extent of endogenous aging as a result of the reform. Regarding absences, 55-year-old workers in the post-reform period are similar to 58-year-old workers in the pre-reform period. Regarding health outcomes, 55-year-old workers in the post-reform period are similar to 57-year-old blue-collar workers.

Table 3
Approach I: Construction versus non-construction workers, 2000–2006 (2003–2006 for Panel B).

	Ages 20–40	Ages 40–60	Ages 51–55	Ages 56–60	Ages 61–65	Ages 56–60	Ages 61–65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Sickness absence							
TREAT	0.0130**	−0.00274	−0.0179	−0.0152	−0.0726**		
	(0.00517)	(0.00653)	(0.0155)	(0.0158)	(0.0362)		
POST	−0.00232	−0.0203**	−0.00577	−0.0387**	−0.0626		
	(0.00661)	(0.00846)	(0.0213)	(0.0192)	(0.0446)		
TREAT × POST	−0.00923	0.00859	0.0222	0.0502**	0.0756*	0.0604**	0.0793
	(0.00622)	(0.00825)	(0.0205)	(0.0209)	(0.0441)	(0.0268)	(0.0971)
R2	0.005	0.012	0.025	0.020	0.042	0.438	0.544
Observations	7385	6848	1454	1215	551	950	304
Pre-reform mean (construction)	2.74%	3.55%	2.39%	3.49%	3.65%	3.49%	3.65%
B. Health problems							
TREAT	0.0180	0.0133	0.0379	−0.0520	−0.161***		
	(0.0117)	(0.0146)	(0.0343)	(0.0366)	(0.0562)		
POST	0.0127	0.00732	0.0251	−0.0614	−0.0773		
	(0.0112)	(0.0149)	(0.0321)	(0.0382)	(0.0704)		
TREAT × POST	−0.0171	−0.0152	−0.0437	0.102**	0.164***	0.137***	−0.0376
	(0.0129)	(0.0162)	(0.0376)	(0.0429)	(0.0613)	(0.0506)	(0.105)
R2	0.013	0.023	0.031	0.069	0.055	0.600	0.694
Observations	5634	5300	1122	907	414	503	210
Pre-reform mean (construction)	5.74%	8.75%	9.94%	7.59%	0.245%	7.59%	0.245%
Job characteristics	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes
Year FE	no	no	no	no	no	yes	yes
Individual FE	no	no	no	no	no	yes	yes

Notes: Estimations in panels A and B show linear probability regressions using Eq. (1). We include blue-collar workers in 2000–2006 (2003–2006 for panel B). 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 individual level and shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

would have retired before 60 if there was no reform have stronger incentives to continue working until the age of 60 in order to benefit from the new pension scheme. If individuals systematically behave in this manner, our estimates might suffer from a selection problem. However, as we have shown in section 5.1, the reform negatively impacts employment rates in the age group 56–60, not least because individuals can leave the labor market before the age of 60 but bridge the time to their new standard retirement (at age of 60) with UI benefits for two years. Hence, the pension reform has induced individuals with above average health problems to leave the labor market earlier rather than to stick to it for additional years, which mitigates the selection problem and makes our tests conservative. Moreover, we can mitigate selection effects by accounting for the panel structure of our dataset and including individual and year fixed effects. Thus, the estimates in columns (6) and (7) only rely on within-individual variations. The *TREAT* is time-invariant and *POST* is multi-collinear to the year-fixed effect, that is, the base effects are omitted in the regression. Even though the base effects are not displayed, the interaction term can be interpreted in terms of size. The fixed effects model for the 56–60 year-old cohort shown in Table 3 (column 6) is very similar to the linear probability model. This gives us a strong indication that our results are not driven by selection bias.

It is interesting to also look at the age group 61–65. After the reform, it consists of three types of individuals: First, those who only benefit from a reduction of their statutory pension age to 61, 62, or 63 due to the stepwise implementation of the reform. These individuals can be expected to represent a normal selection with respect to health. Second, there are those who are eligible for a lower retirement age but decide to continue working full time or part time beyond their (new) statutory retirement age. They can be expected to be a positive selection with respect to health. Third, similar to the discussion of the 56–60-year-old group, the reform may incentivize those who are eligible to retire at 61–63 to work longer than otherwise expected in order to become eligible for the full pension at 61–63. However, there are two arguments in favor of this group being small: The reform lowers the retirement age in 2003 directly from 65 to 63. Therefore, the maximum "waiting period to retirement" did not exceed three years, two of which can be bridged by unemployment insurance with a shortening of the future FAR pension by 1/180 per month of UI. Actually, labor market participation among those 61 to 63 did also decrease due to the pension reform (data is available from the authors upon request). Therefore, we expect the role of a bad selection (i.e., this third type) in the 61–65 age group to be limited and the positive selection (i.e., type two) to outweigh the negative one. Thus, we expect to observe a lower bound for the size of the negative effect of the pension reform on health.

