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THE CAUSAL EFFECT OF WORKLOAD ON THE LABOUR SUPPLY OF OLDER EMPLOYEES

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The causal effect of workload on the labour supply of older employees

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ABSTRACT

Several policies aim to keep older workers in the labour force, but little is known about their effects. We investigate the effects of a particular programme in Norway that reduces the workload of teachers at age 55 but maintains the same wage. Evaluation of this programme is well suited to a difference-in-difference analysis, where the control group is teachers slightly too young to be eligible for the workload reduction. Using full population register data for the period 2005–2013, we analyse the effects of the programme on sickness absence, contracted hours, mental health, and musculoskeletal problems. We find that the programme reduces sickness absence and mental health problems for men, but not for women, and there is no effect on contracted hours. The results are robust to a number of checks.

1. Introduction

Achieving increased labour market activity of older individuals has been high on the political agenda for several years, with the Organization for Economic Co-operation and Development (OECD) noting that population ageing is one of the most important challenges facing its members (OECD, 2006). For instance, in Europe, the ratio of older inactive persons per worker could rise to almost one by 2050. While the OECD notes that even though immigration may help offset the negative effects of population ageing, it remains important to improve the employment prospects of older workers. This involves improving employability as well as policies for retaining employed workers in the labour force.

Often undesired labour market exits, such as disability pension entries, are the result of a long-lasting process, typically preceded by long-term sickness absence or the transition to part-time employment. Thus, measures to prevent long-term sickness absence are potentially important, and the aim of this paper is to analyse an example of such a preventive measure. In addition, policies targeting employees in specific firms or industries may also be more (cost) effective than those offered to all elderly persons in the working population, as they can better accommodate the preferences of both specific employers and employees. The current analysis exploits a rare opportunity to investigate the causal effect of such a preventive initiative, being the reduction in workload given to teachers in Norway at the age of 55, i.e., seven years before they have the option of early retirement.

This initiative is implemented at the industry level, it is quantifiable, and standardized. An important feature is that workers receive full compensation in terms of income with the change in workload. This means we can separate the workload effect from the income effect. The outcomes considered are certified sickness absence and contracted hours. To our knowledge, this is the first analysis of the causal effect of workload on the labour supply of senior employees. In order to identify the effect, we apply a difference-in-difference (DD) approach, which exploits the fact that teachers only a year or less apart in age are treated very differently in the same school year.

We find that among men, sickness absence fell because of the workload reduction. The mean number of sick days per month and the number of sick notes per year declined considerably, by 18% and 16%, respectively, compared with their pre-treatment levels. This effect appears strongest among teachers in compulsory schools. We also identify a reduction in the number of GP visits for mental health problems among men. However, among women, we do not find any effect on the studied outcomes.

This paper proceeds as follows. Section 2 outlines some related literature. Section 3 provides some institutional background on the Norwegian labour market in general and the education sector in particular. Section 4 presents our empirical strategy and Section 5 the requisite data. Section 6 details our results and provides some robustness checks. Section 7 concludes.

2. Related literature

Health is of course an important determinant of labour supply (see, for instance, McGarry, 2004; Kalwij and Vermeulen, 2008; Trevisan and Zantomio, 2016), although institutional arrangements also matter and are highly policy relevant. In recent years, there is a growing literature on the effect of reforms intended to incentivize the labour market participation of the elderly, including the reform of disability schemes, examples being the Netherlands (Koning and Lindeboom, 2015) and the UK (Banks, Blundell, and Emmerson, 2015).[1] Many countries have implemented pension reform (OECD, 2015; Woodland, 2016), including Norway, where a 2011 reform has led to a large increase in the employment of older workers in the private sector (Hernæs *et al.*, 2016). In Norway, public sector and some private sector employees have also had the option of a favourable contractual early retirement scheme (the "Avtalefestet pensjon" or "AFP"), which has been extended over time. Since 1998, this scheme, which allows for part-time work, has entitled workers to an early retirement pension from the age of 62. In Norway and elsewhere, the labour supply of older employees has been shown to be very responsive to the option of early retirement (Blundell, Meghir, and Smith, 2004; Bratberg, Holmås, and Thøgersen, 2004; Autor and Duggan, 2006; Vestad, 2013).

In contrast, we know much less about the impact of retention measures directed at senior employees before they reach the early retirement age. According to Conen, Henkens, and Schippers (2012), such measures include training, workload reduction, extra leave, and the reduction of working hours. Those authors also report the attitudes among employers towards such measures in surveys of eight European countries, and Hermansen and Midtsundstad (2015) do likewise for Norway. Unfortunately, it is difficult to obtain an overview of the efficacy of these actions as some interventions are not quantifiable (for instance, a change in work tasks), and initiatives are often implemented only at the firm level and typically not evaluated (Hermansen and Midtsundstad, 2015). Regardless, the evaluation of such case studies is inherently difficult because of problems with endogeneity.

[1] In 2015, i.e., after our study period, a reform of the disability pension was implemented in Norway, intended to facilitate a combination of social security receipt and work.

Somewhat related to our analysis, a small strand of this literature addresses the effects of a reduction in weekly (and annual) statutory working hours. See Sánchez (2013) for an overview. An interesting case is a French reform reducing the standard working week from 39 to 35 hours, with full income compensation and subsidies to employers. Large firms adopted the decrease in 2000 and smaller firms two years later, thereby constituting a comparison group. On this basis, Estevao and Sa (2008) conclude that aggregate employment was unaffected but labour turnover increased as firms shed workers who had now become more costly. Similarly, Chemin and Wasmer (2009) exploit geographical disparities in a triple-DD approach and likewise fail to reject the null hypothesis of no effect on employment.[2] Nonetheless, the health impacts of the French reform appear to be generally favourable, as measured by survey data on smoking, body mass index, and self-reported health (Berniell and Bietenbeck, 2017).

Elsewhere, Lee and Lee (2016) exploit a quasi-natural experiment in South Korea, where standard hours fell at different times, depending on industry and the size of the establishment. They find that a one-hour reduction in weekly working hours decreases the injury rate by about 8%. Hummels, Munch, and Xiang (2017) take a different approach when investigating the effect of working life conditions on health. Using Danish registry data, they studied the effect of exogenous shocks to within-firm labour demand, and find that when firm exports rise for exogenous reasons, workers work longer hours and take fewer sick leave days, but have higher rates of injury, while women experience a higher risk of hospitalization due to heart attack or stroke. Regardless, none of the reforms or labour demand changes discussed target older employees, and the responsiveness of senior workers are not central to any of the analyses.

3. Institutional background

Norwegian labour market

Labour force participation among seniors in Norway is high, with the average effective labour force exit ages of 64.2 years for men and 64.3 years for women being about six months longer than the OECD (2013) mean. However, the share of older workers on disability benefits is also high, accounting for 19.6% of those aged 55–59 years and 30.5% of those aged 60–64 years in 2012 (OECD, 2013). This is because, unlike many countries where long-term unemployment precedes early retirement, in Norway the early retirement pathway is typically via health-related benefits, i.e.,

[2] Other analyses focusing on employment and/or wages include Raposo and van Ours (2010) for Portugal and Sánchez (2013) for Chile.

long-term sickness absence followed by disability benefits (OECD, 2013). Therefore, a reduction in sickness absence is the subject of much political attention.

