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The effect of central government grants on local educational policy[1]

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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 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 (group size) in primary school (grades 1-4). The grant was given to the 100 municipalities with the highest student-teacher ratio out of more than 400 municipalities. Using a difference-in-differences approach, we find that the grant did not have the intended effect of reducing the actual group size. Our results suggest that strong enforcement mechanisms are necessary for earmarked grants to affect local allocation of resources as intended by central governments, although this may come at the cost of reducing local flexibility.

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Key words: Central government grants, school policy; teacher density

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1 Introduction

Public finance theory suggests that leaving tax and spending decisions to lower level governments (fiscal decentralization) improves allocative efficiency, i.e. improves 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 Thaler (1995) and 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 and Hyman 2017). Hence, 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 made 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, allowing municipalities to increase teaching staff by approximately 700 teacher person-years in total. 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, like the institutional framework in many European countries.[2] Most of the existing empirical evidence on the effects of intergovernmental grants are from the US

[2] 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.

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) and 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 *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 of the change in these schemes in the first place.[3]

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 increase the student-teacher ratio (group size) in grades 1-4, with no clear restrictions on the allocation between schools within the municipalities. Kirkebøen et al. (2016) evaluate another central government grant, introduced in 2012, aimed at increasing the student-teacher ratio (group size) 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. Kirkebøen et al. (2016) found that this grant had the intended effect on the targeted schools by decreasing student-teacher ratios by roughly 10%, although they did not find any effect on student performance. The interesting 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 a similar effect on the student-teacher ratio to the 2012 grant.

[3] 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.

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 and 6 present analyses of robustness and heterogeneity. In Sections 7 and 8, we discuss the results and present a conclusion.

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 (see 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 are allowed to set their tax rates within a narrow band. However, since 1979 all municipalities have applied the maximum rate.

This system of equalization of the tax base and of spending implies that municipalities' opportunity to influence current revenue is very limited. As the income levels available for taxation vary across

[4] There are 428 local governments and 19 county authorities (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, population density and travel distance.

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 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 the 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

[7] In reality, the wag- 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.

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.

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 student-teacher ratios above the national average in the school years 2012/13-2014/15. 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. For more details on the grant, see Appendix A.

Municipalities were told to spend the extra funds on increasing the number of teachers 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, which was fairly unexpected by local policy-makers. The official press release about the grant is dated March 2015[10], whereas municipalities reported the student-teacher ratios for the school year 2014/2015 in October 2014[11]. It follows that the municipalities that were close to the cut-off could not manipulate treatment by inflating their reported student-teacher ratios for 2014/2015. Thus it represents a positive resource shock, and the grant policy can be used as a natural experiment to understand how earmarked central government grants affect local educational spending. However, a possible problem with this kind of reasoning 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 Kunnskapsdepartementet (2010) and Borge et al. (2012). Thus, since similar policy proposals had been on the political agenda some years

[9] https://www.regjeringen.no/no/aktuelt/flere-larere-til-de-yngste-elevene/id2400739/
[10] https://www.regjeringen.no/no/aktuelt/flere-larere-til-de-yngste-elevene/id2400739/
[11] https://gsi.udir.no/registrering/

earlier, we cannot completely rule out the possibility that some municipalities might have decreased teacher densities in order to gain from expected future grants. This problem will be addressed in the robustness analysis in Section 5 below.

3 Empirical specification and data

3.1 Empirical specification

We compare treated municipalities with untreated municipalities in a differences-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 control group in the absence of the additional resources.

The difference-in-differences model using outcomes measured at municipality level can be specified in a regression framework as follows:

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

where m indexes municipalities and t indexes school years. $treated_m$ is an indicator variable that is 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_t is a row vector of time-varying municipality control variables with corresponding coefficient vector δ and ε_{mt} is the error term. School-year fixed effects τ_t captures 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 annual 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.[12]

The outcome variables we use to measure educational spending are different definitions of student-teacher ratios.[13] Specifically, we use the two different measures of group size commonly used by the

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

[13] Alternatively, as local governments annually report income and expenditures by sector of activity to Statistics Norway, we could use education expenditure as outcome variables. 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.

central government, the student-teacher ratio and the adjusted student-teacher ratio.[14] The 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.[15] As municipalities were instructed to spend the extra resources on increasing teacher person-years for regular teaching[16] 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 student-teacher ratio as an outcome variable. Data on the student-teacher ratio and adjusted 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), provide us with 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 2016/2017school 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 non-treated municipalities. In our main specification we will therefore not include observations beyond the school year 2015/2016.

[14] See the Norwegian Directorate for Education and Training (2016) for more information.
[15] The *student-teacher ratio* is defined as the ratio of the total number of student hours to the total number of teaching hours. This quantity includes all student hours and all teaching hours. This definition gives a picture of the total use of resources at a school or in a local government. The *adjusted student-teacher ratio* excludes resources for special needs education and basic Norwegian for language minorities and is an indication of students per teacher in ordinary education. The adjusted student-teacher ratio was used as the teacher density measure when deciding which local governments would receive extra funding.
[16] Defined as all regular teaching excluding special education and Norwegian for second language learners.

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

	(1) Treated municipalities Mean (sd)	(2) All untreated municipalities Mean (sd)
Outcome variables:		
Adjusted student-teacher ratio	16.90	12.62
	(1.68)	(2.60)
Student-teacher ratio	14.14	10.74
	(1.45)	(2.04)
Teacher man years in teaching	99.03	20.68
	(174.17)	(22.04)
Teacher person-years in ordinary teaching	77.51 (127.84)	16.84 (18.03)
Municipality characteristics		
Non-certified teachers measured at regional level (%)	3.01	3.05
Population	31.80	5.15
	(65.99)	(5.82)
Share ages 1-5 (%)	6.31	5.52
Share ages 6-15 (%)	13.39	13.01
Share ages 67-79 (%)	8.70	10.61
Share age 80+ (%)	4.25	5.87
Share immigrant (%)	9.46	6.15
Share divorced (%)	11.69	9.82
Share disabled (%)	9.95	11.53
Share higher education (%)	0.24	0.18
Population density (%)	76.11	43.11
Observations	1100	3608

Note: Standard deviation in parentheses. Detailed definitions of variables are shown in Appendix 2.

The dataset covers all 428 municipalities in Norway during the period 2004-2016, and descriptive statistics for outcome variables and municipality characteristics 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. 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, population density 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 Figure 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.

