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The causal effect of workload on labour supply of older employees

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ABSTRACT

Several policies are implemented to keep older workers in the labour force, but little is known about their effects. We investigate the effects of a particular program: at the age of 55, Norwegian teachers' workload is reduced while wages remain the same. The program is well suited for a difference-in-difference analysis, where the control group is teachers who are slightly too young to be eligible for the workload reduction. Using full population register data for the years 2005-2013, we analyze the effects on sickness absence, contracted hours, mental health and musculoskeletal problems. We find that the program 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.

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1. Introduction.

Increased labour market activity of older individuals has been high on the political agenda for several years. The Organization for Economic Co-operation and Development (OECD) notes that population ageing is one of the most important challenges facing its member countries (OECD 2006). In Europe, the ratio of older inactive persons per worker could rise to almost one in 2050. OECD notes that even though immigration may help offset 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. Undesired labour market exits, such as disability pension entries, are often the result of a long-lasting process, and are typically preceded by long-term sickness absence or a transition to part-time job. Thus, measures to prevent long term sickness absence are potentially important, and the aim of this paper is to analyse such a preventive measure. Policies targeting employees in specific firms or industries may be more (cost) effective than policies offered to all elderly in the working population, since they can be accommodated to the preferences of employers and employees. This analysis exploits a rare opportunity to investigate the causal effect of a preventive initiative; namely 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 income is fully compensated, so that the effect of workload can be separated from the effect of income. The outcomes studied 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-differences (DD) approach, which exploits the fact that teachers who are a year or less apart are treated differently in the same school year.

We find that, among men, sickness absence fell as a result of the workload reduction. The mean number of sickdays per month and number of sick notes per year was reduced considerably, by 18 % and 16 % respectively, compared to pre-treatment levels. The effect appears to be strongest among teachers in compulsory schools. We also find a reduction in the number of GP visits for mental health problems among men. Among women, we do not find any effect on the outcomes studied.

This paper proceeds as follows. We first sketch some related literature in section 2. In section 3, we give institutional background on the Norwegian labour market in general and on the educational sector. Our empirical strategy is presented in section 4, and the data in section 5. We report our results and robustness checks in section 6. Section 7 concludes the paper.

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 matter as well, and are highly policy-relevant. In recent years, there is a growing literature on the effect of reforms intended to incentivize labour market participation of elderly. Disability schemes have been reformed, examples are the Netherlands (Koning and Lindeboom, 2015) and UK (Banks, Blundell, and Emmerson, 2015). [1] Many countries have implemented pension reform (OECD 2015; Woodland 2016), including Norway, where the 2011 reform has lead to a large rise in employment of elder workers in the private sector (Hernæs *et al.*, 2016). In Norway, public sector and some private sector employees have had the option of a favourable contractual early retirement scheme (AFP), which has been extended over time. Since 1998, this scheme, which allows for part-time work, has entitled them to early retirement pension from the age of 62. Labour supply of elder employees has been shown to be very responsive to the option of early retirement (Blundell, Meghir, and Smith, 2004; Bratberg et al., 2004; Autor and Duggan, 2006; Vestad, 2013).

In contrast, very little is known about the impact of retention measures directed at senior employees before they reach the early retirement age. Such measures include training, workload reduction, extra leave or reduction of working hours (Conen, Henkens, and Schippers, 2012). The attitudes among employers towards such measures are reported in surveys for eight European countries in Conen *et al*. (2012) and for Norway in Hermansen and Midtsundstad (2015). It is difficult to get an overview of the efficacy of these actions; some interventions are not quantifiable (for instance, change of work tasks), and initiatives are often implemented at the firm level, and typically not evaluated (Hermansen and Midtsundstad, 2015). Evaluation of such case studies is inherently difficult because of endogeneity issues.

A small strand of literature, somewhat related to our analysis, deals with the effect of a reduction of weekly (and annual) statutory working hours, see Sánchez (2013) for an overview. An interesting case is the French reform where the standard workweek was reduced 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, therefore constituting a comparison group. Estevao and Sa (2008) conclude that aggregate employment was unaffected but labour turnover increased, as firms shed workers who became more expensive. Chemin and Wasmer (2009) exploit geographical disparities in a triple difference-in- difference approach, and they as well fail to reject the null-hypothesis of no

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

effect on employment.[2] The health impacts of the French reform appear to be favorable, as measured by survey data on smoking, body mass index, and self-reported health (Berniell and Bietenbeck, 2017). Lee and Lee (2016) exploit a quasi-natural experiment in South Korea, where standard hours were reduced at different times, depending on industry and establishment size. 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 study the effect of exogenous shocks to within-firm labor demand, and find that when firm exports rise for exogenous reasons, workers work longer hours, take fewer sick-leave days but have higher rates of injury, and women experience higher risk of hospitalization due to heart attack or stroke.

The reforms or labour demand changes mentioned above are not targeted at older employees, and the responsiveness of senior workers are not central to the analyses.

3. Institutional background

Labour force participation among seniors is high in Norway. The average effective labour force exit ages is 64.2 for men and 64.3 for women, about half-a-year higher than the OECD mean (OECD; 2013). However, the share of older people on disability benefit is high; in 2012 it was 19.6% of those aged 55-59, and 30.5% of those aged 60-64 (ibid.). Whereas in many countries, early retirement is preceded by long-term unemployment, in Norway the pathway is typically via health-related benefits, i.e., long-term sickness absence followed by disability benefit (OECD, 2013). Therefore, reduction of sickness absence is given much political attention.

