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The effect of central government grants on local educational policy☆

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ABSTRACT

The use of intergovernmental grants in educational policies may give rise to a conflict between gains attributable to local flexibility and the central government's intention to narrow gaps in school spending and resource use across local jurisdictions. This paper estimates the impact on school resources of a Norwegian central government grant intended to decrease the student-teacher ratio in primary school (grades 1–4). The grant was given to the 100 municipalities with the highest student-teacher ratios out of more than 400 municipalities. Using a difference-in-differences approach, our results show that Norwegian municipalities did not increase teacher density in primary schools, despite receiving extra grants for this purpose. Though we cannot rule out that there was some take-up of our grant, we can exclude full take up. Our results suggest that strong enforcement mechanisms may be necessary for earmarked grants to affect local allocation of resources as intended by central governments, although this might come at the cost of reducing local flexibility.

1. Introduction

Public finance theory suggests that leaving tax and spending decisions to lower level governments (fiscal decentralization) improves allocative efficiency, i.e. the matching of goods and services provided by lower level jurisdictions to the preferences of residents in these jurisdictions; see Oates (1999). However, theory also suggests that intergovernmental grants can be used as a policy tool by upper level governments to internalize externalities across lower level governments or to reach certain distributional goals. An important empirical question is to what extent do receiving lower level governments allocate grants in the way intended or recommended by the upper level government?

There is a large empirical literature analysing the extent to which receiving lower level governments allocate targeted grants in the ways intended or recommended by upper level governments. A common finding, especially in the early literature, is that additional grants are to a large extent spent as intended by the granting government, commonly referred to as the "flypaper effect" (see Hines and

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Thaler 1995; Fisher and Papke 2000). However, recent empirical studies exploiting quasi-experimental methods to identify the causal effects of grants find mixed effects of intergovernmental grants on lower level government spending (Card and Payne 2002; Gordon 2004; Cascio et al., 2013; Brunner et al., 2017; Hyman 2017). These studies indicate that the effect of intergovernmental grants may be sensitive to the design and target of the grant, as well as the economic and institutional setting. Thus, more evidence is needed to reach a proper understanding of the relationship between the design of grant policies and the real effects of the grants on spending and resource allocation at the local level.

In 2015, the Norwegian government gave a grant to the 100 municipalities with higher than average student-teacher ratios for grades 1–4 with the aim of strengthening early intervention and improving student learning. The grant was distributed as a sum per student based on the average number of students in grades 1–4 the previous three years. By exploiting the design of this grant, this paper adds to the existing literature on intergovernmental grants in several ways.

First, we provide quasi-experimental evidence on the effect of earmarked central government grants in a system with multipurpose municipalities with limited flexibility on the revenue side, but relatively extensive flexibility on the spending side, similar to the institutional framework in many European countries.[1] Most of the existing empirical evidence on the effects of intergovernmental grants are from the US where school districts are *single-purpose* institutions, and where local property taxes remain the core of most state financing systems, as pointed out by Hoxby (2001).

Second, motivated by recent evidence from the US showing substantial effect heterogeneity of state and federal school finance policy changes (see e.g. Cascio et al., 2013; Brunner et al., 2017), we analyze the extent to which the grant effect depends on teacher supply constraints as well as population size and the availability of additional revenue sources.

Third, detailed Norwegian register data allow us to investigate how the grant translates into resource use at the *school level*. Knowledge of the effect of the grant on student-teacher ratios at the service-producing unit (the school) in the grant-receiving jurisdiction (the municipality) is crucial to an understanding of the extent to which grant policies have the intended effects. However, little evidence exists of the actual distribution of additional funds across schools. To our knowledge, Hyman (2017) is the only study available so far to investigate this issue. He finds that additional school district resources originating from changes in state education finance schemes were distributed to schools that were not the intended beneficiaries in the first place.[2]

Finally, the Norwegian setting makes it possible to compare the effects on the allocation of school resources of two central government grant policies with different designs, but with similar intentions. The grant we study was received by municipalities in 2015 and was intended to decrease the student-teacher ratio in primary school (grades 1–4), with no clear restrictions on the allocation between schools within the municipalities. Kirkebøen et al. (2017) evaluate another central government grant, introduced in 2012, aimed at decreasing the student-teacher ratio in lower secondary schools (grades 8–10). Importantly, the 2012 central government grant was designed with a clear instruction that the grant received by the municipalities should be distributed to *schools* with less than average student-teacher ratios and less than average student performance in the pre-policy period. The final question we raise in this paper is to what extent the 2015 grant, leaving much more discretion to municipalities with respect to the distribution of funds across schools, had similar effects on the student-teacher ratio.[3]

Our results show that Norwegian municipalities did not increase teacher density in primary schools, despite receiving extra grants for this purpose. Though we cannot rule out that there was some take-up of our grant, we can exclude full take-up. Our finding is robust to several robustness checks and heterogeneity analysis. Kirkebøen et al., 2017 found that their grant had the intended effect, decreasing student-teacher ratios by roughly 10% at the targeted schools, although they did not find any effect on student performance. The different experiences from these two grant policies suggest that stronger enforcement mechanisms may be necessary in order for targeted grants to affect local allocation of school resources as intended by higher level governments, although this may be at the expense of reduced gains ensuing from local flexibility.

The paper is organized as follows: Section 2 describes the institutional setting and the policy intervention. Section 3 presents the empirical strategy and data, and Section 4 outlines the results. Sections 5 presents analyses of robustness. Finally, Section 6 presents concluding remarks.

[1] School spending decisions in Norway, as in most European countries, are made by multipurpose local governments providing a wide range of welfare services such as health care, care for the elderly, kindergartens, culture and infrastructure in addition to compulsory schooling. These local governments enjoy substantial discretion on the spending side of the budget, while the revenue side is closely regulated by tax-sharing arrangements and the formal grant scheme.

[2] Hyman (2017) explores a court-ordered change in the Michigan education finance scheme in the 1990's intended to reduce education inequalities by equalizing spending across school districts. His findings suggest that school districts directed additional funds from this change towards schools serving less-poor populations within the districts that would not have been the intended beneficiaries of the change in the state finance scheme in the first place.

[3] Our paper also relates to a study by Lunder (2016), who investigates whether a national benefit norm for municipal social assistance payments issued by the Norwegian central government affected social assistance payments at the municipal level. While a large share of municipalities apparently complied with the national norm in terms of having local benefit norms, this did not translate into a change in actual payments, leading Lunder to conclude that the national benefit norm had little impact on municipality level outcomes.

