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Firm responses to a more generous insurance against high sick pay costs

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Firm responses to a more generous insurance against high sick pay costs[a]

by

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Abstract

This paper presents evidence on how firms react to a more generous insurance against high sick pay costs. We exploit a reform launched in Sweden in 2015, which introduced different thresholds for insurance reimbursement depending on firm size. By comparing workers in smaller firms with workers in large firms over time, we evaluate the effects of the reform. We find no indication of changed behaviour among employees in the smallest firms (on average 15 employees), but an increase in sickness absence among those employed in middle-sized firms (on average 38 employees). The increased absence in middle-sized firms is entirely driven by new hires, but the newly hired employees do not seem to be differently selected. We find no evidence indicating that the more generous insurance made firms more inclined to employ more sick-prone individuals. Further analysis suggests that the absence of behavioural responses among employees in the smallest firms might be related to a large production loss from an absent worker, which the insurance cannot fully compensate for. Taken together, we find no support for any societal benefits of a more generous insurance against high sick pay costs in terms of an increased employment-probability among more sick-prone individuals.

Keywords: sickness absence, sick pay, firm size, insurance, recruitment
JEL-codes: J22, J23, L23, M51

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1 Introduction

Production loss due to sickness absence among employees poses challenges for many employers. In most OECD countries, employers are responsible for sick pay during the initial stage of an employee's sick leave, but a national insurance scheme (or other type of insurance) is often in place to cover costs after a certain duration of absence. Although the incentive structure is similar in many countries, the extent to which employers bear the cost of sick pay expenditures differs across countries. For example, in Norway the sick pay period extends to 16 days, in Germany to 6 weeks, in the UK to 28 weeks, while in Canada there is no period of continued wage payment (see OECD 2010 for an overview). In countries where employers have sick pay liabilities, insurance schemes (of various design) are often in place to reimburse firms for high sick pay expenses, and insurance solutions sometimes differ for smaller compared to larger firms.[1] A contributing factor to this diverged set of solutions across countries could be limited knowledge regarding the connection between firm incentives and employee absenteeism.

Employers' responsibility for sick pay creates incentives for them to try to prevent absenteeism. However, extensive sick pay liability may also lead them to try to avoid hiring individuals who they perceive to have a high risk of being absent. How to optimally balance these two aspects is a key challenge in the design of employers' sick pay liabilities, but this question has received surprisingly little attention in the literature (see Fevang, Markussen, and Røed 2014, and Böheim and Leoni 2020 for two exceptions).

The aim of this paper is to empirically investigate if employee short-term sickness absence is affected by firms receiving a more generous insurance against high costs for sick pay. A special focus is on how responses differ depending on firm size. It is well-established that small firms tend to have a lower absence level than larger firms (e.g., Lindgren 2012; Dionne and Dostie 2007; Ose 2005). Barmby and Stephen (2000) hypothesize that this gradient in sickness absence is due to a higher unit cost of absence in smaller firms. In this paper we demonstrate that this may be an important aspect to consider also in insurance designs.

We exploit a reform introduced in Sweden in 2015, which introduced different thresholds for insurance reimbursement depending on firm size. In Sweden, the employer is responsible for sick pay during the first two weeks of absence, but a national insurance is in place to insure firms against very high costs.[2] Before the reform, all firms (irrespective of size) faced the same

[1] In the Nordic countries, an insurance is generally provided by the state. In, e.g., the US, the UK, and the Netherlands, the employers reinsure their sick pay liability with private insurance companies. Furthermore, Austria, Denmark, and Sweden are examples of countries that have special insurance solutions for small firms; see Böheim and Leoni (2020) and Pertold and Westergaard-Nielsen (2018).

[2] However, the first day of sickness is always uncompensated.

threshold for reimbursement, determined by the average absence level in the economy. In practice, this implied that small firms were not covered by the insurance since their level of sickness absence rarely reached the average level. The reform aimed to make the insurance relevant also for smaller firms by lowering the threshold for reimbursement: all firms received a more generous insurance, but the insurance generosity increased more in smaller than in larger firms. We exploit the difference in insurance generosity after the reform depending on firm size and compare the behaviour of employers and employees in firms of different sizes over time in a difference-in-differences setting. The analysis conditions on a large set of aggregated controls, including workplace fixed effects.

A more generous insurance might reduce firms' incentives to prevent absenteeism, which could lead to a higher level of sickness absence. It may also affect their recruitment behaviour, making them more likely to employ 'sick prone' individuals, which could also lead to more absenteeism at the firm. We argue that it is not obvious that we should expect sickness absence in the smallest firms to be as responsive to changes in the insurance generosity as in larger firms. The reason is that the *production loss* of an absentee is in general larger in smaller than in larger firms. If there is complementarity between employees in the production process, the production loss may be larger than the absent worker's own output (Hensvik and Rosenqvist 2019). However, if the absent worker can be replaced by a colleague with the same competence, the production loss can be mitigated.[3] Since the number of substitutes tends to be lower in smaller firms, the production loss of an absentee is likely to be larger in smaller than in larger firms. This means that even if the insurance is made more generous for smaller firms, it may still not compensate for their larger production loss of an absentee. It is therefore an empirical question how potential behavioural responses due to the reform differ between firms of different sizes.

We find that sickness absence remained on a similar level after the reform in the largest firms. In comparison to the largest firms, sickness absence increased in middle-size firms (which received a larger reduction in sick pay liability), but not in the smallest firms even though their sick pay liability was reduced the most.[4] We find no evidence suggesting that the reform effect in middle-sized firms is driven by changes in the composition of employees. This suggests that the increase in sickness absence is instead driven by behavioural responses at the workplace, e.g., changed policies, norms, or monitoring. Moreover, we show that the lack of substitutability between co-workers in small firms explains a significant part of their (generally) lower level of

[3] Hensvik and Rosenqvist (2019) show that the number of workers with the same competence at the workplace is important for explaining the level of sickness absence: the fewer substitutes an employee has, the lower the absence level.
[4] The average number of employees is 38 in middle-sized firms and 15 in small firms.

sickness absence. It seems reasonable that this mechanism is important also for understanding why we do not observe any change in behaviour due to the reform in the smallest firms.

This paper contributes to several strands of literature. Firstly, we contribute to the literature on firms' sick pay liability and worker absenteeism due to sickness. Earlier studies (Böheim and Leoni 2020; Pertold and Westergaard-Nielsen 2018; Fevang, Markussen, and Røed 2014) have focused on the margin of introducing or abolishing an insurance. This is the first study on the margin of providing a more generous insurance that also addresses the importance of firm size. As we stated earlier, several countries have special insurance solutions for small firms (e.g., Austria, Sweden, and Denmark) but few studies focus on how (and why) firms of different size react to a more generous insurance. This paper aims to fill this gap. The previous studies (mentioned above) find that firm sick pay liability is an important determinant of employee sickness absence. Our results are in line with these patterns when it comes to middle-sized firms, but not for the smallest firms. Secondly, we document how short-term sickness absence varies depending on firm size. This phenomenon has been documented in several studies (Hensvik and Rosenqvist 2019; Lindgren 2012; Ose 2005; Dionne and Dostie 2007; Barmby and Stephen 2000), but not on Swedish data on short-term sickness absence.[5] Thirdly, very little attention has been paid to the underlying mechanisms behind this phenomenon. One exception is Hensvik and Rosenqvist (2019) who show that the number of substitutes is key for explaining long-term sickness absence, and that both selection of employees and firm-specific behavioural responses matter. We build on their work and investigate whether these mechanisms are important also for understanding differences in short-term sickness absence across firms of different size, and for understanding how firms of different size react to the provision of a more generous insurance.

Our analysis is based on a quarterly survey on short-term sickness absence (i.e., sick leave during the first 14 days in a sickness spell, when the employer is responsible for sick pay) conducted by Statistic Sweden among private firms. The survey is linked to register information on annually updated employer and employee information, including firm and workplace identifiers, industry codes as well as individual background information on demographic variables, earnings, earlier use of sickness benefits etc.

The paper is organized as follows: We start by discussing why sickness absence varies depending on firm size (section 2). We continue with a discussion about the role of an insurance against high sick pay costs in this context, and the Swedish insurance solution (section 3). Section

[5] Lindgren (2012) and Hensvik and Rosenqvist (2019) have documented this phenomenon when it comes to the use of sickness benefits paid by the Social Insurance Agency, which reflects long-term sickness absence (absence lasting longer than 14 days). We focus on short-term absence, i.e., absence due to sickness during the first 14 days in a sick spell. For this period the employer has the pay liability.

4 presents the data used for the analyses and the empirical strategy. In section 5, we present the results, and section 6 concludes the paper.

2 Sickness absence and firm and workplace size

As discussed in the introduction, it is well-established that the level of sickness absence is closely linked to firm size (e.g., Barmby and Stephen 2000; Lindgren 2012). Theoretically, the firm size gradient in sickness absence has an intuitive and straightforward explanation: larger firms face a lower cost of insuring against absence, simply because they can more easily diversify the risk within the firm (Barmby and Stephen 2000).[6] Building on this argument, Hensvik and Rosenqvist (2019) hypothesize that the production loss from sickness absence (and hence the cost of absence) depends on the number of employees at the workplace who can do the same job, and they find that absence rates differ substantially between more and less substitutable employees. This observation suggests that smaller firms, which tend to have fewer substitutable employees, have stronger incentives to prevent sick leave compared to larger firms with more substitutes. Furthermore, Hensvik and Rosenqvist (2019) show that higher absence-related costs in unique positions affect both hiring and separations: people with a history of sickness absence are less likely to be hired to a unique position, and realized sickness absence increases the probability to leave a unique position.[7] The differences in absenteeism between smaller and larger firms could thus be due to both sorting of workers (i.e., 'sick-prone' individuals are less likely to work in small firms) and on-the-job behavioural changes (for example adjustments to firm-specific policies and norms). It is also likely that the cost of monitoring is lower in small firms, and that workers in smaller firms to a higher extent internalize the costs associated with sickness absence, which may trigger behavioural effects (Hensvik and Rosenqvist 2019).

The literature has also suggested more sociological explanations for the firm size gradient in sickness absence (see, e.g., the discussion in Lindgren 2012 and Barmby and Stephen 2000). For example, the sense of moral involvement may be higher in smaller firms with less anonymity, which would predict lower absence in smaller firms (Ingham 1970). These types of explanations are, however, hard to address empirically.

[6] Barmby and Stephen (2000) present a theoretical model with this conclusion.
[7] There is also evidence suggesting that sickness history is an important factor in recruitment: Eriksson, Johansson, and Langenskiöld (2017) find – in a hypothetical choice experiment – that employers are reluctant to hire persons with a history of absence due to sickness.

Figure 1 Short-term sickness absence days per quarter depending on workplace size

Note: Own calculations based on Wage and Salary Statistics and Short-term Sickness Absence Data (KSJU) from Statistics Sweden 2011–2018. The sample consists of firms with <200 employees and workplaces with <100 employees. Only sickness absence among regular employees (not managers) is included. 'Short-term absence' means that only absence days during the first two weeks of an absence spell (when the employer is responsible for sick pay) are included.

