The Long-Run Impact of Increasing School Funding on Labor Market Outcomes

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August 29, 2023

Abstract

I investigate the impact of increasing funding for education on short-term local administration responses and long-term student outcomes. I exploit a 1986 intergovernmental transfer reform in Norway that shifted the allocation of school funding to municipalities, resulting in a revenue shock of almost 10% of local education budget, benefiting those with higher prior share of primary school students. Using detailed municipal and individual-level data, and applying an event-study design with a variety of controls and fixed effects, I show that municipalities with higher funding increase teaching hours and construct new schools. On the individual level, an increase of $1000 in expected education funding leads to around a $13,000 increase on earnings by the age of 35, which is equivalent to 1/3 of average wages and 0.6 of its standard deviation. Furthermore, I find positive and significant impacts on students' educational attainment and migration to larger cities, especially for those exposed to the shock in primary school. Quantile regressions indicate that the effects on earnings are stronger and more significant at the lower end of the distribution, and are more pronounced for children of under-educated parents, indicating that increased school funding has an equality-enhancing impact. Both municipal and individual results are driven primarily by rural municipalities, as the increase in intergovernmental transfer is offset by lower own education spending from local administrations in more central municipalities, as they show higher municipal tax revenues and less dependency on grants.

JEL Classification: H75, I21, I26, I28
Keywords: Education, Intergovernmental Transfers, School Funding

1 Introduction

Education is widely recognized as a key determinant of individual and societal well-being, with higher levels of education associated with improved health outcomes, higher earnings, and greater social mobility. However, ensuring that all individuals have access to high-quality education remains a significant challenge for many countries around the world. One important factor in the provision of education is funding, which can impact the resources available to schools and the quality of instruction that students receive.

The topic of education funding has been of interest to researchers and policymakers for decades, with a large body of literature exploring the relationship between funding levels and student outcomes. However, there is still much to learn about the long-term effects of increasing education funding on student success, particularly in terms of the long run impact on earnings. An intergovernmental transfer reform that took place in 1986, in Norway, provides a unique opportunity to study the effects of a large, exogenous increase in education funding on long-term outcomes for students. By exploiting this natural experiment and using detailed data on both municipalities and individuals, this study aims to shed light on the relationship between education funding and student success.

In this paper, I exploit a reform in intergovernmental transfers in Norway to estimate the effect of increasing funding destined to municipal education on school inputs and students' labor market outcomes later on, matching municipal and individual Norwegian register data. I document that the intergovernmental transfers reform increased funding to municipalities with a higher share of primary school-aged students before the reform (out of the total number of compulsory school students).

Employing an event-study design, with a wide range of controls and fixed-effects, I show that additional $1000 dollars in education funding led to a higher teachers-pupil ratio (by 0.1) and 20 more weekly per student gross hours of work among school employees, as well as an increase in the number of public schools. However, the funding did not affect school staff income or class size, indicating that more teachers and school staff were, on average, destined to increasing instructional hours intensity. These results were largely driven by rural municipalities. Local administration in more central localities seems to offset the transfer with decreased own spending on education, since they have higher tax revenues and are less dependent on grants.

At the individual level, I interact expected revenue shock based on the student's municipality of residence just prior to the reform with different cohort groups. I explore that some cohorts were too old to be in compulsory school by the time of the reform, as others were exposed either while at primary or at lower-secondary school. Controlling for various individual characteristics, such as parental education interacted with year of birth, I find that an expected additional $1000 in education funding leads to a higher probability of being employed and $12,000 to $13,500 higher yearly earnings, by the age of 35, for students exposed in primary and lower-secondary school, respectively. Quantile regressions suggest that the effects on earnings are higher and more significant at the lower end of the distribution, indicating an equality-enhancing impact on the labor market.

The results also show that the shock led to increased migration to larger cities and higher educational attainment, particularly for children from lower socio-economic status parents. The effects, measured in terms of standard deviation, range from 0.5 to 1, with larger effects for children exposed to the shock in primary school. Those patterns are consistent with the recent literature [Jackson and Mackevicius, 2023], although

Higher and more significant point estimates are primarily found for students from rural municipalities, where there was no evidence of crowding out due to increased central administration transfers. For robustness checks, I show that the funding did not increase spending in any other sector, then I change the the cohort groups structure and randomize the treatment, finding consistency with the prior set of results. I final robustness check test the effect of the shock on ability and height for men, finding no statistically significantly relationship. Also, using those variables as control does not significantly change the estimates.