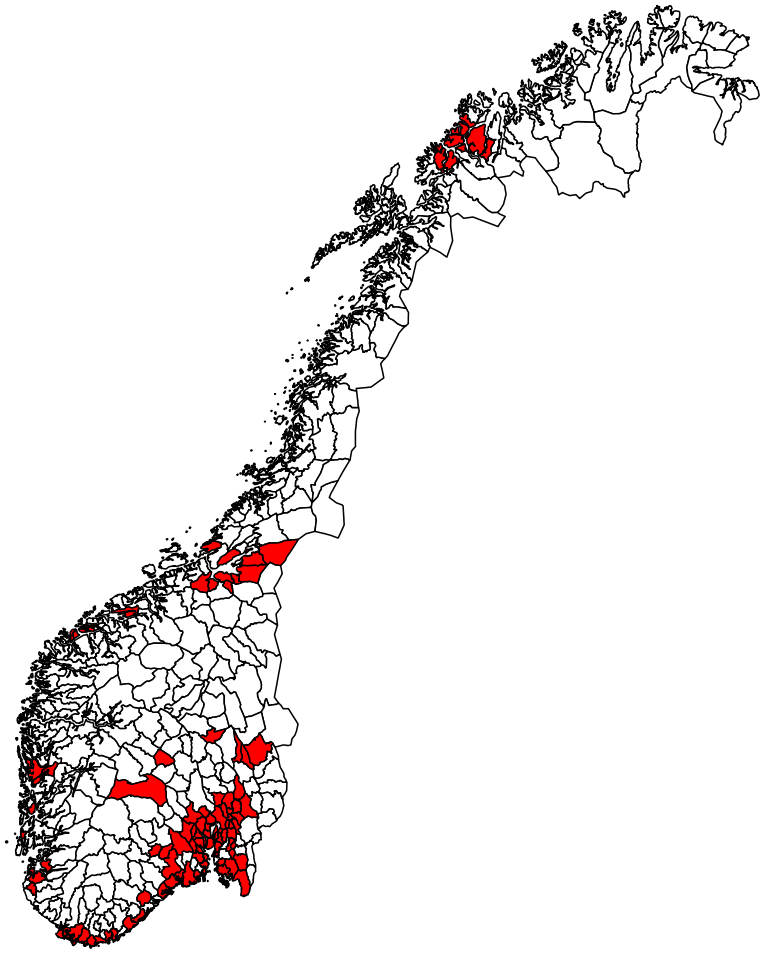

Figure 1: Geographical location of 2015-grant receiving municipalities (red), as listed in Appendix Table A1.

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. One way to handle this is to include time-varying control variables to account for the observed differences between treatment and comparison municipalities. 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.

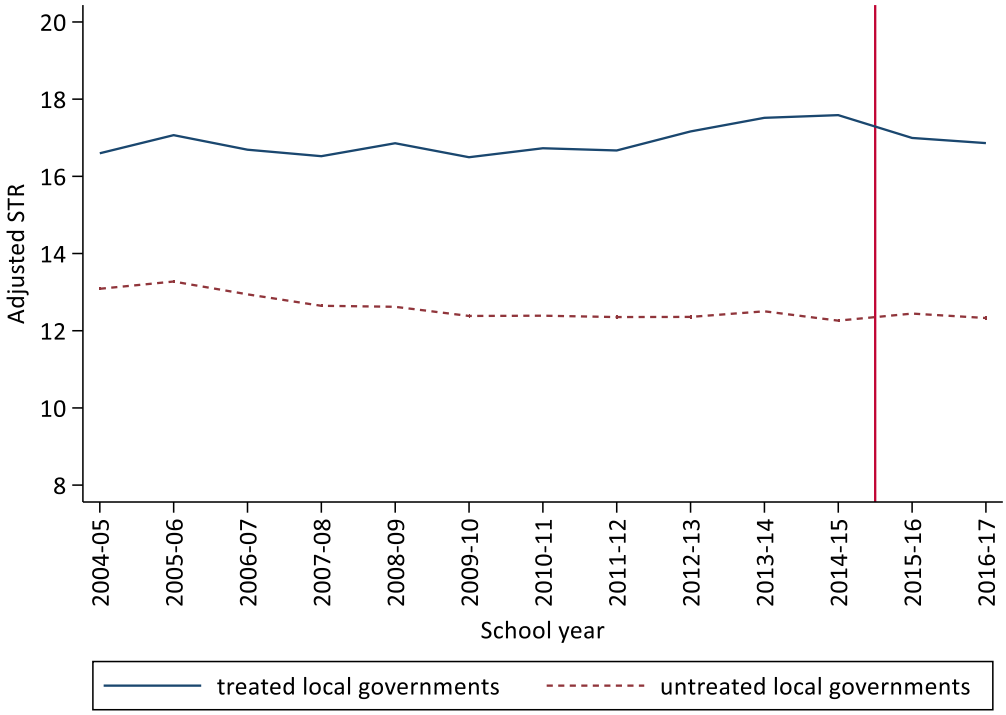

Figure 2: Adjusted student-teacher ratio (STR) in treated and all untreated municipalities, with the red vertical line indicating the first year of treatment.

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. One way to handle this is to include time-varying control variables to account for the observed differences between treatment and comparison municipalities. 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.

The next step is to check whether the common trend assumption holds 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 Figure 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 the treated municipalities in the period 2012-13 to 2014-15, it remains relatively stable in the control group. Hence, the graphical presentation of the

adjusted student-teacher ratio in Figure 1, suggests that all untreated municipalities may not constitute an appropriate comparison group.