Figure 1 presents the relationship between short-term sickness absence (i.e., days sick during the first two weeks of an absence spell) and workplace size using Swedish data.[8] We see that absence due to sickness increases sharply up to about 20 colleagues, then it levels out somewhat around 0.7 days per quarter and person, and above 30 colleagues the absence level stabilizes around 1 day per quarter and employee. The relationship is very similar if we instead of workplaces compare firms of different size, see Figure A1 in appendix.[9] Hence, the firm size gradient in sickness absence is present also in our data, although it mainly appears to be a phenomenon among small and middle-sized firms.

[8] Data on short-term sickness absence is based on the number of days the employer has paid out sick pay. Since sick pay days must be reported to the tax authorities, we have no reason to suspect under-reporting in these numbers.
[9] The reason is that firm and workplace tend to coincide in smaller firms.

3 Insurance against sick pay costs

From an insurance design perspective, the social planner faces a trade-off. On the one hand, it is desirable to create incentives for employers to provide a healthy work environment to prevent sickness absence among employees, which would give rise to a positive labor supply response. This could potentially be achieved by letting the employer pay a larger share of the costs associated with sickness absence. On the other hand, if a larger burden is put on the employer, there is a risk that less healthy individuals will not get hired, which can be seen as a kind of market failure or negative labor demand response. The optimal design of an insurance against high sick pay costs depends on whether the positive labor supply or the negative labor demand responses dominate. Although this trade-off is clear from a theoretical perspective, it has only been addressed empirically by a limited number of papers, and no earlier study has recognized the different set of challenges faced by firms of different sizes in this context.

Several countries have insurances against high sick pay cost, where the rules are more generous for smaller firms: e.g., Austria (blue collar workers in small firms), Sweden (different rules depending on firm size), and Denmark (special insurance program for small firms).[10] The extent to which the more generous rules for small firms contribute to reduce the firm size gradient in sickness absence, will depend on whether or not the more favourable rules are generous enough to compensate for the presumed larger cost faced by smaller firms when an employee is absent. As we discussed in the introduction, the *production loss* of an absentee is likely to be related to the substitutability between co-workers. This *production loss* should, ceteris paribus, be 'more or less' independent of the employer's sick pay liability. The reason is that the sick pay liability is the same irrespectively of the number of substitutes the absent worker has at the workplace.[11] However, the employer's sick pay expenses depend on the sick pay liability and, of course, on the sickness absence level among the employees, which in turn may be affected by firm-specific policies.

How can managers then affect employee sickness absence? Earlier literature have discussed preventive work environment measures including both health promoting policies (Aldana and Pronk 2001) and efforts to monitor that employees do not overuse their right to report sick (Heywood and Jirjahn 2004). It is also possible that managers' attitudes to sickness absence could affect norms regarding absence at the workplace. All these margins could be affected if firms' sick pay liabilities are reduced. Reduced sick pay liability could also affect recruitment behaviour, since employing more 'sick prone' individuals becomes less risky for the employer.

[10] See Böheim and Leoni (2020) and Pertold and Westergaard-Nielsen (2018) for a more extended discussion on how insurances against high sick pay costs differ across countries.
[11] Of course, if extensive sick pay liabilities are introduced, some firms may re-organize their production process (for example by employing more substitutes) so that the production loss of an absentee is reduced.

Taken together, this reasoning implies that firms can affect the sickness absence level and firms' efforts to do so are likely to be partly determined by the employers' sick pay liabilities. However, the production loss associated with an absentee is not directly linked to the employers' sick pay liabilities and this production loss of an absentee is likely to *decrease* with firm size.

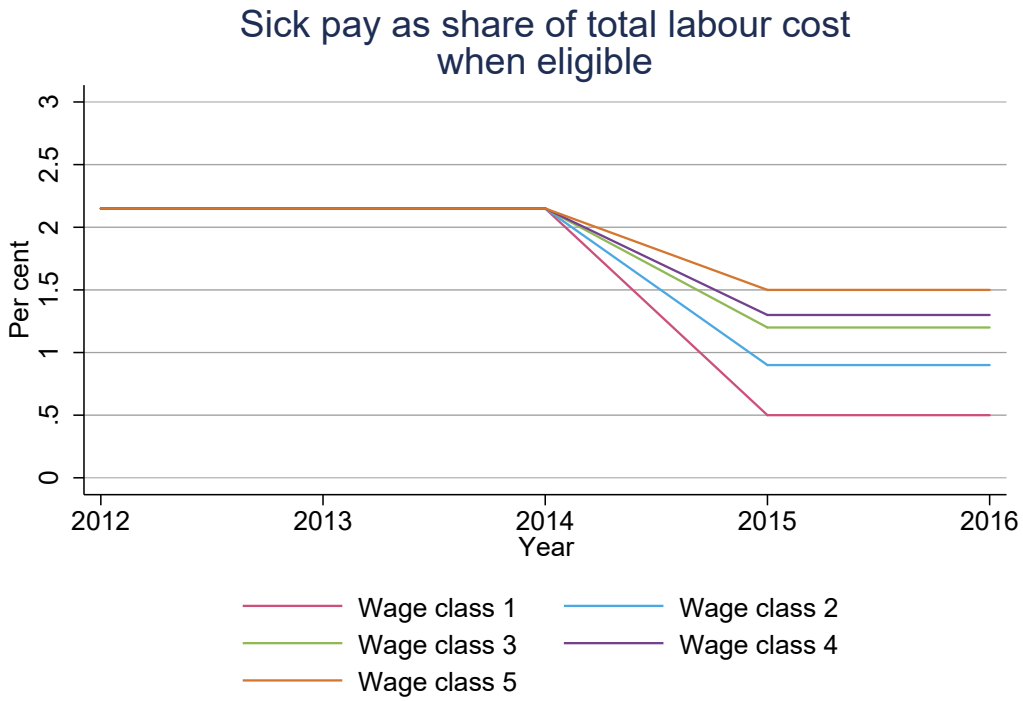
3.1 The Swedish insurance

An insurance against high sick pay costs was introduced in Sweden in 2010. At that time, all firms, irrespective of size, faced the same threshold for reimbursement: each calendar year, sick pay costs that exceeded 2.5 times the average sick pay cost among all firms in the economy (expressed as the share of total labor costs) were refunded. However, since the absence level is much lower in smaller firms, this rule implied that small firms were hardly covered in practice, as their level of absence rarely reached the threshold.

To remedy this problem, differential rules depending on firm size were introduced in January 2015. Since then, employers are (each year) divided into five groups depending on total labor costs. The first wage class (the smallest firms) are firms with wage costs below SEK 3 million (approximately 290,000 EUR). Wage class 2 refers to wage costs in the interval 3–6 million, class 3 to the interval 6–12 million, class 4 to the interval 12–20 million, and the 5th class to firms with wage costs above SEK 20 million.[12] Each wage class has its own threshold for reimbursement. For the smallest firms (wage class 1), the threshold corresponds to sick pay costs amounting to 0.5 percent of total labor costs. The corresponding thresholds for the other classes are: 0.9; 1.2; 1.3 and 1.5, respectively. Hence, all firms received a more generous insurance following the reform, but the insurance generosity increased more in smaller than in larger firms. Figure 2 below summarizes the rules over time for the five wage classes.

[12] Expressed in EUR the following division apply (using the exchange rate on April 4, 2022): <290,000 EUR (wage class 1); 290,000–580,000 (wage class 2); 580,000–1,160,000 (wage class 3); 1,160,000–1,934,000 (wage class 4); >1,934,000 (wage class 5).

Figure 2 Maximum sick pay costs as share of total labor costs depending on firm size



Note: The y-axis shows maximum sick pay as share of total labour costs. The pre-reform level (before 2015) is 2.5 times the average sick pay cost, as share of total labor costs, in the economy, which at the time was around 0.85 according to the public information provided by the Social Insurance Agency to the employers at the time. From 2015 onwards, the thresholds were differentiated depending on which of five different wage classes the firm belongs to. Wage class 1–5 corresponds to wage costs (in SEK) in the following intervals: <3 million (approximately 295,000 EUR), 3–6 million, 6–12 million, 12–20 million, and >20 million.

After the reform, the reimbursement was also paid automatically (in the beginning of the next calendar year) instead of firms needing to apply manually. Firms were also, from this time onwards, allowed to apply for compensation in advance, if the firm needed financial support earlier. These rule simplifications were the same for all firms from 2015 onwards.

The only information we have about insurance reimbursement stem from annual public accounts published by the Social Insurance Agency. In these accounts, firms are grouped by wage class after the reform, while they are grouped by number of employees before the reform. These two divisions largely overlap, but not perfectly. However, from these numbers it is clear that the reform increased the number of firms reimbursed in all wage classes, but more so among the smallest firms. Among the smallest firms (1–4 employees on average), the number of firms reimbursed during a one-year (approximately) period before the reform was 42. The corresponding number for wage class 1 the first year after the reform was 31,785. For the group with the largest firms (>50 employees), the number of firms reimbursed before the reform was

24, and the corresponding number for wage class 5 after the reform was 1,048 (Försäkringskassan 2011; 2016).[13]

4 Data

The analysis is based on register data in combination with a survey conducted among firms by Statistics Sweden on short-term sickness absence (*KSju*[14]). In Sweden, short-term sickness is defined as days reported sick among employees during the period when the employer pays sick pay, i.e., during the first two weeks of a sickness absence period. The observation unit in the survey, as well as in our study, is the number of absence days per employee and quarter. All large firms are included in the survey, but a random stratified (on sector, industry, and firm size) sample is drawn among firms with less than 200 employees. The survey has a panel dimension: selected firms are followed four times a year for five years. To get a more homogeneous sample, we drop the largest firms: those with more than 200 employees, and workplaces with more than 100 employees. We also focus on private firms only.[15]

We are interested in the sickness absence level among regular workers, not employees in managerial positions, since managers are the ones supposed to communicate the prevailing firm policy with regard to, e.g., absenteeism, to other employees. Of special interest in this paper is also the importance of the number of co-workers who can do the same job, and to analyse its potential effect on both the pre-reform level of absence and firm responses to the reform. To get information about regular workers vs. managers and information about substitutability between co-workers, we need another survey, also provided by Statistics Sweden, namely the Wage and Salary Structure data (*Lönestrukturstatistiken*). This survey covers all employees in large firms (>500 employees) but also a stratified random sample (on sector, industry, and firm size) of smaller firms. Altogether, this survey covers about 50 percent of all employees in the private sector. The two surveys are designed in the same way but, unfortunately, they are not synchronized. We therefore must restrict our sample to individuals employed in firms included in both surveys.[16]

[13] For the other groups, the corresponding numbers were the following: Before the reform and for firms with 5–9 employees, the number was 35; for firms with 10–19 employees it was 39, and for firms with 20–49 employees it was 23. After the reform, the number for wage class 2 was 5,763; for wage class 3 2,545, and for wage class 4 1,101.
[14] Konjunkturstatistik över SJUklöner.
[15] In the public sector, our data generally do not allow us to distinguish between workplaces. All employees in a municipality, for example, can be defined as belonging to the same workplace. In our analysis we need workplace identifiers both to define the number of substitutes and to control for workplace fixed effects.
[16] An option is, of course, to not add the Wage and Salary Structure data and only use register data in combination with the KSju sample for estimating the main effects of the reform. However, such a sample would be less appropriate for the focus of this study, namely, to study how sickness absence among regular employees is affected by more generous insurance rules. Including managers is also likely to increase the baseline difference in short-term sickness absence between firms of different size, since managers can be expected to be less absent due to sickness than regular workers (c.f. Hensvik and Rosenqvist, 2019) and they constitute a larger fraction of the personnel in smaller than in larger firms.