This paper contributes to the literature on local administration responses to central administration grants, which has produced mixed results. Gordon [2004] found that the Title I program, which allocates money for compensatory education to school districts, led to substantial and significant crowding out over time. Reiling et al. [2021] found no effect on the intended variable, the student-teacher ratio in primary school, when exploring a central government grant to municipalities in Norway. On the other hand, Cascio et al. [2013] estimated that the Title I program increased education spending in local administrations with less ability to offset grants through local tax reductions. They have also found effects on students' outcomes in those localities, though concentrated on white students. Litschig and Morrison [2013] found that extra transfers introduced in the 1980s in Brazil increased local government spending per capita by about 20 percent over a 4-year period, with no evidence of crowding out, followed by an increase in schooling per capita, literacy rates, and a reduction in poverty rate. This paper shows that, in some types of municipalities, higher central administration education funding can be crowded out, leading to no effect on students in those regions, but not in rural areas, which have lower own revenues and thus more dependency to grants, showing improved outcomes for students exposed to higher transfers.

This paper also contributes to an increasing body of literature, that has been developed since Coleman [1966], on effects of education spending, being one of few papers to estimate the impacts of spending on student outcomes in adulthood on earnings, schooling and migration. There is a substantial amount of evidence that documents the impact of education spending on various outcomes, such as test scores [Card and Payne, 2002], education attainment [Hyman, 2017], wages [Jackson et al., 2015], poverty [Lafortune et al., 2018], and intergenerational mobility [Biasi, 2023]. This paper is particularly closely related to Baron [2022], which explores revenue limits on the annual increase in a school district's per-pupil funding in Wisconsin, which requires districts to hold separate referenda for operational and capital expenditures. The author uses close elections in a dynamic regression discontinuity framework to estimate the causal impact of those two distinct types of expenditures, finding that increases in operational spending have substantial positive effects on test scores, dropout rates, and postsecondary enrollment, but additional capital expenditures have little impact. However, this paper's contribution is to explore further the impacts of increases in education funding on labor market outcomes, from a national reform that allowed municipalities to increase either operational or capital expenditures as they seen fit.

Most of literature of education spending effects is based on school funding formula reforms in the United States since the 1970s, with very few exceptions [Jackson and Mackevicius, 2023; Baron, 2022]. The evidence for other countries is either limited to capital spending [Belmonte et al., 2020] or is based on fewer quasi-experimental designs. For example, Gibbons et al. [2012] and Heinesen and Graversen [2005] found that expenditure per pupil is statistically significantly associated with test scores and educational attainment using British and Danish data, respectively, but the estimated effects are much smaller than those found in this paper.

Norway is among the top education spending countries in the world, both in nominal terms and as a share of GDP. Public funding for education increased from around 5.5% to about 7% of GDP between the 1980s and the 2010s, a level matched only by other Nordic countries. It also has a decentralized but heavily regulated educational system [OECD, 2020] and a heavily unionized and centralized wage bargaining labor market [Balsvik et al., 2015; Nilsen, 2020]. Literature expects lower impacts of increasing funding on education when spending levels are already high [Vegas and Coffin, 2015] and returns to schooling are lower [Becker, 2009]. Thus, Norwegian distinct context provides an opportunity to evaluate the impact of increased education funding on outcomes.

This paper provides strong evidence that increasing funding for education leads to better outcomes for students, even in the context of a heavily regulated education sector and an unionized and wage-centralized bargaining labor market. Despite lower returns to schooling in Norway compared to other OECD countries [Trostel et al., 2002; Psacharopoulos and Patrinos, 2018; OECD, 2021], the results indicates that students do benefit from increased funding, especially from low-educatation backgrounds and at lower ends of the distribution. I find impact on earnings for all students exposed to the funding increase, while those exposed earlier in their education experience positive effects also on employment, years of study and mobility. These findings are in line with the existing literature, which has shown that human capital interventions are most effective when applied in the early stages of a child's life [Carneiro and Heckman, 2003].

Finally, this paper also contributes to the literature on school inputs and their impact on learning and future outcomes. Most articles focus on the effect of class size [Angrist and Lavy, 1999; Fredriksson et al., 2013] and has found mostly positive impacts on learning and earnings. However, there is mixed evidence in Norway [Leuven and Løkken, 2020; Borgen et al., 2022]. This paper shows that, instead of direct policies on school inputs, higher municipal education funding can be effective. Municipalities increase both current and capital spending, not changing its composition, and leading to positive impacts on various school inputs.

The remainder of this paper is organized as follows. In the next section, I describe how primary education was partially funded by Norwegian Central Administration, and how the reform in 1986 affected it. In Section II, I describe the method and the data. I present the results and robustness checks in Section III, Section IV provides robustness checks, and Section V concludes.

2 Institutional Background

2.1 Educational System in Norway

Norway is one of the top spending countries in schools, ranking from top 7th to 3rd in public expenditure on education as a share of GDP over the years. In the 1980s, it was almost 6% of GDP, increasing to about 7% in the following decades. The share of that expenditure to primary education dropped from around 45% to 25%, as the 7-15 years old population decreased from 15 to 12% on that period. Expenditure per student has remained fairly constant since the 1990s, on a level of about 20% of GDP per Capita, ranking from 10th to 5th among all countries.