2. Norwegian municipalities and the central government grant policy intervention

2.1. Institutional setting

The Norwegian public sector is divided into three tiers; the central government, the county government and the municipality. The counties and municipalities constitute the local public sector,[4] whereas the central government has the overriding authority and supervision of municipality and county administration. As in the other Nordic countries, the Norwegian public sector at the municipality level is responsible for providing a wide range of welfare services. Compulsory education is one of the core responsibilities of municipalities, illustrated by its budget share of 22% in 2014. The corresponding budget shares for care for the elderly, childcare, health care, culture and infrastructure are 30%, 13%, 4%, 4% and 9%, respectively (Borge 2015). Schooling is provided free of charge, and less than 1.5% of the students were enrolled in private schools in the empirical period. Compulsory education consists of three stages: lower primary education, grades 1–4 (ages 6–10); upper primary education, grades 5–7 (ages 10–13) and lower secondary education, grades 8–10 (ages 13–16). There are usually several public schools within each municipality. Education is comprehensive with a common curriculum for all students and there is no tracking.

The municipalities' activities are mainly financed by taxes (42% of current revenue) and grants from the central government (37%). User charge (14%), interest (5%) and other revenues (2%) account for the rest.[5] Grants are mostly block grants based on objective criteria,[6] and most tax revenues are income tax payed by individuals. Income tax revenue is shared between municipalities, counties and the central government. Since the 1992 tax reform, income has been taxed at an overall flat rate of 28%, which decomposes into rates of about 13% for municipalities, 3% for counties and 12% for the central government. Norwegian municipalities can set their tax rates within a narrow band. However, since 1979 all municipalities have applied the maximum rate.

Municipalities have very limited opportunity to influence current revenue. As the income levels available for taxation vary across municipalities, equalization is achieved through tax equalization and spending needs equalization. The spending needs equalization system is arranged as a pure redistribution scheme between municipalities. Thus, transfers to municipalities with above average spending needs (per capita) are financed by contributions from municipalities with below average spending needs. The system lifts municipalities at the bottom to 90% of the average tax base while reducing tax bases at the top (Borge et al. 2014).

While the revenue side of the municipal budget is closely regulated, as described above, municipalities enjoy substantial discretion on the spending side. Subject to legal regulations, municipalities have full discretion in the allocation of revenues among different welfare services. For instance, there is no national rule regarding how much of the total budget municipalities should use on compulsory education, or how these resources should be distributed among schools and classes. Municipalities are, however, responsible for providing the resources necessary to enable them to comply with legal requirements, such as the Norwegian Education Act (Education Act, Section 13-10).

The current system is the legacy of public-sector decentralization during the 1980s, intended to strengthen local democracy and improve efficiency. Before 1980, municipality expenditures were mainly financed by earmarked reimbursements from the central government. Beginning in the early 1980s, the central government started to replace these reimbursements with specific grants covering all sectors for which the municipalities were responsible. In 1986, a block grant reform was introduced to decentralize spending decisions and give municipalities incentives to allocate revenue optimally between activities. The Municipality Act of 1992 also allowed more freedom to organize both the administrative and the local political system as it suited the municipality.

Within the educational sector, reduced central government regulation on the spending side has also been accompanied by a shift in the collective bargaining system for teachers. The traditional system of negotiations between the central government and the teachers' unions has been replaced by negotiations between municipalities and unions, formally introduced in 2003.[7] In the same year, the strict maximum class size rule was replaced by a requirement for a justifiable pedagogical group size. However, during the last 15 years, the move towards decentralized decision-making in the educational sector has been constantly under pressure. In the debate, teachers' unions and other stakeholders argue that municipalities have used their local discretion on spending to gradually increase the student-teacher ratio.[8] A typical demand has been that the central government should impose stricter minimum standards and regulate the student-teacher ratio. The central government grant studied in this paper can thus be interpreted as a deviation from the previous path of more decentralized decision making in the public sector and in the provision of educational services in Norway.

[4] There are 428 municipalities and 19 county governments (2016). The capital, Oslo, is both a municipality and a county.

[5] User charges are strictly forbidden in public compulsory schools.

[6] The criteria for educational grants include the population 6–15 years of age, degree of urbanization and travel distance.

[7] In reality, the wage-setting system continued to be quite centralized, since most wage increases for teachers are still decided in national contracts between the Norwegian Association of Local and Regional Authorities (KS) and national unions. However, since 2003 the local units have, at least formally, the possibility of deviating from the basic contracts.

[8] Union of Education Norway («Utdanningsforbundet»), the Christian Democratic Party (Kristelig Folkeparti), the Socialist Left Party (Sosialistisk Venstreparti) and the Labor Party (Arbeiderpartiet) have all argued in favor of a statutory national teacher density norm.

2.2. The central government grant policy intervention

As part of the national budget agreement in the Storting (parliament) in 2015, the Norwegian central government introduced a grant to enable teacher density to be increased in grades 1–4, the intention being to strengthen early intervention and improve student learning.[9] The grant explicitly targeted municipalities with adjusted student-teacher ratios (see Section 3.2) above the national average in the school years 2012/13–2014/15. Following this criterion, 100 out of 428 municipalities were awarded a total of approximately NOK 360 million per year, and the funds were distributed as a sum per student, based on the average number of students in grades 1–4 in the three school years 2012/13–2014/15.

Given that the average expense for a teacher person-year in Norway is approximately NOK 700 000, the grant for the 2015/16 school year would translate into approximately 523 extra teacher person-years, or an average of 5.23 teacher person-years in each of the treated municipalities.[10] If we assume the student-teacher ratio in the treated municipalities would be the same in the 2015/16 school year as in the 2014/15 school year in the absence of the government grant, and that treated municipalities were 100 percent compliant, an average increase in teacher person-years of 5.23 translates into a 5 percent reduction in the unadjusted student-teacher ratio from the 2014/15 school year to the 2015/16 school year.[11]

Municipalities were told to spend the extra funds on increasing the number of teachers in regular teaching in grades 1–4. Beyond that, they had full discretion as to how to distribute resources across schools and how to utilize the new teachers. The Norwegian Directorate for Education and Training (Udir) was responsible for both transferring grants and for monitoring the grant flow. If a municipality failed to use the grant as intended, they were informed by Udir that they would not receive extra grants the following year. Out of the 100 targeted municipalities, 80 reported back to Udir on how the grant was used. 77 of these municipalities reported how many teacher person-years (FTEs) they had employed, while three municipalities only reported back on how much of the grant had been used. The majority of the municipalities reported to Udir that they had employed the same number of teacher person-years as predicted by the grant amount.