<INSERT FIGURE 1 HERE>

Figure 1 depicts the employment rates by age at the population level for three single birth cohorts born in 1951, 1955, and 1959 (we select these because they broadly correspond to our sample birth cohorts 1951–1959). The definition of employment is any wage income. The general picture is that employment falls with age among workers aged 50–60 years, the exceptions being men born in 1951 (or 1955) who actually increased their employment rate at age 56 (or 52). This corresponds to a period when the Norwegian economy experienced a boom in 2007–2008. As also shown, while the employment rate is quite similar for men and women at age 50, it falls more rapidly with age for women than for men, and this holds for all cohorts.

At the national level, the primary policy measures to retain older workers in the workforce are the following. First, a 2011 pension reform provides workers with incentives to stay at work longer. It primarily affects private sector workers and cohorts born after 1953, while public sector workers born 1954 and later will be gradually affected because their pension will be subject to new indexation rules and life-expectancy adjustments (OECD, 2013). Second, the Tripartite Agreement on a More Inclusive Working Life ("IA Agreement"), which was launched in 2001 and has been extended several times since, is intended to promote initiatives at the company level. It is voluntary for employers to sign the agreement, and the majority of labour market programmes are available to employers whether they sign or not. Lastly, by law, workers aged 60 years and above are entitled to one week's extra holiday, irrespective of the sector or industry.

In Norway, sickness insurance is mandatory and regulated by law, covering all employees who have been with the same employer for at least two weeks, with sickness coverage of 100% from the first day. A medical certificate is required for spells of absence of more than three days or eight days, depending on whether the employer has signed the IA Agreement or not. The first 16 days of absence are paid by the employer (the employer period), whereas the remaining period is paid by social insurance. The maximum period of benefits is one year, including the employer period. The level of sickness absence is high, particularly among women (Markussen *et al.*, 2011).

The school system in Norway before higher education is organized in three levels: primary school (pupils aged 6–12 years), lower secondary (13–15 years), and upper secondary (16–19/20 depending on academic or vocational track) education. Compulsory schooling (ages 6–15) is the responsibility of the 428 Norwegian municipalities, whereas upper secondary schools are the responsibility of the

19 counties, with the education sector very much dominated by public schools. Senior policy towards teachers is essentially the same as that towards other employees of municipalities and counties, with the exception of workload regulations, as explained below.

Education sector labour policy

This analysis investigates a negotiated reduction in lessons taught introduced in 2006 and targeted at teachers aged 55 years, such that the 1951 birth cohort was the first to be affected by the intervention. It is explicit in the agreement between teachers' unions and the employers' association that the initiative should result in a workload reduction. Although the local employer (school) might assign the teacher "other pedagogical tasks", for instance mentoring younger colleagues, the reduction in hours taught should not be outweighed by an increase in other tasks and duties.[3] Therefore, *workload* is defined as the regulated number of weekly lessons for a given subject and school type. The teacher should have the option to use the extra time to prepare teaching, correct pupils' homework, etc., while salaries remain unchanged. All teachers of the relevant age have the same *percentage* reduction in workload, including both union and non-union members, and irrespective of the subject taught and school level. The empirical strategy section provides more detail on the initiative.

For decades, the number of lessons that a teacher was to teach during a school week has been regulated in nationwide agreements between teachers' union(s) (since 2001, the Union of Education Norway) and their counterpart (since 2004, the Norwegian Association of Local and Regional Authorities). The agreement covers teachers in primary, lower secondary, and upper secondary schools. The number of lessons taught weekly differs according to subject and school type, and notably, according to the teacher's age, which is what we exploit in this analysis.[4]

The first senior policy initiative for teachers saw its introduction in 1994. In 2002 and 2006, this expanded to include new age groups, but its purpose and implementation remained largely unchanged (Norwegian Association of Local and Regional Authorities, 2008).[5] Initially, the workload reduction was for teachers aged 60, then aged 58, and finally, aged 55.[6] The potential for investigating teachers' workload reduction at age 60 is limited, because at that age, employees in general are also entitled to one week's extra holiday. The workload reduction specific for teachers at age 58 lasted only four

[3] The statement of this principle is in a circular from the employers' association (Norwegian Association of Local and Regional Authorities, 2006).
[4] The workload reductions are as follows: teachers in primary schools teach 1.5 fewer lessons per week irrespective of subject. In lower (upper) secondary schools, teachers of Norwegian, which is considered the most laborious subject, teach 1.2 (1.0) fewer lessons per week, and teachers of other subjects obtain a somewhat larger reduction..
[5] The agreement is found in "special agreements", referred to as SFS 2220 and SFS 2213, for the period 01.05.2004–30.04.2006 and 01.05.2006–30.04.2014, respectively. For the period before 2004, see "F-4073 – tillegg nr. 3 – vedlegg 4".
[6]In 2014, a reduction in teaching load was introduced for teachers in their first teaching job.

years and data on health care use that we apply in some analyses are not available. Therefore, this analysis focuses on the workload reduction at age 55.

In contrast to workload adjustments in individual workplaces, the workload reduction at age 55 is quantifiable and standard across the public education sector. It is also easy to communicate and there is full income compensation; hence, there is every reason to consider that the uptake would be high. In principle, a teacher may turn down the option of reduced workload if he/she prefers teaching the same number of lessons as before rather than having additional "pedagogical tasks". However, the wage income is the same whether the option is accepted or not (for a given number of contracted hours). For those not employed full time, the reduction in workload could make it more attractive to accept a larger position (with more contracted hours). Of course, working overtime as a locum is also an option, but we cannot observe this in the data.

The workload reduction can affect sickness absence in several ways. The first mechanism is via increased leisure. We compare employees who are very close in age and so their productivity should be very similar, such that a reduction in lessons taught corresponds to an increase in leisure (ignoring the time possibly used for other pedagogical tasks).[7] This extra leisure time could be invested in health-enhancing activity.[8] Second, a less stressful work situation should in itself be health improving. Third, workers may interpret the workload reduction as an acknowledgement of greater job demands, with workers who consider that they are treated well (given a workload reduction) responding with having fewer absences or shorter spells of sickness absence (Fehr and Gächter, 2000).[9] Lastly, because absenteeism imposes a burden on their colleagues, workers who enjoy the option of a workload reduction may feel a particular obligation to reduce their absence through sickness if possible.

4. Empirical strategy

The design of the workload reduction agreement makes it well suited for empirical evaluation. The Norwegian school year starts in mid-August and ends in late June. At the school level, a teaching plan is made for the full school year before teaching begins, allocating teachers to classes and courses. This planning considers whether a teacher is entitled to the age-contingent workload reduction in the

[7] If considered over a long age span, presumably lower productivity could outweigh the time saving older workers obtain through a workload reduction, so that hours worked are the same as those of their younger colleagues.
[8] In general, there is a trade-off between leisure (time available for health-enhancing activities) and the consumption of health care goods, which are both elements of the health production function (Grossman, 1972). In our setting, income (consumption) is unaffected by the increase in leisure.
[9] Following the same logic, their colleagues who have to wait another year to receive the same workload reduction could respond by increasing their sickness absence, in which case the effect would be underestimated.

current *calendar* year. Thus, in a given *school* year, teachers whose 55th birthday is before 31 December will receive the 5.8% reduction, whereas their colleagues, who turn 55 during the school year but after 31 December, will not. Having been born in the first rather than the second part of the year therefore acts as a natural experiment available for a DD strategy. By design, the estimated effects of this experiment will relate to the first year of treatment because in later years, both the treatment and the control groups will be eligible for the workload reduction.

To keep the notation simple, let t denote the school year that begins in t (i.e., $t = 2006$ refers to the school year 2006–2007). For school year t, the treatment group includes teachers who turn 55 before New Year's Eve (for $t = 2006$, those who are born July–December 1951). The control group consists of teachers whose 55th birthday is in school year t, but after 31 December (those who are born January–June 1952 for $t = 2006$).

