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Working Paper Effects of compressed work schedules on sickness absence

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Effects of Compressed Work Schedules on Sickness Absence



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Effects of Compressed Work Schedules on Sickness Absence

Elisabeth Fevang^{*1}, Andreas Fidjeland², Karen Hauge¹, and Otto Lillebø²

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Abstract

Steadily increasing demand for personnel has led health care providers to seek more efficient uses of the healthcare workforce. One potential solution is to find ways of organizing work schedules that are more attractive and sustainable for workers. The primary objective of this article is to investigate how compressed work schedules (CWS), a scheduling practice with fewer but longer shifts, affects sickness absence. We do so by leveraging a nation-wide retrospective survey mapping the use and changes between different work schedules in the Norwegian municipal health and care sector, coupled with precise employee-level registry data, to conduct a quasi-experimental analysis of the impact of introducing CWS at the workplace on employees. Our preferred empirical approach involves leveraging observations of employees at workplaces that introduce CWS and workplaces that do not, in a differences-in-differences design. We find no significant effects on sickness absence. The results are robust to different definition of sickness absence.

JEL Codes: I10, J22, J28, J45, J81

Keywords: Healthcare workers, health and care services, shiftwork, sickness absence

^{*} Corresponding author, <u>elisabeth.fevang@frisch.uio.no</u>. We are thankful to Knut Røed, participants at WINC 2023 and the national social security research seminar ("Trygdeforskningsseminaret 2023") for helpful comments and suggestions. We also want to thank Lillebråten, Ingelsrud and Bernstrøm at the Work Research Institute (AFI) for cooperation on collecting data from the establishments. In addition, we are thankful to the Norwegian Union of Municipal and General Employees, Delta and the Norwegian Nurses Association for contributing input to the design of questions in the survey and the procedure for obtaining respondents, as well as respondents who have responded to the survey. Funding from the Norwegian Labour and Welfare Administration (NAV), project number MAB2821, is gratefully acknowledged. Data made available by Statistics Norway have been essential for the research project. The study is pre-registered at the Open Science Foundation (OFS). The pre-analysis plan can be found here: https://osf.io/4wdqc/registrations.

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1. Introduction

The services offered by health and care providers typically require round-the-clock staffing, making the need for shift work unavoidable.¹ Although the flexibility offered by shift work can be attractive for some employees (Stone et al., 2006; Griffiths et al., 2014), an extensive literature associates nonstandard working hours with a range of health issues (Bolino et al., 2020; Wu et al., 2022).² To improve working conditions for their employees and utilize scarce employee resources efficiently, several healthcare providers have reorganized work schedules within this sector or are considering such reorganizations. One way to reorganize work schedules is to implement *compressed work schedules* (CWS). CWS organizes employee work hours in fewer, but longer shifts, allowing for more days off and longer rest in between workdays (Bambra et al., 2008). For example, using 12-hour shifts as opposed to the traditional 8-hour shifts, the number of shifts needed per day reduces from three to two, and the number of days each worker is present in a typical work week decreases from 5 to 3. This way of organizing the work schedule has become increasingly more common in the US and Europe (Dall'Ora et al., 2022).

In this study we analyse how implementing CWS affects sickness absence among employees. Sickness absence is an important indicator for efficient use of resources. For example, sickness absences present substantial direct and indirect costs to employers, including wages for sick and substitute workers, training costs, and in addition to the increased workload on remaining staff, which can compromise care quality due to decreased continuity, decreased time per patient and potentially lower competence of substitutes. The rate of sickness absences might serve as a proxy for employee health and wellbeing, highlighting the importance of favourable working conditions (although low absence rates do not always correlate with positive workplace environments, but rather, e.g., staff composition). The motivation behind implementing CWS has been related to reducing staffing costs (due to fewer overlapping hours), increasing continuity and quality of care, improving nurse recruitment, and reducing turnover (Dall'Ora et al., 2022).

It has been presumed that CWS will be beneficial for employee health, for example due to the fact that more rest between shifts might abate some of the detrimental health effects of shift work (Smith et al., 1998; Vedaa et al., 2019). However, CWS might also improve working conditions for healthcare workers in several other ways. First, decreasing the number of days worked implies lower costs and less time spent on commuting, which may reduce the burden of work. Second, increasing the length of each shift might also allow workers to complete more complex and varied tasks (Knauth, 2007), potentially leading to higher job motivation and satisfaction (Hackman & Oldman, 1976). Third, longer shifts also reduce the number of shift handovers needed, which are not only preferred by most patients, but have also been shown to potentially reduce margins of error (Knauth, 2007). Not the least, fewer working days could allow for more flexibility in balancing work and home life, increasing the time spent with family and friends (Richardson et

¹ Although shift work is particularly common in the health- and care sector it is also widespread elsewhere in the labour market. For context, 21% of all workers in the EU, and 32% in the United States, are working a shift schedule that deviate from the standard workweek (Eurofund, 2017; ILO, 2022). Presumably, our study might therefore be relevant for stakeholders in many other sectors as well.

 $^{^2}$ For example, shift work has been found to be associated with negative health outcomes such as sleep disturbances and fatigue (Shao et al., 2010; James et al., 2017), disrupted eating habits (Souza et al., 2019), and chronical diseases like diabetes type II and coronary heart disease (Kecklund and Axelson, 2016; Torquati et al., 2018). Moreover, there are studies linking shift work to poor mental health outcomes, in particular night work (Torquati et al., 2019). In addition to physiological and psychological disruption, shift work also involve social disruption as it involves

[&]quot;unsocial" working hours, potentially influencing work-life balance. Several studies have found that shift-workers report more family- and social life problems (ILO, 2022; Albertsen et al., 2007), which may influence health and wellbeing among employees (Eby et al., 2005; Jacobsen & Fjeldbraaten, 2018).

al., 2007; ILO, 2022). In sum, such effects would suggest the possibility of substantial organizational gains in terms of higher job satisfaction, fewer absences, and less attrition (Baltes et al., 1999).

It is not, however, obvious that working longer shifts is preferable to shorter shifts. If the marginal detrimental effect of another hour worked on employee health increases with shift length, longer shifts could lead to worsened health outcomes. Moreover, some studies have suggested that worker fatigue and increased risk of accidents could be an issue with longer shifts, with implications for patient safety and the quality of care provided (Rogers et al., 2004; Stimpfel & Aiken, 2013; Griffiths et al., 2014; Dall'Ora et al., 2016). Several studies suggest that extended shift length reduces sleep quality among employees, with potentially adverse implications for their health (see Knauth, 2007, and references therein). As such, introducing compressed schedules might increase absenteeism, but the evidence on such effects remains inconclusive with studies finding both reductions (Vedaa et al., 2019; Peutere et al., 2021) and increases in absences (Ropponen et al., 2018; Dall'Ora et al., 2019; Larsen et al., 2020; Rodrigues Santana et al., 2020).

These conflicting evidence of effects make the impact of CWS on healthcare workers ambiguous. Indeed, the existing evidence on the effects of CWS is best described as mixed and inconclusive (Bendak, 2003; Knauth, 2007; Merkus et al., 2012; Dall'Ora et al., 2016; Deery et al., 2017; Vedaa et al., 2019), with most studies limited to cross-sectional designs with a limited number of workplaces and employees. We extend this literature by applying a credible and rigorous research design that plausibly allows us to uncover estimates that have a causal interpretation. Specifically, we use a staggered difference-in-difference design in which we rely on variation in the timing of when CWS policies are introduced in a particular healthcare unit. This design allows us to recover the effect of introducing CWS in the workplace on important employee outcomes under the assumption that the exact timing of when CWS is introduced is independent of the trends in the outcomes we focus on.

We build our analysis around a novel dataset from the municipal health and care sector in Norway (Bernstrøm et al, 2022). To overcome the lack of information on work time scheduling in central data registries we conducted a survey directed at unit managers wherein they were asked to provide us with information about their unit's work scheduling policies for the period 2018-2022 as well as any changes that had been made in that period. The following services were surveyed: nursing homes, home care services, and various forms of assisted living facilities. We match this establishment-level data to employee-level registry data provided by Statistics Norway. The latter gives us information on employees' work conditions, such as workplace, contracted working hours, earnings, sickness absence, and a range of demographic indicators.

According to our nationwide establishment survey, the number of establishments with CWS has doubled within the period 2018-2022 (Fevang et al., 2024). Common arguments in favour of implementing CWS among workers are increased continuity at work, fewer days/weekends at work, reduced travel time as well as time spent commuting. Arguments among employers are the potential for increasing the share of workers working full time and improved recruitment. Common arguments against CWS are its potential of being harmful for worker health, and potentially increasing the risk of errors. Although this type of work schedule is increasingly popular, there is still some scepticism among both managers and union representatives. Hence, more research on the consequences of introducing CWS is called for.