The policy intervention was the result of a political compromise in the fall of 2014 and was fairly unexpected by local policymakers. The official press release about the grant is dated March 20, 2015,[12] whereas municipalities reported the student-teacher ratios for the school year 2014/2015 in October 20, 2014.[13] It follows that this leaves little room to manipulate treatment status by inflating the reported adjusted student-teacher ratios for 2014/2015. The grant policy thus represents a positive resource shock and can be used as a natural experiment to understand how earmarked central government grants affect local educational spending. A potential limitation with this line of argument is that the former left-wing government had discussed possible ways to introduce national minimum teacher density rules already in the fall of 2010 (see Ministry of Education, 2010 and Borge et al. 2012). Since similar policy proposals had been on the political agenda some years earlier, we cannot completely rule out the possibility that some municipalities might have decreased teacher densities in the hopes to gain from what they perceived as likely future grants, even though it is unlikely that they would be able to predict how the grant would be distributed. This potential threat to identification will be addressed in the robustness analyses in Section 5.

3. Empirical specification and data

3.1. Empirical specification

We specify a regression equation that compares the development in the adjusted student-teacher ratios in treated municipalities with untreated municipalities in a difference-in-differences setting. This approach identifies the causal effect of the intervention under the assumption that the treated municipalities would have continued along a similar time trend to those in the comparison group in the absence of the additional resources. In Section 3.3, we discuss at length the possible reasons why this assumption may not hold as well as possible ways to adjust the comparison group to satisfy the assumption.

At the outset, we base the assignment of municipalities into treatment or comparison group on the explicit implementation of the intervention as described in Section 2. Thus, the treated municipalities (those that receive the grant) had an adjusted student-teacher ratio during the school years 2012/2013–2014/2015 above the national average. The difference-in-differences model with outcomes measured at municipality level can be specified in a regression framework as follows:

$$y_{mt} = \alpha_0 + \beta(treated_m \times d_t) + \eta_m + X_{mt}\delta + \tau_t + \varepsilon_{mt}, \quad (1)$$

where m indexes municipalities and t indexes school years. $treated_m$ is an indicator variable equal to 1 if the municipality is among the 100 municipalities that receive additional resources, and 0 otherwise. d_t is an indicator variable that is equal to 1 after the grant policy is introduced, when $t \geq 2015/2016$, and 0 when $t < 2015/16$. The parameter of interest β gives the average impact of the grant policy on the outcome of interest. X_{mt} is a row vector of time-varying municipality control variables with corresponding coefficient vector δ and

[9] https://www.regjeringen.no/no/aktuelt/flere-larere-til-de-yngste-elevene/id2400739/.

[10] For more details on the grant, see Online Appendix A.

[11] As we do not have detailed information about the denominator and nominator in the adjusted student-teacher ratio, we are not able to calculate the potential change in adjusted student-teacher ratio given total compliance to the grant. We assume that a five percent reduction in student-teacher ratio also applies as an upper bound effect of the grant on the adjusted student-teacher ratio.

[12] https://www.regjeringen.no/no/aktuelt/flere-larere-til-de-yngste-elevene/id2400739/.

[13] https://gsi.udir.no/registrering/.

Table 1
Summary statistics for pre-treatment period 2004/05–2014/15.

	(1) Treated municipalities	(2) All untreated municipalities	(3) Normalized diff.
	Mean (sd)	Mean (sd)	*Treat. – Comp.*
Outcome variables:			
Adjusted student-teacher ratio	16.90 (1.68)	12.62 (2.60)	−1.418
Student-teacher ratio	14.14 (1.45)	10.74 (2.04)	−1.417
Teacher man years in teaching	99.03 (174.17)	20.68 (22.04)	−0.847
Teacher man years in regular teaching	77.51 (127.84)	16.84 (18.03)	−0.883
Municipality characteristics:			
Number of students grades 1-4	1523.08 (2714.99)	256.07 (300.99)	−0.879
Non-certified teachers measured at regional level (%):	3.01 (1.99)	3.05 (2.27)	0.019
Population size (measured in thousands)	31.80 (65.99)	5.15 (5.82)	−0.779
Share aged 1–5 (%)	6.31 (0.73)	5.52 (1.01)	−0.783
Share aged 6–15 (%)	13.39 (1.28)	13.01 (1.52)	−0.257
Share aged 67–79 (%)	8.70 (1.38)	10.61 (2.10)	0.902
Share aged 80+ (%)	4.25 (1.03)	5.87 (1.46)	1.058
Share immigrant (%)	9.46 (4.64)	6.15 (3.38)	−0.834
Share divorced (%)	11.69 (1.84)	9.82 (2.12)	−0.847
Share disabled (%)	9.95 (2.73)	11.53 (3.35)	0.480
Share higher education (%)	0.24 (0.07)	0.18 (0.04)	−1.156
Degree of urbanization (%)	76.11 (17.71)	43.11 (25.12)	−1.203
Observations	1100	3608	

Note: Standard deviations are reported in parentheses. Detailed definitions of variables are shown in Online Appendix B.

ε_{mt} is the error term. School-year fixed effects τ_t capture the common time effects across municipalities while municipality fixed effects captures time-invariant differences between municipalities. In all regression results we report robust standard errors clustered at municipality level to account for serially correlated errors within municipalities.

3.2. Data

Our empirical strategy requires municipality level data on student-teacher ratios as well as data on municipality characteristics. From the Norwegian primary and lower secondary information system (GSI) we have school level information on the number of students, number of teachers and number of non-certified teachers in grades 1–4 and grades 5–7, measured annually on October 1.[14]

The outcome variables we use to measure educational spending are different definitions of student-teacher ratios.[15] In the basic specification of the regression model we use the adjusted student-teacher ratio. The central government uses two different measures of resource use in schools – the unadjusted student-teacher ratio and the adjusted student-teacher ratio. The unadjusted student-teacher ratio expresses the average number of students per teacher, while the adjusted student-teacher ratio expands on this measure by taking special needs education etc. into account and gives a better indication of the group size in a regular teaching situation.[16] As municipalities were instructed to spend the extra resources on increasing teacher person-years for regular teaching[17] in grades 1–4, the adjusted student-teacher ratio is our preferred outcome measure and is used in the baseline model. However, as a robustness check we also use the unadjusted student-teacher ratio as an outcome variable. Data on the adjusted and unadjusted student-teacher ratio are collected directly from the GSI. Furthermore, the Norwegian Social Science Database, Statistics Norway and the database provided by Fiva et al. (2017), give us access to numerous municipality level controls. As the focus in this paper is on decisions made by municipalities with regards to resource allocation, we focus on municipality characteristics that are important for predicting resource allocation.

The analysis uses data for the school years 2004/2005 to 2016/2017. In the 2016/2017 school year, many of the treated municipalities received more teachers through the research projects "1 + 1 Project" and "Two Teachers", which started up in 2016/2017 (Solheim and Opheim 2019). 62 municipalities received additional resources to hire teachers through these research projects: 41 of the 100 treated municipalities and 21 of the untreated municipalities. In our main specification we therefore do not include observations

[14] Information for the school year 2015/2016 was measured on October 1, 2015.