In Appendix, we present details about how we merge the two surveys to create the analysis sample. Table A1 shows the number of firms, given firm size, that are included in KSju and also in the Wage and Salary Structure Statistics. From this table we see that the overlap increases with firm size. However, although the overlap is lower among smaller firms, the representativeness is not necessarily affected since both surveys are based on random samples of firms.

To this sample, we add individual register information on sex, wages and occupations as well as workplace and firm identifiers and codes on the firms' industry (2-digit level: 78 different industries) and sector (25 different sectors) belonging.[17] Industry and sector codes overlap conceptually, but not perfectly in the data. The reason to include both these codes is that the two surveys we use are stratified on both industry and sector. Finally, we follow Hensvik and Rosenqvist (2019) and use the Swedish classification of occupations[18] to calculate, for each individual, the number of employees at the workplace with the same occupation code (3-digit level). For example, an administrator at a workplace that in total employs four administrators will have three substitutes. We have 146 different occupational codes in our data.[19]

The final sample we use for estimation consists of an unbalanced annual panel of firms, workplaces, and individuals. Table 1 presents the number of firms and individuals in this sample. We annually observe around 200–300 firms in wage class 1 and 5, and 50–100 firms per year in class 2–4. The number of individuals in each firm increases with firm size: about 2 000 individuals per year in class 1 and around 50 000 per year in class 5. From year 2015 onwards, the sample size increases in all classes due to an enlargement of the sample for the Wage and Salary Structure statistics, but this should not affect the composition of firms.

[17] Sector refers to Standard Classification by Institutional Sector 2014, INSEKT 2014, consistent with the European System of National and Regional Accounts, ESA 2010. In this data, 25 subgroups divided by ownership and type of activity (for example non-profit or not).
[18] Similar to the International Standard Classification of Occupation (ISCO).
[19] Examples of 3-digit occupations are salespersons, client information clerks and data entry operators.

Table 1 Number of firms and individuals in analysis sample

		Firms					Individuals				
Class:		1	2	3	4	5	1	2	3	4	5
Year	Quarter										
2011	2,3,4	199	89	72	52	203	1 213	1 683	3 038	3 684	37 827
2012	1-4	200	78	80	48	217	1 459	1 982	3 675	3 963	44 251
2013	1-4	189	80	64	38	221	1 460	1 814	2 896	3 924	47 195
2014	1-4	206	68	76	51	229	1 572	1 656	3 436	4 314	47 195
2015	1-4	348	121	142	95	305	2 437	2 877	6 268	7 529	60 555
2016	1-4	390	110	140	88	312	2 824	2 620	6 482	7151	65 680
2017	1-4	356	98	103	73	314	2 616	2 598	5 053	6 245	65 635
2018	1,2,3	300	84	90	86	291	1 634	1 618	3 235	5 531	45 776
Total		2 188	728	767	531	2 092	15 215	16 848	34 083	42 341	414 114

Note: Private sector employees in firms with <200 employees and with workplaces with <100 workers, years 2011–2018. Individuals in managerial positions are not included. *Source*: Salary and Wage Structure Statistics and KSju from Statistics Sweden.

Table 2 presents descriptive statistics for our analysis sample for the pre-reform period, i.e., 2011–2014. Firstly, we note a clear gradient with respect to days reported sick across wage classes: it is lowest, around half a day per quarter on average, in class 1 and increases monotonically for each higher wage class. Sickness absence in class 5 is almost twice as large as in class 1. With respect to age, foreign background (born outside Europe or not), marital status, number of children aged 1–6 (in the household), and prior use of sickness benefits (for long-term sickness), there are no clear differences across wage classes. In the other reported dimensions, we observe a pattern of more positively selected employees in the higher wage classes (i.e., the larger firms): both (full time equivalent) wages and level of education are higher among employees in higher wage classes and the use of disability pension (days used during the last two years) is lower. This observation suggests that other mechanisms than traditional individual determinants explain the firm size gradient in short-term sickness absence.[20] Finally, we note that the average workplace size (i.e., the number of employees at the workplace) increases, as expected, with firm size, as does the number of substitutes. In Table A2 in the Appendix, we present the corresponding descriptive statistics for all employees the in private sector in Sweden. A comparison between Table 2 and Table A2 shows that the patterns we have highlighted in the text are present also in the full sample of employees. Overall, the levels of the different variables are similar in the two samples, but

[20] There is a literature about the determinants of sickness absence that have documented that sickness absence decreases with socioeconomic status; see, e.g., Seglem et al. (2020).

employees in the sample used for our analysis are somewhat younger and less educated, and the analysis sample includes a somewhat lower share of women.

Table 2 Descriptive statistics of workers employed in firms of different size, year 2011–2014

Wage class	1	2	3	4	5
Wage cost in million SEK	<3	3–6	6–12	12–20	>20
Days reported sick[1]	0.434	0.646	0.817	0.835	0.941
	(2.244)	(2.111)	(2.127)	(1.932)	(1.676)
Age	39.563	39.277	39.061	38.916	42.185
	(12.317)	(12.995)	(12.533)	(13.234)	(13.392)
Female	0.334	0.362	0.357	0.457	0.342
	(0.474)	(0.498)	(0.479)	(0.481)	(0.472)
Upper sec. educ.[2]	0.629	0.630	0.599	0.543	0.551
	(0.497)	(0.498)	(0.490)	(0.483)	(0.483)
Post sec. educ.[2]	0.186	0.207	0.253	0.314	0.319
	(0.466)	(0.464)	(0.435)	(0.405)	(0.389)
Wages (SEK)[3]	24 053	25 907	26 828	27 085	29 824
	(9 944)	(9 759)	(7 233)	(6 119)	(5 754)
Annual incoome	239 870	261 076	279 914	267 701	331 612
(SEK)[4]	(163 744)	(161 779)	(125 522)	(119 402)	(112 452)
Born outside Europe	0.050	0.046	0.053	0.067	0.057
	(0.232)	(0.251)	(0.224)	(0.210)	(0.219)
Married	0.367	0.351	0.357	0.354	0.408
	(0.491)	(0.478)	(0.479)	(0.477)	(0.482)
Children aged 1–6	0.187	0.186	0.204	0.191	0.199
	(0.399)	(0.393)	(0.403)	(0.389)	(0.390)
Any sick. benefit[5]	0.057	0.076	0.061	0.069	0.072
	0.232	0.265	0.239	0.253	0.258
Any disab. pension[5]	0.028	0.020	0.018	0.013	0.009
	0.164	0.139	0.134	0.113	0.097
Workplace size	4.923	11.911	20.428	37.399	51.918
	(28.312)	(14.817)	(7.928)	(3.726)	(2.683)
Firm size	4.933	12.037	21.873	42.437	111.662
	(54.191	(14.396)	(7.454)	(3.618)	(2.684)
Substitutability[6]	2.654	5.614	8.798	16.500	19.528
	(18.820)	(13.416)	(6.904)	(3.647)	(2.034)

Notes: 1) This information is observed in the pre-reform period. 2) An indicator for the highest education level achieved. 3) Reported as full-time equivalent wages. 4) Total during a year. 5) Formally named sickness compensation (which you only can get if judged permanently disabled and inable to work). Defined as any benefit during the last 2 years. 6) Number of employees with the same occupation code (3-digit level), a Swedish version similar to the International Standard Classification of Occupation (ISCO) at workplace level.

Figure 3 presents descriptive results based on raw data. We observe a substantial seasonal variation in sickness absence common to all wage classes. The gradient in absence depending on firm size is also visible, with the lowest level of absence among the smallest firms (class 1), and the highest level among the largest (class 5). Important to note is also the stable (except for the seasonal variation) level in class 5, i.e., the wage class that received the smallest increase in

generosity due to the reform.[21] For wage class 1, 2 and 3 – the classes that received the largest improvement in insurance conditions – it is hard to detect whether there is any change in sickness absence behaviour following the reform. The difference between class 5 and 4, on the other hand, seems to diminish after the reform. However, it is not possible, based on this figure, to assess whether or not this is a reform effect or not. To assess whether the reform has altered individual behaviour, we estimate an econometric model that cleans data from seasonal variation and exploits variation within wage classes over time.

Figure 3 Average number of days reported sick (used sick pay days) per employee and quarter depending on firm size

Source: Own estimation based on KSju 2011q3-2019q1 combined with register information about firms' total labour costs.

5 Empirical strategy

The aim of this paper is to estimate the effect of providing firms a more generous insurance against high sick pay costs (a lower threshold for reimbursement) on employee short-term sickness absence. Since all firms received a more generous insurance in 2015, we have no unaffected control group. However, in practice the reform implied a significant amelioration for smaller firms, and a much smaller improvement for larger firms. We exploit this variation by comparing the evolution of sickness absence in smaller and larger firms over time, before and after the

[21] Class 5 is also the largest group with respect to the number of individuals.

reform. The highest wage class – class 5 – will act as a control group, and we are interested in whether employees in firms belonging to the other wage classes changed their sickness absence behaviour in relation to individuals employed in wage class 5.[22] To assess the reform impact, we estimate the following model:

$$y_{i,t} = \beta_1(after_t \times W\_class1\_3_i) + \beta_2(after_t \times W\_class4_i) + \gamma_1 W\_class_{1\_3,i} + \gamma_2 W\_class_{4,i} + \theta quarter_t \times year_t \times female_i \times industry_i + \delta sector_i + \epsilon_{i,t}, \quad (1)$$

where $y_{i,t}$ is the outcome of interest for individual *i*, in time-period *t,* which is a quarter within a year (our observation unit) for the outcome variable but vary by year for the explanatory variables.[23] The parameters β_1 and β_2 capture the conditional difference-in-differences in absence among employees in firms belonging to wage class 1–3 and 4, respectively, in comparison to employees in wage class 5. To streamline the presentation, and increase the precision of the estimates, wage classes 1–3 are grouped together. Figure A3 in Appendix, which also assesses the parallel trends assumption, presents the estimated reform effects for each wage class separately (the conclusions remain the same). $after$ is an indicator for the post-reform period, and W_class1_3 and W_class4 are indicators for being employed in a firm belonging to wage class 1–3 and 4, respectively. The γ_j parameters capture pre-reform differences in sickness absence for wage classes 1–3 and 4, respectively, in relation to class 5. Sickness absence varies a lot depending on season, industry, and gender, and by including fixed effects for season (*quarter*×*year*) interacted with an individual indicator for gender (*female*) and 78 industry categories (*industry*), we effectively control for this heterogeneity.[24] Finally, *sector* captures which sector the individual works in, which we condition on since the survey data we use is stratified on sector. We cluster the standard errors on firms since both the sampling and the treatment is at the firm level (Abadie et al. 2017).