Approach II: Effects on health, control young construction workers

The results of Approach II are summarized in Table 4. In this approach, we compare older construction workers with younger ones. A statistically significant difference between older and younger construction workers before and after the reform implies that the effect of the pension reform on self-reported health is larger for older workers than for younger ones.

The aim of this approach is to tackle the concern about non-parallel trends between construction and other blue-collar workers by focusing on differently affected construction workers. While the parallel trend assumption is confirmed only for health problems in Fig. A1, Table 4 sheds light on the results for older and younger construction workers. In panel A, specifications (1) and (3) confirm that the effect on older workers is statistically different from the effect on younger workers, as the interaction effect Age 56–60 × POST is statistically significantly positive.

The reform has a larger effect on the 56–60 age group than on all the younger cohorts. With the reform, the probability of being absent for the 56–60 age group increases by 2.98 percentage points compared with the 36–40 age group and by 3.7 percentage points compared with the 46–50 age group. These results must be interpreted with caution, as the common trends assumption cannot be tested due to the short data series.

In panel B, we find that the negative effect of the reform on the health of older construction workers statistically significantly differs from that of younger workers. It is important to emphasize that while health problems among younger age groups decrease, the probability of having health issues between the ages 56–60 increases. Hence, the reform affects the 56–60 age group by 7.8–10 percentage points more than all the younger cohorts. Again, fixed effects estimations in columns (5) to (8) confirm these results.

Further discussion and robustness checks

Robustness checks and placebo

In this section, we present the sensitivity analysis and placebo tests in five different stages. First, we provide placebo tests where we incorrectly assume that the reform occurred in 1997 and in 2001 (Table A4). Second, we test for the heterogeneity of the effects and include groups that have been excluded from our main estimates (Table A5). Third, we examine the 56–57 age group, which has been affected in one shot by the full five-year shift of the retirement age due to the reform (also Table A5). Fourth, we verify the interaction of the treatment dummy with a year dummy instead of the POST dummy (Table A6). By doing so, we can examine the variation in the treatment intensity over time (i.e., retirement age is 63 in 2003, 62 in 2004, 61 in 2005, and 60 in 2006). Finally, we interact the treatment dummy with a cohort dummy (i.e., the birth year) to account for the variation in retirement ages for different cohorts (Table A7).

Placebo specification

Turning to the results of the fake treatment, Table A4 reports two sets of incorrect treatments. First, we take exactly five periods before the real treatment period between 1999 and 2003 and define a fake treatment for the year 2001. For each of the two outcome variables, all the specifications are statistically insignificant. In the second row, we include the period between 1991 and 2003, and add a fake treatment in 1997. Once again, all the specifications show no statistically significant results for the fake treatments.

Possible misspecification

As previously discussed, we include or exclude certain groups from our main estimates (e.g., job changers and part-time workers). There is a potential concern that this may affect our results. Table A5 addresses this concern and tests the effect heterogeneity. The first two columns exclude job changers. Columns (3) and (4) provide evidence that the effects are not the result of removing part-time workers from our analysis. Furthermore, column (5) shows the effect on the 56–57 years-of-age cohort that is treated with the full five years of the reform and has a retirement age of 60 after the reform. Columns (6) and (7) in Table A5 show the results for a minimum set of controls. Finally, the last two columns in Table A5 show the results for the German-speaking part of Switzerland alone. As two cantons have already put their own FAR plans in place, we cannot rule out that some workers have moved to these cantons, which could bias our estimates. As both cantons belong to the French-speaking part of Switzerland and migration across language borders is much rarer than migration within the different parts of Switzerland, we look at the German-speaking part only. The results mute the migration argument. The size and statistical significance of the effects changes only slightly. Taken together, the results prove to be robust and confirm our main insight.

Year gradient specification

We address the concern that the gradual decrease in the retirement age may impact the results in Table A6. To this end, we replace the *POST*-dummy with a *YEAR*-dummy and the interaction term with *TREAT* × *YEAR* dummies. We observe a positive and significant effect of the treatment after 2004 for sickness absences and after 2003 for health problems. Table A6 represents the coefficients that have already been plotted in Fig. 5.