[15] Alternatively, as local governments annually report income and expenditures by sector of activity to Statistics Norway, we could use education expenditure as the outcome variable. Unfortunately, we cannot pin down expenditures for grades 1-4 separately. Thus, variants of student-teacher ratios provide the most precise measure of educational resources when we want to isolate the effect of the grant policy.

[16] The unadjusted student-teacher ratio is defined as the ratio of the total number of student hours to the total number of teaching hours and gives a picture of the total use of resources at a school or in a municipality. The adjusted student-teacher ratio, used as the teacher density measure when deciding which municipalities would receive extra funding, excludes resources for special-needs education and Norwegian for second language learners and is an indication of students per teacher in regular education. Technically, the adjusted student-teacher ratio is given as: (Total number of students hours - student hours in special needs education - student hours in Norwegian for second language learners) / (Total number of teacher hours in regular education).

[17] Defined as all regular teaching excluding special needs education and basic Norwegian for language minorities.

beyond the school year 2015/2016.

The dataset covers all 428 municipalities in Norway during the period 2004–2016, and descriptive statistics for outcome variables and municipality characteristics for the pre-treatment period are presented in Table 1. Columns (1) and (2) present averages and standard deviations for the treated municipalities and *all* untreated municipalities prior to treatment. Column (3) displays normalized differences for our controls (Imbens 2015). Focusing on the municipality characteristics, Table 1 suggests that there are obvious differences between treated municipalities and untreated municipalities, especially with regards to "urban" characteristics such as number of students and inhabitants, degree of urbanization and immigrant share. The treated municipalities are larger and more "urban" than all the untreated municipalities. Indeed, when we highlight the treated municipalities in a map of Norway in Fig. 1 it turns out that about 50 percent of the treated municipalities are situated in the south-east of Norway (around the Norwegian capital Oslo). In addition, the fifteen most densely populated municipalities (Oslo, Bergen, Trondheim, Stavanger, Bærum, Kristiansand, Fredrikstad, Sandnes, Tromsø, Drammen, Sandefjord, Asker, Sarpsborg, Skien and Sarpsborg) are all treated municipalities.

3.3. Common trends assumption

Although municipality fixed effects will control for time-invariant differences between the treated municipalities and the comparison municipalities, there is still concern that determinates of the grant policy are systematically related to underlying trends in our measures of educational resources. We may be worried about differences in the characteristics of treatment and comparison groups not captured by the municipality fixed effects. For example, one could argue that municipalities with adjusted student teacher ratios above the national average in the period 2012/13–2014/15 had a systematically different development in terms of student enrolment, student characteristics and faced different teacher supply constraints compared to municipalities below the national average. One way to handle

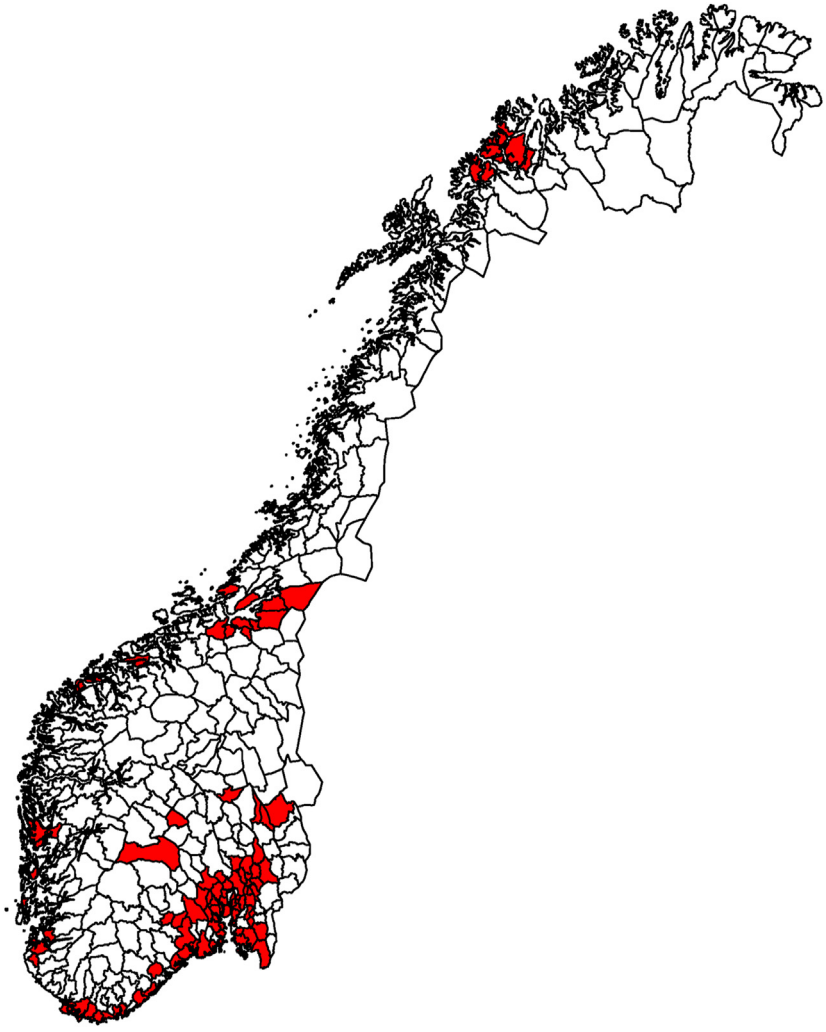

Fig. 1. Geographical location of 2015-grant receiving municipalities (red), as listed in Online Appendix Table A1. (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

this is to include time-varying control variables to account for observed differences between treatment and comparison municipalities. We include a set of municipality-level controls that may affect municipality resource use (variables are displayed in Table 1, and detailed definitions are presented in Online Appendix B). However, as we cannot completely guard against omitted variable bias, a more convincing approach is to combine the control variable approach with some refinements of the comparison group to make it more comparable with the treatment group.

We first check whether the pre-treatment time-trend is similar if we use all untreated municipalities or a subset as a comparison group. First, we investigate the trend in average adjusted student-teacher ratios for the treated municipalities and all untreated municipalities, as illustrated in Fig. 2. The adjusted student-teacher ratios for the treated municipalities and this comparison group follow a similar trend in the outcome variable in the period 2004/05 to 2011/12. However, while the adjusted student-teacher ratio starts to increase in treated municipalities in the period 2012/13–2014/15, it remains relatively stable in the comparison group. As such, the graphical presentation of the adjusted student-teacher ratio in Fig. 2 suggests that all untreated municipalities may not constitute an appropriate comparison group.