[22] Note that we, by using this approach, control for the simplification of insurance rules that affected all firms (see section 3), unless firms of different size respond heterogeneously to these changes. *If* they do respond heterogeneously, we find it most plausible that small firms would benefit more from the automatization of insurance reimbursement compared to larger firms, implying that any potential effect of the automatization would go in the same direction as the more generous insurance rules in terms of a lower threshold for reimbursement. Since the automatization can be seen as a further change of the insurance rules in a more generous direction, we can still interpret the estimates as effects of increased insurance generosity, although the effects of the lower threshold for reimbursement could not be disentangled from the effects of that the reimbursement was paid out automatically.

[23] We estimate annual effects for precision reasons. In the section discussing the identifying assumption, we also estimate quarter effects from which we draw the same conclusions.

[24] See Figure A2 in Appendix for an illustration, based on Swedish data, of how absence due to sickness varies depending on both season and gender. Moreover, it is documented in the literature that sickness absence varies with the business cycle: the level tends to be high in good times and low in worse times. See Hägglund and Johansson (2016) for evidence on this phenomenon based on Swedish data. Since labour market conditions vary across industries, it is reasonable to believe that also sickness absence fluctuates across industries over time.

Specification (1) above is the main model of interest. As robustness tests, we 1) add workplace fixed effects, 2) include industry- and gender-specific linear trends instead of interacting these indicators with the seasonal indicators (*quarter*×*year*), and 3) condition on the workplace being in the sample both before and after the reform. When conditioning on workplace fixed effects, we explore changes within workplaces over time conditional on time-invariant firm and workplace specific heterogeneity with respect to, e.g., business culture, norms, policies, and composition of employees. The specification with industry-specific linear trends constitutes a less flexible way of controlling for general trends that may differ between industries. We estimate this alternative, less demanding, specification to investigate the trade-off between precision versus number of controls added. The last restriction is added to verify that changes in the composition of firms included in the sample before and after the reform is not driving the results.

Potential reform effects could stem from two different types of behavioural responses: behavioural responses among employees working in firms belonging to a particular wage class (wage class 1–3 and 4 in relation to wage class 5), and changes in the selection of employees into and out of firms belonging to these wage classes. The former type of response could be caused by, for example, more (or less) lenient monitoring of employees to not over-report sick leave. The latter could be caused by, for example, changes in the recruitment process, if firms become more (or less) likely to employ more 'sick prone' individuals. We will address these potential mechanisms in section 6.3 and 6.4.

5.1 Identification assumption

Under the assumption of parallel trends in sickness absence between employees in firms belonging to different wage classes in the absence of the reform, the β_j parameters capture reform effects. To asses this assumption, we follow Autor (2003) and use quarterly indicators, $quarter \times year_t$ (qyr_t), and their interactions with the indicators for W_class1_3 and W_class4, respectively, to estimate k leads and q lags of the reform. That is, we estimate the following model:

$$y_{i,t} = \sum_{j=-k}^{q} \beta_j \left(qyr_{t+j} \times W\_class\_Small_i\right) + \gamma_i W\_class_i + \theta quarter_t \times year_t \times female_i \times Industry_i + \delta Sector_i + \epsilon_{i,t}, \quad (2)$$

The model corresponds to our main model (specification 1), but the wage class indicators are now interacted with the quarterly indicators. We estimate this model separately for wage class 1–3 and 4 in relation to wage class 5. The quarterly indicators (in total 30) start (when data on absence is available) the second quarter of 2011 and end the third quarter of 2018. The first quarter in 2014

is the reference quarter. The k lead (pre-reform) parameters help us assess the validity of the underlying parallel trends assumption, by capturing potential divergent trends in sickness absence between wage classes during the period before the reform.

Even if the trends between wage classes before the reform look parallel, there are still potential concerns as to whether the parallel trends assumption will hold. Firstly, we saw in Figure 3 that the pre-reform levels in sickness absence differ between the control group and the two treatment groups. A difference in levels may reflect an underlying factor, which may also explain shifts in levels, potentially coinciding with the reform (Kahn-Lang and Lang, 2020). In our case, the lower level of absence among smaller firms is in line with a well-established pattern in the literature, and is likely to be related to a higher unit cost of absence in smaller firms (Barmby and Stephen 2000). Therefore, it seems unlikely that sickness absence levels would shift in the absence of any reforms affecting the cost of absence. Secondly, Kahn-Lang and Lang (2020) discuss the importance of using the correct functional form, and that it should be consistent with the perceived counterfactual trend. Estimating effects in percent rather than in absolute terms constitutes a relevant alternative in our case. Since many employees have zero sickness absence days in a given quarter, we do this using poisson regression, as suggested by Silva and Tenreyro (2006); Cohn, Liu, and Wardlaw (2021); and Bellégo, Benatia, and Pape (2021). We also estimate the event study model using an indicator for any sickness absence during the relevant quarter as the outcome; hence, focusing on the extensive margin of reporting sick.

6 Results

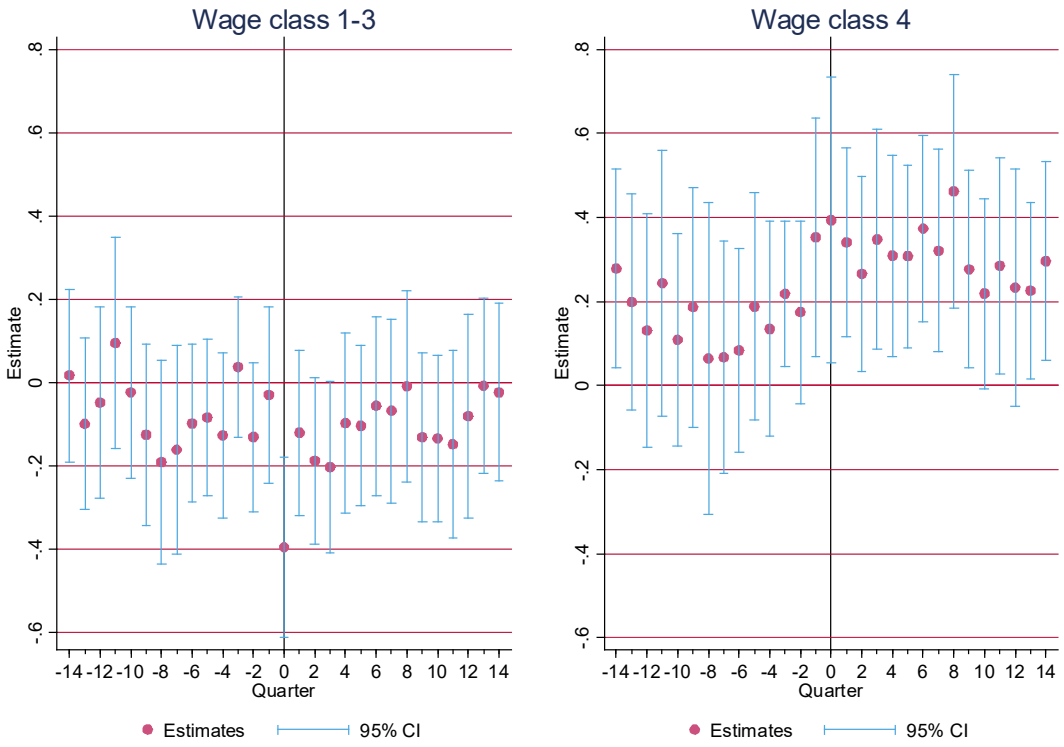
In this section, we present the estimated reform effects from the four different model specifications outlined in section 5. Thereafter, we try to shed light on the mechanisms behind our findings. However, we start by presenting results from the event-study model we use to assess the parallel trends assumption.

6.1 The parallel trends assumption

Figure 4 shows the estimated leads and lag effects of the reform using the model presented in equation 2. The vertical line indicates the first quarter in the new regime (year 2015). The reference value is the first quarter in the last pre-reform year, i.e., 2014. Hence, the estimates to the left of the vertical line (the leads) help us assess the pre-reform trend, and the corresponding estimates to the right show the estimated level of (short-term) sickness absence after the reform. The pattern suggests no reform response for wage class 1–3, but a shift upwards in sickness absence for wage class 4. There is no indication of pre-reform differences in trends, in relation to wage class 5, for neither wage class 1–3 nor class 4, which is reassuring.

In Appendix, we show the corresponding results estimated from four slightly different models. Firstly, in Figure A3, we allow for variation in the fixed effects across wage classes by estimating separate models for each wage class (1–4) in relation to wage class 5. Thereafter, we return to the model used in Figure 4 below, but we do the following modifications: We include workplace fixed effects; these results are presented in Figure A4. We estimate the model using poisson regression; hence, comparing sickness absence across wage classes in percent rather than in absolute terms. These results are presented in Figure A5. Lastly, in Figure A6, we show results when the outcome is replaced by an indicator for any sickness absence during the relevant quarter. In all these models, the pattern is very similar to the one in Figure 4. Thus, the results hold when we estimate effects for each wage class separately, when we add workplace fixed effects, and they are robust to two alternative functional forms. Taken together, we interpret this as empirical support for the common trends assumption, and a first indication of an increase in sickness absence following the reform in wage class 4 but no reform response in wage class 1–3.

Figure 4 Estimated leads and lag effects of the reform for class 1–3 and class 4, in comparison to class 5, on the number of days reported sick per employee and quarter



Note: Estimates and confidence intervals for the regression model presented in equation 2. The model controls for sector and year×quarter×female×industry f.e. The reference value is the first quarter in year 2014 for employees in firms belonging to wage class 5. The horizontal axis indicates the number of quarters since the first quarter included in our analysis data. The vertical line marks the first quarter in the new regime.

6.2 Main results

Table 3 presents the estimated reform effects outlined in section 5. We observe several interesting results. In the first specification, we can first notice the pre-reform difference in sickness absence depending on firm size. The difference, in relation to wage class 5, is largest for class 1–3: on average employees are absent 0.23 days less per quarter in class 1–3, while the corresponding number for class 4 is 0.17. Hence, the estimates show the expected firm size gradient in sickness absence. When adding workplace fixed effects to the model (column 2), the difference between wage class 1–3 and 5 disappears. However, since we now control for everything that is constant within a workplace over time (and wage class is generally constant), it is likely that we do not have enough variation to identify this parameter in our data. Therefore, the first specification (column 1) is likely to be more informative when it comes to general behavioral differences between employees in firms of different sizes.