All children are entitled to free public education from primary to upper-secondary education, and all public tertiary education institutions are free of charge. Municipalities are responsible for primary (1st to 6th grade) and lower-secondary education (7th to 9th grade), both compulsory for children aged between 7 and 15 years old[1]. Upper-secondary education is provided by counties, with an enrollment rate of about 90%. Higher education is provided by the National Ministry of Education and Research, with gross enrollment rate increasing from 25% (as a share of the 18-22 years old population) in 1980 to about 80% after the 2000s. Tertiary vocational education is a short vocational alternative to higher education.

Teachers on all levels have below-average teaching hours and low student-to-teacher ratios. However, school employees' wages have been lower than similarly educated workers.

[1]As of 1997, children start school the year they turn 6 and compulsory schooling lasts ten years

In Norway, primary and secondary education are regulated by the Ministry of Education and Research, which sets national policy and oversees local governance. However, municipalities are primarily responsible for defining the level and distribution of resources. Finally, schools may decide on the internal allocation of budgets, staffing, and student admissions, but all are adhering to the same laws and curriculum.

School sizes are low in general: in 2011, one-third of them had fewer than 100 students and less than one-third (27%) more than 300 students [Seland et al., 2013]. Until 2003, there were class size rules, with a maximum of 30 students in middle schools and 28 in primary schools. Schools have the flexibility to allocate up to 25% of lessons for individual needs, and a quality framework outlines principles for optimal learning environments and achievements. Schools are encouraged to strengthen their partnerships with parents and the local business community.

In primary school, no grades are given, but mandatory subjects in lower-secondary school are assessed. Upon completion, students receive a certificate with their grades and are entitled to three years of upper-secondary education. High-stakes testing is limited to the last year of lower-secondary school and upper-secondary level, but national tests were introduced in 2004 to promote school improvement and identify students who may need additional support.

According to OECD [2020], Norway performs well in reading and mathematics on PISA[2], with a low impact of socio-economic status on reading scores and above average adult literacy skills. Therefore, norwegian education system seems to partially offset family socioeconomic background. Also, high education expenditure has not led to improvement in PISA scores, and there are ongoing challenges in reducing performance gaps, improving upper secondary completion rates and salaries of teaching staff, and aligning school evaluation with competence development.

2.2 Intergovernmental transfers up to 1985

During the 1960s and 1970s, municipal revenues increased steadily, mostly funded by intergovernmental transfers and reimbursement schemes. By the early 1980s, the Central Administration was responsible for funding around 35% of municipal spending, which is a similar level to that in most developed countries with a decentralized government system [Bergvall et al., 2006]. Municipal tax revenues, on the other hand, make up 60% of municipalities' budgets.

The autonomy of municipalities in Norway was gradually reduced by the central government and parliament in the post-war years due to the political objective of universal welfare services. However, Langørgen et al. [2013] documents that the revenue system of the municipalities became increasingly complex, consisting of many small and large earmarked grants, which lacked incentives for cost efficiency.

Regarding intergovernmental transfers for education, regulations in place until 1985 required the Central Administration to cover between 25% and 85% of each municipality's gross expenses on the sector. Graph 7, in the appendix, shows that most of the central administration funding share ranges between 30 and 60% of total municipal spending on that sector.

The transfer amount used to be calculated based on the number of teaching hours, which were valued differently depending on the level of education (the value of teaching hours was referred to as the "Cost Factor"). Other minor criteria were also used to determine smaller portions of the transfer, such as per capita municipal tax revenues and the share of education spending in total municipal expenditure. The formula for the transfer is given in Equation 1:

[2] Programme for International Student Assessment, a triennial international survey which aims to evaluate education systems worldwide by testing the skills and knowledge of 15-year-old students.

$$Transfer_{m,t} = \sum_{l} (\text{Cost Factor}l, t \times Hoursl, m, t) + \epsilon_{l,m,t} \quad (1)$$

where $Transfer_{m,t}$ represents the transfer amount to municipality m for grant size in year t, Cost Factorl, t represents the Cost Factor at the schooling level l in year t, $Hoursl, m, t$ represents the annual teaching hours at level l in municipality m set in year t, and $\epsilon_{l,m,t}$ represents the sum of the other criteria (per capita municipal tax income, the share of education spending in total municipal expenditure, etc.) at level l in each municipality m set in year t.

The Cost Factor was determined by the Central Government each year for primary and lower-secondary levels separately. In 1985, the Cost Factor was set at NOK 130.05 ($29.30 in 2011 PPP dollars) for primary education (for children aged 7 to 12) and NOK 146.80 ($33.07 in 2011 PPP dollars) for lower-secondary education (for children aged 13 to 15).

The municipalities could determine the number of weekly hours pupils received from 1st to 6th grade within a range of 129 to 147 weekly teaching hours, with the central administration grants covering up to 138 hours. At the lower-secondary level, the number of weekly hours was set at 30 for regular teaching at each grade level, in addition to 17.5 hours per week for special education, electives, and other measures.