We use these data to investigate how CWS affect sickness absence at the establishment level. We find that introducing CWS has no significant effect on sickness absence at the establishment level. The results are robust to different measures of sickness absence.

Our study bridges knowledge gaps across two significant strands of literature, providing insights into both healthcare workers' labour supply and the broader economics of work hour organization. We provide evidence on the effects of workplace policies on health sector employees' sickness absence, which is an area of growing concern due to the high incidence in the sector (Krane et al., 2014; ILO, 2023). While tailoring scheduling practices more towards employees' preferences have been proposed as potential solutions for improving job satisfaction and retention among healthcare workers (Lum et al., 1998; Shield & Ward, 2001), the literature lacks comprehensive causal analyses on the effects of compressed work schedules on employee outcomes (Deery et al., 2017), highlighting the need for rigorous evidence before widespread implementation of such policies. Additionally, our research contributes to the line of research on the effect of how scheduling of work hours affects productivity and efficiency, emphasizing the underexplored aspect of how the distribution of work hours affects workplace outcomes beyond immediate productivity, such as the long-term effects of weekly shift schedules. While studies like Spiegel et al. (2014) and Amendola et al. (2011) suggest longer shifts may improve productivity, others (Brachet et al., 2012; Pencavel, 2015; Collewet & Sauermann, 2017) show a productivity decline in extended shifts' latter hours due to worker fatigue. Our paper provides new insights by examining the shortto medium-term outcomes of shift schedules, thereby complementing existing research focused on workday-specific hours and productivity.

2. Institutional settings

2.1 Health and care services

The Norwegian health and care sector is public, funded through taxes and is based on a principle of universal access. The size of this sector has increased during the last decades, and as of 2021 15.4% of all employees in Norway were employed within the health and care sector (see NOU 2023:4, p.26).³ While most hospitals are managed and financed by the central government, local authorities at the municipal level are responsible for providing primary health care. The municipal health and care services include services like nursing homes, home-based services, public physiotherapy service, substance abuse and mental health services and the general practitioner system (NOU 2023:4, p.55). In this paper, we focus on institutions within the municipal health and care services, and housing facilities.

³ The health and care sector are here including the SIC-codes 86, 87 and 88.1. For an explanation of the Standard Industrial Classification 2007 (SIC 2007), see Statistics Norway (2024).

To provide insights into the Norwegian healthcare context, we present some descriptive statistics (as of May 2022) for all employees in Norway, for employees working in the services we focus on in this study, and for employees working in the establishments covered by the survey

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	All employment records	Health- and care services*	Health- and care services covered by the survey*)
Ν	2,600,933	205,448	83 979
Mean age	42.5	41.9	41.8
Women (%)	49.1	83.1	85.2
Shift-work (%)	14.8	81.5	86.4
Contracted working			
hours			
% working full	67.9	28.7	29.5
Time			
Mean full-time equivalent (%)	81.8	62.2	63.6
Sickness absence	1.8	2.8	2.8

Table 1. Descriptive statistics employment records, May 2022

Source: Authors own calculation based on data on all working individuals registered as residents in Norway, aged 20-70 and registered as employed in May 2022 according to the employer/employee register.⁴ *We include nursing homes, home-based services and housing facilities being defined as 86.901, 87.1, 87.2, 87.301, 87.302, 87.303, 88.101 Standard Industrial Classification 2007 (SIC 2007), see Statistics Norway (2024).

According to Table 1, around 8% (205,448 out of 2,600,933) of the entire work force is employed within our focal institutions (nursing homes, home-based services, and housing facilities). Compared to the working population at large, these institutions have a large proportion of women and shift-workers. While 15% of the entire working population work shift, a significant 82% of the study population do so. In addition, a majority (83%) of the study population are women. Compared to the general population, the incidence of sickness absence is high and the share of employees working full-time is low for the sector of interest. The mean calendar days of sickness absence within this sector is 2.8, being approximately 35% higher than the working population in general. In addition, only 29% work full-time and the average full-time equivalent per employee is 62%. We also add a column with the sample of establishments which have answered the survey.

2.2 Regulation of work hours

The services in our study require around-the-clock staffing, thereby making it necessary to organize the work in shifts. Shift work can be structured in several ways.

Traditional work schedules within the health and care sector in Norway. Traditionally, the day is divided into three shifts with day, evening and night shifts. The day and evening shifts usually last 6-8 hours while the night shifts are longer (9-10 hours) (Garde et al., 2019). In contrast to work

⁴ Some of the employments records in the employer/employee register is not active and some is not registered with any contracted working hours. These are excluded from the sample. The sample criteria are described in detail in table A1

schedules where employees work day, evening *or* night shifts throughout a given period (e.g., a week), the health and care sector in Norway operate rotating shift schedules in which the same employee can alternate between different shifts within the same week. There are two common approaches for structuring rotating shift schedules. For the first approach, referred to as a two-shift rotation, a separate staff always works night shifts, and the remaining staff rotates between working day and evening shifts. In the second approach, the three-shift rotation, the staff rotates between working day, evening and night shifts. The rotating schedule last for a period of 6-12 weeks (Moland & Bråthen, 2019).

The Work Environment Act §10-4 regulates work hours in Norway. While a regular work week consists of 37.5 work hours, the work week for shift workers is regulated to be 35.5 hours or down to 33.6 hours⁵. Shift workers have the right to receive a work plan that includes workdays and hours. The main rule is that working hours cannot exceed 10 hours within 24 hours, and not exceed 48 hours within 7 days. Shifts lasting longer than 10 hours require an agreement with the local union, and shifts longer than 12.5 hours require an agreement with central union representatives. Agreements of extended shifts must always be voluntary for the worker, and the agreements must consider the health and welfare of workers in addition to being discussed with, and be recommended by, the employee representative. Extended shifts (12 hours or more) make it possible to reduce the number of shifts per day from three to two. The process of changing the type of shift schedule used within an establishment, such as implementing CWS, is a time consuming process, and can take around 1-2 years when including time for planning, preparation, designing the new schedule, discussions with employees, necessary applications to union representatives, small-scale testing and implementation. As mentioned above, extended shifts require an agreement with local or central union representatives, and there exist several examples of the unions rejecting such applications. Rejections have been based on protecting users and workers, with arguments of avoiding possible harmful effects on employee health and avoiding increased risk of errors.

Within the traditional work schedules for health workers in Norway outlined above, employees usually work every third weekend (Ingstad, 2016). The traditional work schedule combined with round-the-clock operations and limited opportunities to reduce staffing during weekends, has led to extent use of part-time positions to fill "gaps" in the schedules. By introducing CWS (including 12-14 hours shifts), it is possible to reduce the frequency of weekends worked to every fourth weekend and establish a culture of full-time work (Ingstad, 2016). Why the traditional work schedule leads to extensive use of part-time work can be mathematically illustrated (see ibid); Full-time employed shift workers with traditional length of shift usually work 15 days within a three-week period. Two out of these 15 days are weekends, meaning that 13% of the working days are weekends. However, within a week, two out of 7 (28%) days are weekends. As the patients' need for care is constant during the week, 28% of the working time should be performed during weekends. Hence, the work schedule is incompatible with full-time work for all employees. In fact, it may be possible to reduce activity to some extent during weekends, such as not offering baths during weekends, no complete bed change or no doctor's visits. If 20% (instead of 28%) of

⁵ Employees with a three-shift schedule may be entitled to reduced working hours down to 33.6 hours depending on the extent of job during weekends, evenings and Sundays.

the working hours need to be performed during weekends, the traditional work schedule still includes too few working hours during weekends to be compatible with full-time work for everyone.

2.3 Sickness insurance

Norwegian sickness insurance is compulsory and regulated by The National Insurance Act §8, providing coverage for all employees who have worked with the same employer for a minimum of four weeks. It offers 100% coverage from the first day of absence.

Employees have the right to self-certified sick leave, which means that for short spells of absence they can inform their employer they are unable to work due to an illness or injury without providing a medical certificate.⁶

During the initial 16 days of absence, the employer covers the cost of the absence (known as the employer period), while spells of absence exceeding this initial period is covered by the social insurance scheme administered by the Norwegian Labour and Welfare Administration.

Upon presenting a physician's certificate, individuals on sick leave are eligible for a 100% wage replacement and job protection for up to one year. The 100% replacement rate is however capped at six times the base amount in the Norwegian pension system, in 2024 roughly equal to $\in 63\ 000$. However, all public sector employees receive a 100% replacement rate regardless of their income.

