Workload, staff composition, and sickness absence: Findings from employees in child care centers*

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Abstract

Persistently high workload may raise sickness absence with associated costs to firms and society. We proxy workload by the number of adults per child in Norwegian child care centers and find that more educated teachers per child are associated with lower sickness absence. However, more assistants with low or no higher education per child are associated with higher sickness absence, suggesting that observed variation in sickness absence at the center level may be driven by differences in staff composition rather than workload. The importance of the educational composition of employees on sickness absence is supported by findings from fixed-effects models and a fuzzy regression discontinuity design relying on variation from municipal elections.

Keywords: Sickness absence, Workload, Staff composition, Child care centers, FRDD

JEL-codes: I1, I2

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1 Introduction

On a typical working day, around 7 percent of Norwegian employees are absent from work due to sickness, and the associated insurance payments amount to about 2.4 percent of the Norwegian GDP (Markussen et al., 2011). About 15 working days per employee are lost annually in Norway due to sickness absence. It is in the same magnitude as several other OECD countries, with four days in the US, 12 in Sweden, and 18 in Germany.[1] The level of sickness absence varies substantially across groups: for example, it is much higher among high-school drop-outs than those with a degree from higher education (e.g., Cutler and Lleras-Muney, 2010; Kostøl and Telle, 2011).

Workplace conditions are revealed to be an important risk factor for sickness absence (Labriola et al., 2006; Eriksen et al., 2016), but evidence on how various indicators of workload affect sickness absence is scarce (Bratberg et al., 2017; Gørtz and Andersson, 2014; Conen et al., 2012; Defebvre, 2018). We use the number of adults per child as a proxy for workload and investigate the relationship between workload and sickness absence among employees in Norwegian child care centers. Employees include both educated teachers and assistants with no or limited higher education - and the staff composition varies across centers. Several studies have documented that education level correlates strongly with health status, and some evidence suggests a causal link (Cutler and Lleras-Muney, 2010; Clark and Royer, 2013; Gathmann et al., 2015; Brunello et al., 2016). We will, therefore, pay careful attention to the educational composition of the employees in child care centers, noting that even if more adults were randomly assigned across centers, a causal effect on sickness absence of more adults at

[1] OECD stats (2015), see http://stats.oecd.org/index.aspx?queryid=30123.

the center level could be driven by a lower workload as well as changes in the educational composition of adults with inherently low (teachers) or high (assistants) sickness absence. Distinguishing between effects on sickness absence from changes in adults per child versus in the composition of adults with different educational levels may enhance our understanding of determinants of sickness absence and provide insights for the design of policies that aim to combat detrimental working conditions.

We start by exploring the association between the number of adults per child and sickness absence in Norwegian child care centers 2007-2014. We proceed to investigate how the association varies across centers with more teachers versus assistants. Findings from such naive OLS-models show that more adults per child are *not* associated with lower sickness absence. However, more teachers per child are associated with *lower* sickness absence, whereas more assistants per child are associated with *higher* sickness absence. This underlines the crucial need to account for the educational composition of employees when measuring sickness absence at the institutional level.

Centers with a more favorable adults per child ratio may be different from other centers along unobservable dimensions that may affect sickness absence. To get closer to a causal estimate, we first add municipality fixed effects as centers are administrated and funded by the municipality. We find that the significant and opposing association between teachers and assistants per child and sickness absence remains: sickness absence is 1.5 percentage points lower when the number of teachers per child is one percent higher, whereas sickness absence is 5.8 percentage points higher when the number of assistants per child is one percent higher.

In a model that accounts for an individual's time-invariant characteristics like education level and health, the negative and statistically significant association between the number of teachers per child and sickness absence disappears: the point estimate for the number of teachers per child is now close to zero and statistically insignificant, suggesting that the negative correlation is driven by omitted individual characteristics, like time-invariant selection by education level or health. The positive association between assistants per child and sickness absence declines but remains statistically significant at 2.9 percentage points. It is hard to see how more assistants *per se* could causally increase sickness absence. We may thus take this to suggest that more assistants are associated with some contemporaneous events that are detrimental to sickness absence, e.g., more demanding children not being fully compensated for by the additional assistants, or preceding higher sickness absence leading to more assistants being hired (reverse causality). Such endogeneity issues regarding assistants challenge the reliability of the fixed-effects models.

To try to solve endogeneity problems, we apply a fuzzy regression discontinuity design (FRDD) - where child care policies at the municipal level change as one political block gains power by obtaining just above 50 percent of the votes. Several scholars have exploited that non-right local political parties tend to increase the funding to the child care sector (Fiva et al., 2018; Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009). We first exploit that non-right parties promote the hiring of educated teachers over low-educated assistants. Then, using this as the first stage, we retrieve the treatment effects of the number of teachers per child on sickness absence in child care centers in a second stage. Applying the FRDD, we find that in line with these parties' stated political priorities, non-right local governments result in a higher share of educated teachers in centers. There is an increase in the number of teachers per child ranging from 3.8-14.3 percent (depending on the bandwidth) in centers in municipalities where the non-right political block gained the majority. A higher share of educated teachers among the staff reduces sickness absence in child care centers. The reduced form estimates - which capture the direct effect of the non-right block on sickness absence - suggest a reduction in sickness absence in child care centers of around 1-2 percentage points.

A related paper studies how the number of adults influences sickness absence in Danish child care institutions (Gørtz and Andersson, 2014). Using 1-year lagged levels of the number of adults per child as an instrument, and including controls for individual teacher characteristics, workplace characteristics, and background characteristics of the children, they find little evidence that more adults per child reduce sickness absence. We contribute to this literature along three dimensions: first, we emphasize how sickness absence is heterogeneous across educated teachers and assistants with no or limited higher education, and thus how changes in the educational composition of employees at the center level affect the interpretations. Second, we rely on variation over time in fixed-effects models, and third, we implement a novel approach to study the causal effect of workload on sickness absence.

2 Institutional setting

2.1 Sickness absence benefits

The Norwegian National Insurance (NNI) program provides sickness absence benefits. The program covers all residents in Norway, and participation is mandatory. Paid sickness absence is provided from day one of sickness and up to a maximum of one year. Eligibility is determined by being employed for more than four weeks. The coverage is 100 percent up to an established limit (NOK 550 000 in 2016, i.e., €55 000). The employer finances the first 16 days. After this, the NNI covers the expenses. Firing employees due to sickness absence is prohibited by law. After three days (sometimes seven), the employee needs a certificate from a medical doctor to document illness to continue receiving sickness absence benefits.

2.2 Child care in Norway

Child care centers are part of the education system in Norway, although enrollment is voluntary (Drange and Havnes, 2019). In 2004, a large child care reform was introduced, aiming at giving all children who wanted it a child care slot, and thus increasing the number of children enrolled (Norwegian Ministry of Child and Family Affairs, 2003). A substantial amount of resources followed this reform. Likely due to the inflow of children, the share of teachers fell between 2005 and 2007, and in the years following the reform, there has been a strong focus on increasing the share of educated personnel in child care centers, in particular, pursued by the non-right political block. Since September 1st 2009, parents have legal rights to a slot in a child care center in their municipality of residence from August the year the child turns one.[2]

In each municipality, the elected local council decides upon the allocation of funds to the child care sector. Child care centers are further financed by central government transfers and parental co-payment. A municipality usually gathers several child care centers. Private centers are entitled to the same transfer as municipal centers, as long as they meet the quality requirements elaborated on below. Parental co-payment has been capped at a maximum level since 2003, amounting to around NOK 2,500 (€250) per month for a full-time slot.

Quality is regulated with provisions on the number of teachers per child, the size of the

[2]If the child is born in September-December, the child does not have a right to a slot until August the year when the child turns 2, although many children enroll earlier.

play area, etc. There is a nation-wide regulation stating that teachers per child should be no smaller than 1/9 for toddlers aged 1-2, and 1/16 for children aged 3-5.[3] If a center is not able to recruit as many educated teachers as required, it may apply for an exemption. If granted, the teacher position may be held by an employee without formal qualifications. Additional regulations on staffing are decided at the municipality level. That is, the municipality has some discretion on how to organize centers, allocate children to them, and supervise teacher standards. Child care centers may be compensated with more support or staff when more demanding children enroll, but each teacher typically works with two assistants.