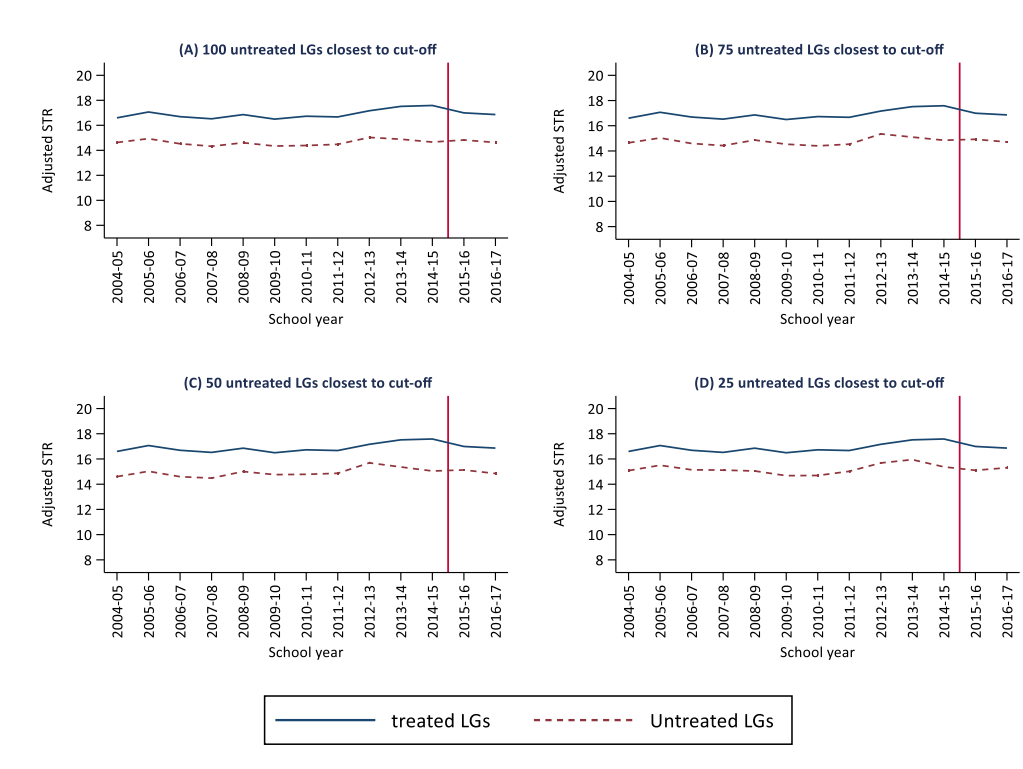

Figure 3: Adjusted student-teacher ratio (STR) in treated and untreated municipalities using alternative control groups, with the red vertical line indicating the first year of treatment.

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. To define a more suitable comparison group, 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, 75, 50 and 25 municipalities closest to the threshold, the national average adjusted student-teacher ratio in the same years (15.94). Figure 3A uses the 100 municipalities closest to the threshold as a comparison group, while Figures 3B, 3C and 3D show the trends in adjusted student-teacher ratios when we use the 75, 50 and 25 municipalities, respectively, that are closest to the threshold as comparison groups.

There is little difference among comparison groups in the pre-treatment trends in adjusted student-teacher ratios. As in the Figure 2, the adjusted student-teacher ratio in the comparison groups follows a similar path to the adjusted student-teacher ratio in the treatment group until the school years 2012/13 and 2013/14 and then takes a slightly different course. The exception is Figure 3d, where the

adjusted student-teacher ratios in the treated municipalities and the comparison municipalities follow a similar path for the entire pre-treatment period.

While Figure 2 and 3 indicate that the parallel time trend assumption might not hold, it can also be tested more formally by means of an event study analysis using the same five 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 the pre-treatment period. 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. Figure 4 reports the result for this specification for each of the comparison groups. The results support the assumption of a common time trend in the outcome variables, strengthening our confidence in the results.

It should also be noted that the "lead" just before treatment is statistically significant at the 5% level when we use the 100 municipalities closest to the cutoff as a comparison group. This indicates that there might be some sort of 'Ashenfelter's dip' just prior to the treatment.[17] 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 this apparent 'dip' affects our findings in addition to carrying out several other specification checks.

Figures 2-4 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.

[17] '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).

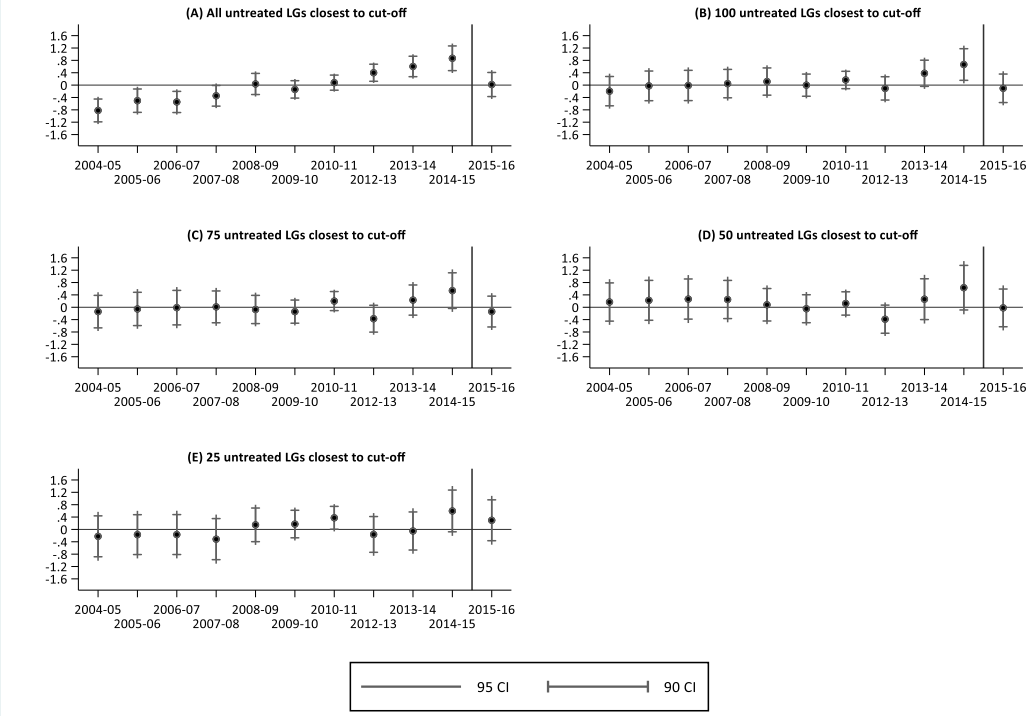

Figure 4: Event study analysis using different comparison groups, with the red vertical line indicating the first year of treatment.

Results

3.4 Baseline results

Table 2: Main results

	(1) Adjusted student-teacher ratio	(2) Adjusted student-teacher ratio	(3) Adjusted student-teacher ratio	(4) Adjusted student-teacher ratio
Treat*After	0.270	0.176	0.174	-0.062
	(0.191)	(0.193)	(0.193)	(0.196)
Treat	4.280***	2.236***	2.284***	
	(0.166)	(0.182)	(0.186)	
Control variables	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
Municipality FE	No	No	No	Yes
N	5 136	5 136	5 136	5 136

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.

Table 2 presents 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 actually 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 control municipalities, and this has basically no influence on the estimated effect of the grant policy. Furthermore, 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 still small and statistically insignificant.