2.3 The 1986 intergovernmental transfers reform

In 1979, the Norwegian Tax Equalization Committee released a report on a new intergovernmental transfers system proposal for counties (NOU 1979: 44), while, in 1982, a similar report was released for municipalities (NOU 1982: 15). Those were used as a basis for the bills 'St.meld. No. 26 (1983-84) - "On a new revenue system for the municipalities and counties"', and 'Ot.prp. No. 48 (1984-85) - "On amendments to laws concerning the revenue system for the municipalities and counties"'.

These bills introduced a new system in 1986 that replaced most prevailing intergovernmental grants, creating an income equalizing grant and three major sector grants: for health, education, and culture (and general purposes). For each sector, cost matrices were constructed based on characteristics that the counties and municipalities would not be able to change over time. With associated weights to those variables, cost matrices provide a number of 'points,' which are used to distribute central administration grants to this day. The criteria and weights were developed with the intention of addressing different municipalities' costs of delivering an equal range of services in each of the three sectors.

Under this new set of rules, in the education cost matrix, there was no differentiation between primary and lower-secondary education, as shown in Table 1. Therefore, municipalities with a higher proportion of younger children (aged 7-15) experienced an exogenous increase in the grant transfer amount.

Table 1: Primary Education Cost Matrix

Criteria	Weight
Approved annual teaching hours in 1985	0.47
Number of inhabitants 7-15 years	0.41
Others	0.12

Source: Langørgen et al. [2013]

It was emphasized that the transition to a new system in 1986 would not lead to major changes in the transfers to local administrations in a short period of time. Changes in criteria and weights were to be phased in over several years: first two years, the new system would be weighted 10 and 20%, respectively, while the old system would be accountable for the higher share. In 1987, however, the previous year level was weighted 80%, and the new rules were fully incorporated in 1988.

3 Data and Methodology

3.1 Data

The analysis uses several registry databases maintained by Statistics Norway. The sample is restricted to municipalities that did not merge, split, or change their borders between 1980 and 1991, which corresponded to 402 out of the total number of 456. This will be done so that there is a pool of municipalities fully treated by the changes.

At the individual level, the sample includes all individuals born from 1964 to 1983 who were living in any of those 402 municipalities in 1985 and in any municipality in Norway by the age of 35. The sample size is approximately 1.1 million individuals, out of which around 990 thousands had a paying job.

Table 2 shows the cohort's age by year, grouped into five categories: those who were never exposed to the reform changes and were born between 1964 and 1967, those who were also not exposed and were born between 1968 and 1970 (serving as the baseline in the regressions), those who were marginally exposed and were born between 1971 and 1975, those who were fully exposed in lower-secondary education and were born between 1976 and 1979, and finally, those who were fully exposed in primary education and were born between 1980 and 1983.

Table 2: Cohort age by year

Cohort	Group	1986	1987	1988	1989	1990	1991
1964	Never Exposed	22	23	24	25	26	27
1965		21	22	23	24	25	26
1966		20	21	22	23	24	23
1967		19	20	21	22	23	24
1968	Not exposed [Baseline in Regressions]	18	19	20	21	22	23
1969		17	18	19	20	21	22
1970		16	17	18	19	20	21
1971	Marginally exposed	15	16	17	18	19	20
1972		14	15	16	17	18	19
1973		13	14	15	16	17	18
1974		12	13	14	15	16	17
1975		11	12	13	14	15	16
1976	Exposed at Lower Secondary School	10	11	12	13	14	15
1977		9	10	11	12	13	14
1978		8	9	10	11	12	13
1979		7	8	9	10	11	12
1980	Exposed at Primary School	6	7	8	9	10	11
1981		5	6	7	8	9	10
1982		4	5	6	7	8	9
1983		3	4	5	6	7	8

Notes: This table shows how cohorts will be grouped in the individual level regressions. Children that were above 15 by the year of 1986 were already out of compulsory school. Children grouped into 'Never exposed' will be used to test for pre-trends.

For fiscal data, the 'Strukturtall for kommunenes økonomi' documents will be used, which are available on the Statisk Sentralbyrå (SSB) website. These documents provide detailed data on municipal per capita gross and net operating expenses by group since 1974. Other municipal-level controls are provided by the kommunendatabasen, which covers a wide range of municipality characteristics and policies since the early 1970s. Any other necessary variables will come from registry data, which also allows linking all residents in Norway to the place they were living each year since birth.

Increasing human capital through school funding may have many diverse effects on individuals, making it necessary to elaborate on the outcomes that are tested. By the 1980s and early 1990s, there is no available data on grades or cognitive/non-cognitive abilities, as presented by Fredriksson et al. [2013]. Therefore, this study will explore the effect of the policy on educational level (in terms of years of study) and earnings, around the ages of 33 to 35, as Haider and Solon [2006] and Böhlmark and Lindquist [2006] show that the association between the returns to schooling in lifetime and current earnings is strongest by the mid-30s. Labor market outcomes, such as employment status and earnings, play a crucial role in individuals' well-being. Thus, it is important to primarily understand the effects of increasing education funding on labor market outcomes.