3. Data and definition of variables

Our analysis utilizes data from two sources: a survey of establishments in municipal nursing homes, home-based services, and assisted living facilities, and administrative records from Statistics Norway. We have merged these datasets at the establishment level. The following sections detail each source independently, followed by an overview of our sample selection methods and data linkage process.

⁶ The number of calendar days of self-certified sickness absence an employee is entitled to, varies between employers, commonly 3 days maximum 4 times a year, or 8 days with a maximum of 24 calendar days within a year, depending on employer agreements. All employees within public sector were entitled to the latter arrangement until January 2019 (Proba, 2016). 99 percent of the establishments who responded to the survey belongs to the public (municipal) sector (Bernstrøm et al., 2022). From January 2019 employers could choose whether their employees are given an extended right to self-certificate absence spells or not.

3.1 Shift-work survey among establishments in the municipal health- and care sector All establishments in Norway registered in the Central Coordinating Register for Legal Entities (Entity Registry) that met the SIC code criteria outlined in Table 1 (excluding certain types such as cantinas, catering services, offices, and administration) were invited to participate in a web-based survey. In May 2022, we emailed survey invitations to all 356 Norwegian municipalities, asking them to forward the invitations to relevant establishments in their municipality. We also encouraged establishment managers to respond. Each email included a list of these establishments, as per the Entity Registry. We customized the invitation letters for small, medium, and large municipalities and sent up to two reminders to establishments that hadn't responded by May-June 2022. The survey remained active until the following August. For more details on the survey, see Bernstrøm et al. (2022).

From our initial list of 4,178 establishments, we received 1,587 survey responses. Of these, 40 responses were excluded due to lack of consent for data linking. We identified 8 responses representing multiple establishments, leading to 23 duplicates. We also removed responses that were blank, consisted solely of 'don't know' answers, or stated that shift work was not part of the work schedule. This resulted in 1,541 valid responses. For 85 establishments, we received multiple responses; these were combined into 196 aggregated responses to represent these establishments accurately. Consequently, we had valid responses from 1,427 establishments, corresponding to a response rate of 34.1%. For a detailed account of how certain establishments were excluded from the analytical sample, we refer to Table A2 in the appendix. When responding, it was possible to answer, "don't know" or "other". In our analysis we exclude "don't know" and "other" answers, resulting in the number of valid responses varying from 1,330 to 1,423, being lowest in 2018 and highest in 2022. As seen in the last column of Table 1, the final sample is representative of the study population. The survey was targeted towards establishments with shift workers, and naturally has a higher rate of shift-workers than the full study population. Except from the higher rate of shift work within the survey sample, there are only minor differences between the survey sample and the sector as a whole.

The survey included questions about the type of work schedule as of May 1st for each of the five years between 2018 and 2022. This provided us with an overview of the situation each year and any changes during the specified period. Nevertheless, the survey does not provide information about the exact timing of when work schedules were changed. Establishments reporting extended shifts were asked some follow-up questions such as whether employees work extended shifts throughout the week or only during weekends, the length of extended shifts and whether the extended shifts were day or night shifts (see Appendix Table 3 for an overview of the survey questions used in the analyses in the paper, and Bernstrøm et al., 2022 for an exact definition of all the question asked in the survey). The survey does not include any information about the reasons for why establishments have or have not implemented changes in their work schedules, and in case who has initiated the implementation.

Similarly as all self-reported data from retrospective surveys, there are certain biases that potentially can influence our data. First, selection bias can be a potential concern. However, as shown in Table 1, last column, the establishments who have answered the survey are representative

of the health and care services in Norway. Second, in some surveys, social desirability bias can be a potential concern. However, in this survey it is not clear whether any answers could be considered more socially desirable than others. Third, recall bias is a potential concern. Indeed, this is a potential bias, as managers in the establishments are asked to report the shift schedules in place in May each year for several years back in time. Managers might not recall perfectly which shift schedules was used in the past, or they might not have worked there in the past, and we can expect that this potential bias increases backwards in time. However, we do not believe that this leads to a systematic underestimation or overestimation of the extent of CWS, but that information further back in time is more imprecise than information given in the year the survey was conducted. This can imply more imprecise results compared to what would have been possible with prospective data.

3.2 Administrative data

We supplement our survey data with high-quality registry data from Statistics Norway from the period 2018-2022. Every resident and establishment in Norway are assigned a unique encrypted personal number. These identifiers enable us to link individual workers to their respective employer establishments together with information from various administrative registries, including social insurance and population registers. Consequently, we can link the de-identified data records of every worker employed by the establishments that took part in our survey.

For each employee, we collect demographic characteristics (including gender, age, marital status, number of children), educational attainment and data that pertains to their employment record. The employer-employee register provides detailed information about each individual's work history, including earnings, start and end dates for specific jobs as well as type of workplace (group of service), weekly contracted working hours and type of work (shift work or not). We also include information about all certified sickness absence spells (see section 2 for a more thorough description about rules regarding sickness absence in Norway).

3.3 Definition of outcomes and compressed work schedules

Definition of compressed work schedules. We define compressed work schedules (CWS) as work schedules that consist of extended shifts (of 10-23 hours) throughout the week (both weekdays and weekends).

Our survey shows that many establishments report having extended shifts, but only during the weekends. We consider extended shifts throughout the workweek and extended shifts only during weekends as two quite different ways of organizing shift schedules. Extended shifts only during weekends will imply fewer days at work but to a much lesser extent compared to extended shifts throughout the week. While extended shifts throughout the workweek will imply around 8 fewer workdays per month, extended shifts on weekends imply around one fewer workday per month (in case of fulltime work). We, therefore, concentrate our analysis on extended shifts throughout the week.

To identify establishments with compressed work schedules, we combine responses from two questions in the survey. The first question inquires how the working day is organized, where respondents could select from the following options: 1) two-shift rotation schedule (some employees rotate between working day and evening, while other employees permanently work night shifts), 2) three-shift rotation schedule (shift workers rotate between day, evening and night shifts), 3) extended shifts (10-23 hours), and 4) round-the-clock shift (24 hours or more). The second question asks whether employees work extended shifts throughout the week or only on weekends. For an exact definition of the questions asked see Table A3 in appendix. We classify an establishment as having compressed work schedules if the respondents answer extended shifts (option 3) in the first question above, and answer having extended shifts throughout the week in question 2 above. We define CWS at the establishment level implying that within an establishment defined as having CWS, there also will be workers who have other schedules. Establishments operating on a 24-hour shift basis (round-the-clock shift) are omitted from both the treatment and control groups.

Outcome variable. Our outcome measure is sickness absence.⁷ We measure sickness absence at the individual level and aggregate it to the establishment level. We use three different measures of sickness absence: 1) Total number of calendar days absent in May each year, 2) Long-term absence measured as being absent throughout May each year, and 3) Total number of calendar days absent throughout the calendar year.⁸

The survey includes questions regarding what type of work schedule each establishment has in May every year. As the survey informs us about the shift schedule in place in May each year, and not on the exact timing of the change in schedule, we only know with certainty what type of shift schedule each establishment has in May. Note that we measure *calendar* days, and not *working* days, as potential working days are fewer for workers with CWS compared to workers with traditional length of shifts (Vedaa et al., 2019). As there is high turnover within this sector, our preferred measure of sickness absence is therefore the total number of calendar days absent in May each year.

Our two other measures of sickness absence capture long-term absence and sickness absence over a longer time horizon, and is used in robustness checks. A common definition of long-term sickness absence, is continuous absence spells for a period of four weeks or more (Larsen et al, 2020). Our second measure of sickness absence measures long-term absence, and is therefore defined as a dummy variable for being absent throughout May each year. Finally, our third measure of sickness absence captures both short- and long term sickness absence over a longer time horizon. We define this measure as the total number of calendar days with sickness absence throughout the calendar year. If the shift schedule leads to more or less sickness absence, but not necessarily immediately, this measure can pick up such effects. However, this variable has other weaknesses making it a more noisy measure. First, as we mentioned above we do not know the exact timing of changes in work schedules. Second, some employees change workplace within a calendar year meaning that their sickness absence may be related to absence elsewhere. Third, some employees

⁷ Our preregistered outcome variables are sickness absence and turnover. In this article we focus on sickness absence only.

⁸ We count calendar days of sickness absence irrespective of whether the employees are fully or partly on sick leave

start or quit working during a year meaning that they are not at risk of sickness absence throughout the year.