Teachers earn more than assistants as they must hold at least a teacher education at the bachelor level. There are no educational requirements for assistants in child care centers. They lack to a large extent higher education, and they cannot get promoted to teacher positions without acquiring the three-year teacher bachelor's degree.

The formal responsibility for planning, implementing, and documenting the pedagogical work in a center lies with the child care teacher. However, according to a survey covering about 600 child care centers, there is only a limited division of labor between teachers and assistants (Steinnes and Haug, 2013), suggesting the workload is similar for teachers and assistants in terms of the tasks they are undertaking in child care centers.

[3] From the 1st of August 2018, teachers per child should be no smaller than 1/7 for toddlers 1-2, and 1/14 for children aged 3-5. This policy change came after the period we are considering but can be considered the product of a persistent focus on this issue over the years.

3 Empirical strategy

We start by exploring the association between the number of adults per child and the sickness absence of employees in child care centers. We estimate the following linear probability model using OLS:

$$y_{ijt} = \alpha_0 + \alpha_1 \Pi_{jt-1} + \alpha_2 X_{ijt} + d_t + \varepsilon_{ijt} \tag{1}$$

where y_{ijt} is a dummy variable taking the value 1 if employee i in child care center j has a sickness absence spell in year t, or zero otherwise. α_0 is an intercept. Π_{jt-1} represents the number of adults per child in center j in year $t-1$. We lag this variable by a year to allow for some time for the change in the number of adults per child to affect sickness absence. Hopefully, relying on the center ratio in year $t-1$ will also make our results more robust to the challenge related to reverse causality, where substitute teachers covering for teachers on sick leave boost the ratio.[4] X_{ijt} represents individual characteristics of employee i and workplace characteristics of child care center j where employee i works. d_t are year dummies, accounting for general variation in sickness due to, e.g., business cycles (Haaland and Telle, 2015). ε_{ijt} is an error term with expectation zero, allowing for correlation within municipality. We cluster on the municipality level as child care centers are administrated and funded by the municipality. Our parameter of interest is α_1, which captures the correlation (contingent on X_{ijt} and d_t) between the number of adults per child and sickness absence.

As education level and sickness absence vary substantially across teachers and assistants, we will not focus on adults but assistants and teachers separately. To study staff composition

[4] Results are similar if we do or do not lag.

effects, we will, therefore, deploy the number of teachers or assistants per child separately and inquire about their relation to sickness absence.

Workplace characteristics are closely associated with sickness absence (Labriola et al., 2006; Markussen et al., 2011). As discussed in Section 2, child care centers are administrated and funded by the municipality that has some discretion on how to set and supervise teacher standards, organize centers, and allocate children to them. The characteristics of the municipality may thus affect the working conditions of the staff in centers. We will, therefore, include municipality fixed effects in some specifications.

Individual characteristics are also important predictors of sickness absence. We account for several well-known observable determinants of sickness absence in X_{ijt}, including age, gender, and center-specific tenure. In addition, we include control variables capturing the share of children aged below three and the share of minority children, as workload may vary with children's age and background. Education level is accounted for in the staff ratios: the number of teachers per child or the number of assistants per child. As discussed in Section 2, teachers hold at least a teacher education at the bachelor level while assistants, to a large extent, lack higher education (more on this in Section 4). Unobservable individual characteristics may also be important. We will thus present a model that accounts for time-invariant individual fixed effects. Time-invariant observable characteristics, such as education level, are now accounted for, hence in contrast to the municipality fixed-effects model, staff composition effects by education level will be netted out. Note that staff composition effects might exist along other dimensions than education.

We will also include center fixed effects in some specifications. It should account for time-invariant selection of children into centers and is useful if we are afraid that children who select into different centers (say small versus large) are different and correlate with other features of the center, such as the type of neighborhood or center premises. As the number of adults, teachers, or assistants per child are calculated at the center level, and the main difference between teachers and assistants is likely to be their education level, a center fixed-effects model will also net out staff composition effects by education level.

Some of the assistants may have specialized child care related education at the high school level. To inquire if education level or a better match quality between teachers (with specialized education) and the workplace are driving our results, we go on to study whether the specialized assistants have similar sickness patterns as other assistants or if they behave more as teachers, using a sub-sample consisting of specialized assistants only. We will deploy all three fixed effects models separately.

As an alternative to studying the number of teachers per child or the number of assistants per child using the whole sample, we will study heterogeneous effects using sub-samples based on teachers or assistants separately.

Fixed effects models yield biased estimates of the causal effect if the variation over time in adults per child is driven by contemporaneous variation in an omitted determinant of sickness absence. For example, if a center learns that it will receive more demanding children in the future, it may respond by hiring more assistants. Unless the additional assistants can fully compensate for more demanding children, such dynamics will erroneously show that more assistants increase sickness absence. A similar bias will occur if a center experiences or foresees higher sickness absence and responds by hiring more assistants or teachers. To try to account for such endogeneity, and thus get closer to causal estimates, we will apply a fuzzy regression discontinuity design (FRDD). That is, because the staff ratios - teachers per child and assistants per child - can potentially correlate with unobservables, we instrument these ratios to get a consistent estimate of the staff composition effects.

There is a burgeoning literature on how political parties in power at the local level shape political outcomes (Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009; Fiva et al., 2018). Evidence from Norway suggests that non-right parties increase spending on child care (Fiva et al., 2018). Following Fiva et al. (2018), we use the share of votes to non-right parties in local government elections (i.e., municipalities) as a source of exogenous variation. We study electoral effects based on a majority rule of 50% of the votes and exploit that non-right parties for a long time have promoted the hiring of educated teachers over low-educated assistants, that is, have advocated for a rise in the share of educated personnel in child care centers. First, we estimate the impact of non-right parties winning the majority in local elections on the number of teachers per child (Π). Then, using this as the first stage, we retrieve estimates of the effect of the number of teachers per child on sickness absence (y) in a second stage. More precisely, we first compare the number of teachers per child in child care centers in municipalities below the majority threshold (untreated) with the number of teachers per child in child care centers in municipalities above the majority thresholds (treated). If the non-right variable is continuous, the direct effect of the non-right block winning the majority may show up as a jump in the number of teachers per child. Then, we estimate how the treatment (i.e., the number of teachers per child) affects sickness absence among the staff in child care centers in a second stage. Note that the majority threshold may not perfectly determine treatment exposure, but it creates a discontinuity in the probability of treatment exposure. Such a fuzzy regression discontinuity design can be illustrated with the following model (using the same notation as in Eq. 1):

$$\Pi_{kjt} = \beta_0 + \beta_1 NR_{kt-1} + \beta_2 nonright_{kt-1} + \beta_3 NR_{kt-1} * (nonright_{kt-1} - 0.5) + \beta_4 X_{ijt} + d_t + e_{kjt}$$

$$y_{ikjt} = \gamma_0 + \gamma_1 \hat{\Pi}_{kjt} + \gamma_2 nonright_{kt-1} + \gamma_3 NR_{kt-1} * (nonright_{kt-1} - 0.5) + \gamma_4 X_{ijt} + d_t + v_{ikjt}$$

where $nonright_{kt-1}$ is the share of votes to the non-right parties in the previous election (running variable). Subscript k indicates municipality. NR_{kt-1} is a dummy variable equal to 1 if $nonright_{kt-1} \geq 0.5$ (otherwise zero). This is our instrument. Crossing the threshold at 0.5 indicates that the non-right parties obtained the majority, thereby influencing the probability of treatment. The interaction term allows for different slopes at either side of the threshold. The parameter of interest is γ_1. It captures the effect on sickness absence of the exogenous variation in the number of teachers per child that follows from the non-right obtaining majority. In the actual analysis, we will apply non-parametric specifications of local-linear regressions with a kernel smoother (triangular). We explore different bandwidths, including a mean-square-error optimal bandwidth selector, see Imbens and Lemieux (2008). We need to keep in mind that the treatment effect is likely to be upward biased since the non-right parties may prioritize children and employees in other ways than solely through more teachers. To give the second stage estimates a causal interpretation, we have to assume that the entire effect on sickness absence in centers is coming from changes in staff composition when the non-right block gains the majority. We will also report reduced form estimates where we can relax this assumption.