(figure 1 here)

Figure 1 shows employment rates by age at the population level for three single cohorts, born1951, 1955 or 1959. (We select these cohorts because our sample of analysis originates from cohorts born 1951-1959, as explained below). Employment is defined as having any wage income. The general picture is that employment falls with age among workers aged 50-60. The exceptions are men born 1951 (or 1955) who actually increase their employment rate at ages 56 (or 52), when the economy experienced a boom, in 2007-2008. 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.

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

At the national level, main policy measures to retain older workers are the following. First, the pension reform of 2011 gives workers 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 as their pension will be subject to new indexation rules and life-expectancy adjustment (OECD, 2013). Second, The Tripartite Agreement on a More Inclusive Working Life ("IA-agreement"), which was launched 2001 and has been extended several times since then, is intended to promote initiatives at the company level. It is voluntary for employers to sign the agreement, and the majority of labour market programs are available to employers whether they sign it or not. Third, by law, workers aged 60 and above are entitled to one week extra holiday, irrespective of sector or industry.

Norwegian 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 below higher education is organized in three levels: primary schools (pupils aged 6-12), lower secondary (age 13-15), and upper secondary education (age 16-19/20 depending on academic or vocational track). Compulsory schooling (age 6-15) is the responsibility of the 428 municipalities whereas upper secondary schools are the responsibility of 19 counties, and the educational sector is highly dominated by public schools. Senior policy towards teachers is essentially the same as towards other employees of municipalities and counties, except workload regulations, as explained below.

The policy initiative studied

This analysis investigates a negotiated reduction in lessons taught introduced in 2006 and targeted at teachers at age 55, so that the 1951 birth cohort was the first to be affected by the intervention. It was explicitly stated in the agreement between teachers' unions and 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 young colleagues, the reduction in hours taught should not be outweighed by an increase in other tasks and duties.[3] Therefore, *workload* is

[3] This principle is stated in a circular from the employers' association (The Norwegian Association of Local and Regional Authorities, 2006).

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' home-work, etc., while salaries remained unchanged. All teachers of the relevant age are given the same *percentage* reduction in workload, union members and non-union members alike, and irrespective of subject taught and school level. The empirical strategy section provides more detail on the initiative.

For decades, the number of lessons that a teacher is to teach during a school week has been regulated in nation-wide 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 comprises teachers in all three school levels; primary schools, lower secondary schools, and upper secondary schools. The number of lessons taught weekly differs according to subject and school type, and notably, also according to teacher's age, which is what we exploit in this analysis.[4]

The first senior policy initiative for teachers was introduced in 1994. In 2002 and 2006, the initiative was extended to comprise new age groups, but its purpose or how it was implemented remained unchanged (The Norwegian Association of Local and Regional Authorities, 2008).[5] First, the workload reduction was given to teachers aged 60, then at age 58, and finally at age 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 extra holiday. The workload reduction specific for teachers at age 58 lasted only four years and data on health care use that we apply in some analyses is not available. Therefore, this analysis focuses on the workload reduction at age 55.

In contrast to workload adjustments at individual workplaces, the workload reduction at age 55 is quantifiable and is standard in the public education sector. It is easy to communicate and income is fully compensated, hence there is every reason to consider the up-take to 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 to possibly having more "pedagogical tasks". However, wage income is the same whether the option is accepted or not (for a given number of contracted hours). For those who are not full-time employed, the reduction in workload could make it more attractive to accept a larger position

[4] The workload reduction can be exemplified 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.

[5] The agreement is found in "special agreements" called SFS 2220 and SFS 2213 for the periods 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]Furthermore, in 2014, a reduction in teaching load was introduced to teachers in their first job as teachers.

(with more contracted hours). Of course, working overtime as a locum is also an option, but this cannot be observed in the data.

The workload reduction can impact on sickness absence in several ways. The first mechanism goes via increased leisure. We compare employees who are very close in age and their productivity should be very similar, so that a reduction in lessons taught corresponds to an increase in leisure (ignoring the time possibly used for "other pedagogical tasks")[7]. This this extra time can be used for health-enhancing activities.[8] Second, a less stressful work situation should in itself be health-improving. Third, workers may interpret the workload reduction as an acknowledgement of high job demands. Workers who experience that they are treated well (given a workload reduction) may respond by having fewer absences or shorter sickness absence spells (Fehr and Gächter, 2000)[9]. Lastly, since absenteeism imposes a burden on their colleagues, workers who enjoy the option of a workload reduction may feel a particular obligation to reduce sickness absence, 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 school level a teaching plan is made for the full school year before teaching begins, allocating teachers to classes and courses. When planning, it is taken into account whether a teacher is entitled to the age-contingent workload reduction in the current *calendar* year. Thus, in a given *school* year, teachers whose 55th birthday is before December 31th will get the 5.8% reduction, whereas their colleagues, who turn 55 during the school year but after December 31th, will not. Having been born in the first rather than the last part of the year therefore acts as a natural experiment, which can be used in a DD strategy. By design, estimated effects of this experiment will relate to the first year of treatment, since in later years, both the treatment and the according control group will be eligible for the workload reduction.

To keep notation simple, let t denote the school year that begins in t (e.g., 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, are born July-December 1951). The control group consists of teachers whose 55th birthday is in school year t, but after December 31 (are born January-June 1952 for t = 2006).