3.5 Control groups

As shown in section 3, 'All untreated municipalities' does not constitute a satisfactory comparison group. To ensure that the findings presented in Table 2 are not artefacts of the choice of treatment and control group, columns (1)-(4) in Table 3 present results for model specifications where we use different comparison groups. All model specifications presented in Table 3 include control variables, year fixed effects and municipality fixed effects.

While the estimated effect of the grant policy is negative when we use the 100, 75 and 50 municipalities with adjusted student-teacher ratios closest to the grant cut-off as comparison groups (columns (1)-(3)), the effect actually turns positive when we use the 25 municipalities closest to the cut-off as our comparison group (column (4)). In any case, the effect is still numerically small and not statistically significant. Hence, the 'zero-effect' results reported in Table 2 also hold when we use more refined control groups.

Table 3: Alternative control groups

	(1) 100 LGs closest to cutoff	(2) 75 LGs closest to cutoff	(3) 50 LGs closest to cutoff	(4) 25 LGs closest to cutoff
Treat*After	-0.251	-0.192	-0.184	0.228
	(0.218)	(0.224)	(0.254)	(0.329)
After	1.584*	1.628	2.112*	1.336
	(0.891)	(1.010)	(1.097)	(1.115)
Control variables	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
N	2 400	2 100	1 800	1 500

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.

4 Robustness analyses

4.1 Drop the years before the grant policy was implemented

As pointed out in Section 2, the former left-wing government had already in 2011 discussed different ways to regulate the student-teacher ratio. Thus, to some extent municipalities might have anticipated the policy implemented in 2015. To account for the potential pre-policy behavior of municipalities designed to game the grant system in the future, Table 4 reports the results when the school years

(2012/13-2014/15) are removed from the estimation prior to the implementation of the grant policy, creating an asymmetric 'donut hole'. Panel A in Table 4 reports the results when the school year prior to treatment is excluded, while Panels B and C present results when the two and three school years, respectively prior to treatment are excluded. The effect is similar in magnitude to the main model specification, and still not statistically significant. Hence, the 'dip' argument related to possible anticipation of the intervention does not seem to explain our main results.

Table 4: Dropping of school years before the grant policy was implemented

	(1) All untreated LGs	(2) 100 LGs closest to cutoff	(3) 75 LGs closest to cutoff	(4) 50 LGs closest to cutoff	(5) 25 LGs closest to cutoff
Panel A: Drop school year 2014/15					
Treat*After	0.029	-0.176	-0.117	-0.125	0.246
	(0.208)	(0.231)	(0.239)	(0.276)	(0.329)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	4708	2200	1925	1650	1375
Panel A: Drop school years 2012/13-2014/15					
Treat*After	0.091	-0.144	-0.098	-0.126	0.217
	(0.220)	(0.244)	(0.250)	(0.292)	(0.339)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	4280	2000	1750	1500	1250
Panel A: Drop school years 2011/12-2014/15					
Treat*After	0.185	-0.167	-0.151	-0.178	0.229
	(0.234)	(0.255)	(0.261)	(0.300)	(0.333)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	3852	1800	1575	1350	1125

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.

4.2 Dropping of municipalities closest to cut-off

Another approach to determining whether possible anticipatory effects of the grant policy affect the findings presented in Table 2 and 3 is to exclude municipalities close to the cutoff, to take account of possible manipulation effects. Columns (1) to (4) in Table 5 therefore present the results for model specifications in which we exclude the 50 municipalities closest to the cutoff (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 are still numerically small and not significant. This is also the case when, in column (5), we only include municipalities just around the cut-off. The estimated effect in this case is very similar to the estimated effects presented in column (4) in Table 3.

Table 5: Remove 50 municipalities closest to cut-off

	(1) Drop 50 municipalities closest to cut off	(2) Drop 50 municipalities closest to cut off	(3) Drop 50 municipalities closest to cut off	(4) Drop 50 municipalities closest to cut off	(5) Only 50 municipalities closest to cut off
Treat*After	-0.069	-0.406	-0.373	-0.465	0.120
	(0.240)	(0.266)	(0.277)	(0.336)	(0.363)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	4536	1800	1500	1200	600

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.

4.3 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 Table 6. The results in Panel A correspond to our baseline specifications when participating municipalities are excluded, while we include the school year 2016/17 in Panel B. When we do not include the 2016/17 school year, the effect of the policy grant is basically the same as in our baseline model. Including school year 2016/2017 leads to a decrease in the estimates and more precision. All estimates remain insignificant, confirming our previous results.

Table 6: Exclude municipalities participating in "1+1" and "Two Teachers" projects

	(1) All untreated LGs	(2) 100 LGs closest to cutoff	(3) 75 LGs closest to cutoff	(4) 50 LGs closest to cutoff	(5) 25 LGs closest to cutoff
Panel A: Excluding school year 2016/17					
Treat*After	-0.056	-0.233	-0.172	-0.115	0.250
	(0.226)	(0.249)	(0.255)	(0.282)	(0.374)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	4392	1764	1500	1236	984
Panel B: Including school year 2016/17					
Treat*After	0.005	-0.127	-0.073	0.045	0.047
	(0.193)	(0.220)	(0.226)	(0.245)	(0.310)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	4758	1911	1625	1339	1066

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.

4.4 Alternative outcomes

We may worry that the dependent variable does not measure 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 educational resources and therefore fails to capture the effect of the grant policy. Panel A of Table 7 reports the effect of the grant policy on the unadjusted student-teacher ratio. It is also interesting to investigate the effect on the denominator in both measures of student-teacher ratio, to see whether the number of teachers increased. Panels B and C of Table 7 report the effect of the grant policy on teacher person-years for both the adjusted and the non-adjusted student-teacher radio.

Table 7: Alternative outcomes

	(1) All LGs	(2) 100 LGs closest to cutoff	(3) 75 LGs closest to cutoff	(4) 50 LGs closest to cutoff	(5) 25 LGs closest to cutoff
Panel A: Student-teacher ratio (non-adjusted)					
Treat*After	-0.153	-0.338**	-0.308*	-0.274	-0.159
	(0.138)	(0.163)	(0.169)	(0.192)	(0.229)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	5 136	2 400	2 100	1 800	1 500
Panel B: Teacher man years (adjusted)					
Treat*After	1.131*	0.970	0.891	1.416**	1.307
	(0.606)	(0.629)	(0.654)	(0.658)	(0.812)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	5 136	2 400	2 100	1 800.	1 500
Panel C: Teacher man years (non-adjusted)					
Treat*After	1.136	1.071	1.028	1.720**	1.899*
	(0.73)	(0.77)	(0.82)	(0.82)	(1.06)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	5 136	2 400	2 100	1 800	1 500

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 unadjusted student-teacher ratio

In Panel A of Table 7, the outcome variable is the student-teacher ratio, defined as the ratio of total number of student hours to total number of teaching hours. This definition provides a picture of the total use of resources at a school or by a municipality. Regardless of which municipalities we use as a comparison group, the effect of the grant policy is negative, indicating that the grant increased the total number of teacher hours relative to the total number of student hours. However, the effect is

only statistically significant at the 5% level when we use the 100 municipalities closest to the cutoff as comparison group (column (2). While this result suggests that the grant policy may have a numerically small effect on teacher density, the event study specification (see **Feil! Fant ikke referansekilden.**) indicates that this effect may be a result of deviating trends in the treatment and comparison group prior to the implementation of the grant policy. Overall, the results presented in Panel A of Table 7 suggest that the grant policy has limited effects, even for this broader measure of teacher density.