Turning to the reform effects, the estimates for class 1–3 are small and statistically insignificant in all specifications, suggesting no change in sickness absence behaviour among employees in the smallest firms after the reform. The results differ for wage class 4. The estimated pre-reform difference (in column 1) suggests 0.168 fewer days per quarter. The corresponding reform effect is of almost the same magnitude but in the opposite direction (0.143 more days per quarter), implying that the pre-reform difference in sickness absence between employees in wage class 4 and 5 largely disappears after the reform. The estimated reform effect for class 4 is rather stable across specifications: it decreases somewhat in magnitude when we add workplace fixed effects (column 2) but increases slightly again in the (less demanding) model with industry-female-specific linear slopes (instead of fixed effects for each industry×female×year×quarter) (column 3). When we restrict the sample to workplaces that are sampled both before and after the reform (column 4) the estimate also increases slightly.

To sum up, the reform seems to have increased sickness absence in firms belonging to wage class 4, but we find no evidence of behavioural changes due to the reform among the smallest firms (wage class 1–3).

Table 3 Estimated reform effects for wage class 1-3 and 4, respectively in relation to wage class 5

	(1)	(2)	(3)	(4)
Dependent variable: Average number of days absent due to sickness per quarter and employee				
Class4×after reform	0.143**	0.105*	0.114**	0.133**
	(0.0682)	(0.0598)	(0.0577)	(0.0674)
Class 4	-0.168***	-0.147**	-0.168**	-0.257**
	(0.0564)	(0.0616)	(0.0670)	(0.105)
Class1–3×after reform	-0.0483	-0.0780	-0.0109	-0.0294
	(0.0482)	(0.0527)	(0.0491)	(0.0506)
Class 1–3	-0.228***	-0.00700	-0.0972	-0.159
	(0.0388)	(0.0931)	(0.0977)	(0.153)
Observations	522,509	522,395	522,488	205,251
R-squared	0.034	0.074	0.065	0.058
Sector	yes	yes	yes	yes
Year×quarter×female× industry f.e.	yes	yes	no	no
Year×quarter×female× industry specific slopes	no	no	yes	yes
Workplace f.e.	no	yes	yes	yes
Pre & post sample restriction[1]	no	no	no	yes
Outcome mean, class 5	0.941	0.941	0.941	0.920

Note: 1) Workplaces included in the analysis exist in the dataset both before and after the reform. Sector refers to Standard Classification by Institutional Sector 2014, INSEKT 2014, consistent with the European System of National and Regional Accounts, ESA 2010. In this data 25 subgroups divided by ownership and type of activity (for example non-profit or not). Robust standard errors in parenthesis, clustered on firm. *** p<0.01, ** p<0.05, * p<0.1. 1) Mean outcome is measured in pre-reform period for the control group (wage class 5).

6.3 Mechanisms: the role of sorting and co-worker substitutability

In this section, we aim to further understand the differential effects depending on firm size by investigating the role of sorting of workers as well as substitutability between co-workers. We try to address the relevance of these mechanisms both when it comes to explaining the general (pre-reform) differences in sickness absence across wage classes, and the estimated differential reform effects depending on firm size.

Sorting of workers

It is possible that the observed gradient in sickness absence across wage classes is, at least partly, explained by how workers are sorted across firms: smaller firms may be more reluctant to employ more 'sick prone' individuals compared to larger firms, and/or individuals who are more inclined to report sick may themselves prefer to work in larger firms. If this type of sorting is important, the estimated firm size gradient in sickness absence should decline if we add controls for worker

characteristics that are predictive of sickness absence to our model.[25] To test this, we estimate our main model specification (i.e., column 1 in Table 3) both with and without controls for a range of individual characteristics. More specifically, we add controls for: age (linearly and quadratically), education level (two dummies), immigrant background (born outside Europe), and presence of preschool children at home, as well as indicators for having used any sickness benefits or disability pension during the last two years.

The results from this exercise are shown in the first two columns of Table 4. We have here chosen to present results for each wage class separately, to further highlight the differences between firms of different size. Column 1 (without individual controls) shows a clear pre-reform gradient in absence depending on wage class: in comparison to wage class 5 with a mean of 0.941, the absence level is 0.42 days lower in class 1 but only about 0.168 days lower in wage class 4. Interestingly, the pre-reform estimates stay rather similar in column 2, where we have added individual covariates; if anything, the gradient is somewhat increased. This suggests that there is no important selection of workers to firms belonging to different wage classes, at least none that we can capture with our individual covariates.[26] Hence, the observed pre-reform gradient in sickness absence seems to be explained by mechanisms taking place at the workplace (or firm) rather than by the selection of workers.

In line with the results presented in Table 3, column 1 shows no indication of changed behaviour following the reform in wage class 1–3 but an increase in sickness absence in class 4. Adding individual controls does not change these findings. For wage class 4, the reform estimate stays very similar, suggesting that the increased sickness absence for this wage class is not driven by changed recruitment behaviour. This suggests that it may instead be driven by behavioural responses at the workplace, e.g., changed policies, norms, or monitoring. For wage class 1–3 we can only note that individual controls do not alter the small and statistically insignificant estimates, confirming that there is no support for any reform effects among the smallest firms.

Substitutability between co-workers

We have also discussed the degree of substitutability between co-workers as a potential explanation for the firm size gradient in sickness absence. To assess the importance of this mechanism for our findings, we add individual controls for the number of co-workers with the

[25] It would be even better to add individual fixed effects. We would then examine potential changes in absence behaviour among individuals who change workplace between firms of different size; the firm size effects in such a model could thus not be driven by selection. However, such an analysis requires repeated observations per employee, and since our data is based on a (random) sample of firms each year we often cannot follow individuals over time.

[26] Of course, we cannot exclude differences in unobserved factors or that individual characteristics change due to personal reasons. For example, health may change over time and such a change can cause employees to move from smaller to larger firms. We ignore this type of explanation in this reasoning since we find them unlikely as main explanations.

same occupational code (at the workplace) to our model.[27] If the number of substitutes is an important mechanism behind the difference in absenteeism related to firm size, we expect the pre-reform differences to decrease with this additional control. Column 3 in Table 4 shows that this is also what happens: When we add controls for the number of substitutes, on top of the other individual covariates, the pre-reform gradient in sickness absence is reduced. The reduction of the estimates is largest among the smallest firms (class 1–3) where the estimates are reduced by 30–40 percent.[28] Hence, the number of substitutes seems to be *one* explanation for why short-term sickness absence generally differs between firms of different size.[29]

The reform estimates remain stable also when we condition on the number of substitutes. This means that the increased sickness absence among firms in wage class 4 is not driven by changes in the substitutability between co-workers. However, given that the lack of substitutes seems to explain at least part of the lower level of sickness absence in smaller firms, it is possible that the lack of reform response among the smallest firms could be due to their more limited availability of substitutes. With few substitutes, the production loss associated with an absent worker may be much larger than the absent worker's own output. If the more generous insurance against high sick pay costs does not fully compensate for this production loss, it is likely to play a less important role for the behaviour in smaller than in larger firms.

Taken together, the results presented in this, and the previous, section suggest that the pre-reform differences in absenteeism between firms of different size (wage classes) are mainly explained by mechanisms taking place at the workplace rather than by the selection of employees. A part of the firm size gradient in absenteeism seems to be caused by differences in the number of substitutes. Furthermore, our results do not indicate that the reform affected firms' recruitment behaviour. Since the estimated increase in sickness absence in wage class 4 does not seem to be driven by changes in the characteristics of the workers employed, it rather seems to be driven by behavioural responses at the workplace, e.g., changed policies, norms, or monitoring.

[27] We do this in a flexible manner, by adding a separate indicator for the exact number of substitutes. The variation at individual level in the number of substitutes is 0–95: 10 percent have 0 substitutes and 40 percent have less than 5.
[28] The t-value for the difference between the estimated parameters in front of wage class 1 between model 2 and 3 is 1.6.
[29] This result is in line with the findings in Hensvik and Rosenqvist (2019). They show that long-term sickness absence rates differ significantly between more and less substitutable employees.

Table 4 Estimated pre- and post-reform effects across wage classes and adding controls for background characteristics and number of substitutes

	(1)	(2)	(3)
Dependent variable: number of days reported sick per individual and quarter			
	Model specification 1	Add individual controls	Add control for number of substitutes
Class 1	-0.420***	-0.489***	-0.342***
	(0.0638)	(0.0642)	(0.0656)
Class 2	-0.266***	-0.350***	-0.261***
	(0.0546)	(0.0522)	(0.0529)
Class 3	-0.141***	-0.170***	-0.105**
	(0.0484)	(0.0464)	(0.0464)
Class 4	-0.168***	-0.180***	-0.161***
	(0.0564)	(0.0550)	(0.0544)
Class1×after reform	-0.0229	-0.0740	-0.0767
	(0.0708)	(0.0712)	(0.0709)
Class2×after reform	-0.0857	-0.0999	-0.109*
	(0.0682)	(0.0658)	(0.0648)
Class3×after reform	-0.0394	-0.0757	-0.0883
	(0.0626)	(0.0616)	(0.0604)
Class4×after reform	0.143**	0.130*	0.136**
	(0.0682)	(0.0670)	(0.0661)
Observations	522,509	517,037	517,037
R-squared	0.035	0.050	0.052
Sector	yes	yes	yes
year×quarter×female× industry f.e.	yes	yes	yes
Outcome mean, class 5	0.941	0.942	0.942

Note: The individual covariates are age-controls linearly and quadratically, indicators for foreign born (outside Europe), education level (two dummies), having children aged 0-6 at home, any use of sickness benefit and disability pension, respectively, during the last 2 years. Number of substitutes are controlled for in a flexible manner by adding separate indicators for the exact number of substitutes. Sector refers to Standard Classification by Institutional Sector 2014, INSEKT 2014, consistent with the European System of National and Regional Accounts, ESA 2010. In this data 25 subgroups divided by ownership and type of activity (for example non-profit or not). Robust standard errors in parenthesis, clustered on firm. *** p<0.01, ** p<0.05, * p<0.1. 1) Mean outcome is measured in pre-reform period for the control group (wage class 5).

6.4 What drives the reform effect for middle-sized firms?

In this section, we further investigate the underlying mechanisms behind the reform response among employees in wage class 4. We first investigate if the reform effect seems to be driven by newly hired employees or employees who have worked longer at the firm. Thereafter, we take a closer look at whether the reform affected the composition of new hires.