4 Data and descriptive statistics

We use Statistics Norway's employer-employee register covering all employees in child care centers from 2007-2014. The employer-employee register contains a personal identifier, an employment code, an organizational identifier, and start and stop dates for each employment spell. Besides, we have information on gender, age, education (field and level), and sickness absence for each employee in child care centers. We also exploit a register with information at the center level containing information on the total number of children enrolled, the number of children in each age group 1-5, and the share of minority children. Based on these two registers, we calculate the number of adults, teachers, and assistants per child in each center. Teachers have a teacher education or a degree in pedagogy, while we categorize other employees, mostly without any higher education, as assistants. We allow teachers and assistants to exit and enter the child care sector over the years. Our main sample consists of 304 470 observations.

Included in the sample are those working more than 30 hours a week (i.e., full time). Avoiding teachers and assistants working only a few hours per week or in "emergencies" enable us to compare teachers and assistants with the same workload in terms of working hours. 90 percent of the teachers and 65 percent of the assistants work full time.[5]

Our sample includes 3,904 child care centers.[6] Few centers are closing down (2%), and few new centers are opening (14 %) during the period that we study. On average, each child care center has 52 children, where 34 are three years old or above, and 18 are below the age of three. There are, on average, 13 employees (employed full-time) per child care center. In other terms, 2.5 adults for every ten children, where 1 is a teacher and 1.5 are assistants.

[5] Our estimates remain very similar if we also include part-time workers.

[6] Family-driven centers are not included, as they are very small and often do not have independent organization identifiers, making it hard to link employees to centers.

92% of the assistants lack higher education, 7% have a bachelor's degree, and 1% a master's degree. In contrast, all teachers have higher education. 99% have a teacher education at the bachelor level and 1% at the master level. So teachers are indeed more specialized and educated. Assistants can be educated and specialized but at a lower level. There is a specialized education for becoming an assistant in child care centers at the high school level. Almost 20% of the assistants in the sample are registered with this education. Very few individuals go from being a low-educated assistant to becoming an educated teacher.

Our outcome variable is a dummy capturing whether an employee has been certified as sick by a medical doctor during a calendar year or not. Spells shorter than three days (sometimes seven days) rarely needs to be certified by a doctor. Thus such self-reported sick days are not registered in our data. Sick days equal the number of contracted working days per year an employee is absent because of sickness as certified by a doctor.

In the regressions, we include a set of control variables (cf. X_{ijt} in Eq. 1) consisting of age (categorical), gender (dummy), and center-specific tenure (first and second-order polynomials). To account for varying workload related to children, we include a control variable capturing the share of children aged below three (cf. the regulation that there should be more teachers and adults per child if the child is below three years old) and the share of minority children at the center level. These control variables come in addition to the education level of the staff embedded in the staff ratios teachers and assistants per child.

We consider three ratios: the number of adults, teachers, and assistants per child. They are calculated at the center level, and in all regressions, we take the natural logarithms to allow for an increasing effect with diminishing returns of adding more adults per child and to facilitate interpretations as semi-elasticities. The first ratio (adults per child) proxies the overall workload in a child care center while the two latter ratios (teachers or assistants per child) enable us to study staff composition effects. In the analysis, we compare educated teachers and low-educated assistants in centers facing the same workload in terms of children enrolled, hours worked (i.e., full time), and the tasks they undertake (Steinnes and Haug, 2013). Hence, the main difference between teachers and assistants is likely to be their education level.

Table 1 displays summary statistics for employees in child care centers. We see that the average age of an employee is about 40. They have, on average, 4.3 years of tenure in a particular child care center, and centers are primarily staffed by women, with a share above 90 percent. As expected, 30 percent are teacher-educated. 33 percent of the employees have at least one sickness spell per year.[7] Among those with at least one sickness spell, the average number of sick days per year is 21. Table 1 also displays summary statistics for teachers and assistants separately. We see that high-educated teachers and low-educated assistants have similar observable characteristics in terms of age, tenure, and gender. Although on average assistants have only a somewhat higher number of sick days, per year, than teachers if absent due to sickness, the share with sickness absence is much higher among assistants than teachers.

[7] This is about three times larger than the long-term sickness rate reported by Gørtz and Andersson (2014) (their Figure 3) for employees in child care centers in Denmark. However, their long-term measure captures only spells of more than two weeks, while we capture all spells certified by a medical doctor. Certification is required for spells exceeding three days, sometimes seven days, but is also used for shorter spells, especially among employees with high sickness absence. From their Table 1, it is evident that short-term sickness by far exceeds long-term in Denmark, and the underlying sickness absence may thus not be very different in Denmark and Norway. Both Denmark and Norway have generous welfare systems compared to other OECD countries.

Table 1: Summary statistics - employees in child care centers

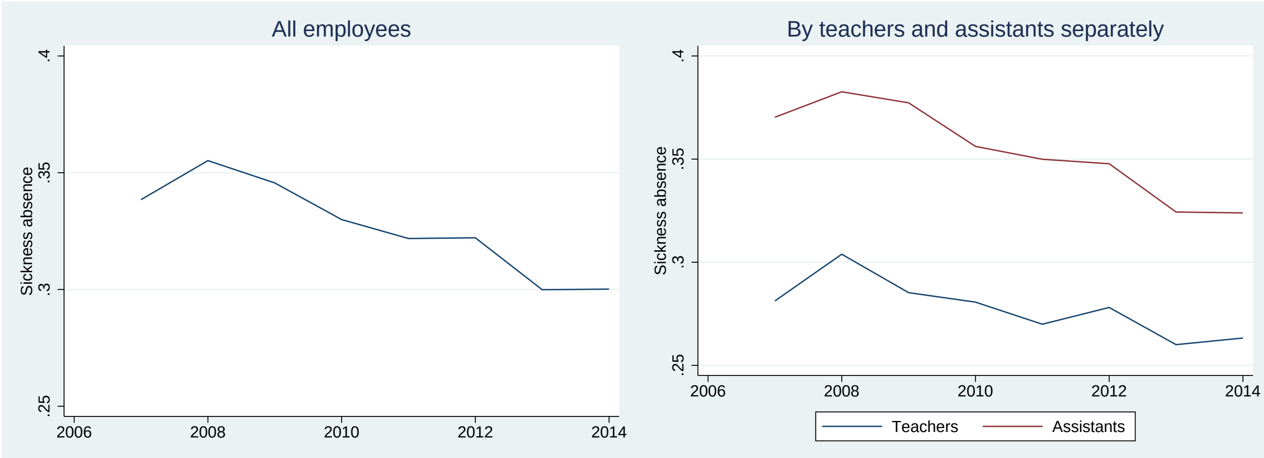
	The full sample		Teachers only		Assistants only	
	Mean	Std.dev.	Mean	Std.dev.	Mean	Std.dev.
Average age	39.80	(11.0)	39.76	(9.8)	39.83	(11.7)
Average years of tenure	4.30	(5.4)	4.51	(4.3)	4.26	(5.4)
Female share	0.92	(0.27)	0.94	(0.21)	0.91	(0.28)
Share with a teacher degree	0.30	(0.11)		1		0
Share with sickness absence per year	0.33	(0.47)	0.27	(0.45)	0.35	(0.48)
Average number of sick days per year	21	(48)	19	(45)	22	(49)
Average adults per child ratio	0.24	(0.06)				
Average teachers per child ratio	0.08	(0.03)				
Average assistants per child ratio	0.16	(0.05)				
Number of observations	304 470		110 430		194 040	