Teacher person-years in teaching and regular teaching

In Panel B of Table 7 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. All estimates are positive, suggesting that teacher employment increased as a result of the grant policy, but are only significant for the comparison group using the 50 municipalities closest to the cut-off. In Panel C of Table 7 we use the unadjusted number of teacher person-years as our outcome variable. Again, results suggest that teacher employment increased as a result of the grant policy. However, the effect on teacher employment is only statistically significant when we use the 50 and 25 municipalities closest to the cut off. In any case, while the targeted municipalities increased teacher employment, the increase seems to have left the student-teacher ratios relatively unaffected. Thus, the increase in teacher employment was not large enough to offset the increase in the number of students.

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, such as the degree of teacher supply constraints, population size and teachers' union strength and fiscal constraints. Moreover, zero mean effects at municipality level may be the result of the method used for the distribution of resources across schools within municipalities. In this section we investigate effect heterogeneity across both municipalities and schools within municipalities. We examine heterogeneity by municipality characteristics in section 6.1 and across schools in section 6.2.

5.1 Heterogeneity across municipalities

In this section we investigate whether the treatment effects depend on teacher supply constraints and other municipality characteristics, represented by Z. For each municipality characteristic, we estimate the following model:

$$(2)\quad y_{mt} = \alpha_0 + \beta(treated_m \times d_t) + \eta(treated_m \times d_t) \times Z_{mt} + X_t\delta + \tau_t + \eta_m + \varepsilon_{mt}$$

In Table 8, we present the level effect of $treated_m \times d_t$ and the interaction effect of $(treated_m \times d_t) \times Z_{mt}$. The coefficient corresponding to the interaction term shows how the treatment effect depends on different municipality characteristics.

Teacher supply constraints

One possible explanation for the zero mean effect presented in Table 2 is that municipalities were not able to increase the teacher-student ratio because of teacher supply constraints. In a setting with municipalities facing an upward sloping supply curve for teachers, a policy-induced demand shift for teachers could potentially increase teacher wages and lead to higher costs. However, since teacher wages in Norway are to a large extent determined by collective bargaining between national unions and the Norwegian Association of Local and Regional Authorities (KS), there is not much wage discretion left at local level. Within this system, actual teacher supply in the short run depends on the decisions of current teachers to migrate between municipalities and the decisions of teachers in non-teaching jobs to take a teaching job, see Falch and Strøm (2005) and Falch et al. (2009). While a complete characterization of the local labor market for teachers is difficult and beyond the scope of this paper, a simple measure of possible teacher supply constraints is the share of non-certified teachers in the pre-treatment period. According to legislation on education in Norway, schools can only employ persons without a teacher's certificate if no certified teachers apply for a vacant position, and non-certified teachers can only be employed for up to one school year (Falch et al., 2009). Thus, the only possible response to shortages of certified teachers is to hire non-certified teachers on short-terms contracts. In this context, the share of non-certified teachers can thus be used as a measure of teacher shortage in the relevant geographical area. If teacher supply constraints explain the zero policy effect, we would expect municipalities with high teacher shortages in the pretreatment period to be less affected by the policy. The estimated treatment effect interacted with the share of non-certified teachers is presented in Panel A of Table 8. The treatment effect is positive, but close to zero while the interaction effect with our measure of teacher shortage is negative. However, none of the coefficients are statistically significant, and indicate that teacher supply constraints are not able to explain much of the zero effect of the grant policy.

Municipality population size

As explained in Section 4, the municipalities receiving the 2015 grant were to a large extent urban municipalities in densely populated areas relative to the rest of the country. It is nevertheless of interest to investigate whether the treatment effect varies with population size across this restricted sample of municipalities. Ross and Sonstelie (2010) argue theoretically that teachers' unions are more

powerful relative to voters in larger school districts because voters' incentives and ability to monitor the efficiency of educational services are smaller in larger school districts. Thus, the ability of teachers' unions to implement their preferred policy could be greater in large municipalities. In Norway, local teachers' unions have considerable influence on the allocation of resources at local level, while teachers' salaries are more or less completely determined at national level. The Norwegian teachers' unions are one of the strongest advocates of national regulations of teacher-student ratios. Thus, a more powerful local teachers' union is expected to be better able to ensure that the 2015 grant is used in the way intended by the central government. Unfortunately, data on teachers' union density or other direct measures of union power at municipality level are not available. However, if union power is higher in large than in small districts, as argued in Rose and Sonstelie (2010), the effect of the 2015 grant on student-teacher ratios should be systematically more negative in large relative to small municipalities.

Motivated by this reasoning and the general literature on the relationship between size and educational outcomes, we investigate whether treatment effects vary by population size.[18] The estimated treatment effect interacted with population size is presented in Panel A of Table 8. The interaction term is small, and except for the specification using all other municipalities as a comparison group, the interaction term is not significant. Thus, we do not find robust evidence that the grant policy effect depends systematically on municipality size, apart from the fact that large and densely populated municipalities were the recipients of the grant in the first place.

Property tax

Cascio et al. (2013) find that the spending effect of federal grants allocated through the introduction of the Federal Title 1 program was highest in school districts with the lowest ability to raise local revenue, as measured by the share of revenue raised locally before the introduction of the program. While the school finance system in Norway is generally highly centralized through a tax sharing system, Norwegian municipalities can decide whether to have property taxes or not, although the tax rate is limited to between 0.2% and 0.7% of a property's value. We investigate whether the effect of the 2015 grant differs across school districts, depending on whether or not they had access to local property tax revenue in the pre-treatment period. The results are presented in Panel C in Table 8. The interaction effect is numerically small, positive and not statistically significant.