To analyse if the reform response differs between new hires and other employees, we split the sample between those who were hired during the last 12 months and employees who have worked

at the firm for at least a year.[30] Table 5 presents the results. We show results for the split sample both without (column 1 and 2) and with individual covariates (column 3 and 4). The covariates included are the same as in Table 4. We perform this analysis using our most demanding (and, thus, most reliable) model specification, i.e., the model that includes workplace fixed effects (model specification 2 in Table 3).[31]

Table 5 Estimated reform effects without and with individual controls and new hires separately

Dependent variable: number of days reported sick per individual and quarter				
	(1)	(2)	(3)	(4)
	No new hires	New hires	No new hires	New hires
Class4×after reform	0.0915	0.264**	0.0915	0.282**
	(0.0624)	(0.121)	(0.0649)	(0.129)
Class 4	-0.142**	-0.137	-0.166**	-0.119
	(0.0684)	(0.116)	(0.0710)	(0.123)
Class1–3×after reform	-0.0781	-0.0585	-0.0903	-0.0314
	(0.0564)	(0.0997)	(0.0560)	(0.0998)
Class 1–3	0.0249	0.0187	0.00246	0.0181
	(0.101)	(0.159)	(0.106)	(0.161)
Observations	447,585	70,591	447,585	68,879
R-squared	0.079	0.178	0.092	0.185
Sector	Yes	yes	yes	yes
year×quarter×female×industry f.e.	Yes	yes	yes	yes
Workplace f.e.	Yes	yes	yes	yes
Individual covariates[1]	No	no	yes	yes
Outcome mean, class 5	0.987	0.619	0.987	0.622

Note: 1)The individual controls are age-controls linearly and quadratically, indicators for foreign born (outside Europe), education level (two dummies), having children aged 0-6 at home, any use of sickness benefit and disability pension, respectively, during the last 2 years. Sector refers to Standard Classification by Institutional Sector 2014, INSEKT 2014, consistent with the European System of National and Regional Accounts, ESA 2010. In this data 25 subgroups divided by ownership and type of activity (for example non-profit or not). Robust standard errors in parenthesis, clustered on firm. *** p<0.01, ** p<0.05, * p<0.1. 1) Mean outcome is measured in pre-reform period for the control group (wage class 5).

The pre-reform estimates for wage class 4 in the first two columns suggest that both groups of employees – i.e., new recruits and employees who have worked longer at the firm – are less absent compared to the same groups of employees in wage class 5 (although the difference for new recruits is not statistically significant). Comparing column 1 and 2, we also see that the reform effect for wage class 4 is entirely driven by new hires. For employees who have worked at the firm for a year or longer, the estimated reform effect is much smaller in magnitude and not

[30] The estimated impact of the reform can be given a causal interpretation if the assumption of parallel trends holds in each of the two sup-groups. Results from estimating model (2) separately for new hires and other employees give us no reason to question this assumption, but as the group of new hires is relatively small the standard errors are rather large for this group.

[31] We get similar results for wage class 4 if we instead use model specification 1, but the magnitude and the precision of the estimates increase when we include workplace fixed effects.

statistically significant. Compared to a pre-reform baseline of 0.482[32], the absence level among new newly recruited employees increases by around 50 percent (0.264/0.482). Hence, the individuals who were newly hired after the reform are absent approximately 50 percent more compared to those who were newly hired before the reform. A comparison of the estimates with and without individual controls suggests that the reform effect among new recruits is driven by behavioural responses rather than selection (the estimates stay very similar), but we can, from the set of controls, of course not rule out the selection mechanism entirely. Again, we observe no indication of changed behaviour among employees in the smallest firms, i.e., wage class 1–3.

In order to analyse the personal characteristics of new hires more closely, we limit the sample to new recruits and replace the outcome variable with indicators of their personal characteristics. If the lower cost associated with sickness absence due to the reform made firms more inclined to employ more 'sick prone' individuals, we may find a positive impact on prior use of sickness benefits or disability pension as well as a higher share of older (> age 55) employees among the new hires. For the other characteristics included there is no clear hypothesis regarding expected changes following the reform. For this reason, we apply an explorative approach and estimate the potential effects on several of the individual characteristics presented in Table 2.

Table 6 presents results for wage class 4 in comparison to wage class 5. We again present results for the full model, i.e., specification 3 in Table 3. The results do not suggest that the new hires are more sick-prone or more likely to have young children, nor do they seem to be selected with respect to wage or education level. They results indicate that they are middle-aged to a higher extent than before the reform: significantly fewer of the new recruits are above age 55.

[32] We get the pre-reform baseline for wage class 4 by subtracting the pre-reform estimate for wage class 4 (-0.137) from the pre-reform baseline for wage class 5 (0.619).

Table 6 Effects on the composition of new hires only

Dependent variable:	(1) Age >55	(2) Age <30	(3) Post sec educ	(4) Upper sec educ	(5) Wage	(6) Child age<6	(7) Any Sick benefit	(8) Any Disability pension
Class 4×after	-0.0713***	-0.00777	-0.00562	0.0482	-0.0598	-0.0264	-0.0217	-0.0247
	(0.0257)	(0.0452)	(0.0294)	(0.0379)	(0.0367)	(0.0386)	(0.0208)	(0.0244)
Class 4	0.0630***	-0.0576	0.0215	-0.0735*	0.00500	-0.0390	0.0390**	0.00635
	(0.0215)	(0.0565)	(0.0303)	(0.0429)	(0.0281)	(0.0337)	(0.0188)	(0.0213)
Observations	61,369	61,369	61,369	61,369	61,369	59,985	61,369	61,369
R-squared	0.197	0.302	0.213	0.274	0.539	0.160	0.833	0.205
Sector	yes	yes	yes	yes	yes	yes	yes	yes
Workplace f.e.	yes	yes	yes	yes	yes	yes	yes	yes
year×quarter× female× industry specific slopes	yes	yes	yes	yes	yes	yes	yes	yes
New hires only	yes	yes	yes	yes	yes	yes	yes	yes
Outcome: mean, class 5	0.0713	0.472	0.115	0.480	10.17	0.205	0.0575	0.00905

Note: 1)The outcomes 1–8: are indicators for age<55, age<30, education (compulsory and secondary, respectively), full time equivalent wage, having children aged 0–6 at home and having used any sickness benefit and disability pension during the last 2 years. Sector refers to Standard Classification by Institutional Sector 2014, INSEKT 2014, consistent with the European System of National and Regional Accounts, ESA 2010. In this data 25 subgroups divided by ownership and type of activity (for example non-profit or not). Robust standard errors in parenthesis, clustered on firm. *** p<0.01, ** p<0.05, * p<0.1. 1) Mean outcome is measured in pre-reform period for the control group (wage class 5).

We conclude that neither the analysis in Table 5 (when adding individual controls) nor this analysis (that explicitly focuses on the characteristic of the new hires in wage class 4) suggest that the reform impact is driven by any important changes in firms' recruitment behaviour. Hence, our results suggest that it is the behaviour, rather than the characteristics, of the newly hired employees that changed following the reform. Our data does not allow for a closer examination of how the reform impacted interactions between managers and employees, but a possible explanation for the estimated reform effect could be a more lenient policy or communication regarding sickness absence (e.g., changes in monitoring or norms), and that new hires are more easily affected by such communication than workers who have worked longer at the firm.

7 Concluding discussion

This paper investigates how firms are affected by a more generous insurance against high sick pay costs. To this end, we exploit a reform of the insurance against high sick pay costs in Sweden in 2015. The reform introduced differential rules depending on firm size when it comes to the threshold for insurance reimbursement, instead of equal rules for all firms regardless of size as was the case before the reform. We observe an increase in short-term sickness absence among workers in middle-sized firms (on average 38 employees) due to the reform. We do, however, not observe any indication of changed behaviour among the smallest firms (on average 15

employees), even though this group received the most generous improvement in insurance conditions in terms of a lower threshold for reimbursement.

The few previous empirical studies on this topic suggest that firm sick pay liability is an important determinant of employee sickness absence: Pertold and Westergaard-Nielsen (2018) find that sickness absence is more prevalent in insured compared to uninsured firms in Denmark, and Fevang, Markussen, and Røed (2014) show that short-term sickness absence rose significantly among pregnant workers in Norway when firms' pay liability for pregnant-related absences was removed. Böheim and Leoni (2020) study the consequences of abolishing a compulsory insurance against sick pay expenses for blue-collar workers in Austria and find that the reform led to a significant decline in absenteeism. Our results are in line with these patterns when it comes to middle-sized firms (on average 38 employees), but not for the smallest firms (on average 15 employees).

To understand why the estimated reform effect varies with firm size (wage class), we investigate potential mechanisms behind the firm size gradient in sickness absence. In line with the previous literature, we document more short-term absence due to sickness in smaller than in larger firms. This gradient seems to be explained mainly by mechanisms taking place at the workplace, rather than by the selection of workers. Our analyses also suggest that the number of co-workers with the same occupation, and who can therefore replace an absent colleague, explains a part of the firm size gradient in sickness absence. If a worker has few substitutes at the workplace, which is often the case in small firms, and there is complementarity between workers in the production process, the production loss due to sickness absence is likely to be much higher and thus more costly for the employer. It is possible that this higher cost is internalized in the behaviour of employees in small firms. Since the insurance – even if made more generous – cannot fully compensate for the production loss that arises when there are few substitutes, it is possible that lack of substitutes also explains why the behaviour in small firms did not change due to the reform.

We find that the increase in sickness absence among middle-sized firms is driven mainly by newly hired employees rather than by employees who have worked longer at the firms. However, our analyses do not indicate that the reform affected firms' recruitment behaviour. For example, the new recruits dot not seem to be more 'sick-prone': they are not more likely to have used sickness benefits or disability pension during the last two years compared to those who were recruited before the reform. Hence, it seems to be the behaviour, rather than the background characteristics, of the new recruits that have changed. One potential explanation behind our findings is that firms communicate their policy or norms related to short-term sickness absence

more leniently when they have access to a more generous insurance, and new hires may be more easily affected by such communication than workers who have been employed at the firm longer.

To conclude, our results suggest that a more generous insurance against high sick pay costs for smaller compared to larger firms reduces the firm size gradient in sickness absence to some extent, but probably not in the way that was intended or in a way that benefits society. The increase in sickness absence among middle-sized firms seems to be driven by behavioural changes among employees rather than by recruitment of more 'sick-prone' individuals. This conclusion highlights the importance of understanding the underlying mechanism for differences in observed firm behaviour when designing insurance schemes to compensate for such differences.

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Appendix

Merging KSju and the Wage and Salary Structure Statistics

KSju is a quarterly survey among firms on reported sick days among employees. Individual information is provided by another annual survey, the Wage and Salary Structure Statistics (WSST). Both surveys are conducted by Statistics Sweden. KSju covers all firms with more than 200 employees in the private sector, while WSST covers all firms with at least 500 employees. Among smaller firms a random sample stratified on sector, industry and size is selected. This procedure is about the same in both surveys, but they are, unfortunately, not synchronized. Our analysis sample include all sampled firms and all their employees in KSju that also show up in WSST. We end up with an annual panel of individuals for which we have quarterly information about days reported sick. Table A1 presents the share from WSST that matches with KSju and the number of firms that match. As expected, the match increases with firm size (since small firms are from two different stratified random samples). We have no reason to suspect any systematic mismatch across years, but a low match a specific year could naturally induce imbalances across years, which may violate the comparison between the smallest (class 1 and 2) and large firms over time.