5 Results

5.1 Descriptive findings

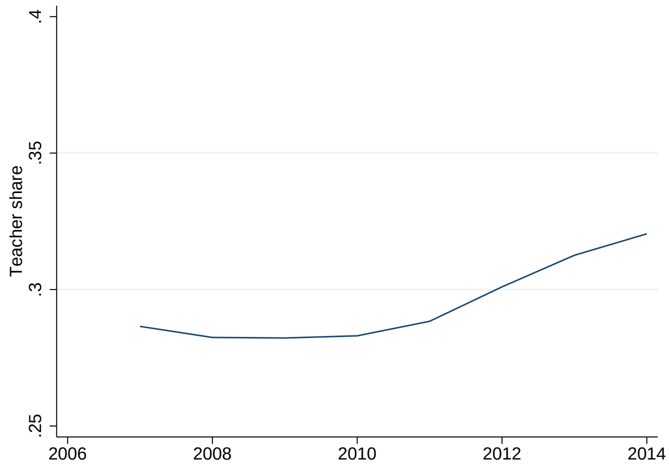
Figure 1 shows the share of employees in child care centers with sickness absence during the years 2007-2014. We see a clear downward trend: there is a reduction in sickness absence among the staff of about 4 percentage points from a level of around 34 percent. This trend applies to both teachers and assistants, but sickness absence is substantially lower for educated teachers than for low-educated assistants.

Figure 1: Percent of employees with at least one sickness absence spell in a year



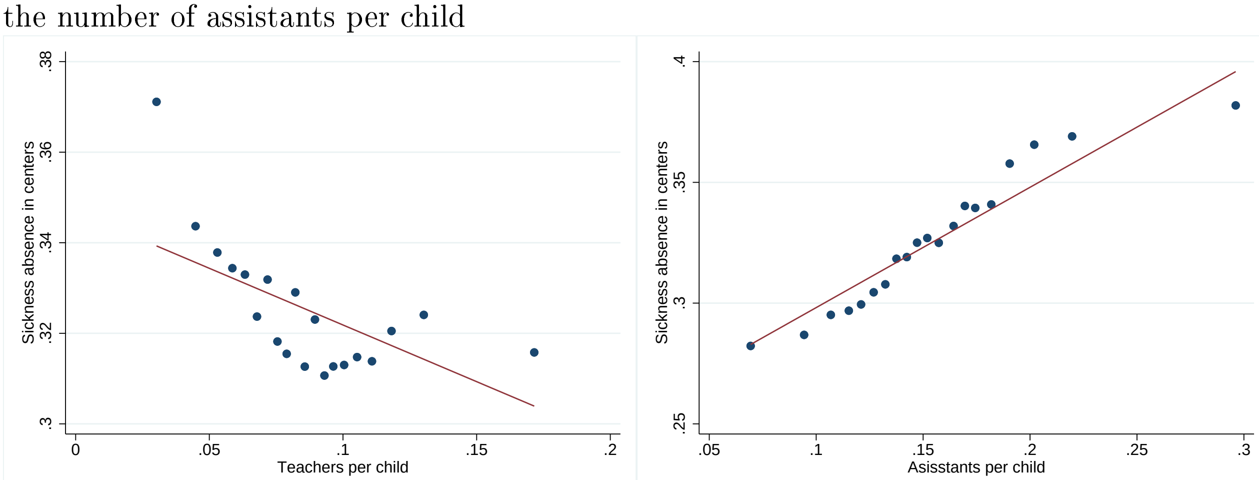
Although there is considerable variation in staff composition across centers, see Figure A.1 in the Appendix, Figure 2 shows that there has been an increase in the share of teachers over the period, in line with what we would expect given the institutional changes over the period (see Section 2.2). The increasing share of educated teachers with low sickness absence over low-educated assistants with high sickness absence can partly explain the overall fall in sickness absence in Figure 1. If the share of teachers had been constant over the period, the overall fall in sickness absence would have been 9 percent instead of the actual fall of 12 percent. This illustrates how changes in the composition of employees with varying educational levels can significantly affect the change in sickness absence on the institutional level, even if no causal effects of workload.

Figure 2: The share of all employees in child care centers that are educated teachers



We can also illustrate the importance of staff composition by looking at associations between sickness absence and the number of teachers or assistants per child. Figure 3 shows that centers with many educated teachers per child have lower sickness absence than centers with few educated teachers per child. The relationship is the opposite for assistants: centers with many low-educated assistants per child have higher sickness absence than centers with few low-educated assistants per child.

Figure 3: Sickness absence in child care centers across the number of teachers per child and the number of assistants per child



5.2 Regression results

From Table 2, we see that sickness absence in a child care center is 7.2 percentage points higher when the number of adults per child is one percent higher. When adding in control variables specified in Section 4 in Column 2, we see that the estimate decreases to 6.1 percentage points, in line with what we would expect if observable individual characteristics and workplace characteristics correct for some of the likely selection. From the two lower rows in Table 2, and as indicated in Figure 3, the positive association between the number of adults per child and sickness absence in child care centers stems from opposite correlations for teachers and assistants: a higher share of assistants is associated with a higher probability of sickness absence among the staff, whereas a higher share of teachers is associated with a lower probability of sickness absence among the staff.[8,9]

[8]Standard errors in Column 1 and 2 are similar whether we cluster on the municipality or center level.

[9]Changing the outcome variable to the number of sick days during a year instead of a dummy variable for being on sick leave or not reveals a similar pattern. There is an increase of about six sick days in centers when the number of assistants per child increases by one percent, whereas there is a negligible, close to a zero increase in sick days in centers when the number of teachers per child increases by one percent.

Table 2: Association between sickness absence (outcome variable) and log of adults, teachers, or assistants per child

	No controls		With controls		Municipality fixed effects		Individual fixed effects		Center fixed effects	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
The full sample:										
Log adults-per-child	0.072	0.009*	0.061	0.008*	0.058	0.008*	0.038	0.006*	0.076	0.007*
Log teachers-per-child	-0.029	0.010*	-0.023	0.011*	-0.015	0.005*	0.004	0.003	0.004	0.004
Log assistants-per-child	0.080	0.009*	0.065	0.009*	0.058	0.006*	0.029	0.004*	0.059	0.005*
Number of observations	304 470		304 470		304 470		304 470		304 470	
Mean outcome	0.33		0.33		0.33		0.33		0.33	
Only specialized assistants:										
Log assistants-per-child	0.050	0.014*	0.034	0.013*	0.042	0.011*	0.030	0.014*	0.060	0.021*
Number of observations	32 728		32 728		32 728		32 728		32 728	
Mean outcome	0.35		0.35		0.35		0.35		0.35	

Note: Estimates from OLS regressions of sickness absence depending on log of adults per child, teachers per child, or assistants per child. Year dummies are included. Control variables (age, gender, center-specific tenure, the share of children below the age of 3, and the share of minority children at the center level) included in all specifications except Column 1. Each coefficient from separate regression. Standard errors clustered on municipality level, except for the individual and center fixed effects models where we have clustered at the individual and center level respectively. * denotes significant at the 5 percent level. Specialized assistants are those with a child care education from upper secondary education.

Municipality characteristics may be important determinants of the workplace for the staff as child care centers are administrated and funded by their municipality. The municipality has some discretion on how to organize centers, supervise teacher standards, and allocate children to them. In Column 3 of Table 2, we report estimates that account for time-invariant (observed and unobserved) municipality characteristics. The association between assistants per child and sickness absence remains positive and statistically significant at 5.8 percentage points but declines somewhat compared to the model in Column 2 (with only observed controls). The same holds for teachers per child: the estimate declines but remains negative and statistically significant at 1.5 percentage points.