[7] If considered over a long age span, the time saving elder workers get through a workload reduction could be outweighed by their presumably lower productivity, so that hours worked are the same as their younger colleagues.
[8] In general, there is a trade-off between leisure (time available for health-enhancing activities) and consumption of health care goods, which are both elements of the health production function (Grossman, 1972). In the setting we analyse, income (consumption) is not affected by the increase in leisure.
[9] Following the same logic, their colleagues who have to wait another year to get the same workload reduction, could respond by increasing their sickness absence, in which case the effect would be underestimated.

Let Y_{it} denote the outcome of teacher *i*, who turns 55 in school year *t*. The main focus in the analysis is on sickness absence and sick notes, but we also report results for certain diagnoses and for 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 belonging to the treatment group, and *After* = 1 for the treatment year, *After* = 0 for the year before, i.e., the school year when the teachers turn 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 dummy variables for month of birth (m) and school year. 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 month of birth, both in terms of "social age" and "biological age" (see Røed 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 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 that are not related to the natural experiment (e.g., Askildsen et al, 2005). The model is estimated with OLS with standard errors clustered at the individual level. Teaching at compulsory schools (1st to 10th grade) differs in many respects from teaching at the upper secondary level; we therefore include sub-analyses by school level. We also consider potential heterogeneity by the full time/part time distinction. All outcomes are estimated separately for men and women.

In DD, the identifying assumption is that the treatment and control groups share a common trend: in 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 time frame where we include two additional pre-treatment years, thus we can get an impression of whether the common trend assumption holds before treatment. We also perform sensitivity analysis to see if the results are driven by particular cohorts or by individuals with very long sickness absences. Finally, we report triple differences (DDD) for the main outcomes. Teachers are compared to other public employees, who are not offered the workload reduction, in a joint regression of all public employees that meet the afore-mentioned cohort criteria. In the regressions, this means interacting all variables in the empirical specification above with a teacher dummy. The effect, δ, now is 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 fairly close in age. Furthermore, it would complicate interpretation is if the same individual could act as control in *t* and as treated in *t* + 1.

5. Data

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 renders information on gender, month and year of birth, in addition to 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), level and type of education, etc.

This dataset can be merged with other administrative data sets by means of the personal identifier. The KUHR register, administered by the Norwegian Health Economics Administration (HELFO), holds information on all invoices sent by GP for remuneration, i.e., 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.

Our two main outcome variables, mean number of sickdays per month averaged over the school year, and number of sick notes per year, are generated from the NAV data and the KUHR data, respectively. While the variable *sickdays* is based on remunerations to employers from National Insurance for episodes that exceed the first 16 days, *sick notes* is based on GP certificates, and represents the number of certificates issued per year without regard to the length of sick leave. From KUHR data we define two variables reflecting the number of GP consultations per year due to mental health problems 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 FD-Trygd data. This variable shows how many hours per week the employee has contracted for, which may divert from actual hours worked, for instance due to 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 for it (born within a

6+6 months' "window" around New Year's Eve). The population of teachers are defined based on information of industry, sector, education, and (if possible) occupation.[10] Since the workload reduction is based on specific employer-union agreements, the sample is restricted to teachers in public schools, i.e., schools owned by municipalities or counties.

The sample of analysis consists of monthly observations of teachers who are employed in August, at the start of the school year. The criteria outlined above give a sample of 18352 individuals; a treatment group of 8581 individuals born 1951-1958 and a control group of 9771 individuals born 1952-1959. The whole sample is used in the analysis of sickdays and contracted hours. Since we observe sick notes and the according diagnoses one school year less in our data set[11], the sample used for these outcomes is somewhat smaller (in total 15579 individuals) and includes birth cohorts 1952-1958 in the treatment group and birth cohorts 1953-1959 in the control group.

Descriptive statistics

(Table 1 here)

Table 1 shows 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 (sickdays per month and sick notes per year) is higher in the treatment group than in the control group, and they have more GP consultations due to a mental health problem than men in the control group. There are no such differences for women. We note that pre-treatment, women have more sickness absence than men. There are also relatively more part-time workers among women than among men. In the empirical analysis, where outcomes are estimated separately for men and women, our research design will take into account any pre-treatment differences in sickness absence between treatment and control groups.

6. Results.

(Table 2 here)

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[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 does not cover the whole (pre-treatment) school year 2005/2006.

Our main results are reported in table 2, which shows difference-in-difference 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 sickdays for men is large – 0.3 days reduction per month or 3 days for a school year of ten months. This is a 18% reduction from the average pre-treatment level for men in the treatment group, which is 1.60. The effect on number of sick notes is also large. The reduction is - 0.11, amounting to 16% reduction from the average of 0,72.

(Table 3 here)

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

(Table 4 here)

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

(Table 5 here).

Teaching responsibilities differ by school type, and it may differ how the workload reduction is implemented, for instance, in terms of whether the teacher is assigned “other pedagogical tasks”, and what these tasks are. Table 5 therefore shows 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 time and part time workers, but the results show no clear pattern.

Robustness checks

(Table 6 here)

The results this far indicate a negative treatment effect on male teachers' sickness absence. If the common trend assumption is violated, however, this finding may be spurious. As one check of the identifying assumption we have performed a placebo test. Here, 'treatment' is in the school year when teachers turn 54. Since in reality, there is no treatment at that age, significant coefficients on the placebo treatment would indicate that the identifying assumption is broken. Table 6 shows the placebo results. No coefficients on sickdays or sick notes are statistically significant, and some of the point estimates are positive. It could be 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 that. We conclude that the placebo tests provide no evidence against the main results.