Table 1A Number of firms in KSju each year and the yearly share also in the Wage and Salary Structure Statistics

	Number of employees:	1–9	10–49	50–99	100–199
Year					
2011	Number of firms	790	410	169	164
	Match KSju and Wage Structure Stat.	0.32	0.59	0.84	0.88
2012	Number of firms	940	468	208	198
	Match KSju and Wage Structure Stat.	0.25	0.52	0.78	0.79
2013	Number of firms	845	397	201	206
	Match KSju and Wage Structure Stat.	0.29	0.56	0.76	0.79
2014	Number of firms	754	382	206	199
	Match KSju and Wage Structure Stat.	0.31	0.54	0.77	0.74
2015	Number of firms	1259	720	272	258
	Match KSju and Wage Structure Stat.	0.32	0.50	0.76	0.74
2016	Number of firms	1175	699	249	220
	Match KSju and Wage Structure Stat.	0.32	0.47	0.73	0.78
2017	Number of firms	1132	750	311	294
	Match KSju and Wage Structure Stat.	0.38	0.47	0.77	0.78
2018	Number of firms	984	664	287	272
	Match KSju and Wage Structure Stat.	0.38	0.45	0.76	0.78

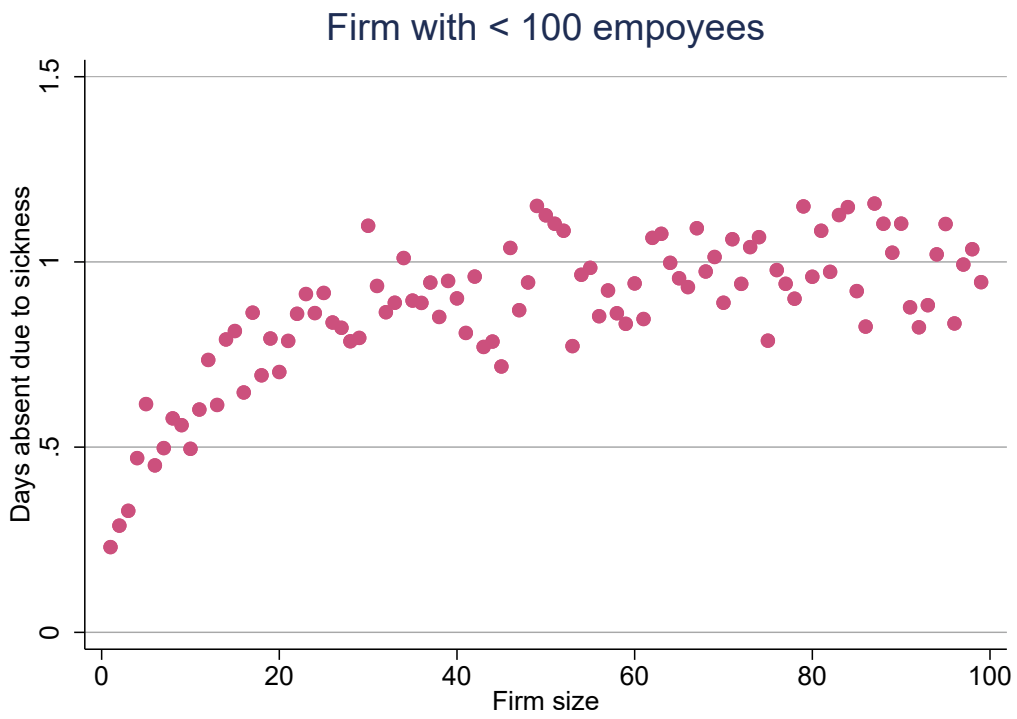
Note: The division of firms into groups depending on number of employees is the one used in the KSju.

Table 2A Descriptive statistics of employees in private sector in Sweden 2011–2014

Wage class	1	2	3	4	5
Age	41.387	43.193	43.074	42.076	42.061
	(13.046)	(12.856)	(12.526)	(12.597)	(12.009)
Female	0.424	0.459	0.467	0.469	0.405
	(0.494)	(0.498)	(0.499)	(0.499)	(0.491)
Upper secondary educ.	0.575	0.533	0.519	0.512	0.492
	(0.494)	(0.499)	(0.500)	(0.500)	(0.500)
Having post-sec. educ.	0.274	0.321	0.347	0.356	0.393
	(0.446)	(0.467)	(0.476)	(0.479)	(0.488)
Wage (SEK)[1]	25 352	27 228	27 986	28 112	31 129
	(7 012)	(7 869)	(8 619)	(8 869)	(11 677)
Annual income (SEK)	244 954	274 210	289 083	290 691	341 051
	(118 974)	(130 101)	(138 664)	(146 575)	(188 350)
Born outside Europe	0.053	0.042	0.036	0.054	0.051
	(0.223)	(0.201)	(0.187)	(0.226)	(0.220)
Married	0.400	0.454	0.456	0.423	0.426
	(0.490)	(0.498)	(0.498)	(0.494)	(0.494)
Children aged 1–6	0.183	0.167	0.185	0.189	0.209
	(0.387)	(0.373)	(0.389)	(0.392)	(0.407)
Any sickness benefit[2]	0.062	0.072	0.075	0.075	0.071
	(0.241)	(0.259)	(0.264)	(0.264)	(0.257)
Any disability pension[3]	0.038	0.028	0.024	0.020	0.013
	(0.192)	(0.165)	(0.154)	(0.142)	(0.111)
Workplace size	5.262	10.912	18.599	31.819	52.039
	(2.639)	(4.432)	(8.374)	(15.395)	(27.836)
Firm size	5.334	12.350	22.438	39.394	92.318
	(2.645)	(3.860)	(7.008)	(11.670)	(39.483)
Subst. at workplace[4]	2.608	4.084	6.877	12.651	18.689
	(1.995)	(3.657)	(6.990)	(13.194)	(19.140)
N	10 986	22 184	51 392	65 740	367 344

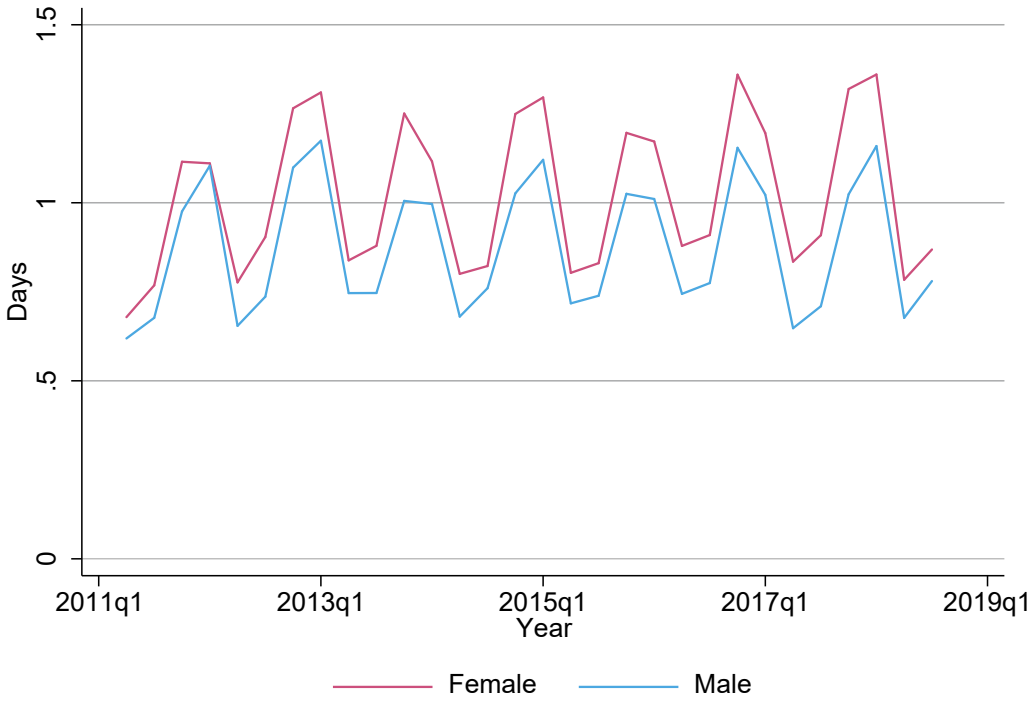
Notes: 1) Reported as full-time equivalent wage. 2) Defined as any sickness benefits during the last 2 years. 3) Formally named sickness compensation (which you only can get if judged permanently disabled and inable to work). Defined as any disability person during the last 2 years. 4) Number of employees with the same occupation code (3-digit level), a Swedish version similar to the International Standard Classification of Occupation (ISCO) at workplace level.

Figure A5 Short-term sickness absence days per quarter depending on firm size



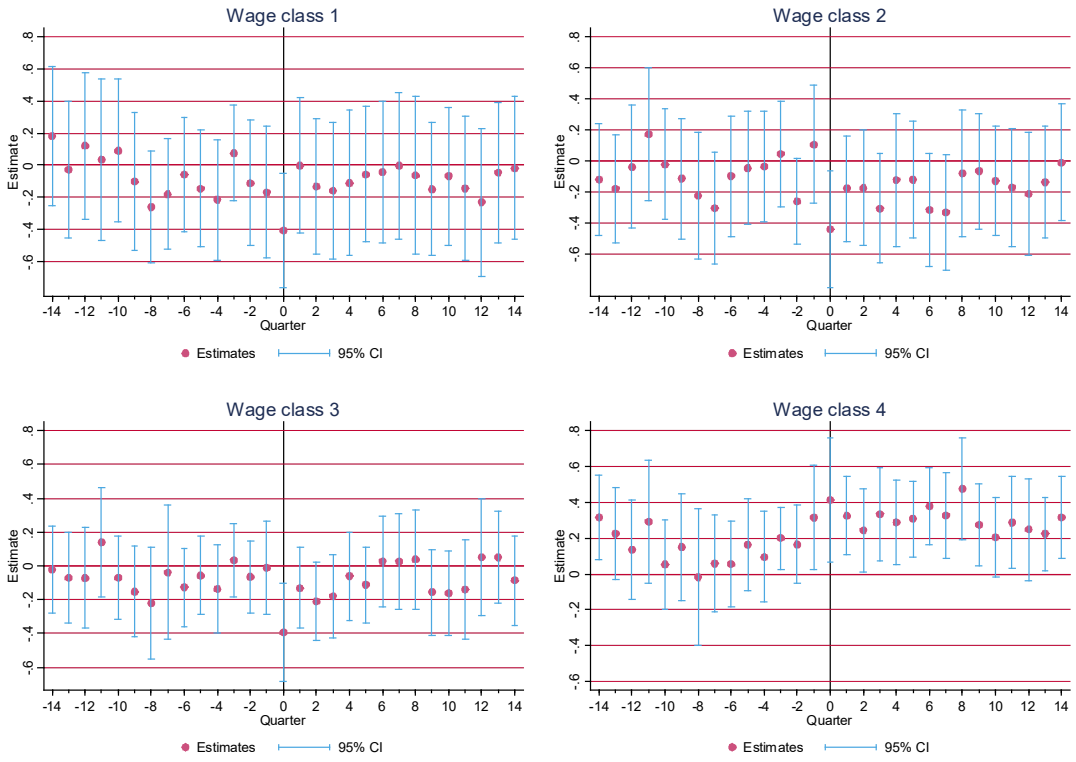
Note: Own calculations based on Wage and Salary Statistics and Short-term Sickness Absence Data (KSJU) from Statistics Sweden 2011–2018. The sample consists of firms with <200 employees and workplaces with <100 employees. Only sickness absence among regular employees (not managers) is included. 'Short-term absence' means that only absence days during the first two weeks of an absence spell are included.