Next, we consider estimates from a model that accounts for time-invariant individual characteristics. We see in Column 4 of Table 2 that the result for assistants per child drops but remains positive at 2.9 percentage points. More teachers per child, however, is no longer associated with lower sickness absence. The point estimate is close to zero, the standard error is low and it is statistically insignificant, suggesting that the negative estimates are biased by omitted individual characteristics, like time-invariant selection by education level or health. We will comment on the point estimates for assistants per child later but note here that the point estimates for teachers per child support our hypothesis that the education level is integral in explaining differences in sickness absence between assistants and teachers. When staff composition effects by education level are *not* netted-out (the municipality fixed effects model), the point estimate for teachers per child is negative at 1.5 percentage points, whereas it is a precise zero when time-invariant selection by education level is netted-out (the individual fixed-effects model). Note that some of the other individual-level unobservables that are accounted for (e.g., cognitive and non-cognitive skills such as grit and perseverance) can be thought to be partly proxied by education, and all of them are captured by the individual fixed effects.

Recall that we compare educated teachers and low-educated assistants in the same child care center, taking care of the same children. They have the same workload in terms of the tasks they perform (Steinnes and Haug, 2013) and the number of hours they work (i.e., full time). The remaining difference between teachers and assistants is likely to be their education level. Several studies have documented that education level correlates strongly with health status, and some evidence suggests a causal link (Cutler and Lleras-Muney, 2010; Clark and Royer, 2013; Gathmann et al., 2015; Brunello et al., 2016). Even if education does not causally affect sickness absence, differences between assistants with no or limited higher education and educated teachers can be explained by non-cognitive skills such as motivation, grit, and perseverance, which may influence both education (as suggested by Heckman et al. (2006)) and the likelihood of sickness absence.

Running a model with center fixed effects gives similar results as the individual fixed effects model, see Column 5 of Table 2, at least for teachers. The estimates for teachers per child are close to zero. It is expected as the staff ratios are calculated at the center level. Hence, staff composition effects by education level are netted-out in both of these models. Comparing the estimates from the municipality fixed-effects model with the center fixed-effects model suggests similar results for assistants per child, but not for teachers per child. This difference might indicate that time-invariant center characteristics such as neighborhood or premises explain the negative association we see for teachers per child and sickness absence, whereas findings for assistants are robust to the inclusion of these fixed effects.[10]

There is a specialized education at the high school level for assistants in child care centers. To look at whether a match quality between the field of education and workplace drives the staff composition effects, we now turn to the lower level of Table 2. Coefficients are similar to the main results for assistants, although coefficient sizes are somewhat smaller. We take this to suggest that the education level is more important than the matching between the field of education and workplace when we want to understand the staff composition effects.

If we run sub-sample analyses based on the municipality fixed effects model, including teachers or assistants only, we find no association between the number of teachers per child and teachers' sickness absence: the point estimate is close to zero and statistically insignificant.

[10]When running a model where we include all three fixed effects simultaneously, the point estimates for teachers and assistants per child are similar to the individual fixed-effects model (presented in Column 4 of Table 2). Running a model with only individual and municipality fixed effects or individual- and center fixed effects also give similar results to the individual fixed-effects model.

However, more assistants per child are associated with higher sickness absence among assistants. More precisely, sickness absence increases by 4.8 percentage points when the number of assistants per child is one percent higher, see Table 3 and the bold typeface. These sub-sample analyses also net out staff composition effects and confirm our previous results from specifications where composition effects by education level are netted out (i.e., the individual and center fixed effects models).

Based on these two sub-samples, we can further inquire about spillover effects. We find that more assistants are associated with higher sickness absence among teachers, whereas there is no significant association between assistants' sickness absence and the number of teachers per child in child care centers, see Table 3 and the non-bold typeface.

Table 3: Sub-sample analyses: association between sickness absence (outcome variable) and log of teachers or assistants per child

	Teachers only		Assistants only	
	Coef.	Std. Err.	Coef.	Std. Err.
Log teachers-per-child	**0.003**	**0.007**	0.002	0.004
Log assistants-per-child	0.033	0.006*	**0.048**	**0.006***
Number of observations	110 430		194 040	
Mean outcome	0.27		0.35	

Note: Estimations based on the municipality fixed effects model. Year dummies and control variables are included. See note to Table 2. Standard errors are clustered on municipality level. * denotes significant at the 5 percent level. Point estimates with non-bold typeface are spillover effects.

It is hard to see how more assistants *per se* could causally increase sickness absence. We may thus take this to suggest that more assistants are associated with some contemporaneous events that are detrimental to sickness absence. Such events could be more demanding children not being fully compensated for by the additional assistants, or that assistants are hired to replace sick assistants or teachers (i.e., reverse causality). Child care centers may be compensated with more staff when more demanding children enroll, and assistants rather than teachers are likely to be hired in this setting. Also, in the case of new hiring due to sickness absence among the staff, assistants rather than teachers are likely to be employed. The reason is that assistants do not need any particular level or type of education to be hired. Hence, the pool of assistants is larger, making it easier and less expensive to hire an assistant than a teacher.

Seen together, our results suggest that staff composition, rather than workload, is essential in understanding the observed relationship between the number of adults and sickness absence.

5.3 Fuzzy regression discontinuity design (FRDD)

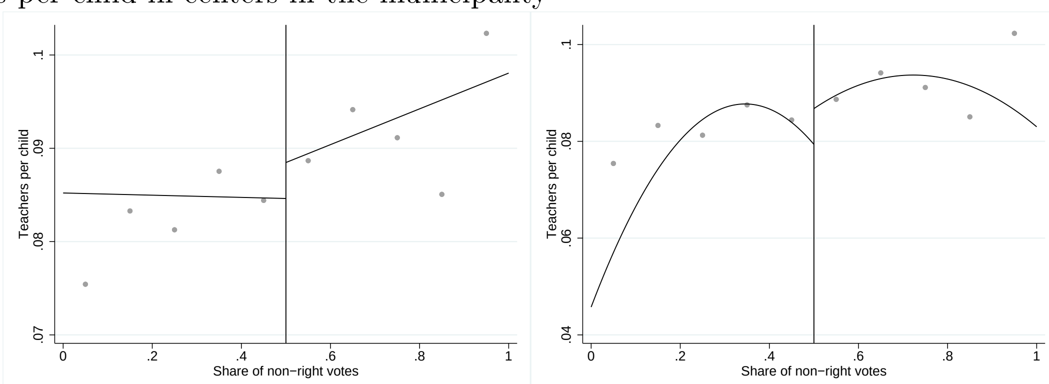
In an attempt to shed some light on the causal effect of staff composition on sickness absence, we follow Fiva et al. (2018) and exploit variation in the political block in power in local councils in Norwegian municipalities during the period 2007-2014.[11] We first estimate the impact of non-right parties winning the majority in local elections on the number of teachers or assistants per child as the non-right block, has a long time, advocated for the hiring of educated teachers over low-educated assistants to increase the share of educated personnel in child care centers. Then in a second stage, we retrieve estimates of how the number of teachers or assistants per child affects sickness absence in child care centers.

[11] There were 424 municipalities in Norway in 2007-2014. Elections are held in September every fourth year (2007 and 2011), and we look at the number of teachers or assistants per child in the years following the election (up to and including the next election year). We group political parties into two political blocks according to their inclination to hire educated teachers over low-educated assistants in publicly funded child care centers: a non-right block (high priority) and a right block (lower priority). The latter consists of the conservative party (H) and the progress party (FRP). The data are from Fiva et al. (2018): see http://www.jon.fiva.no/data.htm.

FRDD results

Figure 4 shows the number of teachers per child as a function of the running variable; the share of votes to the non-right parties in local elections. The Figure indicates that there are more teachers per child in child care centers in municipalities with a non-right local government, i.e., in municipalities where the non-right parties won the majority of the votes, in line with these parties' stated political priorities.[12]

Figure 4: Share of votes in municipal elections to non-right parties and subsequent number of teachers per child in centers in the municipality



Note: The solid line depicts a first/second-order polynomial fit using control and treated units separately. The gray dots represent the sample average for each disjoint bin. There are 222 792 observations on the left and 78 072 observations on the right of the cut-off.

Table 4 shows the estimated results from the FRDD-framework for different bandwidths. The first stage suggests, as indicated by Figure 4, that if the non-right block obtained at least 50 percent of the votes in the previous election, the number of teachers per child increases. The point estimates range from 3.8 to 14.3 percent, depending on the bandwidth. For assistants, we find the opposite: a non-right majority in local elections leads to fewer assistants per child in child care centers (except when the bandwidth is very narrow in Column 4).