[18] Brunner et al. (2017) use a direct measure of teachers' union bargaining power and find that the effects of school finance reforms on actual school district spending are strongest in states with strong teachers' unions. The evidence of scale effects in public sector production in general is mixed; see Blom-Hansen et al (2016) and the references therein. The evidence from the limited literature on the effect of district size on student performance is also inconclusive. For example, Driscoll et al. (2003) find that test scores are negatively related to district size in California. Using Danish data, Heinesen (2005) concludes that educational attainment is higher for students from larger districts, i.e. districts with populations above 15,000. Berry and West (2010) exploit variation in the timing of consolidation across U.S. states and find that larger districts have some modest gains with respect to returns to education and completed years of schooling.

Table 8: Heterogenous effects of the grant policy (municipality level)

	(1) All untreated LGs	(2) 100 LGs closest to cutoff	(3) 75 LGs closest to cutoff	(4) 50 LGs closest to cutoff	(5) 25 LGs closest to cutoff
Panel A: Interaction with share of non-certified teachers					
Treat*After	0,423	0,249	0,275	0,213	0,586
	(0,337)	(0,359)	(0,365)	(0,382)	(0,423)
Interaction with share of non-certified teachers	-0,164	-0,171	-0,161	-0,138	-0,125
	(0,102)	(0,104)	(0,105)	(0,106)	(0,106)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	5136	2400	2100	1800	1500
Panel B: Interaction with population					
Treat*After	0,034	-0,232	-0,180	-0,173	0,242
	(0,214)	(0,234)	(0,239)	(0,266)	(0,336)
Interaction with population	-0,004***	-0,001	-0,000	-0,000	-0,001
	(0,001)	(0,002)	(0,002)	(0,002)	(0,002)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	5136.000	2400	2100	1800	1500
Panel C: Interaction with property tax					
Treat*After	-0,429	-0,602*	-0,522*	-0,507	-0,055
	(0,281)	(0,308)	(0,314)	(0,338)	(0,408)
Interaction with property tax	0,554*	0,527	0,496	0,474	0,397
	(0,328)	(0,331)	(0,331)	(0,327)	(0,325)
Control variables	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
N	5128	2400	2100	1800	1500

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.

5.2 Intra-municipality heterogeneity: School level analysis

The 2015 grant was received by municipalities, but then allocated to individual schools to enable them to hire additional teachers. 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. For instance, funds might have been disproportionally spent on large schools, lower performing schools or schools with a high share of children from immigrant families or from families with low socioeconomic status. While budgetary data at school level is not available, we have information on real resource use in the form of student-teacher ratios (group sizes) at school level. Motivated by the approach and findings in Hyman (2017), we use the following school level model specification to investigate the distribution of resources across schools within municipalities:

$$(3)\quad y_{smt} = \alpha_0 + \beta Treated \times d_t + X_{mt}\delta + \rho d_t + \eta_m + \varepsilon_{smt}$$

We divide the sample into schools with above/below average municipality characteristics in the pre-treatment period and estimate the model within the similar difference-in- differences framework as in

the municipality level analysis. The dependent variable in the model is adjusted student-teacher ratio in school *i*, in municipality *m* in school year *t*. The treatment variable is the same as in the municipality level analysis.

Table 9: Heterogenous effects of the grant policy (school level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Control group: All LGs		Control group: 100 LGs closest to cut-off		Control group: 75 LGs closest to cut-off		Control group: 50 LGs closest to cut-off		Control group: 25 LGs closest to cut-off	
	>mean	<mean	>mean	<mean	>mean	<mean	>mean	<mean	>mean	<mean
Panel A: Schools above and below average adjusted student-teacher ratio in municipality										
Treat*After	-0.251	-0.118	-0.568	0.104	-0.721	0.146	-0.075	0.137	0.122	0.184
	(0.323)	(0.220)	(0.612)	(0.253)	(0.792)	(0.258)	(0.347)	(0.300)	(0.522)	(0.371)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12255	12328	8667	8029	7924	7394	7276	6780	6651	6216
Panel B: Schools above and below national average adjusted student-teacher ratio										
Treat*After	-0.605	0.605**	-0.849	0.607**	-1.130	0.595*	0.079	0.481	0.246	0.531
	(0.946)	(0.260)	(1.127)	(0.292)	(1.335)	(0.305)	(0.345)	(0.345)	(0.423)	(0.460)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10148	14435	9841	6855	9590	5728	9314	4742	8988	3879
Panel C: Schools above and below average student-teacher ratio in municipality										
Treat*After	-0.242	-0.143	-0.565	0.180	-0.744	0.248	-0.030	0.126	0.190	0.145
	(0.316)	(0.239)	(0.585)	(0.273)	(0.761)	(0.276)	(0.320)	(0.309)	(0.469)	(0.402)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12538	12045	8934	7762	8170	7148	7534	6522	6895	5972
Panel D: Schools above and below average #students in municipality										
Treat*After	-0.226	-0.186	-0.489	-0.017	-0.555	-0.066	0.221	-0.122	0.533	-0.127
	(0.350)	(0.226)	(0.666)	(0.274)	(0.867)	(0.285)	(0.298)	(0.344)	(0.412)	(0.471)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10795	13788	7835	8861	7212	8106	6661	7395	6098	6769
Panel E: Schools above and below national average #students										
Treat*After	-0.193		-0.203		-0.277		0.034		0.165	
	(0.208)		(0.342)		(0.419)		(0.248)		(0.350)	
Control variables	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes	
N	24590		16701		15322		14060		12870	

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.

We begin by estimating equation (3) without heterogeneity, equivalent to carrying out the main analysis at school level rather than at municipality level. Panel E of Table 9 reports the results, and estimates are comparable to the municipality level estimates reported in Tables 2 and 3.

Pre-treatment teacher density

Did the treated governments distribute the extra resources to schools with low teacher density, measured as a high adjusted student-teacher ratio in the pre-treatment period, as would be expected if they strictly followed the intentions of the central government? To investigate this, in Panel A of

Table 11 we first divide schools according to whether they were above or below the average student-teacher ratio in the pre-treatment period. We then estimate a version of the model in Panel B of Table 11, where we group schools according to whether they were above or below the national average adjusted student-teacher ratio in the school years 2012/13-2014/15. Finally, in Panel C of Table 11 we group schools according to whether they were above or below the average student-teacher ratio. While our findings indicate that the adjusted student-teacher ratio increased at schools below the national average adjusted student-teacher ratio in the treated municipalities, the results also suggest that the allocation of teachers across schools with different initial group sizes was not affected by the treatment, as the estimated effects are small and statistically insignificant.