Additionally, since higher human capital tends to increase individual mobility, this study will also explore the potential impact of the policy on migration to one of the four largest cities in Norway[3] at three different stages of life, from early to later adult years.

3.2 Descriptive Statistics

Education spending accounted for around 29% of municipal expenditures between 1980 and 1985, while tax revenues were only 45% of total municipal revenues. Table 3 shows the trends in some key variables.

Table 3: Municipal-Level Sample Averages

Year	(1) Yearly Expenditure on Education	(2) Share of Primary and Lower-Secondary School Students over Population	(3) Share of Primary School Students over (2)	(4) Public Schools	(5) Students per Teacher	(6) Teaching Hours Per Pupil Proxy	(7) Class Size
1979	5630.96	0.153		7.38			18.69
1980	5721.06	0.153		7.68			18.69
1981	5700.90	0.152	0.659	7.69	10.96		18.67
1982	5875.88	0.150	0.651	7.71	10.79		18.54
1983	6010.19	0.148	0.646	7.72	10.62	4.38	18.43
1984	6212.85	0.144	0.637	7.68	10.31	4.71	18.24
1985	6628.84	0.140	0.632	7.65	9.99	4.90	18.18
1986	6703.40	0.136	0.627	7.61	9.36	5.29	17.70
1987	7106.92	0.133	0.627	7.60	8.90	5.59	17.40
1988	7388.35	0.129	0.633	7.59	8.53	5.91	17.17
1989	7710.50	0.125	0.642	7.50	8.41	6.23	17.11
1990	7891.25	0.122	0.653	7.43	8.18	6.40	16.92
1991	10054.06	0.120	0.658	7.40	7.75	6.49	16.87

Notes: Expenditure values in 2011 PPP dollars. Teaching Hours Per Pupil Proxy defined as sum of contracted hours for employees in Primary and Lower-Secondary Schools.

As one may see, municipal per pupil spending on education almost doubled from 1979 to 1991, while the share of students in primary and lower-secondary school dropped from around 15% of total population to 12% in 1991. Although students per teacher and class size dropped, along with increasing teaching hours proxy[4], the average number of public schools dropped after 1983.

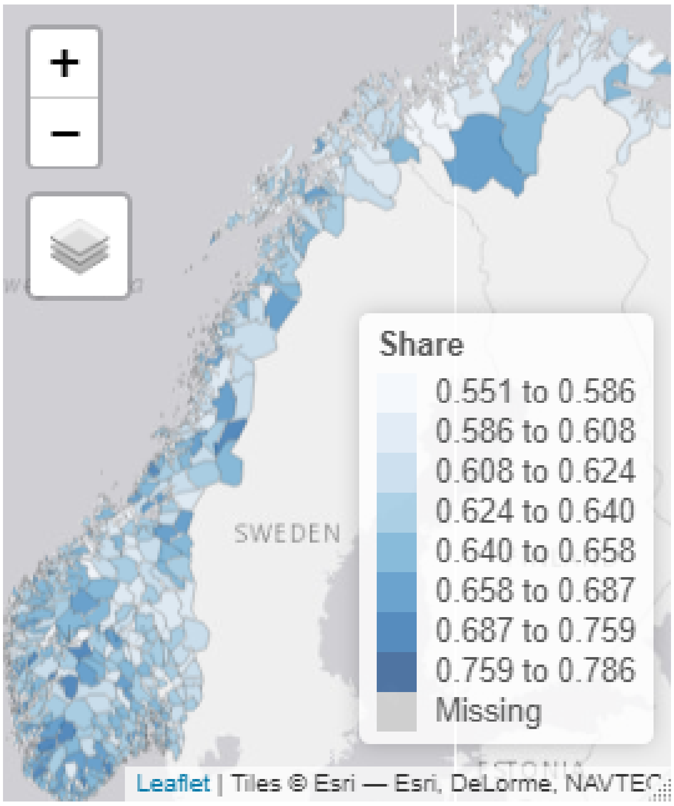
Finally, map 1 shows the share of 7-12 years old children over all within primary and lower-secondary school age, which will be the treatment intensity variable, by municipality

[3]Oslo, Bergen, Trondheim, and Stavanger

[4]Contracted hours from employed workers in primary and lower-secondary schools

in 1985. It shows a distribution with no regional patterns.