Let Y_{it} denote the outcome of teacher i, who turns 55 in school year t. The focus in the analysis is on sickness absence and sick notes, but we also provide results for specific diagnoses and the number of contracted working hours. For individual i, Y_{it} may be compared to the outcome in t–1. In standard notation, we let the dummy variable *Treat* indicate the treatment group, *After* = 1 for the treatment year, and *After* = 0 for the year before the treatment year, i.e., the school year when the teacher turns 54. We obtain the DD estimate of the effect of reduced workload as δ in

$$Y_{itm} = \beta_0 + \beta_1 Treat_i + \beta_2 After_t + \delta(Treat_i \times After_t) + \gamma_m + \gamma_t + u_{it},$$

where we have included month of birth and school year fixed effects. Each individual is observed in two school years, t and t–1 in our notation. Previous research has shown that labour market outcomes tend to vary by birth month, both in terms of "social age" and "biological age". See Larsen and Solli (2017) for earnings. We control for such variation in two ways: the month-of-birth indicators pick up variation within a birth cohort, and the DD design takes care of variation between birth cohorts in the mean pre-treatment age.

It is also important to control for school year effects because there could be general trends in sickness absence rates and other outcomes unrelated to the natural experiment (e.g., Askildsen, Bratberg, and Nilsen, 2005). We estimate the model using OLS with standard errors clustered at the individual level. Teaching in compulsory schools (1st to 10th grades) differs in many respects from teaching at the upper secondary level; we therefore include sub-analyses by school level. We also consider potential heterogeneity using the distinction in full- and part-time employment. We also estimate the outcomes separately for men and women.

In DD, the identifying assumption is that the treatment and control groups share a common trend: in the absence of treatment, the treatment group outcome would be on the same trend as the control group. We perform two robustness checks for this assumption. First, a placebo test where the placebo treatment is at age 54. Second, an extended timeframe where we include two additional pre-treatment years; thus, we can obtain an impression of whether the common trend assumption holds before treatment. We also perform sensitivity analysis to see if particular cohorts or individuals with very long sickness absences drive the results. Finally, we report triple differences (DDD) for the main outcomes. In this, we compare teachers to other public sector employees not offered the workload reduction in a joint regression of all public employees that meet the aforementioned cohort criteria. In the regressions, this means interacting all variables in the above empirical specification with a teacher dummy. The effect, δ, is then the coefficient on the teacher dummy interacted with $(Treat_i \times After_t)$.

We restrict the analysis to teachers born in July–December (treated) and January–July (controls), that is, only teachers who are 54 when the previous school year begins and turn 55 during the current school year. Thus, we obtain a sample where the treated and the controls are quite close in age. Furthermore, it would complicate interpretation if the same individual could act as a control in t and be treated in t+1.

5. Data

Sources

The key data source is the FD-Trygd database, which links administrative information from the Norwegian Labour and Welfare Administration (NAV) and Statistics Norway. This database covers all Norwegians from 1992 onwards, and provides information on gender, and month and year of birth, along with detailed information on certified sick leave covered by social insurance (start and stop dates), disability, work history (date of job entry and exit, sector, industry, occupation, contracted hours), and the level and type of education, etc.

We can merge this dataset with other administrative datasets by means of the personal identifier. The KUHR register, administered by the Norwegian Health Economics Administration, holds information on all invoices sent by GPs for remuneration, i.e., for each patient contact (consultation), whether at the GP's regular office or at an emergency centre. At each consultation, the GP registers a diagnosis according to the ICPC-2 classification. For each consultation, we also know whether a sickness certificate has been issued.

We generate our two main outcome variables, the mean number of sick days per month averaged over the school year, and the number of sick notes per year, from the NAV and KUHR data, respectively. While the variable *sickdays* depends on the remunerations to employers from National Insurance for episodes that exceed the first 16 days, *sick notes* are from GP certificates, and represent the number of certificates issued per year without regard to the length of sick leave. From the KUHR data we define two variables reflecting the number of GP consultations per year from mental health or musculoskeletal problems (all diagnoses within ICPC-2 Chapters P or L), respectively. These two groups of diagnoses are the most common in certified sickness absence. *Contracted hours* originate from the FD-Trygd data. This variable shows how many hours per week the employee has contracted, which may diverge from the actual hours worked, for instance, because of sickness absence or overtime work.

Sample selection

For each school year starting 2006–2013, we identify a group of teachers who are affected by the workload reduction and another group that is slightly too young to be eligible (individuals in these two groups are born within a 6+6 months' "window" around New Year's Eve). The population of teachers is defined based on information about industry, sector, education, and (if possible) occupation.[10] As the workload reduction draws on specific employer–union agreements, the sample is restricted to teachers in public schools, i.e., schools owned and operated by either municipalities or counties.

The sample of analysis consists of monthly observations of teachers employed in August at the start of the school year. The criteria outlined above yield a sample of 18,352 individuals, comprising a treatment group of 8,581 individuals born 1951–1958 and a control group of 9,771 individuals born 1952–1959. We use the whole sample in the analysis of sick days and contracted hours. As we observe sick notes and the corresponding diagnoses for one school year less in our dataset,[11] the sample used for these outcomes is somewhat smaller (in total, 15,579 individuals) and includes birth cohorts 1952–1958 in the treatment group and birth cohorts 1953–1959 in the control group.

[10] Within the relevant industries (compulsory schooling, upper secondary, academic track and upper secondary, vocational track), we restrict the sample to individuals whose level of education is compatible with teaching. In this respect, a report written on behalf of the Norwegian Directorate for Education and Training has been helpful (Sjaastad *et al.*, 2016).

[11] Our sick leave data do not cover the whole (pre-treatment) school year 2005/2006.

Descriptive statistics

<INSERT TABLE 1 HERE>

Table 1 provides summary statistics at the beginning of the school year before treatment. Except for month of birth, the treatment and control groups are similar with respect to marital status, nationality, education, school type, and labour supply. For men, sickness absence (sick days per month and sick notes per year) is higher in the treatment group than in the control group, and they have more GP consultations because of mental health problems than in the control group. There are no such differences for women. We note that pre-treatment, women have more sickness absences than do men. There are also relatively more part-time workers among women. In the empirical analysis, where we estimate the outcomes separately for men and women, our research design considers any pre-treatment differences in sickness absence between the treatment and control groups.

6. Results

<INSERT TABLE 2 HERE>

Table 2 presents the main results, being the DD estimates for our two measures of sickness absence. For both outcomes, we find that the workload reduction has a negative and statistically significant effect for men, but no effect for women. The treatment effect on sick days for men is also large, corresponding to –0.3 days reduction per month or three days for a school year of ten months. This is an 18% reduction from the average pre-treatment level for men in the treatment group, which is 1.60. The effect on the number of sick notes is also large, with a reduction of –0.11, amounting to a 16% reduction from the average of 0.72.

<INSERT TABLE 3 HERE>

We would expect a potentially positive health effect to manifest itself at the primary care level. Table 3 reports the results for the number of visits with psychological and musculoskeletal diagnoses, the most frequent illnesses identified in certified sickness absence. The results indicate that the number of GP visits due to mental health problems declined in the male treatment group. At the same time, an additional regression (not reported here) showed that the total number of GP visits is not affected.

<INSERT TABLE 4 HERE>

The workload reduction in itself does not change contracted hours for the treatment group. Instead, it introduces a new standard for a full-time position in terms of lessons taught. Only if the teacher changes his or her labour supply relative to this new standard is there a change in contracted hours. We therefore expect a zero or positive effect on contracted hours, as it might be possible to offset the reduction in workload by increasing contracted hours. However, we find no such effect using the results in Table 4.