(Figures 2a and 2b and Table 7 here)

We have also checked the common trend assumption by including data on two additional years before treatment. Figure 2 graphs average absence days per month from t = -2 to t = 1 (the treatment year). For men, the pre-treatment averages appear not to be parallel (decreasing from t = - 2 to t = - 1 for the control group, increasing for the treatment group). However, the confidence intervals overlap and we think 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]

(Table 7 here).

The main results are based on 8 treatment cohorts with according control cohorts. To examine whether the results are driven by particular cohorts, we have reproduced the trends in Figure 2a, dropping one cohort at a time. The resulting graphs, presented in Appendix figures 1a-1d, do not lead us to suspect that the results are driven by a particular cohort. We also have plotted similar graphs as in Figure 2 by school type. The results in Appendix figure 2 do not give clear indications of heterogeneity in the pre-treatment trends.

(Figures 3a and 3b here)

It could be the case that our estimate of the average effect on sickness absence is driven mainly by teachers with very long absences. To get an impression of how the effect is distributed over absence spells of different lengths, we have estimated a series of linear probability models where the outcomes are dummy variables indicating, respectively, mean monthly absence $\geq$ 1, 5, 10, 15, 20, 25. If the

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

treatment effect is driven mainly by long absences, we would expect the effect in the LPM models to increase as the cut-off for *y* increases. Figure 5a shows that this is not the case for men: the effect is largest for Pr(days ≥ 5), it 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 on mean absence, the LPM models confirm that this is true for the full distribution, see figure 5b.

We have found that a reduction in workload reduces sickness absence (both number of days and 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 specific for teachers or whether a similar development can be seen 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 difference-in-difference approach, where the identifying assumption is that in the absence of treatment, the difference in *Y* between the treatment and control group from one school year to another should be the same for teachers and for the comparison group of non-teachers. The comparison group meets two criteria: they have the same type of employer as teachers (municipalities and counties) and they have completed at least some higher education[13]. As with teachers, in the treatment year they are either 55 years (the treatment group) or 54 years (the control group), and we observe them the year before treatment as well.

(table 8 here)

Table 8 shows the DDD estimates for the effects on several outcomes. It strengthens our belief that the DD estimates for male teachers on sickness absence and mental health problems are really effects of the workload reduction. The estimated effects are actually stronger in DDD than in DD; the reduction in sickdays per month (measured during the school year, August-June) is -0,5 sickdays, i.e., similar to the estimate in table 7 but larger than the estimate of -0,3 reported in table 2. The number of sick notes is found to be reduced by 0,16, while the estimate reported in table 2 is -0,11, and the DDD estimate for number of GP visits due to mental health problems is of larger magnitude and statistically more significant than the DD estimate.

7. Discussion and conclusion

The contributions of this analysis are two-fold. First, it investigates a research question that is important but seldom analysed, namely the effect of senior policy initiatives; in this case an age-dependent reduction of workload. Second, in order to estimate the causal impact of workload on

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

labour supply of elder employees, we utilize a setting where workload varies by employee's month of birth, which is exogenous to employer and employees. The estimated effect is an intention-to-treat effect, since 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 due to less psychological strain. There is no effect on contracted hours for either gender. These results are supported by a placebo test, and we do not find any indication that very long spells or individual year-of-birth cohorts drive the effect. Results from a triple difference-in-difference estimation support the interpretation that the estimated effect for men is caused by the workload reduction.

The difference in effect estimates between men and women cannot be easily explained by differences in job characteristics. We compare employees within the same occupation, and have made separate estimations by gender for school type and full-time versus part-time workers. Our finding that male and female employees react differently to institutional changes is in line with previous research. Not only are women's sickness absence levels higher than men's (see for instance Markussen *et al*., 2011). More importantly, women also appear less responsive to negative organizational shocks/ change in job security (Bratberg and Monstad, 2015; Ichino and Riphahn, 2005), their absenteeism react differently to social security reform (Johansson and Palme, 2005) and their labour supply responses to a negative health shock is different from men's (Trevisan and Zantomio, 2016). In general, this gender difference could be caused by 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 who remain employed at age 53-55, are of particularly good health or strong work motivation. At the population level, we observe that employment rates fall more sharply among women than men during pre-treatment years. However, the health and motivation of the sample participants are not fully observed and we cannot conclude that selection explains the gender difference in our results.

Some caveats are in place. We study employees at ages 53-55 and use a narrow time window to trace a causal effect, which relates to the first ten months after treatment. Because of the nature of the intervention, we cannot conclude with respect to later labour market outcomes such as retirement age and disability pension uptake. Despite the fact that the reduction in workload studied is modest and affects only parts of the work duties of a teacher (those directly related to classroom teaching), we find 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. Still, it is noteworthy that men who are given less workload appear to have improved their health in the sense that consultations due to mental health problem are less frequent and a sick note is issued less often.

Our results indicate that preventive measures taken to retain older workers can indeed have an effect on labour supply. However, they should be implemented with caution, since such measures make older workers more costly and may be a competitive disadvantage in the labour market.