Table 4

Approach II: Old versus young construction workers, 2000–2006 (2003–2006 for Panel B).

	Linear probability model				Fixed effects model			
	36–40 vs.56–60 (1)	**41–45 vs.56–60** (2)	**46–50 vs.56–60** (3)	**51–55 vs.56–60** (4)	**36–40 vs.56–60** (5)	**41–45 vs.56–60** (6)	**46–50 vs.56–60** (7)	**51–55 vs.56–60** (8)
A. Sickness absence								
Age 56–60	−0.00297	−0.0465	−0.00935	0.00408				
	(0.0297)	(0.0420)	(0.0279)	(0.0173)				
POST	−0.0138	−0.00924	−0.0247	0.0145				
	(0.0133)	(0.0172)	(0.0192)	(0.0214)				
Age 56–60 × POST	0.0298*	0.0169	0.0377*	0.00510	0.0692**	0.0465	0.0199	0.0255
	(0.0174)	(0.0185)	(0.0203)	(0.0220)	(0.0306)	(0.0341)	(0.0382)	(0.0378)
R2	0.025	0.016	0.025	0.019	0.500	0.464	0.507	0.462
Observations	1735	1644	1318	1237	1057	1000	809	855
B. Health problems								
Age 56–60	−0.200***	−0.185*	−0.0703	−0.108***				
	(0.0707)	(0.0954)	(0.0737)	(0.0369)				
POST	0.00105	0.0174	0.00945	0.0154				
	(0.0250)	(0.0301)	(0.0354)	(0.0347)				
Age 56–60 × POST	0.0746**	0.0602*	0.0807**	0.0826**	0.104**	0.0915*	0.0738	0.0962**
	(0.0339)	(0.0353)	(0.0394)	(0.0416)	(0.0464)	(0.0493)	(0.0528)	(0.0475)
R2	0.045	0.037	0.052	0.046	0.625	0.633	0.656	0.620
Observations	1348	1269	1051	976	748	687	602	639
Job characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	no	no	no	no	yes	yes	yes	yes
Individual FE	no	no	no	no	yes	yes	yes	yes

Notes: Estimations in panels A and B show linear probability regressions using Eq. (1). We include construction workers only in 2000–2006 (2003–2006 for panel B). 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 individual level and shown in parentheses.*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Cohort gradient specification

Another approach to account for the gradual decrease of the retirement age is to interact the *TREAT*-dummy with the continuous variable year of birth between 1936 and 1949. Table A7 shows the results for ages 56–60 (column 1), 61–65 (column 2), and for the whole cohort of ages 56–65 (column 3). For the sickness absences in panel (a), the effect is positive but not statistically significant for each of the age groups 56–60 and 61–65. However, it is statistically significant for the entire 56–65 age group. A positive interaction effect for the 56–65 year olds indicates that for each younger birth year, the probability of being absent due to sickness increases by 0.4 percentage points for construction workers as compared to other blue-collar workers. Panel (b) shows a positive interaction effect for all three columns. For each later year of birth, the probability of having health problems increases between 0.06 and 0.12 percentage points for construction workers compared to other blue-collar workers (see Table A7). This supports the argument that the earlier results are not solely driven by the misspecification of not including the gradual decrease of the reform.

Actual or claimed health?

To date, we find robust evidence that the reform in the construction sector increases sickness absences and self-reported health problems. Given the intention of the reform to improve the health of construction workers by offering them early retirement, this result is unintended. However, as previously discussed, it is critical to disentangle the different channels that may be at play. On the one hand, we learn from human capital theory that changing the working horizon changes incentives to invest in older workers for the employers and the older workers themselves (Bauer and Eichenberger, 2017, Bertoni et al., 2018, Brunello and Comi, 2015) or can be stressful (Falba et al., 2009). In this case, the lower working horizon may negatively affect actual health of construction workers. On the other hand, as we observe self-reported health measures, it is indeed possible that our results are also affected by health problems that are only claimed, that is we observe morally hazardous behavior by construction workers who remain more often absent from work not because of actual sickness, but rather because the opportunity cost to do so diminishes with the shrinking work horizon. Moreover, they may try to justify their absence by self-reporting more extensive health problems (Black et al., 2017, Gupta and Larsen, 2010, Johansson et al., 2020). So far, we have not yet thoroughly attributed the effects found to these mechanisms.