3.4 Descriptive statistics

In this section we provide some descriptive statistics, starting with the establishment survey. Some establishments have several types of shift schedules simultaneously. The most common way to organize shifts within our sample is a two-shift rotation schedule. Between 80 and 90 per cent of the establishment managers report having this type of work schedule in 2018-2022. Extended shifts (10-23 hours) have become steadily more widespread. During the study period 2018-2022, the share of establishments reporting extended shifts has more than doubled, from 21 to 43%. 24-hours shifts are quite rare within this sample and are mainly concentrated within assisted living facilities for persons with intellectual disabilities (Fevang et al., 2024).

Figure 1 displays the share of establishments who in the survey report having employees who work extended shifts. A large share of establishments report having extended shifts on weekends only, which is probably related to filling "gaps" in the work schedules during weekend (see Ingstad, 2016). Having extended shifts throughout the week (CWS) is much less common, but the fraction of CWS has also doubled. In 2018, 6% of the establishments reported having CWS which increased to 12% in 2022.



Figure 1. Share of establishments reporting extended shifts

Table A4 in the appendix provides an overview of establishments reporting extended shifts in May 2022. Most of the establishment report having extended shifts combined with a two-shift or three-shift rotation schedule. Only 14% had extended shifts only. For employees working extended shifts, establishment managers report that the most common frequency of weekends was every fourth weekend. The establishment managers were also asked about the frequency of weekends among workers not working extended shifts. In this group the most common frequency of

weekends was every third weekend (Fevang et al., 2024). 95% of the extended shifts last between 12 and 14.5 hours, where most of them (70%) lasts 12-12.5 hours. Shifts of this length are compatible with dividing the 24-hour day into two shifts rather than three. The survey also included a question about the share of workers within each occupation working extended shifts. Here we present numbers for the largest occupations within this sector; registered nurses and auxiliary nurses. Around 40% of the establishment managers reported that more than 60% of registered nurses and auxiliary nurses worked extended shift (for details, see Appendix Table A4).

Table 2 gives a description of the sample where the establishment survey is merged with registry data as of May 2022. The sample is restricted to shift-workers aged 20-70.⁹ Mean age in this sample was 41 and the share of females 85%. Almost 60% had formal health and social care education. When it comes to type of services, nursing homes account for 43% of the employees in this sample, home based services (29%) and the rest was employed within various assisted living facilities. Finally, we present the mean and the distribution of our outcome variable. Mean calendar days of sickness absence in May 2022 was 2.8 where 15% is absent for 1 day or more and 5.7% is absent throughout the whole May.

Table 2. Descriptive statistics. Establishment survey combined with register data. May				
2022				
Number of employment records	72,291			
Number of establishments	1,427			
Mean age	41.0			
Distribution of age (%)				
20-29	27.9			
30-39	21.3			
40-49	18.5			
50-59	20.1			
60+	12.2			
Women (%)	85.2			
Group of health- and social care professional (%)**				
Health- and social care professionals with (at least) bachelor degree	26.7			
Health- and social care professionals with diploma	32.4			
Employees without formal health and social care education	40.9			
Type of service (%)				
Nursing homes (ISIC-codes 87101, 87102, 87201, 87301)	42.5			
Home-based services (ISIC-codes 86901, 88101)	29.2			
Assisted living facilities for people with intellectual	13.2			
disabilities (ISIC-code 8/203)	15.1			
Assisted living facilities for elderly and people with functional disabilities (ISIC-codes 87202, 87302, 87303)	15.1			
Mean calendar days of sickness absence	2.8			
Calendar days of sickness absence (%)				
0	84.5			
1-7	4.5			

⁹ 87% in the sample of establishments are shift-workers (see table A6).

8-30	4.5
31 (the entire month)	5.7

*The sample criteria is described in Table A5, **Definition of health-and social care professional is described in table A6

4. Empirical strategy

In this section we describe our empirical framework. We will discuss the standard event study methods and their limitations in our context. We exploit the recent advances in the difference-indifference literature to overcome these limitations and discuss how they apply to our settings.

A common method to estimate the impacts of changing treatments involves utilizing an event study regression, which incorporates both unit and time fixed effects. In the specific scenario we are examining, the equation used to assess the effect of changing to CWS on the outcomes of interest, is:

$$Y_{it} = \sum_{j=-3}^{3} \beta_j 1(CWS)_{ij} + \mu X_i + \delta_i + \gamma_t + \varepsilon_{it}$$
(1)

where Y_{it} is the outcome measure for unit *i* in year *t*. In our setting a unit is an establishment. The indicator variable $1(CWS)_{ij}$ tracks the year surrounding the change to CWS for unit *i*, in our case with indicators for the set $j \in \{-3,3\}$. In each case, the indicator takes the value 1 for units exposed to the treatment, *j* periods removed from the initial treatment exposure, and 0 otherwise. X_i is a vector of covariates and δ_i and γ_t are unit and year fixed effects respectively. The coefficient of interest is β_j , representing the Average Treatment Effect on the Treated (ATT), that is, the average effect of the treatment among units exposed to a switch to CWS. The estimated $\hat{\beta}_j$ is interpreted as the average effect of the treatment on the outcome of interest *j* periods following the switch to CWS. The primary assumption in such an event-study design is that without the treatment, the counterfactual outcome trajectory for the treated group would align with the actual trajectory experienced by the untreated group (those not participating in the treatment), referred to as the parallel trend assumption.

A limitation of this approach is that it might not yield precise results in scenarios where treatment effect heterogeneity is likely, that is, where the effects vary across different establishments or evolve over time. Particularly, traditional event studies tend to use units treated earlier as control groups for those treated at a later stage, a method that might lead to biased estimates due to the dynamic nature of the treatment effects. To overcome this limitation, we utilize the difference-in-differences and event study procedure introduced by Callaway & Sant'Anna (2021) (see also Baker et al., 2022, and Roth et al., 2023, for an overview of recent developments in this methodology). This approach acknowledges the variability in treatment timing, thus effectively capturing dynamic treatment effects. It facilitates the calculation of the group-specific Average Treatment Effect on the Treated (ATT(g,t)), signifying the group-time average treatment effects for a specific period t for the group of units first treated at time g. For example, in our context, we observe switches to CWS in the years 2019, 2020, 2021, and 2022. We can therefore group the

treated establishments into treatment cohorts *g*, consisting of the sets of establishments making the switch in the same year. Additionally, we monitor the periods leading up to their transition to CWS (referred to as years *t*). Hence, the ATT is defined as follows:

$$ATT(g,t) = E[Y_t^*(1) - Y_t^*(0)|G_q = 1]$$
(2)

where G_g is a dummy variable that equals one if the unit belongs to the treatment group treated in time period g, $Y_t^*(1)$ and $Y_t^*(0)$ represent the actual outcome at time t for the treated units and the potential outcomes for those units not yet treated, respectively. In our analysis, we employ a control group of units that have never been treated (denoted as C) as a proxy for what would have happened had the unit never introduced CWS in the first place. Callaway & Sant'Anna (2021) demonstrate that, under the assumption that both control and treatment groups follow counterfactual parallel trends, in the general approach of Equation (2) is expressed as follows:

$$ATT(g,t) = E[Y_t - Y_{t-1} | G_g = 1] - E[Y_t - Y_{t-1} | C = 1]$$
(3)

where $[Y_t - Y_{t-1}|G_g = 1]$ represents the progression of the outcome for the treatment group, whereas, and $[Y_t - Y_{t-1}|C = 1]$ is the outcome for the group that was never treated. That is, (3) estimates the average treatment effect at time *t* for members of cohort G treated in period *g*. In the context of estimating the effect of establishments changing to CWS, ATT (2020,2022) is the average treatment effect in 2022 for the cohort of establishments that changed to CWS in 2020. Importantly, we use this method to avoid comparisons across groups treated at different times, thus addressing the issue of using units treated earlier as controls for those treated later. Equation (3) also provides unique estimates for each cohort of units that transitioned to CWS within the same year.

The estimated results from equation (3) reflect the group-time average treatment effects. However, for ease of exposition we aggregate these group-time average treatment effects into a smaller number of parameters, following the dynamic aggregation approach proposed by Callaway & Sant'Anna (2021). This is done because we anticipate dynamics in the causal effect of transitioning to CWS depending on the number of years since the change to CWS occurred. Therefore, for each time period *t* relative to an establishment's switch to CWS, we calculate the average of the ATT across all groups. This average is then weighted by the sample share of each cohort, yielding the average treatment effect among those establishments that have experienced CWS for exactly *t* periods. We also calculate a single, overall point estimate, by taking the average of the aggregated relative time estimates when $t \ge g^{10}$.