The treatment effect estimates, i.e., the second stage, suggest that more teachers per child

[12] See Figure A.2 in the Appendix for the corresponding plot of assistants.

reduce sickness absence in child care centers (except when there is not an increase in *the share* of teachers, see Column 4), while more assistants per child increase it. A non-right majority may thus change the composition of the staff in centers, and sickness absence will drop if and only if the share of educated teachers in centers increases. That is when both more educated teachers (with low sickness absence) and fewer low-educated assistants (with high sickness absence) are being hired.

The treatment effect estimates are large in magnitude. Looking at the number of teachers per child, we see that there is a reduction in sickness absence ranging from 10-27 percentage points in Column 1-3, from a baseline of 33 percent. Keep in mind that this is a local average treatment effect that applies to the subgroup of compliers. That is, it applies to municipalities where the non-right block wins the majority and child care centers are exposed to an increase in the number of teachers per child.

The treatment effect is likely to be upward biased since the non-right parties may prioritize children and employees in other ways than solely through more teachers. To give the second stage estimates a causal interpretation, we need to assume that the entire effect on sickness absence in child care centers is coming from changes in staff composition when the non-right block gains the majority. It is a bold assumption. Reduced form estimates, however, are capturing the direct effect of having a non-right local government on sickness absence in centers. These estimates suggest that sickness absence in child care centers is lowered by 1-2 percentage points if the non-right block obtains majority and the share of teachers in centers increases (Columns 1-3 of Table 4). We think of this as an upper bound of the effect of a higher share of teachers.

Regarding assistants, if the share of assistants increases (Column 4 of Table 4), the direct effect of the non-right block winning the majority on sickness absence in centers is positive and amounts to around 5 percentage points. This estimate is similar to the assistants per child estimate in the municipality fixed-effects model in Table 2. As already noted, this estimate is likely to be biased due to reverse causality. In the FRDD-analysis, the non-right block may suffer from a teacher shortage: whereas having more educated staff in child care centers is high on the political agenda for the non-right parties, there might be a teacher shortage which impedes them from fulfilling this political objective. Although the number of teachers per child in child care centers increases (in Column 4 of Table 4), the share of teachers is not increasing - as more assistants are also being hired.[13,14]

Still, our results are robust across the different specifications regarding the number of teachers per child and suggest that if the share of educated teachers among the staff augments, sickness absence at the center level declines.[15]

[13]Concerning the non-right block and the number of adults per child, the first stage estimates (for the different bandwidths) are all positive. That is, when the non-right block gains the majority, the number of adults per child in child care centers increases. Regarding the second stage estimates for sickness absence in child care centers, the number of adults per child negatively affects sickness absence if the share of teachers increases (Column 1-3 in Table 4) and positively affects sickness absence if there is an increase in the share of assistants (Column 4 in Table 4).

[14]Some populated areas are suffering from a teacher shortage given the standard of teachers per child should be no smaller than 1/9 for toddlers aged 1-2 and 1/16 for children aged 3-5 (Gulbrandsen et al., 2016). Unfortunately, we do not have good enough data to study teacher shortage and the hiring of assistants over teachers as Norwegian registers lack job-application data.

[15]We have also tried to exploit random variation in the population of children à la Hoxby (2000). More precisely, we have exploited variation in the number of 1-year-olds to see if the variation in cohorts across municipalities affects the number of teachers or assistants per child in the first stage. Unfortunately, we do not have strong enough first-stage F-statistics (i.e., compliance ratio is too low) to exploit this strategy further.

Table 4: Effect estimates from the FRDD

Bandwidth		1		0.25		0.1		0.06	
Outcome	Independent variable	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
First stage:									
Log teachers-per-child	Non-right	0.065	0.001*	0.088	0.003*	0.143	0.004*	0.038	0.005*
Mean outcome		0.08		0.08		0.08		0.08	
Log assistants-per-child	Non-right	-0.059	0.001*	-0.047	0.002*	-0.052	0.003*	0.218	0.005*
Mean outcome		0.16		0.16		0.16		0.16	
Reduced form:									
Sickness absence	Non-right	-0.017	0.001*	-0.013	0.003*	-0.014	0.005*	0.047	0.008*
Second stage:									
Sickness absence	Log teachers-per-child	-0.267	0.008*	-0.151	0.041*	-0.099	0.028*	0.573	0.195*
Sickness absence	Log assistants-per-child	0.293	0.007*	0.271	0.074*	0.265	0.060*	0.342	0.043*
Mean outcome		0.33		0.33		0.33		0.33	
Eff. number of observations (left and right of the cut-off)		222 792 + 78 072		204 078 + 74 550		107 047 + 55 296		78 242 + 41 594	

Note: Point estimates based on non-parametric local-linear regressions with triangular kernel function of the given bandwidth. The bandwidth, or smoothing parameter, is manually set in Column 1-3. In Column 4, a mean-square-error optimal bandwidth selector is used. Results are similar with uniform kernel function and almost identical with or without covariates (age, gender, center-specific tenure, the share of children below the age of 3, and the share of minority children at the center level). Year dummies are included. * = statistically significant at the 5 percent level. In Column 4, the bandwidth becomes so narrow that we can no longer cluster on municipality level due to too few effective observations. The optimal bandwidth varies for teachers and assistants per child in Column 4. Stated in the table is the optimal bandwidth for teachers per child (0.06) and the effective number of observations for teachers per child (78 242 + 41 594). The optimal bandwidth is 0.04 and the effective number of observations is 35259+28908 for assistants per child. For the reduced form estimates in Column 4, the optimal bandwidth is 0.05 and the effective number of observations is 64498+33066.

Validity and robustness

Table 5 illustrates the validity of the instrument. As already stated, because the staff ratios - teachers per child and assistants per child - can potentially correlate with unobservables, we instrument the ratios to get a consistent estimate of the staff composition effect. The first stage estimates (displayed in Table 4) indicate that the instrument - the indicator variable of non-right, i.e., above the majority threshold - is relevant. A check for instrument exogeneity is to inquire whether the instrument is uncorrelated with other variables that are strong predictors of the outcome variable (sickness absence), i.e., individuals on both sides of the cut-off point should be very similar on average in observable characteristics. Column 2 of Table 5 shows that the instrument is indeed orthogonal to teacher and children characteristics.

Table 5: Validity of the instrument

	Sickness absence as outcome		Majority for the non-right block as outcome	
	Coef.	Std. err.	Coef.	Std. err.
Male	-0.05	(0.005)*	-0.008	(0.006)
Age	-0.002	(0.0002)*	-0.00003	(0.0002)
Experience	-0.008	(0.0005)*	-0.0007	(0.0005)
Experience^2	-0.0003	(0.00002)*	0.00003	(0.00003)
Share of children under 3	0.11	(0.012)*	0.03	(0.01)
Share of minority children	0.03	(0.005)*	0.01	(0.009)
Number of observations	300 864		300 864	
Mean outcome	0.33		0.43	

Note: The regressions include a constant and year dummies. Standard errors clustered on municipality level. * denotes statistically significant at the 5 percent level.

The running variable, the share of non-right votes, must be continuous around the cut-off point. There should be no sign of individuals manipulating the running variable to increase their chances of being treated. For identifying electoral effects, it means that relevant actors should not have precise control over election results. Several papers point out that the incumbent party is more likely to win. However, Eggers et al. (2015) conclude that the assumptions behind the RD design are likely to be met in a wide variety of electoral settings. Still, we use a donuts strategy (that is, exclude a window of $\pm$ 0.01 around the discontinuity) to check for manipulation of the running variable around the threshold. Table A.1 in the Appendix shows that the point estimates only marginally change when we exclude observations close to the thresholds, and the main results are not meaningfully changed (compared to Table 4). Note, however, that in Column 4 of Table A.1, there is now a reduction in the number of assistants per child. Hence, there is a negative effect on sickness absence when the number of teachers per child increases as the share of educated teachers among the staff now augments. Moreover, the reduced form effect of the non-right block winning the majority in local elections on sickness absence in centers is slightly higher. Figure A.3 in the Appendix shows the density of the running variable (the share of non-right votes) and indicates that it is fairly smooth around the cut-off.