Our perspective that the lower working horizon is not only affecting health claims but also actual health is supported by three pieces of evidence: First, the two health measures underlying our analysis are largely independent from each other as they refer to two different time periods. While the questionnaire participants were asked about sickness absences during the *last week*, they were asked questions pertaining to health problems during the *entire past year*. Thus, of the individuals who were absent from work in the previous week due to sickness, only shares of 38 percent (ages 56–60) and 27 percent (ages 61–65) indicate health problems. Conversely, of the individuals reporting health problems in the previous year, only 6 percent (ages 56–60) and 3 percent (ages 61–65) were absent in the previous week. These results suggest that individuals' health statements are not solely driven by them attempting to justify absence from work.

Second, by including only individuals working full time, we are able to limit the threat of over-reported health problems. Working individuals tend to under- rather than over-report health problems because they fear career repercussions or job losses if they are found to suffer from chronic diseases (Gupta and Jürges, 2012). Moreover, sickness absence is a more objective measure that is less likely affected by a justification bias (Ilmakunnas and Ilmakunnas, 2018).

Third and most importantly, we examine whether the intended health-improving effects of the reform materialize at least after retirement. Even though post-retirement health is not the main focus of the

Table 5
OLS analysis of the relationship between reform and health outcomes in construction and other blue-collar sectors.

	(1) Poor health	(2) Chronic disease	(3) Back problems	(4) Impediment in daily life	(5) Number of bad days
a) Ages 66–70					
TREAT × BIRTH ≥ July 1938	0.164	0.110	0.478**	1.345*	9.581
	(0.231)	(0.106)	(0.163)	(0.637)	(9.097)
R2	0.081	0.074	0.087	0.075	0.056
Observations	378	378	378	378	378
b) Ages 66–75					
TREAT × BIRTH ≥ July 1938	0.189	0.231*	0.390*	0.856	6.157
	(0.154)	(0.114)	(0.190)	(0.672)	(6.403)
R2	0.080	0.052	0.061	0.045	0.029
Observations	612	612	612	612	612
c) Ages 66–80					
TREAT × BIRTH ≥ July 1938	−0.0128	0.122	0.275	0.614	−1.054
	(0.143)	(0.128)	(0.184)	(0.739)	(8.176)
R^2	0.057	0.046	0.048	0.052	0.024
Observations	736	736	736	736	736

Notes: Estimations show linear probability regressions using Eq. (1). We include blue-collar workers in 2004–2015. The treatment group comprises construction workers, whereas the control group consists of the other blue-collar workers. BIRTH ≥ July 1938 stands for individuals born after July 1938, which were eligible to a lower retirement age. Personal characteristics include the variables age, nationality, education, and marital status. Standard errors are clustered on the individual level and shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A1
Overview of reform.

Birth year of cohort	Retirement age of cohort	Year of retirement	Statutory retirement age in this year
1935	65	2000	65
1936	65	2001	65
1937	65	2002	65
1938	Until June 65; from July: gradual decrease to 64.5	2003	Until June: 65; from July: 63
1939	Gradual decrease from 64.5 to 63.5	2003	Until June: 65; from July: 63
1940	Until June: gradual decrease from 63.5 to 63; from July: 63	2003	Until June: 65; from July: 63
1941	Gradual decrease from 63 to 62	2004	62
1942	62	2004	62
1943	Gradual decrease from 62 to 61	2005	61
1944	61	2005	61
1945	Gradual decrease from 61 to 60	2006	60
1946	60	2006	60
1947	60	2007	60
1948	60	2008	60
1949	60	2009	60
1950	60	2010	60

Table A2
Descriptive statistics Swiss Household Panel.

	Pre-reform			Post-reform			
	Non-Construction	Construction	Diff (T-C)	Non-Construction	Construction	Diff (T-C)	Diff-in-Diff
Poor health	2.038	2.127	0.089	1.963	2.005	0.042	−0.047
Chronic disease	0.483	0.345	−0.137*	0.455	0.415	−0.040	0.098
Back problems	1.585	1.364	−0.222**	1.417	1.434	0.017	0.239**
Impediment in daily life	2.812	2.327	−0.485	2.091	2.185	0.094	0.579
Number of bad days	10.944	8.164	−2.781	9.252	4.790	−4.462	−1.681
Covariates							
Education	4.850	4.745	−0.105	4.442	4.790	0.348	0.453
Married	0.855	0.764	−0.091	0.822	0.741	−0.081**	0.010
Swiss	1.000	1.000	0.000	0.934	0.849	−0.085***	−0.085**
Age	74.269	74.364	0.094	68.955	69.073	0.119	0.024
Years in job	51.094	50.655	−0.439	46.256	47.473	1.217**	1.656
Years	2004–2015						
Ages	66–80						

Notes: We include the years 2004–2015 in the age group 66–80. The treatment group comprises construction workers (T), whereas the control group includes the other blue-collar workers (C). The pre-reform period includes individuals born before July 1938 while post-reform period includes those born after July 1938. *p < 0.1, **p < 0.05, ***p < 0.01.