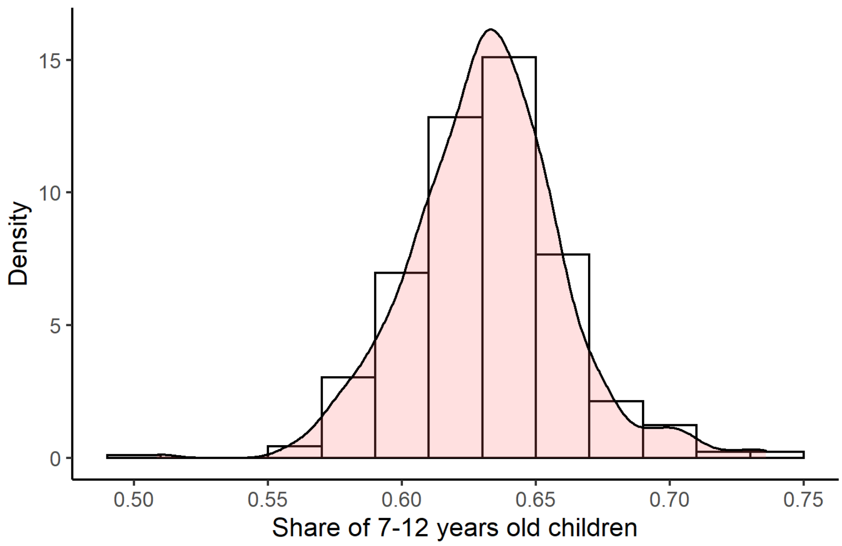
Figure 1: Distribution of the share of children aged between 7 and 12 years old in 1985



Source: Kommunedatabasen. Share over 7-15 years old population

Most municipalities had a share between 55 and 70% in 1985, a small range for the treatment variable, whose standard deviation is about 0.029. However, a few municipalities have more extreme shares, around either 50% or 75%.

Figure 2: Density of the share of children aged between 7 and 12 years old in 1985



Source: Kommunedatabasen. Share over 7-15 years old population

Table 4, additionally, shows the descriptive statistics by cohort group, with all variables fixed at ages between 33 and 35. Similarly to trends shown above, average schooling increased over 1 year of study from Norwegian residents born between 1964 to 1967 to those born between 1980 and 1983, with a similar pattern observed on parents' educational level. Yearly earnings, on the other hand, almost doubled between those cohorts.

Table 4: Individual-Level Sample Averages

Cohort Group (year of birth)	1964-67	1968-70	1971-1975	1976-79	1980-83
Number of Observations	262,506	199,475	307,030	207,059	200,986
Years of Study (at age 33-35)	12.8	13.2	13.6	13.9	14.0
Yearly Earnings (at age 33-35)	22,463.5	25,793.8	31,477.7	37,744.6	41,431.1
Man (Share)	51.4 %	51.3 %	51.0 %	51.1 %	51.3 %
Mothers' Years of Study	11.1	11.3	11.6	11.9	12.2
Fathers' Years of Study	11.7	11.9	12.2	12.5	12.6
Nordic Foreigners	0.9 %	0.7 %	0.6 %	0.4 %	0.2 %
Other Foreigners	2.3 %	2.2 %	2.3 %	2.0 %	1.5 %

Obs: Earnings in 2011 PPP dollars, outliers (bottom and top 1%) excluded

3.3 Empirical Procedure

3.3.1 Assessing transfer amount determinants

I will take advantage of the fact that it is possible to observe the education grant amount until 1985 to estimate its relationship to the share of primary school students. This methodology is in a similar fashion as presented in Frean et al. [2017] for a "simulated" measure of eligibility. These simulated policy measures can serve as instruments for each municipality's actual education grant. The first-stage regression for such a 2SLS estimate is close to one for each policy measure

Formula 2 describes the model to assess the determinants of the education transfer amount ($y_{i,t}$) to municipalities.

$$y_{i,t} = X'_{i,k}\alpha_k + \gamma_m + \delta_t + \epsilon_{m,t} \quad (2)$$

where $X_{i,k}$ is a matrix of variables that may influence the distribution of resources for education, that is, the share of students aged between 7 and 12 as a share of total primary and secondary school students, teachers per pupil (a proxy for teaching hours), per capita municipal tax income and education spending's share of total municipal expenditure. There are also municipal and year fixed effects included.

3.3.2 Municipal-level Analysis

Primary and lower-secondary school is compulsory in Norway and grade retention is uncommon at those levels. Therefore, I exploit cross-municipality variation in pre-reform share of children aged between 7 and 12 years old over the total of 7-15 aged students, which I will interact with the coefficient associated with the shock associated with it in equation 2, constructing the revenue shock variable ($Shock_m = \hat{\alpha} \times \frac{\text{7 to 12 years old population in 1985}}{\text{7 to 15 years old population in 1985}}$). This variable will be used as a measure of treatment intensity in a event-study framework. At the municipality-year level, I estimate models of the following form:

$$Y_{m,t} = \sum_{t \neq 1985} [\pi_t Shock_m] + \sum_{t \neq 1985} [X'_{m,\bar{t}}\phi_t] + \alpha_1 \Delta Pop_{m,t} + \alpha_2 Sh.715_{m,t} + \gamma_m + \delta_t + \vartheta_{ct,t} + \epsilon_{m,t} \quad (3)$$

where $X_{k,m,\bar{t}}$ is matrix of pre-reform yearly averaged variables in municipality m, which are supposed to control for all criteria that may influence education spending, while $\Delta Pop_{m,t}$ is a pre-reform predicted annual change in the share of demographic groups, $Sh.715_{m,t}$ is the share of children aged between 7 and 15 of the municipality m in year t, and γ_m, δ_t and $\vartheta_{ct,t}$ are municipal, year and county-by-year fixed effects, which control for any change in the same region.