Finally, we run placebo tests. That is, we use fake thresholds, where the non-right block is getting 0.15, 0.2, 0.35, 0.65, 0.8, or 0.85 shares of the votes. See Figure A.4 in the Appendix. We find that there is no longer a jump in the number of teachers per child in child care centers, except for at 0.35. The existence of a placebo jump does not invalidate the FRDD and can probably be explained by non-right parties being in coalition with the right-wing and still triumph in cases regarding child care and notably regarding the importance of increasing the share of teachers in centers.

Seen together, our results suggest that staff composition, rather than workload, is essential in understanding the level of sickness absence in child care centers. Increasing the share of educated teachers with inherently low sickness absence reduces the sickness absence, whereas increasing the share of low-educated assistants with inherently high sickness absence increases the sickness absence in child care centers.

6 Conclusion

Workplace conditions are risk factors for sickness absence (Labriola et al., 2006; Eriksen et al., 2016), but little is known about the effects of the various indicators of workload (Bratberg et al., 2017; Gørtz and Andersson, 2014; Conen et al., 2012; Defebvre, 2018). We use the number of adults per child as a proxy for workload in child care centers to inquire if it influences sickness absence. We also distinguish between teachers and assistants and estimate associations separately for these groups. The integral difference between teachers and assistants is their education level: teachers have at least three years of higher education, whereas only 8% of the assistants have tertiary education. Our study can not explain the reason for the difference in sickness absence across employees with and without higher education, but several studies have documented that education level correlates strongly with health status, and some evidence suggests a causal link (Cutler and Lleras-Muney, 2010; Clark and Royer, 2013; Gathmann et al., 2015; Brunello et al., 2016). Moreover, non-cognitive skills such as motivation, grit, and perseverance influence the decision to engage in education, and we can not rule out that such traits also influence sickness absence. The distinction between teachers and assistants thus allows us to pay attention to methodological challenges related to changes in the composition of employees with high/low education level and inherently low/high sickness absence.

Our results show that more adults per child are not associated with lower sickness absence, in line with the findings of Gørtz and Andersson (2014). However, decomposing the number of adults into educated teachers and low-educated assistants reveals that more teachers per child are associated with lower sickness absence in child care centers, while it is the opposite for assistants. When accounting for municipality fixed effects (because child care centers are administrated and funded by the municipality), the opposing associations for teachers and assistants prevail: sickness absence is 1.5 percentage points lower when the number of teachers per child is one percent higher, whereas sickness absence is 5.8 percentage points higher when the number of assistants per child is one percent higher. When accounting for individual fixed effects, where no time-invariant variation in education level or health status remains, i.e., staff composition effects are netted out, the estimate for teachers per child becomes close to zero and statistically insignificant, suggesting that the negative estimates for teachers per child are biased by omitted individual characteristics, like time-invariant selection by education level or health. In contrast, the estimate for assistants per child drops but remains positive at 2.9 percentage points and statistically significant, suggesting that the estimates for assistants per child may suffer from omitted variable bias or reverse causality. For example, if a center learns that it will receive more demanding children in the future, it may respond by hiring more assistants. Unless the additional assistants can fully compensate for the more demanding children, such dynamics will erroneously show that more assistants increase sickness absence. A similar bias will occur if a center experiences or foresees higher sickness absence among the staff and responds by hiring more assistants.

Trying to address possible model deficiencies, we use results from municipal elections where non-right parties obtained at least 50 percent of the votes (Fiva et al., 2018). Based on a fuzzy regression discontinuity design, we find that non-right parties increase the share of educated teachers in line with these parties' stated political priorities of increasing the number of educated personnel in child care centers. The number of teachers per child increases by 3.8-14.3 percent (depending on the bandwidth) in centers in municipalities where the non-right block obtained the majority. In the second stage, we find that more educated teachers among the staff reduce sickness absence in child care centers. The treatment effect of more teachers per child on sickness absence is likely to be upward biased since the non-right parties may prioritize children and employees in other ways than solely through more teachers. Nevertheless, reduced form estimates suggest that there is a beneficial effect of 1-2 percentage points on sickness absence of having a non-right local council, possibly for at least two reasons: more educated teachers with low sickness absence and fewer low-educated assistants with high sickness absence are being hired.

Our analysis underlines that evaluating the effects of workload on sickness absence is difficult. Policymakers and leaders may want to improve working conditions or the quality of a produced good by increasing the number of employees. It is typically very costly, but costs calculated by mechanically looking at the increase in salaries may not be accurate. Substituting workers with high sickness absence, often low-educated, with workers with low sickness absence, often high-educated, may not increase costs to the same extent as the higher salaries of high-skill workers since their sickness absence is lower. Our analysis suggests that such composition effects may be more important than causal effects of workload on sickness absence in Norwegian child care centers.

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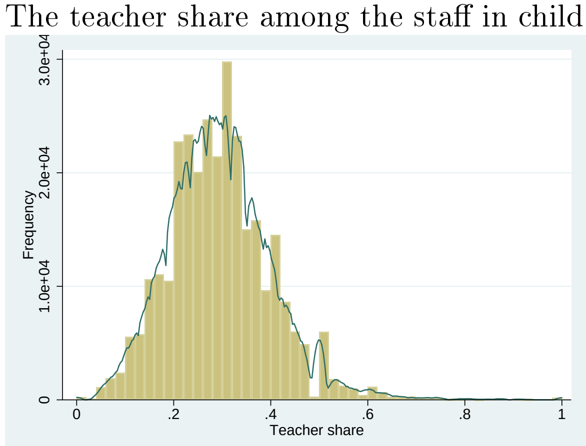
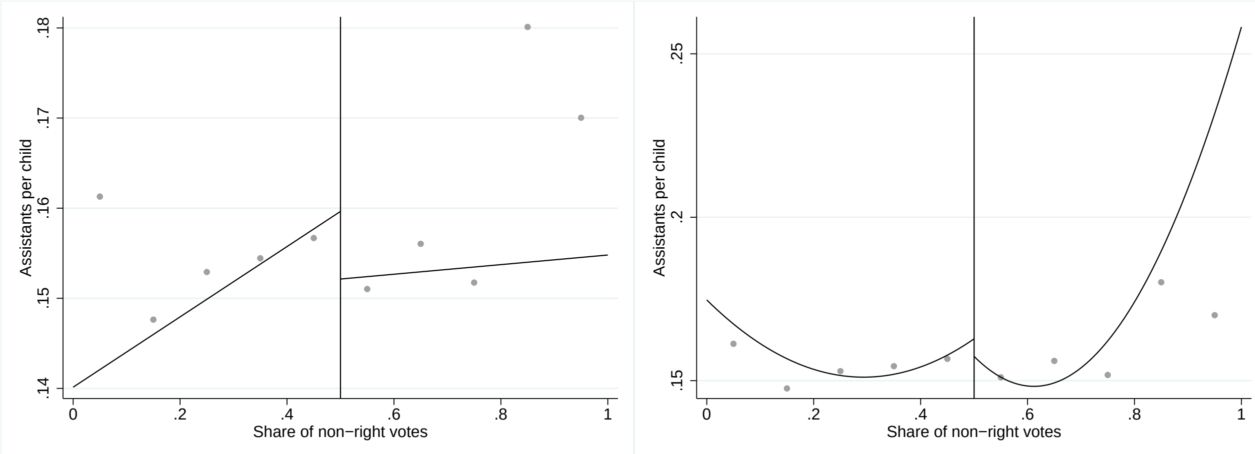
Appendix

Figure A.1: The teacher share among the staff in child care centers



Note: On average, the share of teachers is 0.3. There are few centers with more teachers than assistants.