Table A3
Robustness: Main estimations with extending years.

	Years 2001–2005		Years 1997–2009		Years 1995–2011		Years 1991–2015	
	Ages 56–60 (1)	Ages 61–65 (2)	Ages 56–60 (3)	Ages 61–65 (4)	Ages 56–60 (5)	Ages 61–65 (6)	Ages 56–60 (7)	Ages 61–65 (8)
A. Sickness Absence								
TREAT × POST	0.0681***	0.0330	0.0238	0.0455	0.0112	0.0513*	0.0272**	0.0506**
	(0.0232)	(0.0421)	(0.0178)	(0.0339)	(0.0153)	(0.0299)	(0.0131)	(0.0244)
R2	0.033	0.047	0.014	0.022	0.014	0.015	0.012	0.013
Observations	950	430	1946	909	2762	1347	4176	2070
B. Health problems								
TREAT × POST	0.112***	0.0551	0.0725**	0.110**	0.0665**	0.106**	0.0541**	0.0863**
	(0.0397)	(0.0591)	(0.0321)	(0.0472)	(0.0286)	(0.0447)	(0.0266)	(0.0380)
R2	0.060	0.061	0.040	0.027	0.047	0.042	0.053	0.051
Observations	869	394	1756	834	2519	1249	3928	1972
Job characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Year trend	yes	yes	yes	yes	yes	yes	yes	yes
Regional dummy	yes	yes	yes	yes	yes	yes	yes	yes

Notes: Estimations in panels A and B show linear probability regressions using Eq. (1). We include blue-collar workers in different extended years. 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 individual level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Table A4
Placebo estimation with fake treatments.

	A. Sickness absence		B. Health problems	
	Ages 56–60	Ages 61–65	Ages 56–60	Ages 61–65
TREAT × POST01	−0.021	0.039	−0.012	−0.096
	(0.028)	(0.068)	(0.093)	(0.107))
	626	218	484	221
	1999–2003	1999–2003	1999–2003	1999–2003
TREAT × POST97	0.009	−0.035	−0.054	−0.101
	(0.023)	(0.043)	(0.042)	(0.063)
	1077	534	840	437
	1991–2003	1991–2003	1991–2003	1991–2003
Job characteristics	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes
Year trend	yes	yes	yes	yes
Regional dummy	yes	yes	yes	yes

Notes: Estimations show linear probability regressions using Eq. (1). We include blue-collar workers in 1991–2003. The treatment group comprises construction workers, whereas the control group includes 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 regional dummy to control for region-specific differences. Standard errors are clustered on the individual level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Table A5
Robustness: Effect heterogeneity.