In using the event-study framework introduced in Callaway & Sant'Anna (2021), we adopt their recommendation of using a variable base period. This approach differs from standard event studies in that it does not include an omitted category.¹¹ Instead, each coefficient quantifies the effect of the switch to CWS, *t* years subsequent to the change. Here, *t* represents the running variable on the x-axis, which is the number of years. The coefficient is the average treatment effect on the treated

¹⁰ For a full discussion of this method, see Section 2 of Callaway and Sant'Anna (2021) and section 3 of Callaway (2022). We calculate all treatment effects using Callaway and Sant'Anna's R Package did, version 2.1.2. See https://bcallaway11.github.io/did/ for more information on this package.

¹¹ See https://bcallaway11.github.io/posts/event-study-universal-v-varying-base-period for a discussion

(ATT) j years after the change, averaging across the event-time coefficients for cohorts treated in each respective year.

In order to evaluate whether our model can provide causal estimates we examine the assumptions underlying the DID-estimator with multiple periods and varying treatment timings. The first assumption is based on the traditional DID-estimator, while the subsequent assumptions address the variable treatment timing across different establishments.¹² The parallel trend assumption, says that the potential outcomes for both untreated and treated groups would have been similar on average if not for the treatment. In our context, this means that treated and control units would have followed parallel trends without the introduction of CWS. While we can't directly test this assumption, our visual approach allows us to examine outcome trends in the years before the adoption of CWS, and as will be evident in the next section, seems to hold quite well. We also compare observable characteristics between treated and untreated units. Changing shift schedules, and in particular introducing CWS, is a process that takes time, and it is reasonable to assume that the employees are informed about upcoming changes in the work schedule well in advance, e.g., 1-2 years. We assert that the assumption of parallel trend appears reasonable in our setting based on our visual inspection of the outcomes in the two groups before treatment. Likewise, as there is no difference between the treatment and control group before treatment occurs, we do not see any indication of anticipation effects on our outcome variables.

Second, we impose that once an establishment adopts CWS, it continues to use it. In our analysis, we identified 8 cases where establishments switched from CWS to another work schedule. These cases were excluded from our sample.

Finally, the question of covariates in the DID-setting have been extensively discussed in recent DID-literature, see for example Caetano and Callaway (2023) and Roth et al. (2023). We have rich panel-data information about the employees in our sample. Yet, regardless of whether these controls are measured prior to the introduction of the change to CWS, as X_i , or as a time-varying covariate ($X_{i,t}$), introducing these controls may only be done in the event when either X_i or $X_{i,t}$, is unaffected by the treatment. We are not able to assume that these time-varying covariates are not a function of the treatment itself and will thus not include them in the regressions.

4.2 Sample selection in the analysis

Sample selection. The focus of our analysis is to use variation on establishment level to gauge any potential changes in the outcomes of interest for the establishments. We focus on outcomes at the establishment level for three reasons.¹³ The first is the high turnover in the sector, which makes it difficult to set non-arbitrary conditions for assigning employees to establishments when establishing treatment exposure levels. Examples of issues with no clear solution is how many months a worker must be observed with a treated establishment before assigning the worker as treated, and how to use information about sickness absences among treated workers who in later

¹² For a more technical discussion, see Callaway (2023).

¹³ In the pre-analysis plan we outlined analyses both at the individual and establishment level. For the three reasons mentioned in this paragraph, we only run analyses at the establishment level.

periods might work somewhere else. The high turnover makes individual-level analysis particularly complicated due to the second reason, which is that we asked establishments about their working schedule per the month of May each year. As we do not have specific information about when any change occurred, only that it happened at some point between year t and year t - 1, we do not observe to what extent employees working for an establishment for only part of that year was present during the CWS regime. Similarly, our third reason is that the information about Which employees in an establishment level only. We do not have information about which employees in an establishment work the extended shifts. In our data we see that most of the establishments (around 90%) report having employees working extended shifts also report having two- or three- shift rotation schedules. Hence, only a small part of the treated establishments consists only of employees working extended shifts.

As such, we circumvent these issues by identifying all shift-workers between the ages of 20 and 70 who worked in an establishment in May each year (excluding those who quit during that month). ¹⁴

In this study, a treated establishment refers to any establishment transitioning from two- or threeshift schedules to CWS. Conversely, the control group comprises establishments that persist with two- or three-shift schedules for the duration of the sample period. Consequently, certain establishments are excluded from the sample. Specifically, establishments operating on a 24-hour shift basis are omitted from both the treatment and control groups. Despite being a variant of extended shifts, 24-hour shifts represent an extreme case and, in our assessment, diverge significantly from other forms of extended shifts as well as traditional two- or three-shift schedules. Additionally, we exclude establishments transitioning from extended weekend shifts to CWS. This exclusion aims to ensure the integrity of a "pure" control group, characterized by the absence of any form of extended shifts. They are also excluded from the treatment group as a switch from extended shifts during weekends to CWS represent a different treatment intensity then the change from two- or three-shift schedules. Consequently, the mechanisms from the estimation would be difficult to disentangle as the establishments change from two completely different shift schedules.

Out of 1,413 establishments, the limitations we place on the definition of treatment and control leaves us with a total sample of 1,068 establishments, where 70 establishments are treated and 998 constitutes the control group. The establishments consist of 240,641 employment records¹⁵, and we place no restrictions on the sample regarding the number of workers identified in each establishment. As discussed in the previous section, the regression weights the establishments by the number of workers in each establishment. Table 3 below summarizes pre-treatment statistics regarding our sample's treatment and control groups, background characteristics of the establishments and a joint F-test between the two groups.

¹⁴ See Table A5 for a definition of the sample.

¹⁵ Each employee could be recorded up to 5 times (once a year during the period 2018-2022). Some employees also have several employment records within a year, as some have several jobs.

	Control (n=998)		=998)	Т	Treatment (n=70)			
	Ν	Mean	SD	Ν	Mean	SD	F-stat	P-value
Outcome measure- sickness								
absence								
Calendar days in May	998	2.660	1.518	70	2.739	3.721	0.133	0.716
Absence entire May	998	0.056	0.042	70	0.062	0.120	0.838	0.360
Calendar days whole calendar year	998	32.276	13.586	70	31.860	19.097	0.058	0.810
Other measures								
Share with health-related								
education ¹	998	0.548	0.162	70	0.527	0.169	1.074	0.300
Share with higher health-								
related education (at least	998	0.269	0.133	70	0.225	0.142	7.000	0.008***
bachelor) ¹	<i>,,,</i> ,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,	0.20)	0.122	10	0.220	0.1 12	1.000	0.000
Share women	998	0.866	0.131	70	0.856	0.166	0.380	0.538
Age (mean)	998	42.121	4.164	70	42.65	4.772	1.036	
Age- distribution								
< 30	998	0.255	0.117	70	0.251	0.169	0.073	0.787
30-44	998	0.285	0.100	70	0.284	0.133	0.011	0.916
45-59	998	0.332	0.112	70	0.323	0.129	0.418	0.518
60+	998	0.128	0.079	70	0.143	0.088	2.162	0.142
Share immigrants	998	0.147	0.131	70	0.142	0.120	0.112	0.738
Share married	998	0.406	0.129	70	0.428	0.174	1.873	0.171
Share with children under 18	998	0.384	0.116	70	0.407	0.164	2.292	0.130
Share with children under 6	998	0.157	0.078	70	0.182	0.163	5.459	0.020**
Number of staff	998	55.608	49.886	70	43.286	37.240	4.109	0.043*
Type of service								
Nursing homes	986	0.344	0.475	70	0.271	0.448	1.528	0.217
Home-based services	986	0.300	0.459	70	0.229	0.423	1.610	0.205
Assisted living facilities	986	0.356	0.479	70	0.500	0.504	5.867	0.016**

Table 3. Pre-treatment statistics of treatment and control establishments

Note: ¹For a definition of health-related education, see table A6. The variables are defined in 2018, or the first time the establishment is observed. *** p<0.01; ** p<0.05; *p<0.10

Regarding balance between the treatment group and control group before treatment, Table 3 shows that sickness absence on average is slightly higher in the treatment group compared to the control group when measured as calendar days in May and long-term sickness absence defined as absent during the whole May. Sickness absence measured as calendar days during the whole year is slightly higher in the control group. Importantly, these differences are non-significant. For most of the other characteristics (health-related education, age, age-distribution, share of women, share immigrants, share married) there is no significant differences between the treatment and control group. There is a significantly lower share of employees with higher health-related education and the size of the establishments (measured as number of staff) is significantly lower in the treatment group compared to the control group. There is also a significantly higher share of employees with small children (children under the age of 6) in the treatment group compared to the control group, there is a significant higher share of assisted living facilities compared to the control group. As these differences may have an impact on our outcome measure (sickness absence), we use pre-treatment characteristics as control variables in robustness checks.