Tables and figures

FIGURES

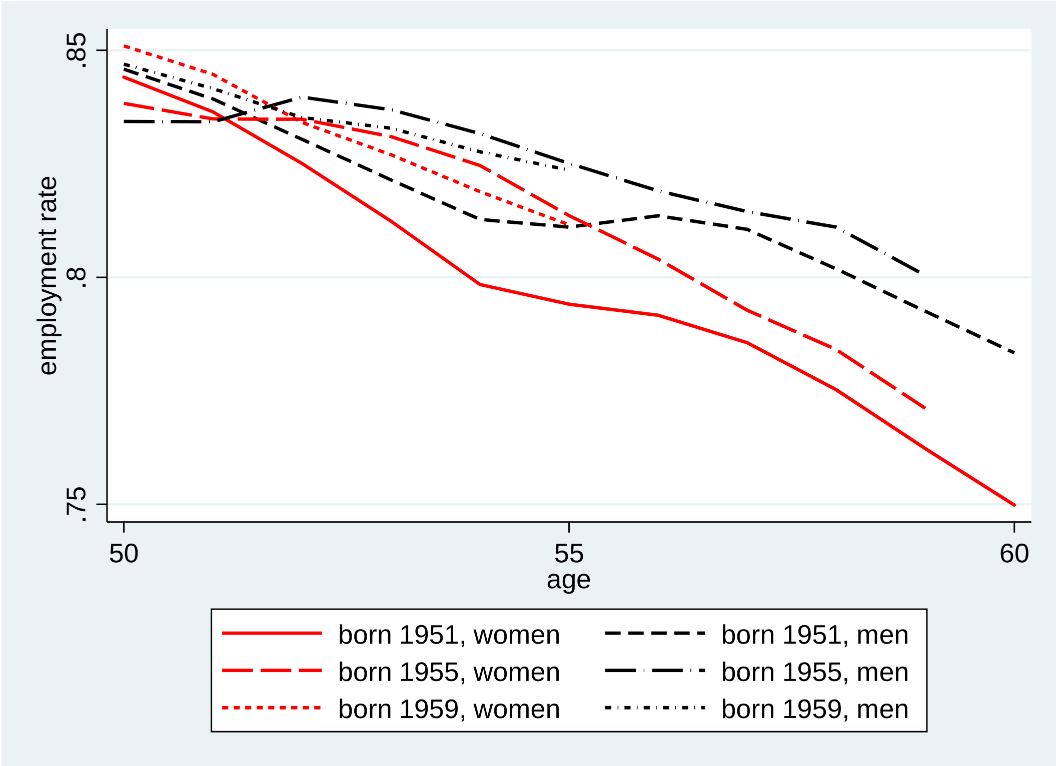

Figure 1. Employment rates by age and gender for selected cohorts, at population level. Observational period is 2001-2013. Source: own calculations.