Figure A.2: Share of votes in municipal elections to non-right parties and subsequent number of assistants per child in centers in the municipality



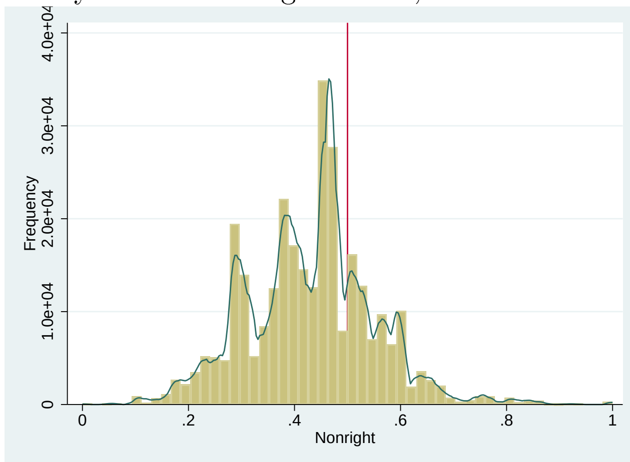
Note: The solid line depicts a first/second-order polynomial fit using control and treated units separately. The gray dots represent the sample average for each disjoint bin. There are 222 792 observations on the left and 78 072 observations on the right of the cut-off.

Table A.1: Donuts strategy: effect estimates from the FRDD

Bandwidth		1		0.25		0.1		0.06	
Outcome	Independent variable	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
First stage:									
Log teachers-per-child	Non-right	0.122	0.002*	0.178	0.003*	0.384	0.005*	0.436	0.008*
Mean outcome		0.08		0.08		0.08		0.08	
Log assistants-per-child	Non-right	-0.114	0.001*	-0.133	0.002*	-0.209	0.004*	-0.151	0.008*
Mean outcome		0.16		0.16		0.16		0.16	
Reduced form:									
Sickness absence	Non-right	-0.032	0.005*	-0.037	0.004*	-0.074	0.007*	-0.055	0.009*
Second stage:									
Sickness absence	Log teachers-per-child	-0.260	0.006*	-0.205	0.024*	-0.193	0.018*	-0.115	0.026*
Sickness absence	Log assistants-per-child	0.280	0.005*	0.274	0.032*	0.354	0.033*	0.264	0.085*
Mean outcome		0.33		0.33		0.33		0.33	
Eff. number of observations (left and right of the cut-off)		218044+66635		199330+63113		102299+43859		68687+25125	

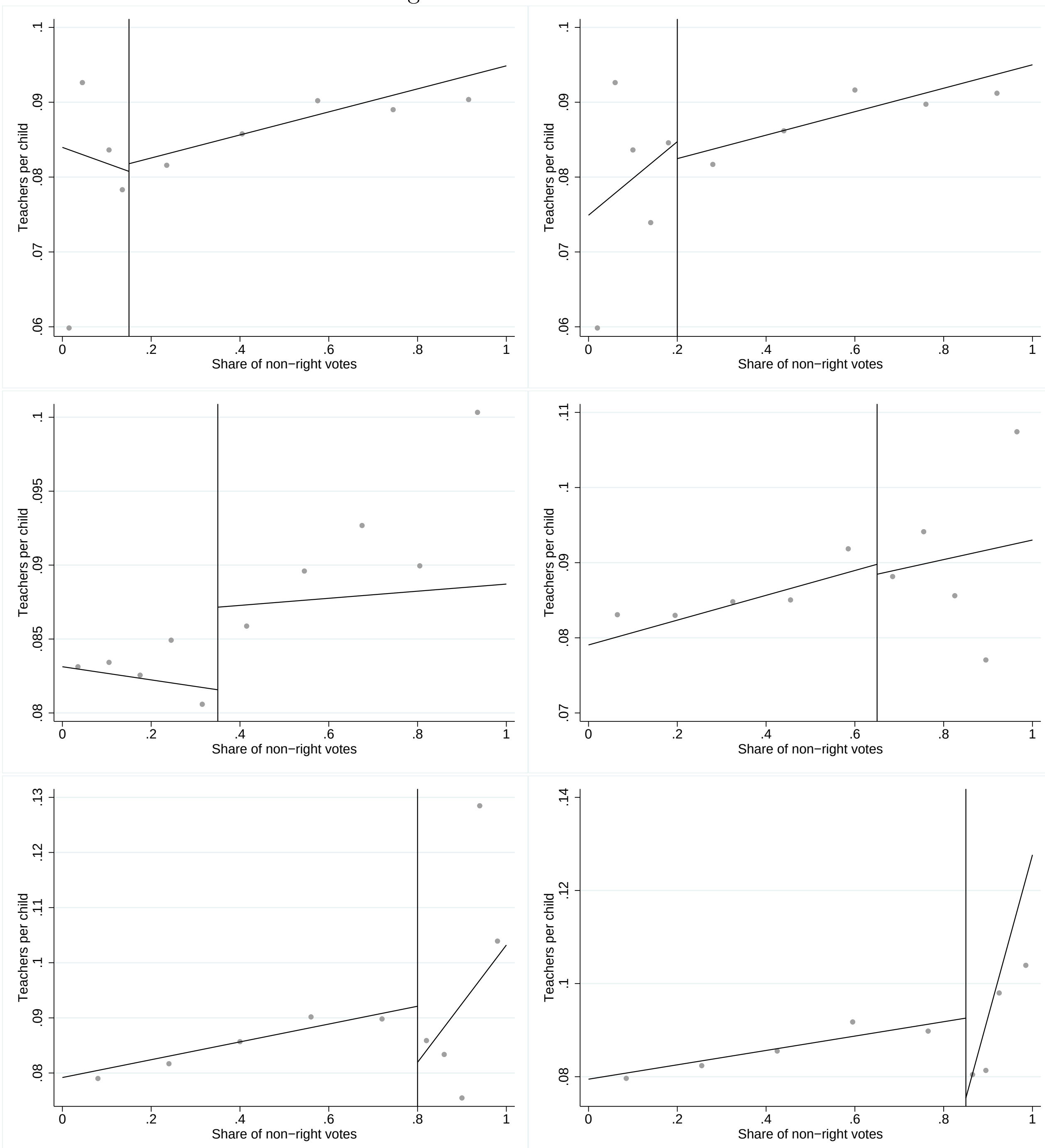
Note: Point estimates based on non-parametric local-linear regressions with triangular kernel function of the given bandwidth. The bandwidth, or smoothing parameter, is manually set in Column 1-3. In Column 4, a mean-square-error optimal bandwidth selector is used. Results are similar with uniform kernel function and almost identical with or without covariates (age, gender, center-specific tenure, the share of children below the age of 3, and the share of minority children at the center level). Year dummies are included. * = statistically significant at the 5 percent level. A window of ± 0.01 around the discontinuity is excluded, i.e., 16 185 observations. Number of observations=284 679. Without the donuts, the number of observations=300 864. In Column 4, the bandwidth becomes so narrow that we can no longer cluster on municipality level due to too few effective observations. The optimal bandwidth varies for teachers and assistants per child in Column 4. Stated in the table is the optimal bandwidth for teachers per child (0.06) and the effective number of observations for teachers per child (68687+25125). The optimal bandwidth is 0.05 and the effective number of observations is 64946+22864 for assistants per child. For the reduced form estimates in Column 4, the optimal bandwidth is 0.065 and the effective number of observations is 74127+30273.

Figure A.3: Density of the running variable, x-line for the majority at 0.5



Note: There are 222 792 observations on the left and 78 072 observations on the right of the cut-off (the red x-line). The threshold at 0.5 indicates that the non-right block obtained the majority in the local election.

Figure A.4: Placebo-tests



Note: We use fake thresholds at 0.15, 0.2, 0.35, 0.65, 0.8, or 0.85. That is, thresholds below and above the actual threshold at 0.5 where the non-right block is winning the majority in local elections to study the development in the number of teachers per child. The solid line depicts a first-order polynomial fit using control and treated units separately. The gray dots represent the sample average for each disjoint bin. See the previous Figure (Figure A.3) for the distribution of the share of votes to the non-right parties.