Figure A6 Gender and seasonal variation in the average number of days absent due to short-term sickness per employee and quarter



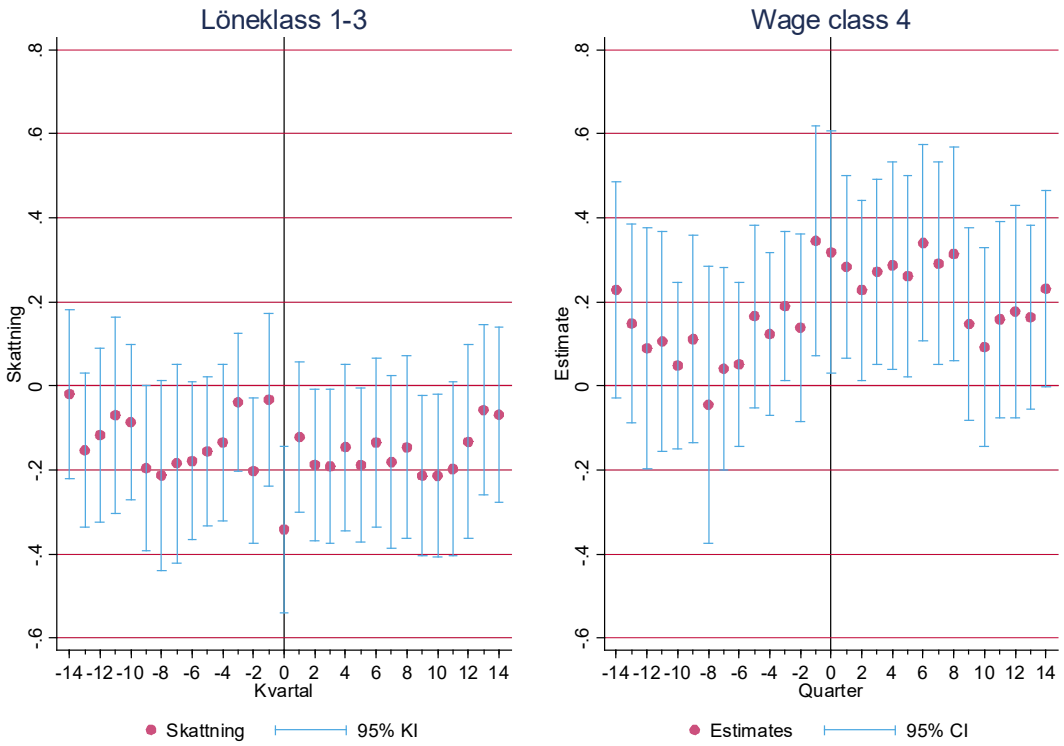
Note: Own estimation based on Short-term Sickness Absence Data (KSJU) 2011q3–2019q1 combined with register information about the individuals' gender. "q" indicates quarter.

Figure A7 Estimated leads and lag effects of the reform for class 1, 2, 3 and 4 separately, in comparison to class 5, on the number of days reported sick per employee and quarter.



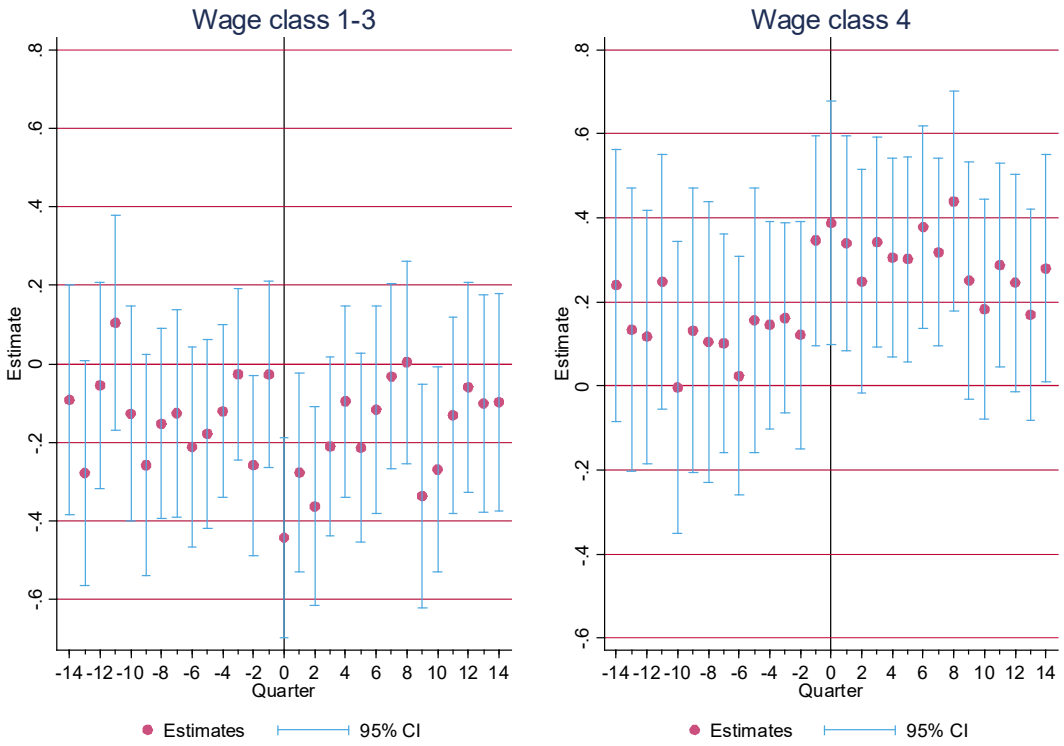
Note: Estimates and confidence intervals from the regression model presented in equation 2 (fixed effects for sector and year×quarter×female×industry) but estimated separately for each wage class 1-4 in relation to wage class 5. The horizontal axis indicates the number of quarters since the first quarter included in our analysis data. The vertical line marks the first quarter in the new regime.

Figure A8 Estimated leads and lag effects of the reform for class 1–3 and class 4, in comparison to class 5, on the number of days reported sick per employee and quarter. Specification with workplace fixed effects.



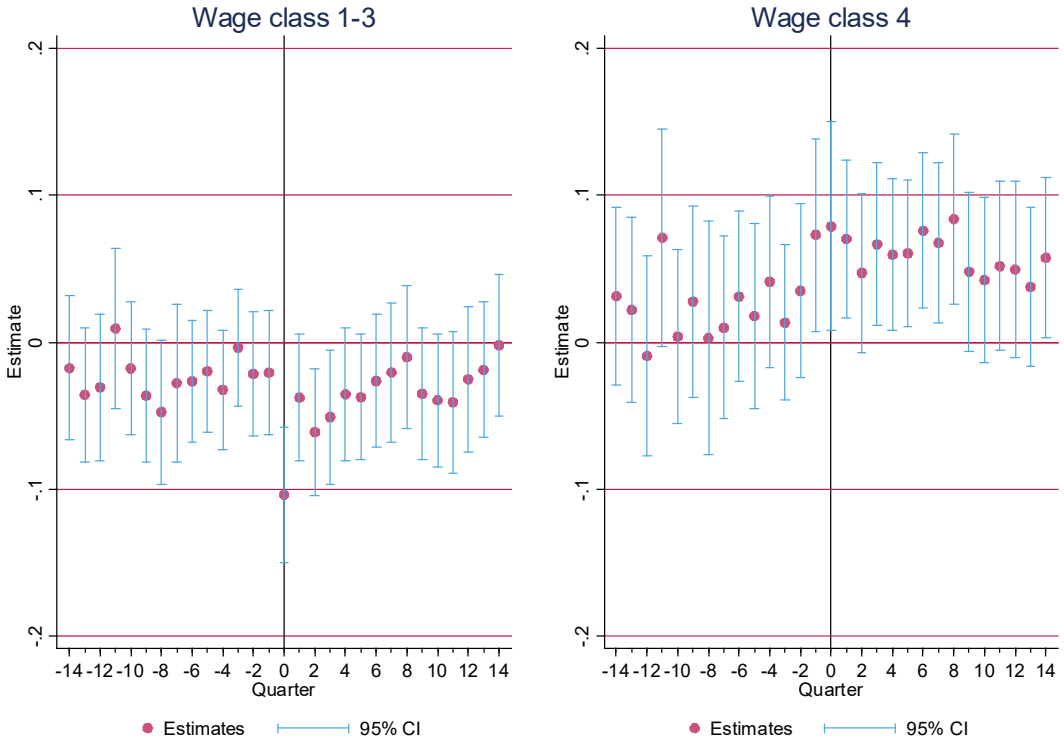
Note: Estimates and confidence intervals for the regression model presented in equation 2 (fixed effects for sector and year×quarter×female×industry). The reference value is the first quarter in year 2014 for employees in firms belonging to wage class 5. The horizontal axis indicates the number of quarters since the first quarter included in our analysis data. The vertical line marks the first quarter in the new regime.

Figure A9 Estimated leads and lag effects of the reform for class 1–3 and class 4, in comparison to class 5, on the number of days reported sick per employee and quarter. Estimates using Poisson regression.



Note: Estimates and confidence intervals from a poisson regression model with fixed effects for sector and year×quarter×female×industry. The reference value is the first quarter in year 2014 for employees in firms belonging to wage class 5. The horizontal axis indicates the number of quarters since the first quarter included in our analysis data. The vertical line marks the first quarter in the new regime.

Figure A10 Estimated leads and lag effects of the reform for class 1–3 and class 4, in comparison to class 5, on the probability of reporting sick per individual and quarter



Note: Estimates and confidence intervals for the regression model presented in equation 2 (fixed effects for sector and year×quarter×female×industry), but the outcome is the probability of having any sick day during the relevant quarter. The reference value is the first quarter in year 2014 for employees in firms belonging to wage class 5. The horizontal axis indicates the number of quarters since the first quarter included in our analysis data. The vertical line marks the first quarter in the new regime.