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

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

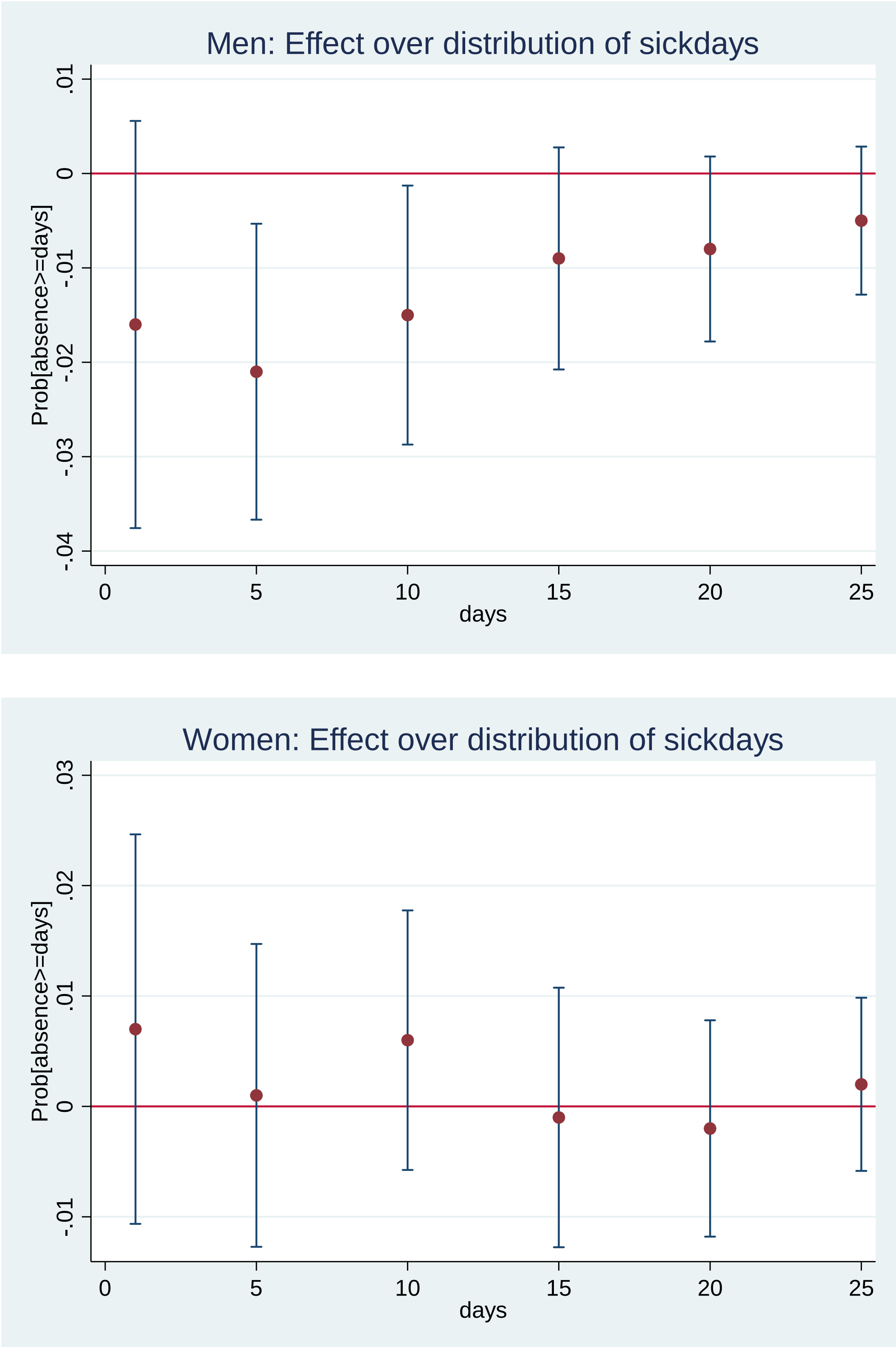

Figure 3. Effects over distribution of sickdays

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						
Sickdays/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 sickdays per month and sick notes per year. Diff in diff estimates

	Sickdays			Sick notes		
	All	Men	Women	All	Men	Women
DID	-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.

Sickdays: mean of monthly sickness absence per year. Sick notes: Number of sickness certificates issued by GP per year. Controlled for school year and month of birth. Sample used in estimation of sickdays: Treatment cohorts: 1951-1958. Control cohorts: 1952-1959. School years 2005/2006 (first pre-treatment year)-2013/2014 (last post-treatment year). Regarding sample used in estimation of sick notes: see table 3.

Table 3. Effect on GP visits due to mental health or musculoskeletal problems. Diff in diff estimates

	GP visits, mental health			GP visits, musculoskeletal		
	All	Men	Women	All	Men	Women