<INSERT TABLE 5 HERE>

Teaching responsibilities differ by school type, and it may affect the implementation of the workload reduction including, for instance, whether the school assigns the teacher other pedagogical tasks and the nature of these tasks. Table 5 panel A therefore provides the results by school type. It appears that the effect is strongest for male teachers working in compulsory school, although the effect is not quite significant when we split by school level. As the reduced workload is a percentage, the magnitude is smaller for part-time employees who therefore receive a smaller "dose" of treatment. We have also split the sample into full- and part-time workers, but the results reported in Table 5 panel B exhibit no clear pattern.

Robustness checks

<INSERT TABLE 6 HERE>

The results thus far indicate a negative treatment effect on male teachers' sickness absence. However, this finding may be spurious if the assumption of a common trend is violated. As one check of the identifying assumption, we perform a placebo test. Here, "treatment" is in the school year when teachers turn 54. Given that, in reality, there is no treatment at that age, significant coefficients on the placebo treatment would indicate that the identifying assumption does not hold. Table 6 details the placebo results. As shown, none of the coefficients for sick days or sick notes is statistically significant, and some of the point estimates are positive. This could mean that teachers strategically increase contracted hours at age 54 in anticipation of the workload reduction, but the placebo results for contracted hours do not indicate this. We conclude that the placebo tests provide no evidence calling into question the main results.

<INSERT FIGURES 2A AND 2B HERE>

<INSERT TABLE 7 HERE>

We also checked the common trend assumption by including data on two additional years prior to treatment. Figure 2 plots the average absence days per month from $t = -2$ to $t = 1$ (the treatment year). For men, the pre-treatment averages appear not to parallel (decreasing from $t = -2$ to $t = -1$) the control group, instead increasing for the treatment group. However, the confidence intervals overlap and we consider that this graph does not provide evidence against the common trend assumption. For women, the pre-treatment trends are closer. We have also estimated the DD effect using the extended data, finding results that are consistent with those found previously, as reported in Table 7.[12]

The main results draw on eight treatment cohorts with corresponding control cohorts. To examine whether the results are driven by particular cohorts, we reproduce the trends in Figure 2a, dropping one cohort at a time. The resulting graphs, presented in Figures 1a–1d in the Appendix, do not lead us to suspect that a particular cohort drives the results. We also plotted similar graphs to those in Figure 2 by school type. The results in Figure 2 in the Appendix provide no clear indication of heterogeneity in the pre-treatment trends.

<INSERT FIGURES 3A AND 3B HERE>

It could be the case that teachers with very long absences mainly drive our estimate of the average effect on sickness absence. To obtain an impression of how the effect is distributed over absence spells of different lengths, we estimate a series of linear probability models (LPMs) where the outcomes are dummy variables indicating mean absences of ≥1, 5, 10, 15, 20, and 25 months. If long absences mainly drove the treatment effect, we would expect the effect in the LPMs to increase as the cut-off for *y* increases. Figure 3a shows that this is not the case for men: the effect is largest for Pr(days ≥ 5), but then decreases and becomes insignificant for Pr(days ≥ 10), etc. On the other hand, the effect on Pr(days ≥ 1) is also smaller and insignificant, indicating that the treatment effect is less clear for short absences. For women, where DD did not identify any treatment effect for mean absence, the LPMs confirm that this is true for the full distribution, see Figure 3b.

We have found that a reduction in workload reduces sickness absence (both the number of days and the number of sick notes) for male teachers, but not for female teachers. The question arises whether the change in trend that we observe for male teachers at age 55 is only for teachers or whether we can observe a similar development for men outside of the educational sector, in which case the change cannot be attributed to the workload reduction. To investigate this, we apply a triple DD approach (DDD), where the identifying assumption is that in the absence of treatment, the difference in Y between the treatment and control groups from one school year to the next should be the same for teachers and the comparison group of non-teachers. The comparison group meets two criteria: they

[12] The data series on sick notes is not sufficiently long to undertake the same exercise.

have the same type of employer as teachers (municipalities or counties) and they have completed at least some higher education.[13] The same cohorts are included as in the teacher sample.

<INSERT TABLE 8 HERE>

Table 8 details the DDD estimates for the effects on several outcomes. These results strengthen our belief that the DD estimates for male teachers on sickness absence and mental health problems are actually an effect of the workload reduction. The estimated effects are indeedstronger in DDD than in DD, the reduction in sick days per month (measured during the school year, August–June) being –0.5 sick days, i.e., similar to the estimate in Table 7 but larger than the estimate of –0.3 in Table 2. We find the number of sick notes to be reduced by 0.16, while the estimate reported in Table 2 is –0.11, and the DDD estimate for the number of GP visits due to mental health problems is of a larger magnitude and statistically more significant than the DD estimate.

7. Discussion and conclusion

The contributions of this analysis are twofold. First, we investigate a research question that is important but seldom analysed, namely the effect of policy initiatives aimed at older workers, in this case, an age-dependent reduction in workload. Second, to estimate the causal impact of workload on the labour supply of older employees, we utilize a setting where workload varies by employee's month of birth, which is exogenous with respect to both the employer and employees. The estimated effect is an intention-to-treat effect, as we cannot observe whether the individual employee makes use of the option.

Our main result is that the workload reduction causes a decrease in sickness absence among males, and there are indications that this is from less psychological strain. There is no effect on contracted hours for either gender. We support these results with a placebo test, and we do not find any indication that very long spells or individual year-of-birth cohorts drive the effect. The results from a triple difference-in-difference estimation support the interpretation that the workload reduction causes the estimated effect for men.

Differences in job characteristics do not easily explain the difference in effect estimates between men and women. We compare employees within the same occupation and perform separate estimations by gender for school type and full- versus part-time workers. Our finding that male and female employees react differently to institutional changes is then in line with previous research. Not only

[13] The comparison group consists of local public sector workers, including nurses, social workers, engineers, administrative staff, etc.

are women's sickness absence levels higher than men's (see, for instance, Markussen *et al*., 2011), but more importantly, women also appear less responsive to workplace changes. Elsewhere, these include negative organizational shocks/change in job security (Ichino and Riphahn, 2005; Bratberg and Monstad, 2015), their absenteeism reacts differently to social security reform (Johansson and Palme, 2005), and their labour supply responses to a negative health shock are different from those of men (Trevisan and Zantomio, 2016).

In general, this gender difference could arise from differences in options or preferences, possibly shaped by gender roles. A related interpretation could be that the selection out of the labour market is stronger among women, so that women remaining employed at age 53–55 have particularly good health or have a very strong work motivation. At the population level, we observe that employment rates fall more sharply among women than men during pre-treatment years. However, we do not observe the health and motivation of the sample participants fully and so cannot conclude that selection explains the gender difference observed in our results.

Some caveats are in place. We study employees at ages 53–55 and use a narrow time window to trace the causal effect, which relates to the first ten months after the start of treatment. Because of the nature of the intervention, we cannot conclude anything with respect to later labour market outcomes such as retirement age and the uptake of the disability pension. Despite the fact that the reduction in workload studied is modest and affects only part of the work duties of a teacher (those directly related to classroom teaching), we identify a large and significant effect among men. Several potential mechanisms could cause the observed decrease in sickness absence, and the analysis cannot distinguish between them. Nonetheless, it is noteworthy that men given less workload appear to have improved their health in the sense that consultations due to mental health problems are less frequent with sick notes issued less often.