Next, we investigate whether it is possible to identify a comparison group that provides a pre-treatment trend that is more comparable to the trend in the treated municipalities. A natural point of departure is to look at municipalities that are close to, but did not meet, the criteria for receiving the grant based on the argument that they are most comparable to the treated ones in terms of unobservable characteristics. To define an alternative comparison group based on this approach, we order the municipalities according to their average adjusted student-teacher ratio in the school years 2012/13–2014/15 and then define comparison groups as the 100 and 75 municipalities closest to the treatment threshold, the national average adjusted student-teacher ratio in the same years (15.94). Fig. 3A uses the 100 municipalities closest to the threshold as a comparison group, while Fig. 3B show the trends in adjusted student-teacher ratios when we use the 75 municipalities that are closest to the threshold as the comparison group.[18] There is little difference among comparison groups in the pre-treatment trends in adjusted student-teacher ratios. The adjusted student-teacher ratio in the comparison groups now follows a similar path to the adjusted student-teacher ratio in the treatment group until the school year 2012/13 and then takes a slightly different course.[19]

While Figs. 2 and 3 indicate that the parallel time trend assumption might not hold, it can also be investigated more formally by means of an event study analysis using the same three comparison groups. We estimate a version of equation (1) in which we include interactions of the treatment indicator and time dummies for the pre-treatment period ("leads") and leave out the interaction for one pre-treatment period as reference. As treatment is based on the average student-teacher ratio for the school years 2012/13–2014/15, we leave out the school year 2011/12. All other interactions are expressed relative to the omitted period, which serves as the baseline. If the common trends assumption is valid, the coefficients corresponding to the included "lead" interaction terms should be insignificant. Fig. 4 reports the result for this specification for each of the comparison groups. The results when using respectively the 100 and 75 municipalities closest to the threshold support the assumption of a common time trend in the outcome variables, strengthening our confidence in the results.[20] When using all other municipalities as a comparison group the results are less convincing.

It should be noted that the "lead" just before treatment is statistically significant at the 5% level when we use the 100 municipalities closest to the threshold as a comparison group. This indicates that there might be some sort of 'Ashenfelter's dip' just prior to the treatment.[21] This might be due to some anticipatory effects of the grant policy and it is possible that some of the units in the treatment group acted strategically and increased teacher density in order to receive extra grants the next year. However, as previously discussed, the timing of reporting and the timing of the grant do not leave much room for such manipulation. In any case, we investigate whether

[18] We have also used the 50 and 25 comparison municipalities closest to the threshold (Figure D1 in the Online Appendix). However, this does not really improve our pre-trend and we start to lose precision as the number of municipalities decreases.

[19] We have considered other methods of making the comparison group more comparable to the treated municipalities. To check whether trimming the sample based on local government characteristics, such as population size and degree of urbanization, improves the pre-trend we followed Crump et al. (2006) by estimating the propensity score on the full sample – both treated and untreated municipalities and trimming the sample by including only those municipalities where $p > 0.1$ or $p < 0.9$. However, this does not improve the pre-trend. Further, to rule out the possibility that top treated municipalities are driving pre-trend differences we have limited treated municipalities to respectively the 100, 75, 50 and 25 closest to the threshold. While pre-trend differences are statistically insignificant also when we limit the treatment group, the confidence intervals increase. Thus, this approach leads to a loss in precision. Also, we have estimated model versions where we include respectively linear regional trends and linear county trends. Again, including trends in our empirical design does not improve pre-trends. Finally, as an additional robustness check we have run some specifications using the synthetic control approach (Abadie et al., 2015, Abadie et al. 2010), which is a data-driven approach for estimating treatment effects. To implement the strategy for our purpose, we first define the 328 untreated municipalities as potential "donors" to the synthetic control. Second, we define the predictor variables used to match the treatment and the synthetic control. To keep it as simple as possible we use lags of the outcome variable as predictor variables to match the treatment and control group. For the estimations, we use synth_runner (Galiani and Quistorff 2017) in Stata. The estimation that provided the best visual match in pre-trends is a specification that matches on trends of the outcome variable and on average values for the outcome variable for each pre-treatment year. The results from this specification suggests that the grant induced a decrease in adjusted student-teacher ratio equal to 2.5 percent in the first treatment year and a 0.5 percent reduction in the second treatment year. While the p-values suggest that these effects are not statistically significant, the standardized p-values, where we correct for poor matching between treated municipalities and donor municipalities, indicate that the effects are statistically significant. So, whereas the estimates indicate that the group size decreased more in the treated municipalities, they do not indicate full take-up which is in line with the main conclusion of our paper. Detailed results are available upon request.

[20] See Online Appendix Figure D2 for results when using the 50 and 25 municipalities closest to the threshold.

[21] 'Ashenfelter's dip' is an empirical regularity that the mean earnings of participants in employment and training programs generally decline during the period just prior to participation (Ashenfelter, 1978).

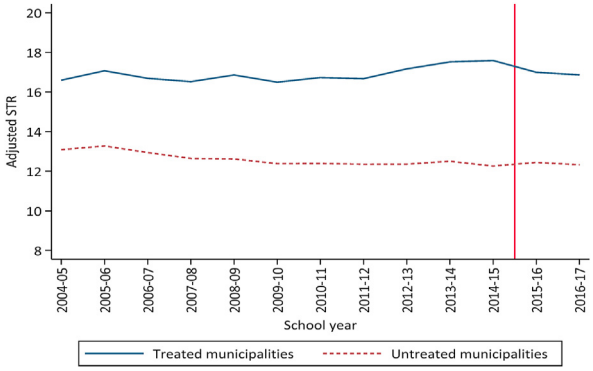

Fig. 2. Adjusted student-teacher ratio (STR) in treated and all untreated municipalities, with the red vertical line indicating the first year of treatment. (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