	No job changer		With part-time job		Other age	Minimum set of controls		German speaking part	
	Ages56–60	Ages61–65	Ages56–60	Ages61–65	Ages56–57	Ages56–60	Ages61–65	Ages56–60	Ages61–65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Sickness Absence									
TREAT	−0.0123	−0.0743**	−0.0137	−0.0605**	−0.0244	−0.0184	−0.0577**	−0.0266	−0.0597
	(0.0164)	(0.0367)	(0.0151)	(0.0308)	(0.0265)	(0.0145)	(0.0293)	(0.0163)	(0.0382)
POST	−0.0361*	−0.0645	−0.0309	−0.0512	−0.0420	−0.0517***	−0.0507	−0.0362**	−0.0534
	(0.0194)	(0.0479)	(0.0192)	(0.0407)	(0.0263)	(0.0185)	(0.0359)	(0.0178)	(0.0434)
TREAT × POST	0.0466**	0.0776*	0.0452**	0.0707*	0.0564*	0.0491***	0.0555	0.0587***	0.0701
	(0.0215)	(0.0451)	(0.0204)	(0.0387)	(0.0327)	(0.0187)	(0.0375)	(0.0219)	(0.0464)
R2	0.021	0.043	0.020	0.035	0.036	0.011	0.024	0.024	0.039
Observations	1180	532	1286	627	521	1482	690	1049	505
B. Health problems									
TREAT	−0.056	−0.160***	−0.0284	−0.127	−0.0396	−0.0610*	−0.128***	−0.0435	−0.156***
	(0.0366)	(0.0567)	(0.0383)	(0.0565)	(0.0487)	(0.033)	(0.043)	(0.0386)	(0.0599)
POST	−0.586	−0.0772	−0.0481	−0.059	0.0047	−0.082**	−0.0423	−0.0692*	−0.0905
	(0.0381)	(0.0707)	(0.0406)	(0.0707)	(0.0558)	(0.0342)	(0.0571)	(0.0398)	(0.0727)
TREAT × POST	0.110**	0.164***	0.0876*	0.116*	0.116**	0.114***	0.129***	0.0984**	0.170***
	(0.043)	(0.0623)	(0.0452)	(0.0643)	(0.0558)	(0.0383)	(0.050)	(0.0451)	(0.0639)
R2	0.062	0.054	0.124	0.089	0.101	0.014	0.022	0.067	0.057
Observations	887	409	961	470	395	1109	536	792	379
Job characteristics	yes	yes	yes	yes	yes	no	no	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	no	no	yes	yes
Year trend	yes	yes	yes	yes	yes	yes	yes	yes	yes
Regional dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: Estimations in panels A and B show linear probability regressions using Eq. (1). We include blue-collar workers in 2000–2006 (2003–2006 for panel B). 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 individual level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

paper, it is interesting for two reasons. First, given the intention of the reform, one may expect health improvements in older cohorts. Therefore, a negative effect on post-retirement health would be bad news for proponents of the reform. Second, looking at post-retirement health provides new insights on whether actual health or only claimed health is affected. Individuals have no reason to claim bad health in order to justify absences from work when they are retired. Therefore, a negative effect of the reform on post-retirement health is a strong indication that the reform is negatively affecting actual health.

In Table 5, we explore the long-term effects of the reform on individuals who are older than 65, that is, older than the Swiss statutory retirement age as well as the previous retirement age in the construction sector. To this end, we use information from the Swiss household panel

Table A6
Robustness: Year gradient specification.

	A. Sickness absence	B. Health problems
	Ages 56–60 (1)	Ages 56–60 (2)
TREAT × 2000	Base	
TREAT × 2001	0.0103	
	(0.0728)	
TREAT × 2002	0.0245	
	(0.0834)	
TREAT × 2003	0.109	Base
	(0.0869)	
TREAT × 2004	0.129	0.128**
	(0.0896)	(0.0634)
TREAT × 2005	0.191**	0.150**
	(0.0949)	(0.0753)
TREAT × 2006	0.174*	0.174**
	(0.0979)	(0.0846)
Year FE	yes	yes
Regional FE	yes	yes

Notes: Estimations in panels A and B show linear probability regressions using Eq. (1). We include blue-collar workers in 2000–2006 (2003–2006 for panel B). The treatment group comprises construction workers, whereas control group consists of other blue-collar workers. POST stands for the post-reform period after July 2003. We control for a minimum set only, including variables such as age, experience, nationality, educational level, income, and marital status. We add year trend and a regional dummy to control for region-specific differences. Standard errors are clustered on the individual level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Table A7
Robustness: Interaction with years of birth between 1936 and 1949

	Ages 56–60	Ages 61–65	Ages 56–65
	(1)	(2)	(3)
A. Sickness Absence			
TREAT × Year of birth	0.00253	0.00519	0.00357*
	(0.00262)	(0.00364)	(0.00211)
R2	0.019	0.019	0.015
Observations	1824	1383	3207
B. **Health problems**			
TREAT × Year of birth	0.00626*	0.0116*	0.00916**
	(0.00378)	(0.00661)	(0.00365)
R2	0.038	0.054	0.047
Observations	1591	1323	2914
Job characteristics	yes	yes	yes
Personal characteristics	yes	yes	yes
Year trend	yes	yes	yes
Regional dummy	yes	yes	yes

Notes: Estimations in panels A and B show linear probability regressions using Eq. (1). We include blue-collar workers in 2000–2006 (2003–2006 for panel B). The treatment group comprises construction workers, whereas control group consists of other blue-collar workers. YEAR OF BIRTH stands for the birth year. 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 regional dummy to control for region-specific differences. Standard errors are clustered on the individual level and shown in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