School size

Of the 100 municipalities that received extra grants as a result of the 2015 grant policy, 17 municipalities reported that they distributed the funds on the basis of the number of students in each school. This anecdotal evidence indicates that municipalities might have used a simpler rule (than the student-teacher ratio) when they distributed the extra resources across schools. To investigate this, in Panel D of Table 11 we divide schools into those above the average school size (measured as the number of students in grades 1-4 in the pre-treatment period) and those below the average school size within a municipality. The results provide no evidence that municipalities favored schools by enrolment size when they allocated the 2015 grant.

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 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. Our findings are quite different from the findings in Kirkebøen et al. (2016) 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 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 strong enforcement mechanisms are 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.

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Appendix A: Details of the grant policy intervention

As part of the national budget agreement in 2015, the Norwegian government decided to introduce a special government grant to increase teacher density in grades 1-4 with the intention of strengthening early intervention and improving student learning.[19] The grant targeted municipalities with student-teacher ratios above the national average in the school years 2012/13-2014/15.[20] Ultimately, 100 out of 428 municipalities were awarded approximately NOK 360 million per year to increase teacher density in grades 1-4.

In April 2015, UDIR transferred NOK 150 000 000 to the targeted municipalities. The funds were divided as a sum per student based on the average number of students in grades 1-4 during these three years. Table A1 displays how much of the grant each of the targeted municipalities received during the school year 2015/16 and how this amount translates into (potential) teacher man years.

For 2015, the sum per student was equal to NOK 946.22. The targeted municipalities would receive 5/12 of the grant in April and 7/12 of the grant in January. If a municipality had an average number of students equal to 1 585 in the school years 2012/13-2014/15, they would receive NOK 1 499 995 in April 2015. In 2016, UDIR transferred NOK 370 440 000 (360 000 000 x 1.028) to the targeted municipalities. NOK 216 090 000 was transferred in January and NOK 154 350 000 was transferred in April. For 2016, the sum per student was equal to NOK 2 336.79. Thus, a municipality with an average number of students equal to 1 585 in the school years 2012/13-2014/15 would receive NOK 3 704 400 in 2016, with NOK 2 160 900 being transferred to the municipality in January and NOK 1 543 500 in April. Thus, for the 2015/16 school year the municipality would receive NOK 3 660 900 to increase the number of teachers this school year. As 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 5.2 teacher person-years. If the municipality had a teaching stock of 82 in the 2014/15 school year, this suggests that the grant allowed the municipality to increase their teaching stock by around 6 percent.

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

[20] The Norwegian Directorate for Education and Training (UDIR) operates with two measures of student-teacher ratios in compulsory education. The *student-teacher ratio* is defined as the ratio of the total number of student hours to the total number of teaching hours. This definition provides a picture of the total use of resources at a school or in a local government. The *adjusted student-teacher ratio* excludes resources for special needs education and basic Norwegian for language minorities and is an indication of students per teacher in ordinary education. The adjusted student-teacher ratio is a better indicator than the student-teacher ratio for assessing the ratio of students to teachers in an ordinary teaching situation. The adjusted student-teacher ratio was used as the teacher density measure when deciding which local governments would receive extra funding.

Table A1: List of local governments receiving extra grants

County	Local government	Average number of students 2012/13-2014/15	Grant size school year 2015/16	Teacher person-years 2014/15 (unadjusted)	Increase in teacher person-years (unadjusted) 2015/16	Increase in teacher person-years (adjusted) 2015/16 (percentage change)
Aust-Agder	Arendal	1 988	4 591 752	128.98	7	5%
Akershus	Asker	3 180	7 342 956	194.02	10	5%
Østfold	Askim	748	1 728 162	51.4	2	5%
Hordaland	Askøy	1 652	3 815 042	107.2	5	5%
Telemark	Bamble	646	1 491 838	44.49	2	5%
Hordaland	Bergen	11 829	27 317 272	756.15	39	5%
Sør-Trøndelag	Bjugn	211	486 503	17.72	1	4%
Akershus	Bærum	6 447	14 888 363	393.23	21	5%
Telemark	Bø i telemark	276	637 380	19.34	1	5%
Buskerud	Drammen	3 126	7 219 020	186.52	10	6%
Akershus	Eidsvoll	1 166	2 691 930	75.32	4	5%
Hedmark	Elverum	993	2 292 412	60.73	3	5%
Akershus	Enebakk	614	1 417 939	41.4	2	5%
Vest-Agder	Farsund	474	1 093 861	32.43	2	5%
Akershus	Fet	597	1 377 911	36.65	2	5%
Østfold	Fredrikstad	3 558	8 215 889	247.34	12	5%
Akershus	Frogn	770	1 778 967	46.11	3	6%
Akershus	Gjerdrum	348	804 423	20.9	1	5%
Buskerud	Gol	224	517 294	14.28	1	5%
Aust-Agder	Grimstad	1 129	2 608 022	75.08	4	5%
Østfold	Halden	1 383	3 193 059	89.27	5	5%
Hedmark	Hamar	1 229	2 837 419	81.5	4	5%
Møre og Romsdal	Hareid	239	552 704	15.84	1	5%
Troms	Harstad	1 101	2 541 823	81.09	4	4%
Rogaland	Haugesund	1 679	3 877 394	119.48	6	5%
Vestfold	Hof	165	380 273	10	1	5%
Buskerud	Hole	318	734 373	25.91	1	4%
Vestfold	Holmestrand	412	952 221	27.14	1	5%
Vestfold	Horten	1 174	2 711 943	79.46	4	5%
Buskerud	Hurum	389	898 336	26.33	1	5%
Østfold	Hvaler	181	417 992	11.29	1	5%
Sør-Trøndelag	Klæbu	384	886 789	25.9	1	5%
Buskerud	Kongsberg	1 224	2 826 641	87.45	4	5%
Telemark	Kragerø	428	989 170	29.34	1	5%
Vest-Agder	Kristiansand	4 135	9 548 383	256.87	14	5%
Vestfold	Larvik	1 950	4 502 459	137.3	6	5%
Nord-Trøndelag	Leksvik	179	412 603	11.19	1	5%
Troms	Lenvik	602	1 389 457	42.13	2	5%