Figure 2. Number of treated establishments year by year

Figure 2 shows the number of treated establishments sorted by calendar year. In 2019 there are 8 establishments introducing CWS. In 2020, 15 establishments introduce CWS, while there are 27 and 20 in 2021 and 2022.



Figure 3. Sickness absence (calendar days in May each year) in treatment and control group

In figure 3 we descriptively investigate the development in sickness absence separately by treatment status. In the control group sickness absence is quite constant at 2.6-2.7 calendar days in May 2018 to May 2020. In May 2021 and May 2022 sickness absence is at a higher level (3.1 and 2.8 calendar days). Sickness absence in the treatment group fluctuates more, but it does not seem to be systematically influenced in any direction. Note that COVID-19 takes place in the middle of the study period (starting in March 2020). However, sickness absence in May 2020 is not higher compared to the year before probably reflecting a quite low infection level in Norway at this time. The peak in sickness absence in May 2021 may reflect quite high incidence levels of COVID at this time.

5 Results

In this section, we present estimates of the impact of transitioning to a compressed work schedule on sickness absence, using the methodological framework of Callaway & Sant'Anna (2021). We start by presenting the average treatment effect on sickness absence and looking into the effects in the context of event studies. Thereafter, we look at the robustness of the main findings.

5.1 Main estimation results

Table 4 presents the estimated aggregated treatment effects (ATT). The results are obtained by estimating the sample analogue of equation (3) from the previous section. The aggregated ATT estimate, reported in the top row, provides the most precise and reliable estimate of the overall effect. We estimate the aggregate effect to be both statistically and economically insignificant, with a point estimate -0.0107 sick days, and a 95% confidence interval ranging from -0.454 to 0.433.

We also explore how the effects vary by the length of exposure to CWS, allowing us to look beyond the immediate impact to investigate the presence of dynamic effects resulting from the switch to CWS. Also, across the event time-specific estimates we find that the aggregated ATT on sickness absence is statistically insignificant at conventional levels. More so, even if they were interpreted at face value, the point estimates reported in column (1) are substantively small, and in most cases within the range of \pm 0.15 days change in absences. However, the event time-estimates are noisy, resulting in large confidence intervals that include values that range from a reduction of 20% to an increase of 20% correspondingly. Note in particular, that the cohort of treated establishments used in the estimation of event time 3 comprise of those switching to CWS in 2019 only, which (as per figure 2) is a small group. We urge caution from the reader in drawing strong inferences from specific event time point estimates. Our main takeaway is that we find little evidence to suggest that switching to CWS has a strong impact on sickness absence, in either direction.

	Estimate	Std. Error	[95% Simult. Conf.	Band]
Aggregate average treatment				
	-0.0503	0.239	-0.5046	0.404
Event time				
-3	-0.206	0.332	-1.014	0.602
-2	0.136	0.263	-0.502	0.774
-1	0.064	0.248	-0.5399	0.667
0	-0.0428	0.225	-0.5900	0.504
1	-0.199	0.192	-0.667	0.267
2	-0.154	0.309	-0.904	0.5955
3	0.196	0.557	-1.159	1.550
Observations (Year*Establishments)	4,474			

Table 4. Estimates of the effect of change to CWS on sickness absence (calendar days of sickness absence in May)

Notes: This table presents event study estimates and aggregate average treatment (ATT) of the effect of CWS on sickness absence. The model is estimated using the Callaway and Sant'Anna (2021) DiD estimator as described in Section 4 at the establishment level. Change to CWS is defined in Section 2. ATT's are estimated for each period using varying base period. The figure shows bootstrapped 95% simultaneous confidence intervals. The results are obtained from estimating the sample-analogue of equation (3) in Section 4. Sampling weights employed and standard errors are clustered at the panel (establishment) level. Capped spikes represent 95% confidence intervals

For ease of interpretation we illustrate the dynamics of the estimated event study model in Figure 4, using the estimates presented in Table 4. In particular, the figure allows for easy assessment of the plausibility of the parallel trend assumption, by investigating whether establishments that switched to CWS and those that never changed were statistically similar in terms of outcome dynamics during the pre-treatment periods.



Figure 4. Event study estimates of a change to CWS, sickness absence (calendar days of sickness absence in May).

The exact point estimates and confidence intervals are provided in table 4

Notes: This figure presents event study estimates of the effect of CWS on sickness absence. The model is estimated using the Callaway and Sant'Anna (2021) DiD estimator as described in Section 4 at the establishment level. Change to CWS is defined in Section 2. ATT's are estimated for each period using varying base period. The figure shows bootstrapped 95% simultaneous confidence intervals. The results are obtained from estimating the sample-analogue of equation (3) in Section 4. Sampling weights employed and standard errors are clustered at the panel (establishment) level. Capped spikes represent 95% confidence intervals.

As was evident from Table 4, the ATT-estimates for the pre-treatment periods are statistically indistinguishable from zero, supporting the plausibility of the parallel trend assumption. A marginal increase is observed from year -3 to year -1, but this increase is not statistically significant, suggesting that establishments appear to not be positively selected based on the dynamics of sickness absence prior to switching to CWS.

5.2 Robustness analyses

In this section we first investigate whether the results affect the composition of staff, which in turn may affect our outcome measures. Second, we explore whether the results are sensitive to alternative measure of sickness absence and sample selection criteria.

In Figure 5, we show how CWS influences the composition of staff measured as the share with relevant education (Panel A), the average age of staff (Panel B) and the share of female workers (Panel C). These variables are relevant as changes in staff composition might predict sickness

absence; employees with relevant education tend to have lower sickness absence compared to employees without education, while being a woman and higher age is associated with higher sickness absence. We find an increase in the share of employees with relevant education for establishments that change to CWS, but this effect is not significant.¹⁶ In terms of mean age, the effect fluctuates around zero over the analytical window, and none of the period-specific effects estimated are significant. Finally, we find suggestive evidence of a drop in the share of female workers, which seems to be persistent and grow in magnitude, but the effect is also not significant. As with the main effects displayed in Figure 3, we see that the estimates for the third year are estimated imprecisely due to the small sample of establishments that changed to CWS in the first year of observations. Overall, there seem to be some changes in the composition of staff as measured by a (non-significantly) higher share with relevant education and a (non-significant) reduction in number of females. With a higher share of employees with relevant education and a lower share of female we should expect a decline in sickness absence. This means that a decline in sickness absence may be due to compositional changes in staff and not attributing to CWS making individual employees healthier. However, as there are no significant changes in the composition of staff we are hesitant to draw strong conclusions based on this analysis.





Notes: This figure presents event study estimates of the effect of CWS on share of health- and social care professionals as measured by relevant education, mean age and share of female workers. The model is estimated using the Callaway and Sant'Anna (2021) DiD estimator as described in Section 4 at the establishment level. Change to CWS is defined in Section 2. ATT's are estimated for each period using varying base period. The figure shows bootstrapped 95% simultaneous confidence intervals. Sampling weights employed and standard errors are clustered at the panel (establishment) level. Capped spikes represent 95% confidence intervals. Relevant education is measured as health and social care professionals with either a diploma, bachelor's or master's degree (see Table A.6). The exact point estimates and confidence intervals are provided in table A7-A9.

We report the results from our other robustness checks in Table 5. First, we include alternative measures of sickness absence. The point-estimates both for long-term sickness absence (being absent throughout whole May) and total calendar days within the calendar year, are negative and close to zero. None of the estimates are significant. Second, we include control variables. With this inclusion the coefficient of sickness absence becomes slightly more negative, but still far from being significant. Third, we extend the sample to include *all* establishments introducing CWS during the period 2019-2022, including those that first implemented extended shifts during weekends only. When these establishments are included in the sample, all effects are estimated with greater precision (due to the increase in sample size). The point estimate for sickness absence is positive, but the effect is nevertheless far from being significant. In our fourth check, we revert

¹⁶ The sign of the point-estimate is in line with one of the intention of introducing CWS, namely to attract more qualified staff.

to our preferred sample of establishments, but restrict the sample of workers included in the analysis to those aged 25-66 and holding at least a 50% position. The last restriction is commonly used within the literature investigating the impact of extended shifts on sickness absence (e.g. Vedaa et al., 2019; Garde et al., 2019). With this restriction we narrow the sample to employees with more stable employment relationships. Hence, we exclude many students, pensioners and others for whom employment is not their main activity. Using this sample, we find a point estimate suggesting that CWS reduces sickness absence by 0.2 calendar days, but the estimate is not significant. For our last check, we also include employees in the analysis who do not work shift. These employees are not directly affected by changes in the work schedules, but there may be spill over effects from the treated workers. For example, CWS may affect the working environment which again may influence the whole group of employees measured by sickness absence. However, as expected, the estimated effects on sickness absence is still zero.