Overall, our results suggest that preventive measures taken to retain older workers can indeed have an effect on the labour supply. However, there is a need for caution as such measures make older workers more costly and thus may yield them a competitive disadvantage in the labour market.

TABLES AND FIGURES

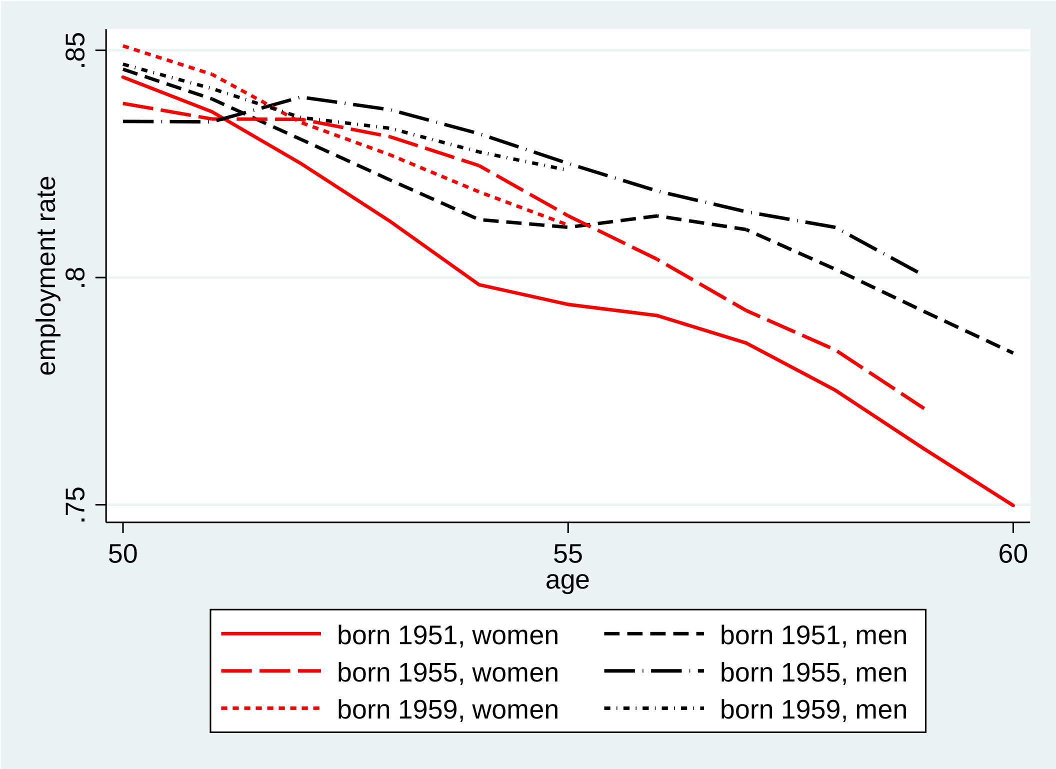

Figure 1. Employment rates by age and gender for selected cohorts, at the population level. Observational period is 2001–2013. Source: Authors' calculations.

Figure 2a. Men: Trends before treatment (treatment year = 1)

Figure 2b. Women: Trends before treatment (treatment year = 1)

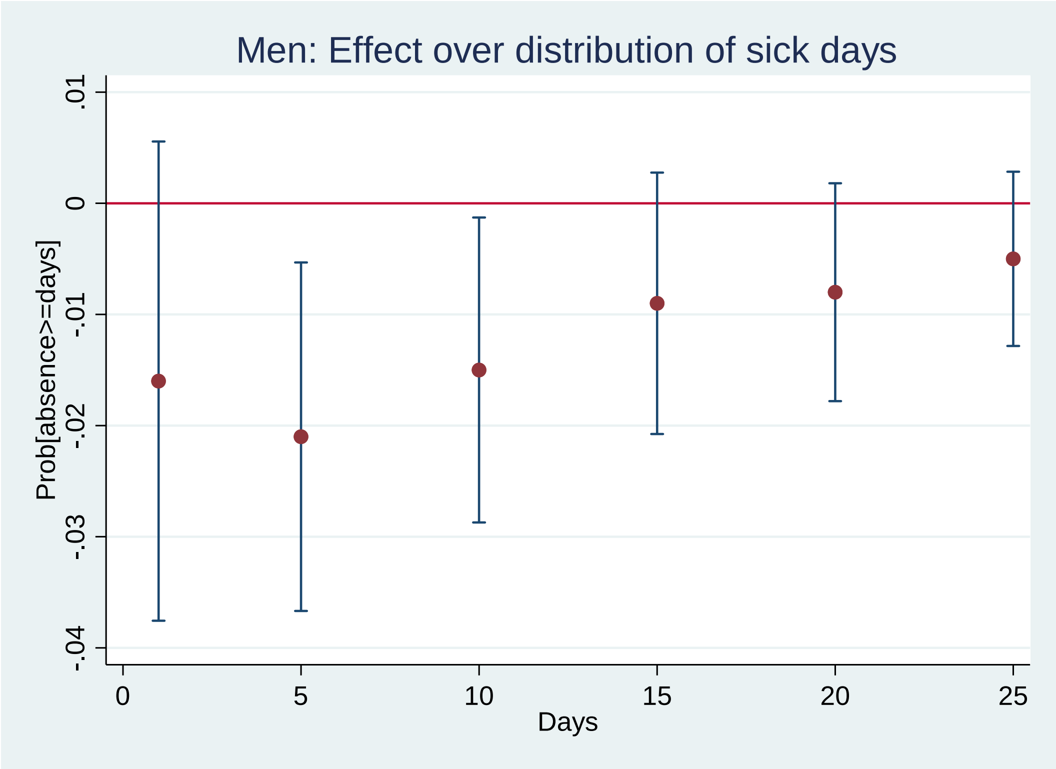

Figure 3a. Men: Effect over distribution of sick days.

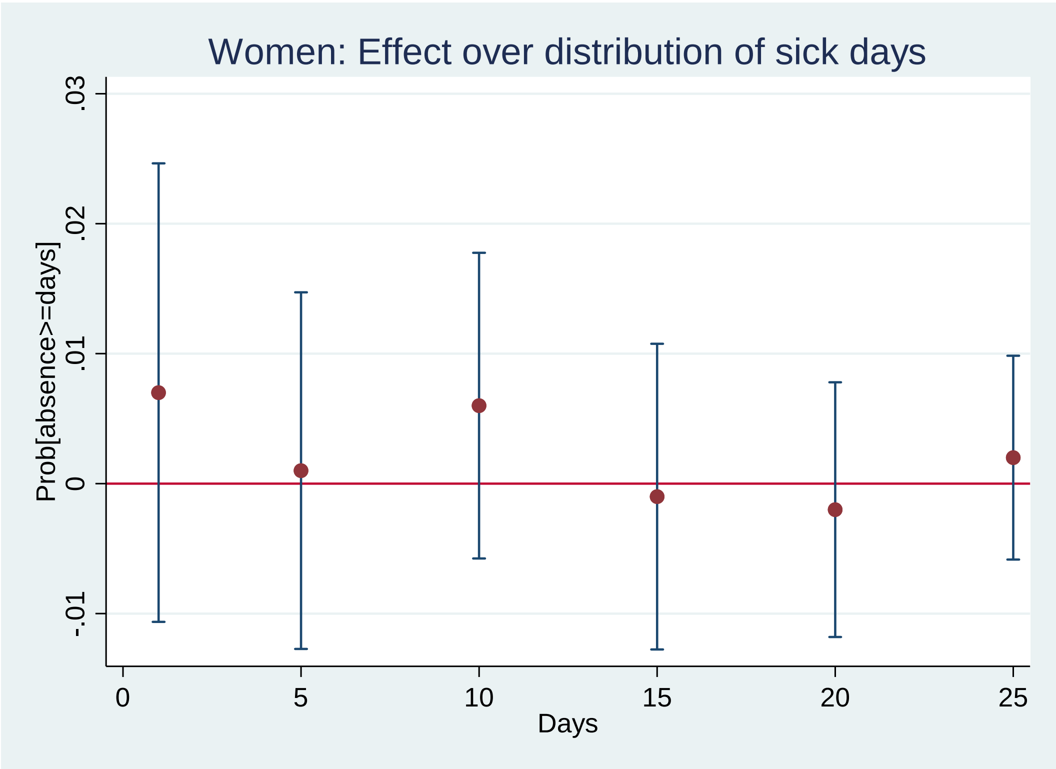

Figure 3b. Women: Effect over distribution of sick days.