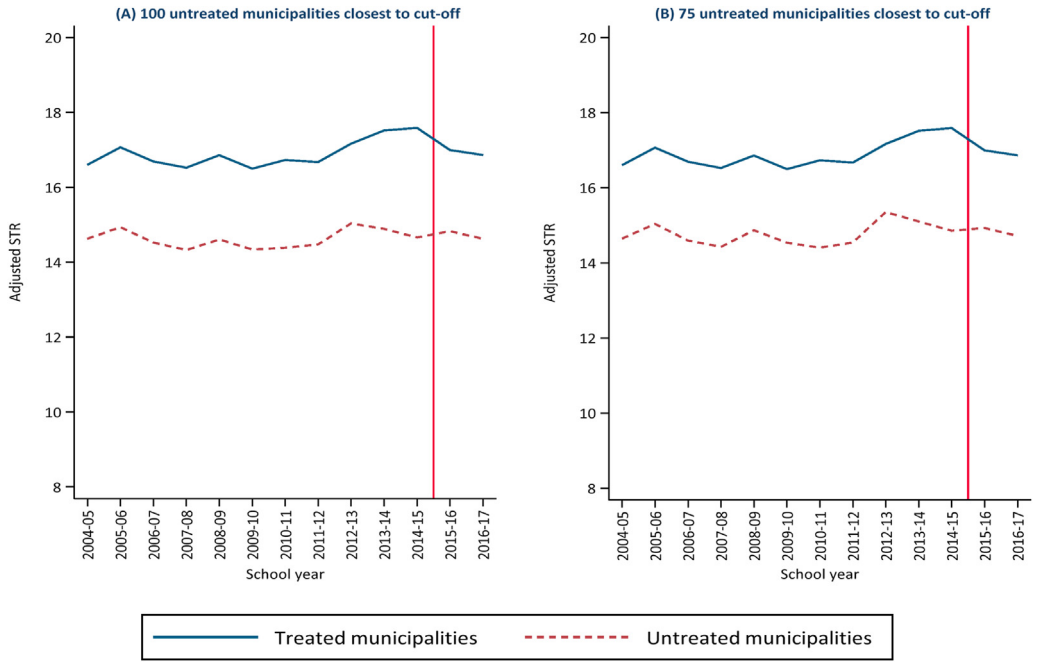

Fig. 3. Adjusted student-teacher ratio (STR) in treated and untreated municipalities using alternative comparison groups, with the red vertical line indicating the first year of treatment. (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

this apparent 'dip' affects our findings in Section 5 by excluding school years 2012/13–2014/15 from our sample in addition to carrying out several other specification checks in Section 4.

Fig. 4 a–c indicate that the adjusted student-teacher ratio appears to change very little in 2015/16 in the treated municipalities relative to the comparison municipalities. This is the first evidence that the policy had little effect on teacher density.

4. Results

4.1. Baseline results

Column (1)–(4) in Table 2 present results based on variants of equation (1) when we use adjusted student-teacher ratio as our outcome variable and all untreated municipalities as a comparison group.

Column (1) represents the simplest specification, where no covariates are included in the regression model. This simple specification indicates that the estimated effect on the adjusted student-teacher ratio is positive, but numerically small and not statistically significant. Column (2) adds controls for municipality characteristics to account for observed time-varying differences between the treated municipalities and untreated municipalities, and this has basically no influence on the estimated effect of the grant policy. Furthermore,

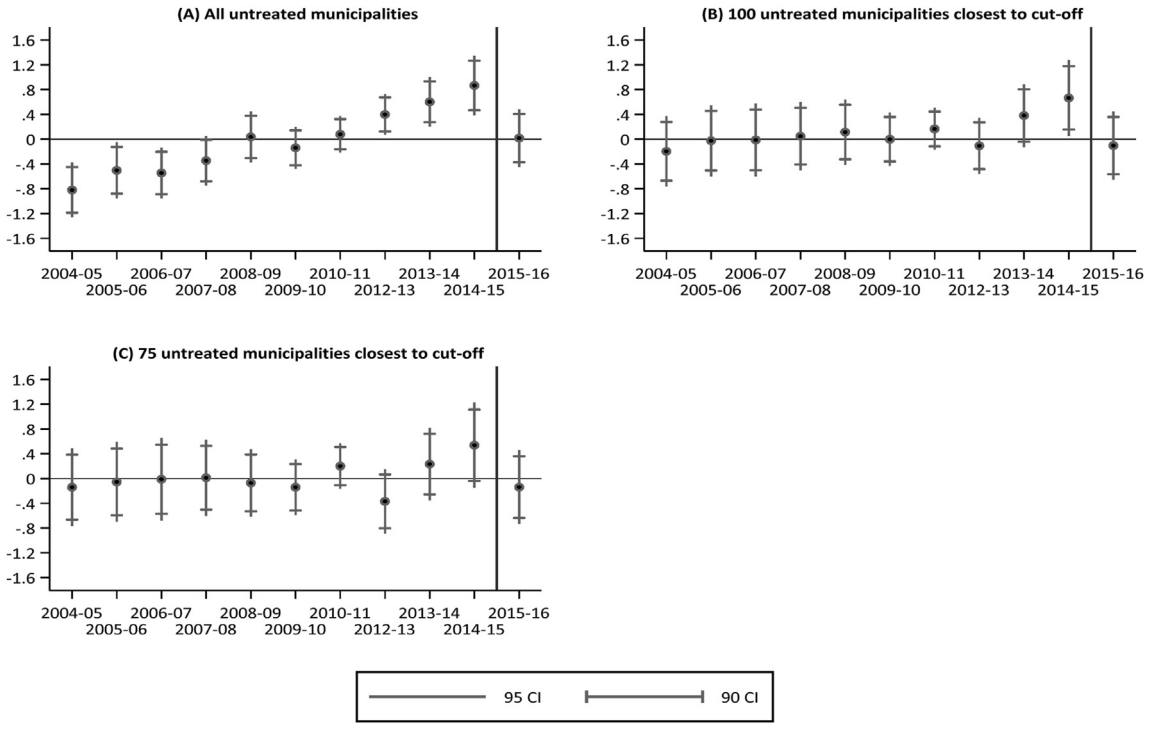

Fig. 4. Event study analysis using different comparison groups, with the vertical line indicating the first year of treatment. The figures plot the coefficients from event-study specifications as well as the 95 and 90 percent confidence intervals. The omitted year is school year 2011–12. The regressions include year and municipality fixed effects, as well as municipality characteristics listed in Table 1 as control variables.

making the regression model more flexible by adding year fixed effects in column (3) does not alter this finding. When municipality fixed effects are included as well in column (4), the effect turns negative, indicating that the grant decreased the adjusted student-teacher ratio, but the effect is small and statistically insignificant.

As argued in Section 3, all untreated municipalities may not constitute a satisfactory comparison group. To ensure that the finding presented in column (4) in Table 2 is not an artefact of the choice of the comparison group, columns (5) and (6) in Table 2 present results for model specifications where we use the 100 and 75 municipalities with adjusted student-teacher ratios closest to threshold as comparison groups.[22] The estimated effects reported in columns (5) and (6) are larger compared to the effect in column (4), and, interpreted literally, suggest that the grant led to a decrease in adjusted student-teacher ratio by about 0.19–0.25. This translates into a 1–1.5 percent reduction in the adjusted student-teacher ratio in the treated municipalities when compared to a pre-treatment level of 16.9 for treated municipalities. Thus, even when we use comparison groups with more convincing pre-trends, the estimated effect is still numerically small and statistically insignificant.

While trimming the sample provides more comparable pre-treatment trends, it also introduces a concern that we are less likely to pick up effects of the grant due to loss of statistical power.[23] When we compare the 95% confidence intervals, we observe that the results presented in column (6) are less precise than those presented in column (4) in Table 2. While we cannot reject effects sizes up to a 2.6 percent reduction in the adjusted student-teacher ratio based on the results in column (4), this increases further to a 3.7 percent reduction in column (6). However, while we lose precision when trimming the sample, the result reported in column (6) indicates that the municipalities were not in full compliance with the grant as we can reject effect sizes above 71.3%.