DID	-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 for 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. Diff in diff estimates

	All	Men	Women
DID	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 for 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 sickdays, different subsamples. Diff in diff 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 per august in baseline year. Controlled for 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. Diff in diff estimates

	Sickdays			Sick notes			Contracted hours		
	All	Men	Women	All	Men	Women	All	Men	Women
DID	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 sickdays per month using 3 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.
Sickdays: mean of monthly sickness absence per year. Controlled for school year and month of birth.

Table 8. Effect of workload reduction, different outcomes. Triple difference-in-difference.

Outcomes:	All	Men	Women
Mean sickdays 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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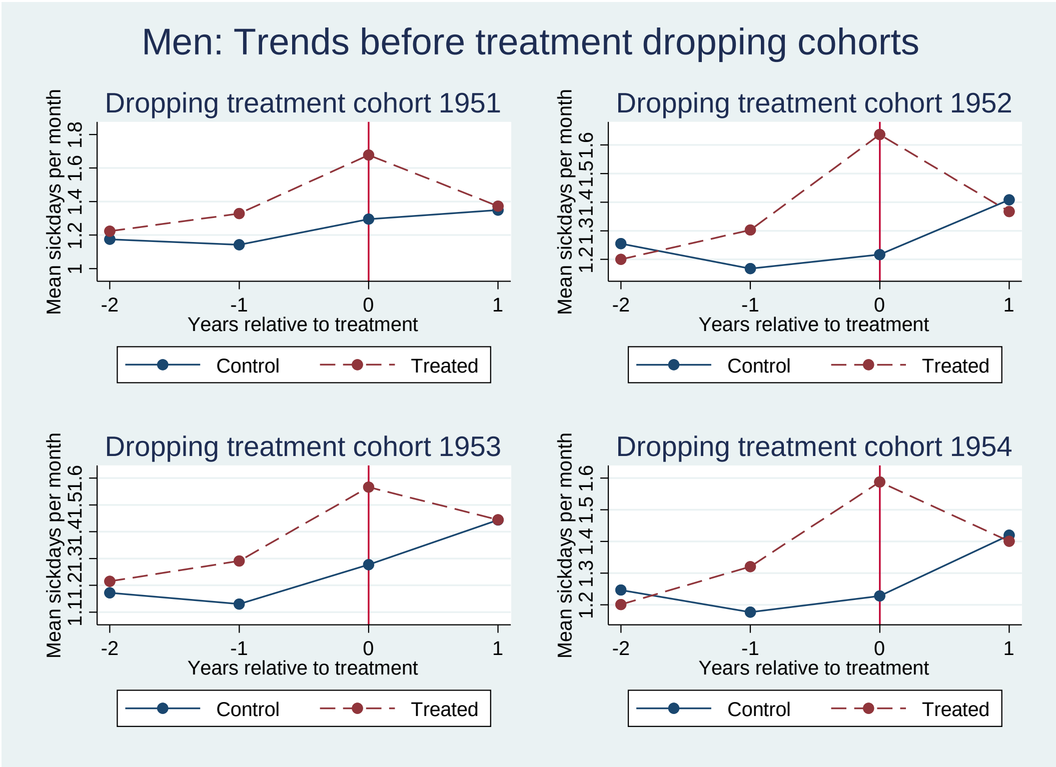
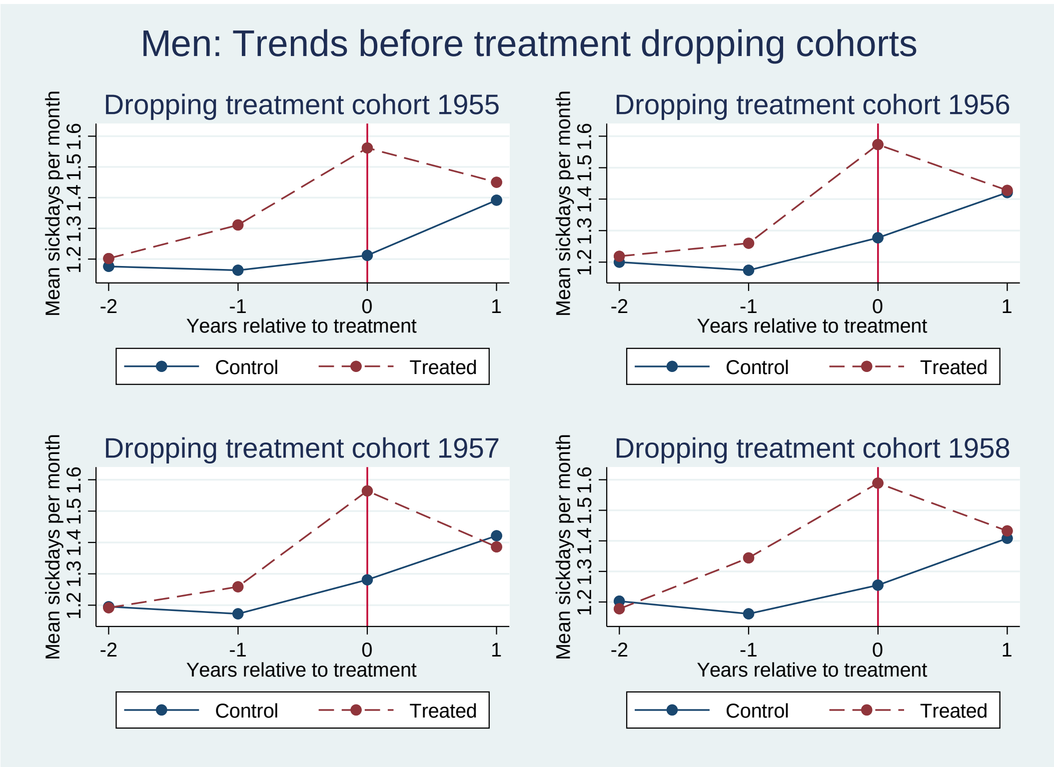
APPENDIX