Table 1. Summary statistics at baseline

	All		Men		Women	
	Control	Treated	Control	Treated	Control	Treated
Female	0.67	0.66				
	(0.471)	(0.475)				
Month of birth	3.57	9.42	3.56	9.44	3.57	9.41
	(1.676)	(1.723)	(1.662)	(1.734)	(1.682)	(1.716)
Norwegian born	0.95	0.95	0.95	0.95	0.95	0.95
	(0.214)	(0.218)	(0.220)	(0.207)	(0.210)	(0.223)
Married	0.48	0.49	0.51	0.51	0.47	0.47
	(0.500)	(0.500)	(0.500)	(0.500)	(0.499)	(0.499)
Education level						
Upper secondary	0.01	0.01	0.03	0.03	0.00	0.00
	(0.106)	(0.117)	(0.160)	(0.176)	(0.0642)	(0.0678)
Bachelor	0.86	0.86	0.79	0.79	0.90	0.89
	(0.343)	(0.348)	(0.411)	(0.405)	(0.296)	(0.309)
Master/higher	0.12	0.13	0.19	0.17	0.09	0.10
	(0.330)	(0.333)	(0.391)	(0.380)	(0.290)	(0.303)
School type						
Compulsory	0.69	0.68	0.50	0.51	0.78	0.77
	(0.463)	(0.467)	(0.500)	(0.500)	(0.412)	(0.422)
Upper secondary	0.31	0.32	0.50	0.49	0.22	0.23
	(0.463)	(0.467)	(0.500)	(0.500)	(0.412)	(0.422)
Labour supply						
Full time	0.78	0.78	0.85	0.84	0.75	0.75
	(0.413)	(0.413)	(0.359)	(0.366)	(0.433)	(0.432)
Part time	0.22	0.22	0.15	0.16	0.25	0.25
	(0.413)	(0.413)	(0.359)	(0.366)	(0.433)	(0.432)
Outcomes						
Sick days/month	1.85	1.95	1.24	1.60	2.15	2.14
	(5.181)	(5.428)	(4.231)	(5.026)	(5.569)	(5.619)
Hours/week	33.64	33.74	34.93	34.81	33.01	33.17
	(7.074)	(7.080)	(6.574)	(6.797)	(7.226)	(7.161)
N	*9771*	*8581*	*3242*	*2953*	*6529*	*5628*
Sick notes/year	0.90	0.93	0.61	0.72	1.04	1.03
	(2.061)	(2.098)	(1.689)	(1.914)	(2.201)	(2.177)
P-diagnoses/year	0.58	0.65	0.41	0.58	0.66	0.69
	(2.171)	(2.479)	(1.875)	(2.671)	(2.293)	(2.375)
L-diagnoses/year	1.05	1.07	0.80	0.80	1.17	1.21
	(2.517)	(2.537)	(2.116)	(2.265)	(2.677)	(2.653)
N	*8270*	*7309*	*2650*	*2449*	*5620*	*4860*

Standard deviations in parentheses

Table 2. Effect on mean sick days per month and sick notes per year. DD estimates

	Sick days			Sick notes		
	All	Men	Women	All	Men	Women
Effect	–0.062	–0.292*	0.060	–0.017	–0.116*	0.033
	(0.63)	(1.97)	(0.47)	(0.44)	(2.09)	(0.67)
N	*36704*	*12390*	*24314*	*33931*	*11294*	*22637*

t-statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. OLS with standard errors clustered at the individual level.
Sick days: mean of monthly sickness absence per year. Sick notes: Number of sickness certificates issued by GP per year. Controlled by school year and month of birth. Sample used in estimation of sick days: Treatment cohorts: 1951–1958. Control cohorts: 1952–1959. School years 2005/2006 (first pre-treatment year)–2013/2014 (last post-treatment year). See Table 3 regarding sample used in estimation of sick notes.

Table 3. Effect on GP visits due to mental health or musculoskeletal problems. DD estimates

	GP visits, mental health			GP visits, musculoskeletal		
	All	Men	Women	All	Men	Women
Effect	–0.041	–0.140*	0.009	–0.018	–0.029	–0.012
	(1.10)	(2.25)	(0.19)	(0.43)	(0.46)	(0.21)
N	*33931*	*11294*	*22637*	*33931*	*11294*	*22637*

t-statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. OLS with standard errors clustered at the individual level.
GP visits, mental health; and GP visits, musculoskeletal: Number of GP visits per year registered with a code within ICPC-2 Chapter P (Psychological) and Chapter L (Musculoskeletal), respectively. Controlled by school year and month of birth. Sample used (as in estimation of sick notes): Treatment cohorts: 1952–1958; Control cohorts: 1953–1959. School years 2006/2007 (first pre-treatment year)–2013/2014 (last post-treatment year).

Table 4. Effect on contracted hours per week. DD estimates

	All	Men	Women
Effect	0.035	0.085	0.013
	(0.69)	(0.98)	(0.21)
N	*36704*	*12390*	*24314*

t-statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. OLS with standard errors clustered at the individual level.
Dependent variable is contracted hours per week. Controlled by school year and month of birth. Treatment cohorts: 1951–1958. Control cohorts: 1952–1959. School years 2005/2006 (first pre-treatment year)–2013/2014 (last post-treatment year.

Table 5. Effect on mean of monthly sick days, different subsamples. DD estimates

A. By school type	All	Men	Women
Compulsory school	–0.086	–0.374	0.006
	(0.71)	(1.72)	(0.04)
N	*25100*	*6226*	*18874*
Upper secondary	–0.012	–0.226	0.233
	(0.07)	(1.11)	(0.83)
N	*11604*	*6164*	*5440*
B. By contracted hours	All	Men	Women
Full time	–0.062	–0.183	0.013
	(0.58)	(1.17)	(0.09)
N	*28753*	*10472*	*18281*
Part time	0.217	–0.193	0.343
	(0.85)	(0.42)	(1.14)
N	*7951*	*1918*	*6033*

t-statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. OLS with standard errors clustered at the individual level.
Dependent variable is mean of monthly sickness absence per year. Full time worker: ≥ 30 hrs/contracted hours in August of baseline year. Controlled by school year and month of birth. Treatment cohorts: 1951–1958; Control cohorts 1952–1959. School years 2005/2006 (first pre-treatment year)–2013/2014 (last post-treatment year).