Nord-Trøndelag	Levanger	955	2 205 427	72.18	3	4%
Buskerud	Lier	1 362	3 145 331	86.37	4	5%
Oppland	Lillehammer	1 160	2 679 612	79.88	4	5%
Vest-Agder	Lindesnes	236	545 776	14.81	1	5%
Oppland	Lunner	459	1 060 760	28.5	2	5%
Vest-Agder	Lyngdal	440	1 016 883	32.41	1	4%
Akershus	Lørenskog	1 751	4 042 898	101.21	6	6%
Hedmark	Løten	316	730 524	21.53	1	5%
Sør-Trøndelag	Malvik	738	1 705 068	53.21	2	5%
Vest-Agder	Mandal	850	1 962 945	50.84	3	6%
Buskerud	Modum	615	1 419 479	43.29	2	5%
Møre og Romsdal	Molde	1 167	2 694 240	72.88	4	5%
Østfold	Moss	1 392	3 214 612	99.94	5	5%
Akershus	Nannestad	658	1 518 781	40.95	2	5%
Buskerud	Nedre Eiker	1 253	2 892 843	81.7	4	5%
Akershus	Nes i Akershus	928	2 143 074	61.88	3	5%
Akershus	Nesodden	904	2 088 420	58.14	3	5%
Akershus	Nittedal	1 266	2 923 634	79.22	4	5%
Buskerud	Nore og Uvdal	105	242 481	9.57	0	4%
Vestfold	Nøtterøy	1 003	2 315 507	68.34	3	5%
Akershus	Oppegård	1 450	3 348 554	82.38	5	6%
Sør-Trøndelag	Orkdal	599	1 382 529	38.54	2	5%
Hordaland	Os i Hordaland	1 102	2 544 132	68.21	4	5%
Oslo	Oslo	25 644	59 221 669	1638.33	85	5%
Telemark	Porsgrunn	1 541	3 558 704	107.99	5	5%
Østfold	Rakkestad	372	858 308	26.3	1	5%
Vestfold	Re	466	1 076 156	31.69	2	5%
Akershus	Rælingen	877	2 025 298	24.94	3	12%
Buskerud	Røyken	1 139	2 630 347	58.23	4	6%
Østfold	Råde	334	772 092	68.76	1	2%
Hordaland	Samnanger	114	262 496	9.87	0	4%
Vestfold	Sande i Vestfold	498	1 150 825	34.57	2	5%
Vestfold	Sandefjord	2 058	4 753 406	148	7	5%
Rogaland	Sandnes	3 825	8 833 254	268.25	13	5%
Østfold	Sarpsborg	2 503	5 779 528	173.6	8	5%
Telemark	Sauherad	176	405 675	12.76	1	5%
Møre og Romsdal	Skaun	448	1 035 357	32.85	1	5%
Akershus	Skedsmo	2 624	6 058 959	172.98	9	5%
Akershus	Ski	1 724	3 980 546	99.71	6	6%
Telemark	Skien	2 335	5 391 557	149.12	8	5%
Østfold	Skiptvet	201	464 179	14.57	1	5%
Østfold	Spydeberg	269	621 984	18.03	1	5%
Rogaland	Stavanger	6 017	13 896 113	411.75	20	5%

Nord-Trøndelag	Stjørdal	1 165	2 690 390	88.19	4	4%
Hordaland	Stord	975	2 250 844	70.41	3	5%
Rogaland	Strand	704	1 626 550	44.56	2	5%
Møre og Romsdal	Sula	484	1 117 724	34.79	2	5%
Vestfold	Svelvik	304	702 811	17.79	1	6%
Vest-Agder	Søgne	543	1 254 745	38.14	2	5%
Akershus	Sørum	974	2 250 074	58.12	3	6%
Vestfold	Time	958	2 211 585	69.41	3	5%
Troms	Tromsø	3 455	7 978 796	235.04	11	5%
Sør-Trøndelag	Trondheim	8 026	18 534 054	554.41	26	5%
Vestfold	Tønsberg	1 833	4 233 803	116.29	6	5%
Akershus	Ullensaker	1 874	4 328 486	114.64	6	5%
Møre og Romsdal	Ulstein	456	1 052 293	33.44	2	4%
Aust-Agder	Vegårshei	115	266 345	8.27	0	5%
Nord-Trøndelag	Verdal	748	1 726 622	54.04	2	5%
Akershus	Vestby	906	2 092 269	62.83	3	5%
Buskerud	Øvre Eiker	880	2 032 996	60.48	3	5%
Møre og Romsdal	Ålesund	2 194	5 067 477	147.39	7	5%
Akershus	Ås	910	2 101 506	67.3	3	4%
Total			**366 089 311**	**10430.6**	**523**	
Average			**3 660 893**	**104.306**	**5**	**5%**

Appendix B: Definitions of explanatory variables

Non-certified teachers measured at regional level (%): (The number of teachers without approved education, divided by the total number of teachers in primary education (grades 1-7) in labour market regions)*100. Registration date is October 1 each year. *Source: Grunnskolens informasjonsstystem (GSI).*

Population (measured in thousands): The total number of inhabitants in the local government, divided by 1000. Measured December 31 each year. *Source: Statistics Norway.*

Share aged 1-5 (%): (The number of inhabitants aged 1-5 divided by the total number of inhabitants in the local government)*100. Measured December 31 each year. *Source: Statistics Norway.*

Share aged 6-15 (%): (The number of inhabitants aged 6-15 divided by the total number of inhabitants in the local government)*100. Measured December 31 each year. *Source: Statistics Norway.*

Share aged 67-79 (%): (The number of inhabitants aged 67-79 divided by the total number of inhabitants in the local government)*100. Measured December 31 each year. *Source: Statistics Norway.*

Share aged 80+ (%): (The number of inhabitants aged 80+ divided by the total number of inhabitants in the local government)*100. Measured December 31 each year. *Source: Statistics Norway.*

Share immigrants (%): (The number of immigrants and Norwegian-born children of immigrant parents divided by the total number of inhabitants in the local government)*100. Measured December 31 each year. *Source: Statistics Norway.*

Share divorced (%): (The number of divorced or separated inhabitants aged 16-66 divided by the total number of inhabitants aged 16-66 in the local government)*100. Measured December 31 each year. *Source: Statistics Norway.*

Share disabled (%): (The number of disability pensioners aged 16-66, divided by the total number of inhabitants aged 16-66 in the local government)*100. Measured December 31 each year. *Source: Statistics Norway.*

Share higher education (%): (The number of inhabitants aged 16-66 with higher education divided by the total number of inhabitants aged 16-66 in the local government)*100. Measured December 31 each year. S*ource: Statistics Norway.*

Population density (%): Source: (The number of inhabitants living in populated areas divided by the total number of inhabitants in the local government)*100. Measured December 31 each year. *Source: Statistics Norway.*