By non-parametrically tracing out the full adjustment path of the treatment effect via equation (1), I am able to examine the reform gradual implementation. As discussed in subsection 2.3, the variation in the underlying criteria does not lead to immediate treatment impact. Adding some structure allows to find aggregate effects over a combination of years, for which I will use the following specification:

$$Y_{m,t} = \pi_0(Shock_m \times I(82-84)) + \pi_1(Shock_m \times I(86-88)) + \pi_2(Shock_m \times I(89-91)) + \sum_{t \neq 1985} [X'_{m,\bar{t}}\phi_t] + \alpha_1 \Delta Pop_{m,t} + \alpha_2 Sh.715_{m,t} + \gamma_m + \delta_t + \vartheta_{ct,t} + \epsilon_{m,t} \quad (4)$$

where π_1 and π_2 express the level changes in the grouped years of 1986-88 and 1989-91, respectively. Both will be reflect the reform effects.

The main assumption underlying the identification approach is similar to that in all event-study analyses: that all trends across municipalities, controlling for introduced covariates and fixed effects, would have remained unchanged in relation to the share of 7-12 year old children (out of 7-15 year olds) after the reform, had it not happened. Therefore, this relative time parameter should be flat and not statistically significantly different from zero in the pre-reform period. In addition to the parallel trend assumption, the validity of the results requires that the reform does not coincide with any shocks or policies that might influence post-reform outcomes.

As controls, I use the 1982-85 average Share of Tax Revenue (as a proportion of all revenues) and the 1980-85 average Share of Education Expenditure (as a proportion of all expenditures), which were part of the criteria for pre-reform grant distribution, both interacted with each year. Since there is a concern that the new rules would also change other sources of central administration funding, controls for Health Sector Matrix Points will also be included, which were part of the criteria for post-reform grant distribution. Since rural and central municipalities have significantly different contexts that might not be perfectly captured by covariates, there will also be fixed effects on dummies identifying the level of centrality[5] interacted with year.

3.3.3 Individual-level analysis

I develop a similar design for individual outcomes, but replacing year by year of birth (cohort fixed effects, c). Also, I will use cohort groups (g) interacted with the expected shock, as shown in equation 5.

$$Y_i = \sum_{g=-1}^{2} [\pi_g Shock_m] + \sum_{c \neq 1970} [X'_i \phi_c] + \gamma_m + \delta_c + \vartheta_{ct,c} + \epsilon_i \quad (5)$$

In addition to the municipal controls and fixed effects discussed earlier, the individual-level analysis will also include gender and foreigner[6] dummies, as well as the educational level of the individual's mother and father and within family birth order, since Black et al.

[5] Centrality refers to a municipality's geographical location in relation to towns of different sizes, with 7 levels. It was measured in 1980 by the Norwegian Statistics Bureau.

[6] Foreigners are categorized into Nordic (born in Sweden, Denmark, Finland, or Iceland) and others

[2011] find a strong and significant effect of birth order on IQ. Since Table 4 shows clear trends in parental educational level and the share of foreigners across cohorts, those controls will be interacted with the year of birth.

The variable $Shock_m$ will be calculated based on the municipality the individual was in 1985, one year prior to the reform. This means that the coefficients will measure an intention-to-treat effect, since not all students lived in the same municipality in the following years (90% in 1988 and 86% in 1991). Full treatment effects would likely be biased upward, since more concerned parents may sort their location based on where education spending or quality is increasing [Nechyba, 2006; Caetano, 2019]. This hypothesis will also be tested.

Other parental responses to the shock may also occur in terms of their own efforts to enhance their children's human capital accumulation. However, the evidence on the sign and magnitude of this response is mixed. While Houtenville and Conway [2008] finds suggestive evidence of a reduction in parental effort in relation to school inputs, Datar and Mason [2008] shows very small effects (3-7% of the standard deviation), with no impact on students' achievement. Finally, Bonesrønning [2004] found no conclusive evidence of parental effort responses to different class sizes, although there is some indication that parents tend to reduce their efforts as class size increases (a complementary response). The Norwegian context of heavily publicly-funded education and low income inequality is indicative of a potentially low magnitude and impact of parental responses on the effort margin.

Finally, I test an alternative structure on the analysis, by interacting the school funding shock with continuous variables of years of exposure and age. This way, instead of simply pooling the cohorts more and less exposed, I examine how the effects of the shock vary depending on the length of time the cohort was exposed and their age at the time of exposure. This approach helps to address the concerns from pooling the cohorts. Also, it provides additional information about the nature of the effects of the school funding shock.