The estimated effect sizes in columns (5) and (6) and their respective standard errors are similar, as are the pre-treatment trends shown in Fig. 4 A and B. In the remaining analysis we therefore only present results when using the 75 municipalities closest to the threshold.

[22] Results where we use respectively 50 and 25 closest local governments are showed in Table C1 in Online Appendix C. The results from these specifications are not substantially different to those presented in this subsection.

[23] We have used the power calculation formula derived by Burlig et al. (2020) to evaluate the statistical power of our design. Assuming a significance level of 5%, these simulations suggest that we should be able to detect effects equal to a reduction of 0.5 students/teachers with 80 percent likelihood when we use all untreated municipalities as a comparison group. An effect size of 0.5 students/teachers is equivalent to a 2.8 percent reduction in adjusted student-teacher ratio. Hence, using all available data, our study is at least powered to pick up an effect of the grant half the size of the calculated upper bound effect of the grant. When using the 75 closest municipalities as the control group our study is powered to detect a 3.5 percent reduction in student-teacher ratio. When we reduce the number of local governments in our sample further, the minimum detectable effect converges toward the upper bound effect of the grant (see Table A2).

Table 2
Main results.

	(1)	(2)	(3)	(4)	(5)	(6)
	Comparison = All municipalities	Comparison = All municipalities	Comparison = All municipalities	Comparison = All municipalities	Comp. = 100 municipalities closest to threshold	Comp. = 75 municipalities closest to threshold
Treat*After	0.270	0.176	0.174	−0.062	−0.251	−0.192
	(0.191)	(0.193)	(0.193)	(0.196)	(0.218)	(0.224)
Treat	4.280***	2.236***	2.284***			
	(0.166)	(0.182)	(0.186)			
Control variables	No	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes	Yes
Municipality FE	No	No	No	Yes	Yes	Yes
N	5136	5136	5136	5136	2400	2100

Note: Robust standard errors adjusting for clustering at municipality level in parentheses. Municipality characteristics included as control variables. Coefficients marked ***, ** and * are statistically significant at the 1%, 5% and 10% levels respectively.

5. Robustness analyses

5.1. Exclude the three years prior to the implementation of the grant policy

As pointed out in Section 2, the former left-wing government had already in 2011 discussed different ways of regulating the student-teacher ratio. Therefore, to some extent municipalities might have anticipated the policy implemented in 2015. To account for any potential pre-policy behavior by municipalities attempting to game the grant system in the future, column (1) in Table 3 reports the results when the school years 2012/13–2014/15 are excluded from the estimation prior to the implementation of the grant policy, creating an asymmetric 'donut hole'.

The estimated effect reported in column (1) is somewhat smaller compared to the corresponding baseline specification presented in column (6) in Table 2, and still not statistically significant. Hence, the 'dip' argument related to possible anticipation of the intervention does not seem to explain our main results.

5.2. Excluding municipalities closest to threshold

It might be easier for municipalities closest to the threshold, compared to those far from the threshold, to adjust the student-teacher ratio in the hope of being granted additional funding in case of future policy initiatives. An alternative approach to account for this possibility to game the system is to exclude units close to the treatment threshold. Column (2) in Table 3 therefore presents the results for a model specification equal to the one presented in column (6) in Table 2, but where we exclude the 50 municipalities closest to the threshold (25 below and 25 above). The effects of the grant policy on the adjusted student-teacher ratio are quite similar to our baseline results and not significant.

5.3. Exclude municipalities participating in "1 + 1" and "Two Teachers" projects

In our baseline specifications we do not include observations beyond the school year 2015/16. As mentioned previously, the reason for this is that the research projects "1 + 1 Project" and "Two Teachers" that started up in 2016/2017 could complicate our analysis. Another way of dealing with this issue is to exclude municipalities participating in these projects, as we do in column (3) in Table 3. The results in column (3) correspond to our baseline specifications when participating municipalities are excluded, while we include the school year 2016/17. While including school year 2016/2017 and excluding municipalities participating in the "1 + 1" and "Two Teachers" projects lead to a decrease in precision, the effect remains insignificant, confirming our previous results.[24]

5.4. Alternative outcomes

We may worry that the dependent variable does not capture the relevant effect. Recall that the adjusted student-teacher ratio is a measure of the ratio of students to teachers in a regular teaching situation. As the municipalities had full discretion on how to distribute resources across schools and how to utilize the new teachers, the adjusted student-teacher ratio may be too narrow a measure of

[24] We have also estimated a version where we extend the period until the school year 2019/2020, excluding participating municipalities. When we investigate the effect of the grant year-by-year in the post-treatment period we observe that the results do not change much, except that we see a statistically significant reduction in the adjusted student-teacher ratio in the school year 2019/2020. However, this is likely due to a to national teacher density norm at the school level that was implemented in 2018/19 and further tightened in 2019/20 in which the treated municipalities in our analysis became more targeted compared to the untreated municipalities. Results are available upon request.

Table 3
Robustness analyses.

	(1)	(2)	(3)
	Exclude school years 2012/13–2014/15	Exclude 50 LGs closest to cut off	Exclude municipalities participating in "1 + 1" and "Two Teachers" projects
Treat*After	−0.151	−0.373	−0.073
	(0.261)	(0.277)	(0.226)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
N	1575	1500	1625

Note: Robust standard errors adjusting for clustering at municipality level in parentheses. Municipality characteristics included as control variables. Coefficients marked ***, ** and * are statistically significant at the 1%, 5% and 10% levels respectively. The 75 untreated municipalities closest to threshold are used as a comparison group.

educational resources and therefore fail to capture the effect of the grant policy.

In particular, absence of parallel trends in the pre-treatment period may be due to different developments in the denominator and nominator of the group size variables. In Figure D3 in Online Appendix D we report trends in the number of students and teacher person-years (adjusted and unadjusted) in treated and untreated municipalities. While the number of students and teacher person-years increase in the treated municipalities, the equivalent trends are much flatter in the untreated municipalities. This indicates that our treated municipalities have higher growth in the student population which in turn may affect their ability to reduce student-teacher ratios. Recruitment problems, labor market tightness and imperfect long-term planning can all constrain the municipalities' ability to reduce student-teacher ratios. Further, our main outcome of interest, which was also the target measure for the grant, excludes hours spent on special needs education and basic Norwegian for language minorities. This may disproportionally underestimate resources relevant for the treated municipalities. As a result, the adjusted group size may therefore underestimate the real effect of the grant. In Table 4, we therefore investigate the effect of the grant policy on the unadjusted student-teacher ratio, and on teacher person-years for the adjusted and the unadjusted student-teacher radio.