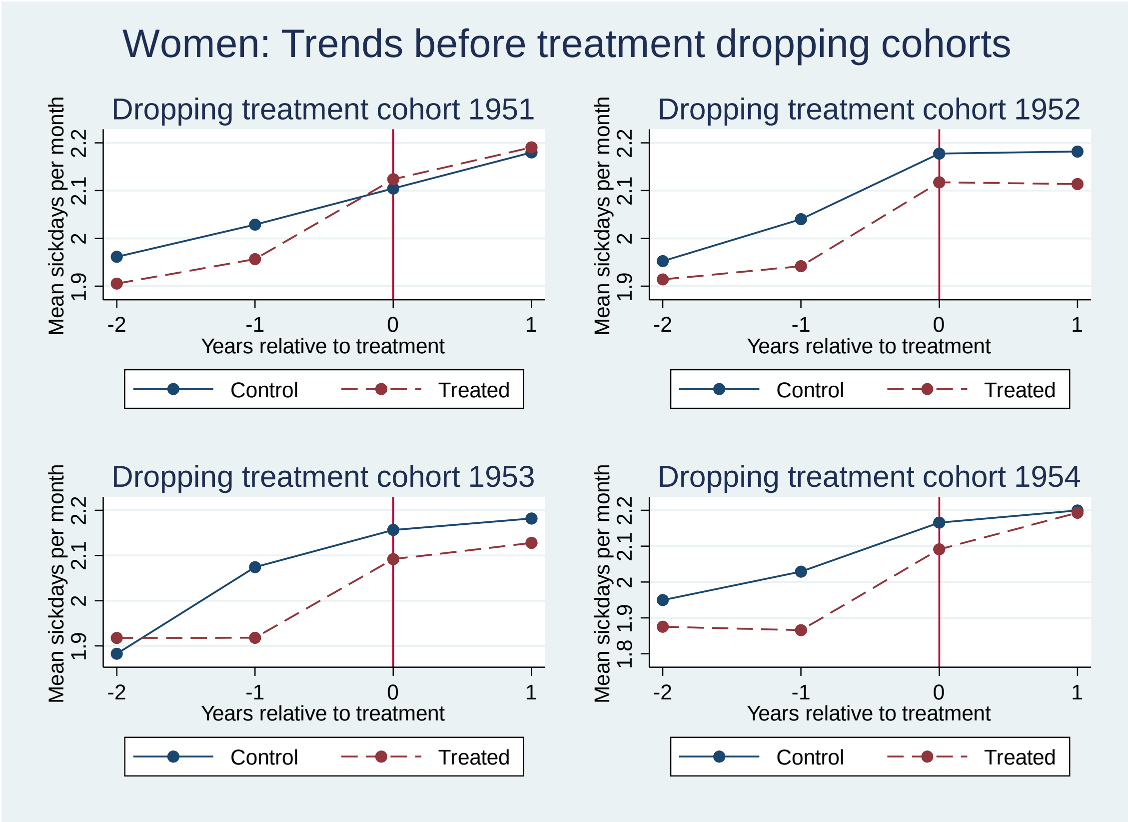
Figures



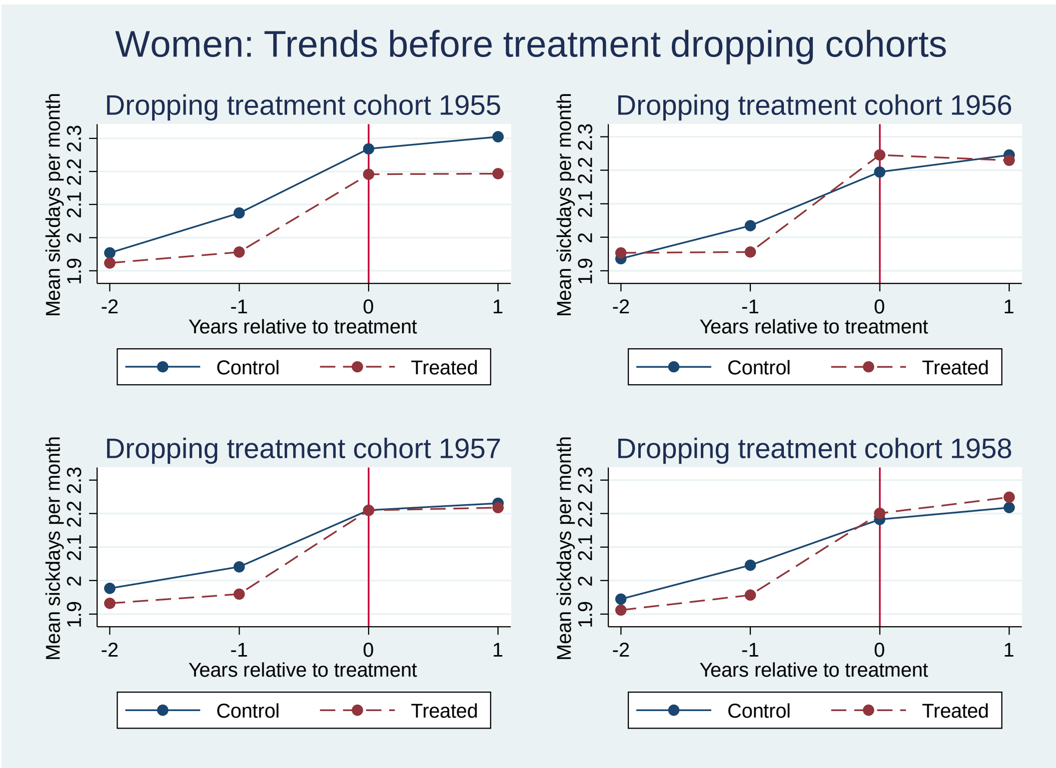
App. figure 1a. Men: Trends before treatment, dropping cohorts 1951-54



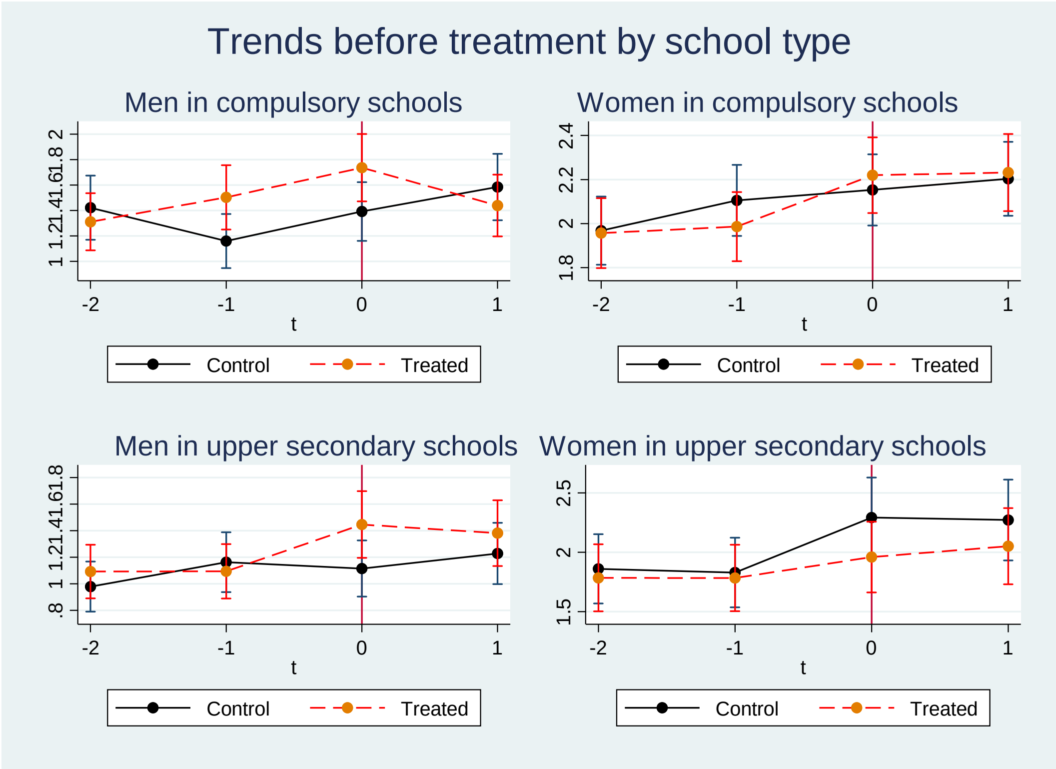
App. figure 1b. Men: Trends before treatment, dropping cohorts 1955-58



App. figure 1c. Women: Trends before treatment, dropping cohorts 1951-54



App. figure 1d. Women: Trends before treatment, dropping cohorts 1955-58



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