Table 6. Placebo treatment. DD estimates

	Sick days			Sick notes			Contracted hours		
	All	Men	Women	All	Men	Women	All	Men	Women
Effect	0.084	0.199	0.019	–0.016	0.058	–0.051	0.080	0.101	0.072
	(0.87)	(1.37)	(0.15)	(0.38)	(0.94)	(0.95)	(1.54)	(1.06)	(1.15)
N	*36882*	*12458*	*24424*	*28549*	*9226*	*19323*	*36882*	*12458*	*24424*

t-statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. OLS with standard errors clustered at the individual level.
Placebo treatment at age 54.

Table 7. Effect on mean sick days per month using three pre-treatment years

	All	Men	Women
Effect	–0.123	–0.46*	0.055
	(0.91)	(2.29)	(0.31)
N	*67712*	*22988*	*44724*

t-statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. OLS with standard errors clustered at the individual level.
Sick days: mean of monthly sickness absence per year. Controlled by school year and month of birth.

Table 8. Effect of workload reduction, different outcomes. DDD estimates

Outcomes:	All	Men	Women
Mean sick days per month	–0.086	–0.496**	0.099
	(0.67)	(2.59)	(0.60)
Contracted hours	0.102	0.135	0.086
	(1.53)	(1.11)	(1.08)
N	*89716*	*26678*	*63038*
Sick notes	–0.009	–0.165*	0.062
	(0.18)	(2.19)	(0.96)
P-diagnoses	0.014	–0.195*	0.105
	(0.29)	(2.48)	(1.72)
L-diagnoses	–0.001	–0.028	0.011
	(0.02)	(0.32)	(0.15)
N	*84224*	*24751*	*59473*

t-statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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APPENDIX