5.4.1. The unadjusted student-teacher ratio

In column (1) in Table 4, the outcome variable is the unadjusted student-teacher ratio, defined as the ratio of total number of student hours to total number of teaching hours. The result in column (1) suggests that the grant policy led to a 2 percent reduction in unadjusted student-teacher ratio as compared to the pre-treatment average of 14.14 and is significant at the 10% level. As the unadjusted student-teacher ratio provides a picture of the total use of resources at a school or by a municipality, this finding indicates that special needs education might be a potential channel for the grant. Because of limitations in our data (we only have information on teacher man years in special needs education, not students in special needs education), we are not able to investigate this more closely. In any case this effect is not robust across comparison groups (see Online Appendix Table C2). When we use the 100 municipalities closest to the threshold as a comparison group, the effect is significant at 5 percent level. When we instead use all untreated municipalities and the 50 and 25 municipalities closest to the threshold as comparison groups, respectively, the effect is statistically insignificant and the coefficients are smaller numerically. As such, the result is not consistent enough to conclude that there is an effect. Also, even though we cannot rule out that the grant had an effect on special needs education, increasing special needs education would not be in accordance with the intention of the grant.

5.4.2. Teacher person-years in teaching and regular teaching

In column (2) of Table 4 we use the number of teacher person-years (adjusted) as the outcome variable. This measure, like the adjusted student-teacher ratio, excludes resources for special needs education and basic Norwegian for language minorities. The estimate is positive, suggesting that teacher employment increased as a result of the grant policy, but is not statistically significant. In column (3) of Table 4 we use the unadjusted number of teacher person-years as our outcome variable. Again, while the positive result suggest that teacher employment increased as a result of the grant policy, the suggested increase seems to have left the student-teacher ratios relatively unaffected. The increase in teacher employment was not large enough to offset the increase in the number of students.

5.5. Heterogeneity analysis

The average effect of the grant on the use of educational resources as estimated above may mask important heterogeneity across municipalities. The degree to which a municipality can or wants to use the grant to increase teacher density could depend on the broader constraints facing municipalities. In particular, the degree of teacher supply constraints may limit the possibility of municipalities to increase the student-teacher ratio, especially in large municipalities with high population growth.

Thus, the zero estimated treatment effects reported so far may to some extent be a result of teacher supply constraints. Earlier research on the teacher labour market in Norway has demonstrated the relevance of teacher supply constraints, see Falch and Strøm (2005) and Falch et al. (2009). Further, teachers' union strength and fiscal constraints may be of importance. Moreover, zero mean effects at municipality level may be the result of the method used for the distribution of resources across schools within municipalities.

Table 4
Alternative outcomes.

	(1)	(2)	(3)
	Unadjusted student-teacher ratio	Adjusted teacher man years	Unadjusted teacher man years
Treat*After	−0.308*	0.891	1.028
	(0.169)	(0.654)	(0.82)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
N	2100	2100	2100

Note: Robust standard errors adjusting for clustering at municipality level in parentheses. Municipality characteristics included as control variables. Coefficients marked ***, ** and * are statistically significant at the 1%, 5% and 10% levels respectively. The 75 untreated municipalities closest to threshold are used as a comparison group.

One hypothesis is that the municipalities distributed the grant to the schools viewed as being most in need of additional resources, irrespective of the intentions of the grant. While budgetary data at school level is not available, we have information on real resource use in the form of student-teacher ratios (adjusted and unadjusted) at school level as well as school size (measured as the number of students at the schools).

To investigate whether the treatment effects depend on teacher supply constraints – as measured by the share of non-certified teachers in the pre-treatment period, population size and access to local property tax, we estimate a version of Equation (1) were we introduce a triple interaction term between $treated_m \times d_t$ and these municipality characteristics. To investigate the distribution of resources across schools within municipalities we follow Hyman (2017) and estimate school model specification where we divide the sample into schools above and below average student-teacher ratios and number of students.

Detailed descriptions of these approaches and results are shown in Online Appendix E and F, respectively. Summing up, we find no differential effects by municipality characteristics such as the share of non-certified teachers, population size or access to local property tax. Nor do we find evidence that the zero finding reported earlier disguises effects of the grant for specific schools within municipalities. In addition, a more recent analysis of the national teacher density norm at the school level implemented in 2018/2019 shows that schools were able to recruit teachers (see Sandsør et al. 2020). As in our analysis, primarily large municipalities were affected by this norm. As such, this indicates that teacher shortage is perhaps not the primary explanation behind our findings, although we cannot rule it out completely.

6. Concluding remarks

General or earmarked central government grants to municipalities are considered to be potentially important policy tools for enabling policy-makers to affect educational spending, and ultimately school performance. An important question is whether recipient municipalities allocate additional grants in the way intended by the central government. Utilizing a grant policy initiated by the Norwegian government in 2015 to decrease the student-teacher ratio in primary schools (grade 1–4), this paper uses quasi-experimental methods to investigate how earmarked grants affect educational resource allocation at the municipality level.

Our results show that Norwegian municipalities did not increase teacher density in primary schools, despite receiving extra grants for this purpose. This finding is robust to several robustness checks and heterogeneity analysis. Although we cannot rule out that there was some take-up of our grant, we can exclude effect sizes equivalent to more than 71% take-up based on our preferred baseline specification, suggesting that the receiving municipalities were not fully compliant to the grant. Furthermore, while including more untreated municipalities in our comparison group gives less support to the common trend assumption, it increases the precision of our estimate. Using all untreated municipalities as a comparison group, rules out effect sizes equivalent to more than 50% take-up. This leads us to conclude that the upper bound effect of the grant lies somewhere in between. Further analyses suggest that this was not a result of teacher supply constraints.

Our findings differ from Kirkebøen et al. (2017), finding that a government grant, introduced in 2012 and aimed at decreasing the student-teacher ratio in lower secondary schools (grades 8–10), generated a reduction in the student-teacher ratio in the treated schools of approximately 10%. Importantly, the 2012 grant policy was designed with a clear instruction that the grant received by the municipalities should be distributed to *schools* with less than the average student-teacher ratio and less than average student performance in the pre-policy period. In contrast, the 2015 grant was not subject to such strong instructions and the municipalities had much more freedom as to the use of the grant.

The different experiences from these two grant policies suggest that stronger enforcement mechanisms may be necessary in order for targeted grants to affect local allocation of school resources as intended by higher level governments, although this may be at the expense of reduced gains ensuing from local flexibility. At a general level, our findings suggest that the effect of targeted grants depends heavily on the detailed design of the grant policy.

Declaration of competing interest

In accordance with guidelines for publication in the European Journal of Political Economy and our ethical obligation as researchers,

the authors of this paper jointly declare that there is no potential conflict of interest.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ejpoleco.2021.102006.

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