	Estimate (std.error)
Main model/gample (from Table 4, N-1068 establishments (4.474	
observations), 70 treated establishments, 240,641 employment records)	[-0.5046 0.404]
1. Alternative definitions of sickness absence	
I)Being absent throughout whole May	-0.009 (0.0086)
	[-0.0263 0.0073]
II) Total number of calendar days throughout the calendar year	-0.6175 (1.902)
	[-4.344 3.109]
2. Adding control variables	
Control for: Age, share female, share immigrants, type of service,	-0.14 (0.244)
relevant education and kids under age 6;	[-0.618 0.3383]
3.Extended sample of establishments	
Include all establishment implementing CWS, also those first	0.081 (0.179)
introducing extended shifts during weekends only	[-0.272 0.431]
(N=1.133 establishments (4.792 observations). 135 treated	
establishments, 261,066 employment records)	
4.Reduced sample of individuals	
Employees 25-66 years and holding at	-0.211 (0.403)
least a 50% position	[-1.005 0.585]
(Same sample as the main model regarding establishments.	[]
Reduced sample of employment records (145,400)	
5.Extended sample of individuals	-0.0380 (0.221)
Employees not registered with shift work is also included.	[-0.497 0.436]
(Same sample as the main model regarding establishments.	
Increased sample of employment records (290,950), same	
sample of establishments as in the main sample)	

Table 5. Estimated effect of CWS on sickness absence-alternative models

Note: This table presents Difference-in-Difference (DiD) estimates of the effect of changing to CWS on three alternative samples, using the same outcomes as in the main estimation and another outcome called share quit from one year to the next. All models are estimated using the Callaway and Sant'Anna (2021) DiD estimator using the

dynamic aggregation. All panels present the aggregated ATT across all time-periods. All results are obtained from estimating the sample analogue of equation (3). *** p<0.01; ** p<0.05; *p<0.10

6 Discussion

To the best of our knowledge, this study is the first to use longitudinal data to estimate how compressed work schedules (CWS) affects sickness absence within the municipal health and care sector. As the effect of CWS might differ across contexts, it is important to have institution-specific knowledge. Gaining knowledge about the effect of such measures in the municipal sector is particularly important for at least three reasons: First, the majority of workers in health and care occupations are employed in the municipal sector (NOU 2023:4). Second, turnover and absences is a particular concern in the municipal sector, more so than other health care sectors (Helsedirektoratet, 2021). Third, as the population ages the demand for personnel will increase rapidly in this sector, as it provides the majority of elderly care services.

Our results add to a literature dominated by cross-sectional studies with subjective sickness absence measures. Although recent developments have seen an increased emphasis on using longitudinal register data to investigate this relationship, these studies are few and mixed in their findings (Ropponen et al., 2019; Vedaa et al., 2019; Dall'Ora et al., 2019, Larsen et al., 2020; Rodriguez Santana et al., 2020) and none of the studies covered the municipal health- and cares sector. Two studies (Vedaa et al., 2019; Peutere et al., 2021) found that extended shifts were associated with lower sickness absence, while the others found extended shifts were associated with higher sickness absence. These studies used data from mental health hospitals (Rodriguez Santana et al., 2020) and ordinary hospital units (Ropponen et al., 2019; Vedaa et al., 2019; Dall'Ora et al., 2019, Larsen et al., 2020). One of the studies (Rodriguez Santana et al., 2020) followed units before and after introducing extended shifts. They utilize a causal inference design (Difference-in-Difference design and Interrupted Time Series) to identify how extended shifts affect short-term sickness absence (measured as sickness absence up to 7 days). They based their study on six inpatient wards within a mental health hospital in England, which introduced extended shifts (from 8 to 12 hours) between June and October 2017. The authors used differential timing in implementing extended shifts using not-yet-treated wards as controls for those who first introduced extended shifts. The wards were observed weekly from 2016-2018, resulting in 463 observations. They find a significant increase in absence hours per week of between 0.73% and 0.98% after implementation. Our study is not directly comparable to their study, and there may be several reasons why our results differ. First, we use a different methodology as we can include information on a set of never-treated units, which Rodriguez-Santana et al. (2020) cannot. Second, their analysis focuses on short-term sickness absence (1-7 days) only, while our data covers certified sickness absence spells potentially lasting up to one year. Different results could also be related to different samples; they study mental hospital wards while we focus on workplaces within the municipalities' health- and care sector.

As mentioned in the introduction there is scepticism towards implementing CWS in Norway because of possible adverse effects, such as employees' health and sickness absence. According to our point estimates, the effects on sickness absence is statistically indistinguishable from zero and the results are robust to different definitions of sickness absence, including control variables and different sample definitions. Our analysis reveals no evidence to suggest that CWS either increases or decreases sickness absence in the municipal health and care sector. possibly indicating that CWS does not have any harmful effects on sickness absence. Our null results may also be driven by a limited number of workers working extended shifts, as extended shifts in Norway is voluntary for each worker.

Our robustness analyses also tested whether staff composition has changed in conjunction implementing CWS. This is important as changes in the staff composition may influence sickness absence. We find some changes in staff composition as measured by a higher share with relevant education and a reduced number of females. As employees with education and males tend to have lower sickness absence, we should expect a decline in sickness absence. As we do not find a decline in sickness absence, the results may hide an increase in sickness absence at the individual level. However, as the staff changes are insignificant we are hesitant to draw strong conclusions based on this analysis.

Although our point estimates for sickness absence are statistically indistinguishable from zero, the confidence intervals are quite wide, and we cannot rule out that CWS has heterogeneous effects.¹⁷ For example, it might be that the effect varies between types of services (nursing homes, homebased care, and various types of assisted living facilities) due to for instance different working tasks, composition of patients, varying workload and different opportunities to take breaks during the workday. Another important factor might be the degree of autonomy in choosing shift patterns, which has been found to reduce sickness absence among employees (Turunen et al., 2020). On the other hand, findings from Ingstad & Amble (2015) suggest that the positive impact of CWS (reduced stress due to e.g., increased continuity and freedom to prioritize tasks) occurs primarily when all employees within a unit work extended shifts.

It is also important to remember that the generalizability of our results may be affected by the COVID-19 pandemic. WHO declared COVID-19 a pandemic on 11 March 2020 (WHO, 2021). In Norway, infection control measures were in place, to varying extent, from March 2020 to February 2022.¹⁸ Extended shifts may be particularly beneficial in preventing infections as the recipients may interact with fewer employees during the day. This also implies that there may have been fewer COVID-19 outbreaks within these establishments. Hence, we should also expect that CWS was beneficial in preventing sickness absence among employees. In turn, this might imply that we underestimate adverse effects on sickness absence. On the other hand, the pandemic may have limited influence on our results for some reasons. First, we investigate sickness absence in May, which is a month with quite low infection rates. Second, infectious diseases (such as influenza) are also relevant without the pandemic and those receiving health and care services are particularly exposed to infections at any times.

¹⁷ The analysis plan includes heterogeneity analyses. We have tried to estimate models for different subgroups (such as nursing homes, homebased services and assisted living facilities), but there are too few observations within subgroups to estimate for our sample period. ¹⁸ See https://www.regjeringen.no/no/aktuelt/smitteverntiltakene-oppheves/id2900873/

7 Conclusion

In this paper we have investigated how working fewer but longer shifts in compressed work schedules (CWS) in the context of municipal health and care services impact sickness absence for the employees. We find no significant effects of introducing CWS on sickness absence.

It is important to acknowledge that our analysis focused solely on establishment-level data. This limitation prevents us from drawing definitive conclusions about the long-term effects of CWS on individual employees. First, since the information about CWS is measured at the establishment level, means we do not have information about which and how many employees work extended shifts. Hence, we are not able to estimate long- run effects for the individuals. However, we have information about the type of work schedules for a large group of establishments covering thousands of employees. Second, although the sample size of establishments in the survey is large, covering a large share of workplaces within the health- and care sector, the number of treated units is limited, leading to large standard errors in some cases. In particular, the precision of our analysis prevents us from more thorough investigations into how the effects of CWS might differ across various groups of workers and establishment. Third, although the recency of our data is beneficial for the relevance and external validity of our analysis, it prevents us from investigating longer-term effects, which will be an important focus for future studies.