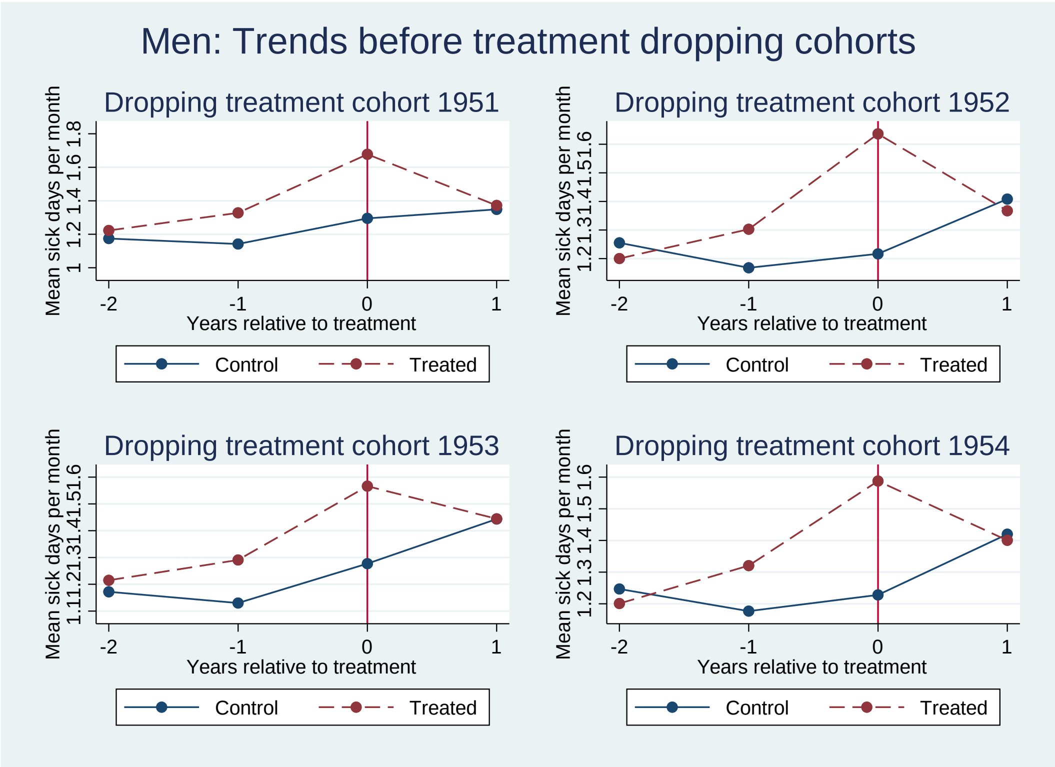

Figure 1a. Men: Trends before treatment, dropping cohorts 1951–54

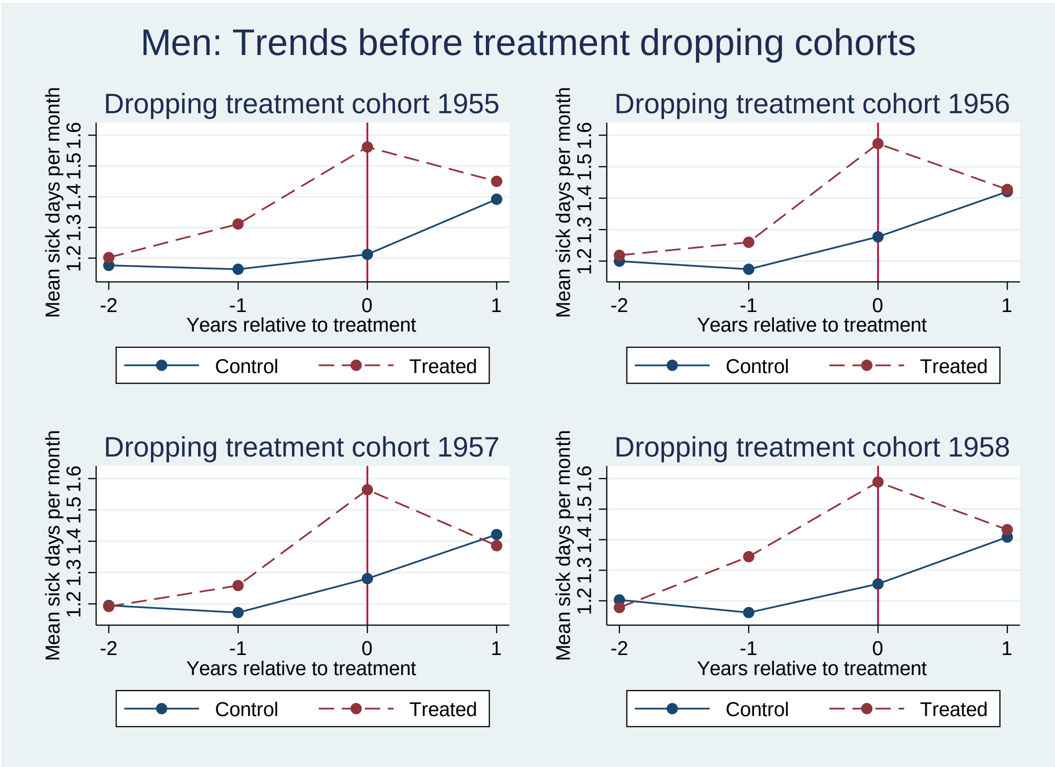

Figure 1b. Men: Trends before treatment, dropping cohorts 1955–58

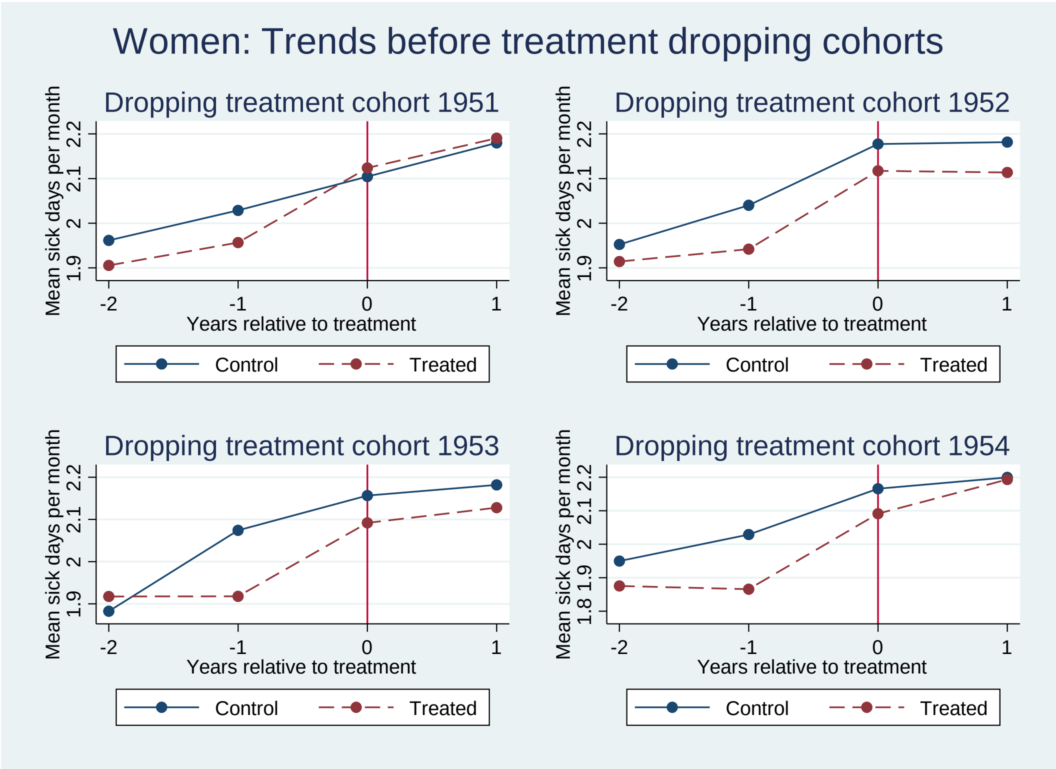

Figure 1c. Women: Trends before treatment, dropping cohorts 1951–54

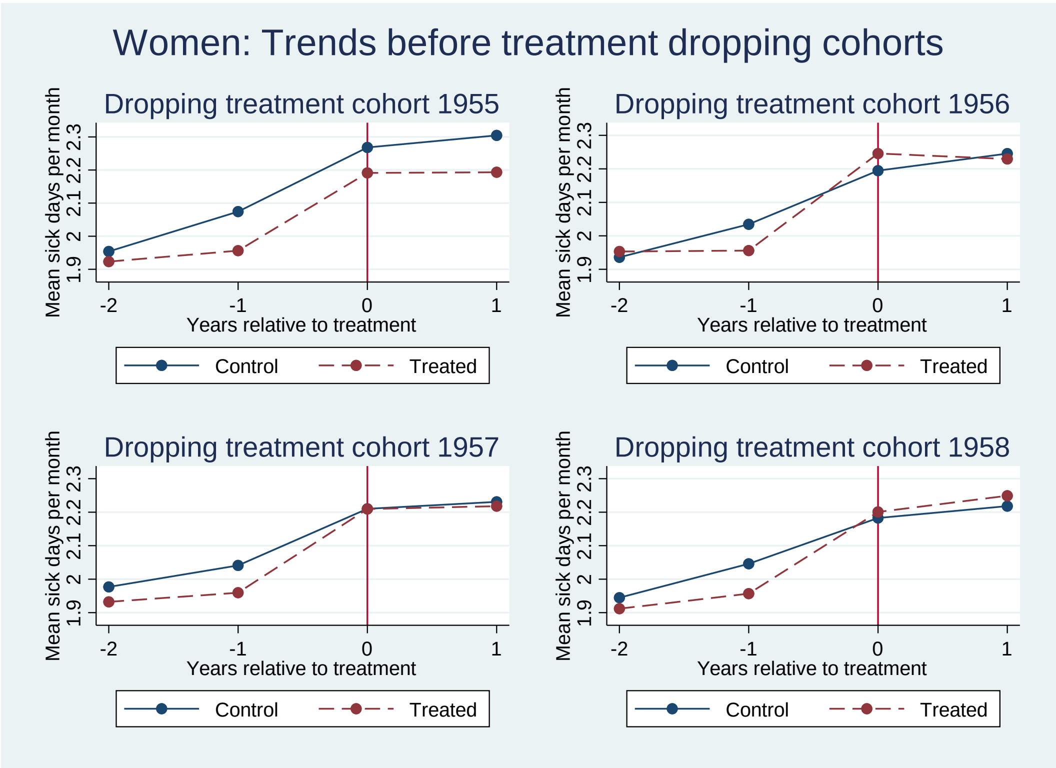

Figure 1d. Women: Trends before treatment, dropping cohorts 1955–58

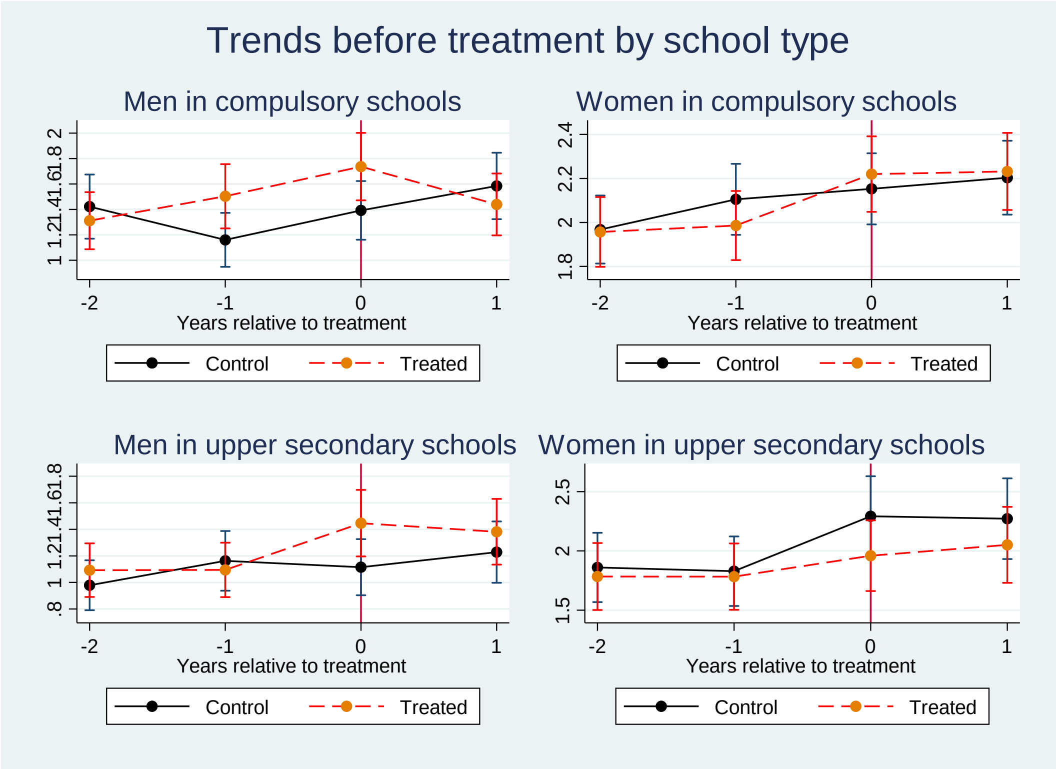

Figure 2. Mean sick days per month. Trends before treatment by school type.

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