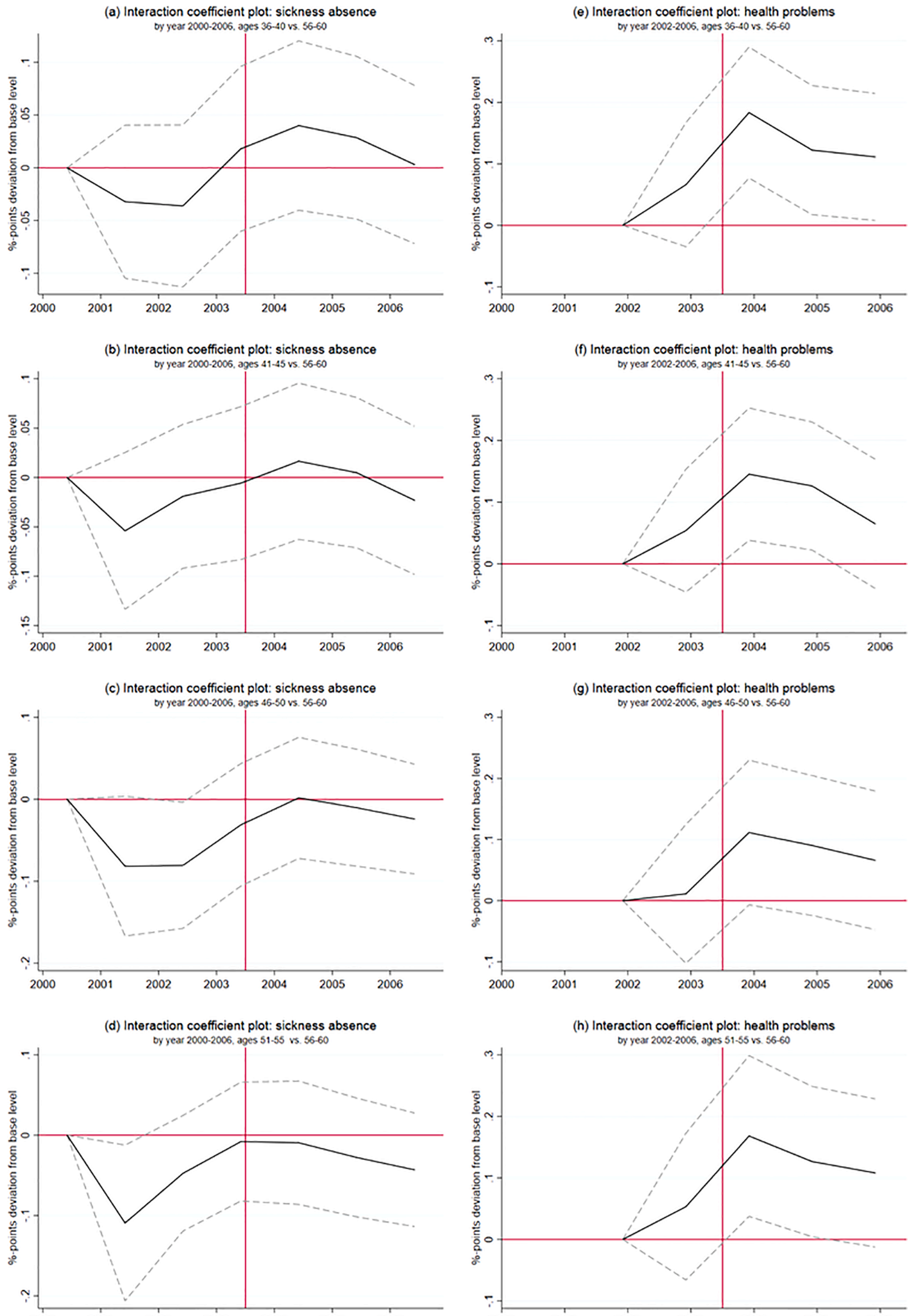

Fig. A1. Average percentage point deviation from the base level (control group in years 2000 and 2002, respectively) of sickness absence (a–d) and health problems (e–h) by year for Approach II. Notes: SLFS (2000–2006) and authors' own compilation. The two panels show the interaction effect *TREAT* × *YEAR* between 2000 and 2006. The base level is set at 0 and deviations from 0 are interpreted as the percentage point deviation from the base level (control group in years 2000 and 2002). Sickness absences (panels a–d) are observed in the *last week* before the survey; health problems (panel e–h) are observed in the *last year* before the survey. As the survey is executed between April and June we adjust the curve in panel (a) accordingly, that is the observations in the mid of the respective year.