Our results have important implications for workplace policies in the health and care sector. Even though we do not find any evidence of CWS being a contributing factor to decrease sickness absence in this sector, we do not find any evidence in the opposite direction either. A zero effect on sickness absence is good news if other factors are affected positively. More research is needed in order to investigate how other factors are influenced by CWS. More research is also needed to investigate whether the effects may be heterogeneous. It may be related to different preferences across groups of employees as well as differences in how CWS is introduced. Future research should seek to identify ways to organize work schedules being beneficial both for sickness absence, full-time work, recruitment, quality of care, etc.

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Appendix

Description	Excluding	Sample (n)
All employment records registered in "A-ordningen" as of May 2018-2022		4,895,490
Zero or negative earnings («kontantlønn»)	(-)1,968,623	2,926,867
Not registered with an employer	(-) 1,891	2,924,976
Employment records not counted as «normal» No information about contracted working hours	(-) 146,935	2,778,041
(arb_arbeidstid)	(-) 25,408	2,752,633
Not employed the whole month (May)	(-) 24,592	2,728,041
Not within the age 20-70	(-) 127,108	2,600,933

Table A1. Definition of sample constructed for Table 1

Table A2. Processing data from the survey

Action	Add/remove	Sample size (establishments)
Survey end		1,587
Remove answers not consenting	(-)40	1,547
Duplicates (i.e. managers answering for several establishments)	(+)15	1,562
Remove establishments answering "don't know" or blank to all questions about organizing work schedules	(-)6	1,556
Remove establishments stating they do not have shift work	(-)15	1,541
Aggregate (i.e. keep only one answer when establishments have provided more than one answer)	(-)111	1,430
Remove establishments with missing link to register.	(-)3	1,427

Table A3. Overview of central questions in the establishment survey

 The first question relates to how the establishment has organized the workday. We ask about different types of work schedules: Two-shift rotation: The 24-hour day is divided into three, where some employees rotate between working day and evening, while other employees permanently work night shifts, Three-shift rotation: The 24-hour day is divided into three, and the employees rotate between working day, evening and night shifts,
 Extended shifts: shifts lasting more than 10 hours are included in the employees' work schedule implying that workers have longer but fewer shifts. Extended shifts can be used throughout the week or just at weekends. We do *not* mean long work sessions that arise due to special needs or that employees sometimes work double shifts. Also, we do not mean co-liver shifts.
 24-hour shifts: a shift schedule where employees work for 24 hours or more, where the shift includes active work and resting work.

	Which of the following shift schedules has your establishment had in the last four years? (Several answers are						
	possible. Check the years the establishment had each of the shift schedules, also if only a small group of						
	employees had that type of shift schedule). Provide status by May 1 each year.						
		Two-shift	Three-shift	Extended	24-hour	Other	Don't know
		rotation	rotation	shifts	shifts		
	May 1 2022						
	May 1 2021						
	May 1 2020						
	May 1 2019						
	May 1 2018						
2	 We now wonder whether the employees worked extended shifts all weekdays or only had extended shifts during weekends (for instance by working extended shifts on Saturdays and Sundays). When did employees work extended shifts during a work week? (Several alternatives are possible). Provide status per May 1 each year ¹⁹ 						
	All weekdays Only during Other Don't know weekends						n't know
	May 1 2022						
	May 1 2021						
	May 1 2020						
	May 1 2019						
	May 1 2018						

Table A4. Descriptive statistics of establishments reporting extended shifts. May 2022				
Number of establishments	615			
Type of service (%)				
Nursing homes	22.3			
Home-based services	17.7			
Assisted living facilities for people with intellectual disabilities	31.4			
Assisted living facilities for elderly and people with functional	28.6			
disabilities Combination with other types of work schedules (%)				
Extended shifts only	14.0			
Two-shift schedule	84.6			
Three-shift schedule	21.8			
24 h shifts	7.0			
Length of extended shifts (hours) (%)				
10-11.5	3.6			
12	20.6			
12.5	49.0			
13-14.5	26.1			
15-23	1.6			
When employees worked extended shifts (%)				

<i>Table A4. Descriptive statistics of establishments reporting extended shifts. May 20</i>

¹⁹ This is a follow-up question asked to respondents reported having or having had extended shifts in the previous question.

All weekdays	28.3
Only during weekends	55.1
Combination of all weekdays and weekends only	10.1
Other	5.9
Extended shifts on days and/or night shifts (%)	
Dayshift	98.4
Nightshift	27.1
Frequency of weekends (several answers possible) (%)	
Every second	3.9
Every third	25.9
Every fourth	73.2
Every fifth	2.4
Every sixth	7.3
Frequency of extended shifts among registered nurses (%) (among establishments who have reported having registered nurses ²⁰)	
0	14.6
1-20%	23.4
21-40%	12.1
41-60%	9.1
61-80%	10.7
81-100%	30.2
Frequency of extended shifts among auxiliary nurses ²¹ (%) (among establishments who have reported having auxiliary nurses within their staff ²²)	
0	3.4
1-20%	27.4
21-40%	17.1
41-60%	13.5
61-80%	12.0
81-100%)	26.5

Table A5. Defin	ition of sample	constructed for	r descriptive statistic	s survey population
	···· · · · · · · · · · · · · · · · · ·		F F F F F F F F F F F F F F F F F F F	

Description	Excluding	Sample (n)
All employment records covered by the survey population and registered in "A-ordningen" as of May		
2018-2022		572,653
Zero or negative earnings	(-) 134,748	437,905
Not registered with an employer	(-) 0	437,905

²⁰ 175 establishments reported not having any registered nurses within their staff.
²¹ For our definition of auxiliary nurses, see Table A6.
²² 31 establishments reported not having any auxiliary nurses within their staff.

Employment records not counted as «normal" ²³	(-) 11,198	426,707
No information about contracted working hours	(-) 3,128	423,579
Not employed the whole month (May)	2,989	420,590
Not within the age 20-70	12,961	407,629
Not shift-work	52,444	355,185
No answer regarding the type of work schedule	8,610	346,575

Table A6. Definition of variables

Group of health- and social care professional	Classification of education (NUS)*	Main groups of professionals
Health and social care professionals	661, 662, 663, 665, 669,	Nurses
with (at least) bachelor degree	761, 762, 763, 765, 769,	
	861, 862, 863, 865, 869,	
Health and social care professionals	461, 462, 463, 465,	
with diploma	561, 562, 563, 565, 569,	Auxiliary nurses
	361201, 361202, 469901, 469910	
Employees without formal health and social care education	Rest	

Note: *For a description of classification of education (NUS), see https://www.ssb.no/klass/klassifikasjoner/36

Table A7. Estimates of the effect of change to CWS on mean age – Event estimates

Event time	Estimate	Std. Error	[95% Simult. Conf. E	Band]
-3	-0.267	0.360	-0.6213	1.143
-2	-0.356	0.457	-1.477	0.764
-1	-0.004	0.282	-0.696	0.686
0	-0.284	0.229	-0.845	0.278
1	0.298	0.359	-0.582	1.178
2	-0.320	0.617	-1.832	1.191
3	0.027	1.042	-2.579	2.252
Observations	4,474			
(Year*Establishments)				

Table A8. Estimates of the effect of change to CWS on share of female – Event estimates

Event time	Estimate	Std. Error	[95% Simult. Conf. B	and]
-3	0.033	0.009	-0.019	0.025
-2	-0.013	0.009	-0.033	0.008
-1	-0.0002	0.008	-0.021	0.016
0	-0.009	0.006	-0.022	0.004

²³ See Statistics Norway, 2024https://www.ssb.no/klass/klassifikasjoner/180

1 -0.02	.3 0.019	-0.067	0.021	
2 -0.02	0.038	-0.107	0.066	
3 -0.10	0.098	-0.326	0.116	
Observations 4,474	1			
(Year*Establishments)				

Table A.9. Estimates of the effect of change to CWS on share of relevant education – Event estimates

Event time	Estimate	Std. Error	[95% Simult. Conf. E	Band]
-3	0.001	0.011	-0.027	0.029
-2	0.017	0.012	-0.014	0.047
-1	-0.009	0.011	-0.037	0.019
0	0.011	0.010	-0.013	0.035
1	0.020	0.013	-0.012	0.052
2	0.044	0.026	-0.021	0.110
3	0.056	0.044	-0.054	0.166
Observations	4,474			
(Year*Establishments)				