(available from 2004 onward) on the five health measures briefly described in section 4.2. As data is not available for the pre-reform period (before July 2003), we replace the POST-dummy with a dummy equal to one for a birth year of 1938 or later (according to Table A1, this is the first cohort to benefit from a lower retirement age). We cannot rule out that the lower retirement age affects post-retirement health of construction workers negatively. While we do not find any statistically significant indication for improving health after retirement due to the reform, we find some clearly negative effects. Specifically for age groups 66–70 and 66–75, we find evidence a greater prevalence of back problems, impediments in daily life and chronic diseases. This supports the hypothesis that actual health prior to retirement is affected by the reform, at least to some extent. However, panel (c) in Table 5 illustrates that in the bigger picture (when including all individuals between 66 and 80 years of age), the negative health effects for the construction workers evaporates, which is not astonishing after so many years. Taken together, our results suggest that – even though we cannot fully rule out the existence of a justification bias and moral hazard – the reform negatively affected the actual health of construction workers to at least some degree.

Conclusion

In this paper, we have analyzed whether lowering the retirement age for construction workers has an unintended negative effect on their health before they reach the new retirement age. 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 compared them to other blue-collar workers, as well as younger construction workers.

Specifically, our results suggest that while the reform has been effective in reducing employment in the 61–65 age cohort, it has also decreased employment in the 56–60 age cohort. This paper complements the literature on the effects of pension reforms to the de facto retirement age (e.g., Atalay and Barrett, 2015, Staubli and Zweimüller, 2013) as it is one of the first to focus on a reform that has lowered the retirement age. Even more interestingly, we have observed a higher prevalence of sickness absences and a greater probability of health problems among cohorts approaching retirement age after the reform. The outcomes are considerable in terms of size. For instance, sickness absences in the 56–60 years of age cohort increase by 33 percent and those in the 61–65 years of age cohort increase by 36 percent compared to the pre-reform period. Likewise, the mean probability of health problems being indicated increases by 54 percent (56–60 cohort) and from almost zero to 8.9 percent (61–65 cohort) due to the reform. Our results have withstood various robustness checks. Moreover, we do not find any health improving effects for post-retirement ages. After the reform, construction workers seem to suffer from more intense health problems and chronic diseases many years after retirement. The empirical results on post-retirement health are, to some extent, in line with the research by Kuhn et al. (2018). They find that offering early retirement by extending unemployment insurance (UI) benefits for older workers raises mortality for men, as the probability of dying before age 73 increases by 1.85 percentage points. Thus, early retirement can have a negative effect on post-retirement health.

We are mainly interested in pre-retirement health. Our results are in line with some previous literature. Bertoni et al. (2018) find that increasing the retirement age affects the pre-retirement health-related behavior of individuals positively. The Swiss reform lowers the retirement age and shows symmetric results in the opposite direction. Thus, due to the lower working horizon, the observed health measures are affected negatively.

There are several mechanisms that could potentially explain why a lower retirement age affects sickness absences and self-reported health problems. On the one hand, there could be an effect on actual health. Human capital theory predicts that a lower working horizon reduces the net present value of investments in older workers, and hence, actual health. For example, firms may invest less in safety or older workers may decrease their health-promoting behavior and be less cautious, running the risk of having more accidents. On the other hand, the lower working horizon can affect claimed health through absenteeism driven by moral hazard. In this case, workers are absent from work even though they are not sick. Moreover, they might be tempted to justify their absences by indicating health problems. Taken together, we cannot fully identify the weight of these different factors and observe only a net effect of the different mechanisms. However, all of them result in social costs. While sickness absences are costly for firms, health problems are expensive for individuals and health care systems. In section 6.2, we provide some evidence that actual health at least partially drives our results.

The insights provided are relevant for all countries struggling with population aging and pension reforms that increase retirement age. Exempting certain groups from such higher retirement ages will remain an important issue. Nevertheless, as this paper shows, the intended benefits of doing so may be outweighed by the costs. While the promotion of early retirement in the construction sector is intended to improve the health of construction workers, we find the contrary to be true. Before workers reach the new retirement age, their sickness absences and self-reported health problems increase. Thus, at the age of 60, they have a lower stock of health capital than their peers before the reform.

CRediT authorship contribution statement

Ann Barbara Bauer: Methodology, Software, Validation, Formal analysis, Writing - original draft, Writing - review & editing, Visualization. **Reiner Eichenberger:** Conceptualization, Writing - review & editing, Supervision.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A

Tables A1–A7, Fig. A1

Appendix B. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jeoa.2020.100296.

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