I specify the interaction terms in the equation 6. I include an interaction between the school funding shock and years of exposure, and then adding another interaction with age. I estimate the the effects on earnings and years of study.

$$Y_i = \pi_1 Shock_m \times Exposure_i + \pi_2 Shock_m \times Exposure_i \times \text{Age in } 1985_i + \sum_{c \neq 1970} [X_i' \phi_c] + \gamma_m + \delta_c + \vartheta_{ct,c} + \epsilon_i \quad (6)$$

where $Exposure_i$ is the number of years for which students were school-aged between 1986 and 1991 and Age in 1985_i is, naturally, how old students were by the end of the year of 1985. π_1 are π_2 the coefficients of interest.

Although this model does not test for pre-trends, it provides additional robustness on previous results. Additionally, it enlightens whether there are any age- or length-specific effects, that could explain the results obtained from pooling the cohorts.

4 Results

4.1 Transfer amount size

Table 5 shows the model results to assess the determinants of the education transfer amount to municipalities. All coefficients are estimated in terms of 2011 PPP dollars per pupil, with all fixed effects discussed in subsection 3.3.1.

Table 5: Education transfer level per pupil between 1982 and 1985

VARIABLES	Education Grant ($)
Teachers per Pupil	4,531***
	(1,426)
Share of 7-12 years old children (over 7-15)	-858.1**
	(428.4)
Share of Education Expenditure (over whole budget)	234.1
	(259.0)
Municipal Per Capita Tax Revenues (ln)	-1,621***
	(334.8)
Share of 7-15 years old children (over population)	-16,448***
	(2,166)
Observations	1,590
R-squared	0.962

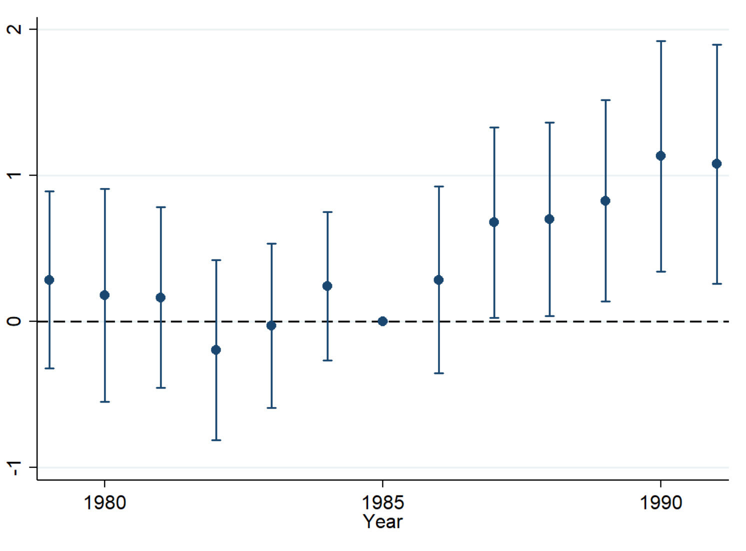
Notes: This figure shows the results from estimating Equation 1. Standard errors clustered by municipality in parentheses. Estimate levels are in 2011 PPP dollars' terms. Sample is 402 Norwegian municipalities that had the same borders throughout the period.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

The table shows that a 10p.p. higher share of primary school students is associated with drop of $85.8 on the transfers per pupil from the central administration to municipal provision of education. Given that this variable ranges mostly from 0.55 to 0.7, the relative shock size after 1986 is between $470 and $600 dollars per pupil, which corresponded to 7-9% of the average municipal budget to education. All results will be re-scaled to a $1000 difference in the transferred resources.

4.2 Municipal-level Results

Graph 3 shows the net per pupil education spending in log points response, each year, to an increase of $1000 in the intergovernmental transfer to education. Coefficients are mostly flat prior to the baseline year, indicating no pre-trend, but they do increase from 1986 on, being statistically significantly positive after 1987 This result is expected, due to the gradual implementation of the reform, discussed in subsection 2.3.

Figure 3: Effect of $ 1000 higher grant on Net Per Pupil Education Expenditure (ln)



Notes: This figure shows the results from estimating Equation 2. Dots represent the π_t estimates; bars represent 95% confidence intervals, clustered at the municipality level. Sample is 402 Norwegian municipalities in 1985 that had the same borders throughout the period.

Table 6 shows aggregate results for municipalities. There is evidence that municipalities use higher resources to increase teaching hours and building schools - or, alternatively, keeping them from shutting down, since average number of public schools dropped in that period. Interestingly, class size seems to be unchanged, meaning that more teaching hours resulted into more tutoring or extracurricular activities. School staff income was also unchanged, which is expected in a context of very rigid wage structure, as it happens to be in Norway [Balsvik et al., 2015].

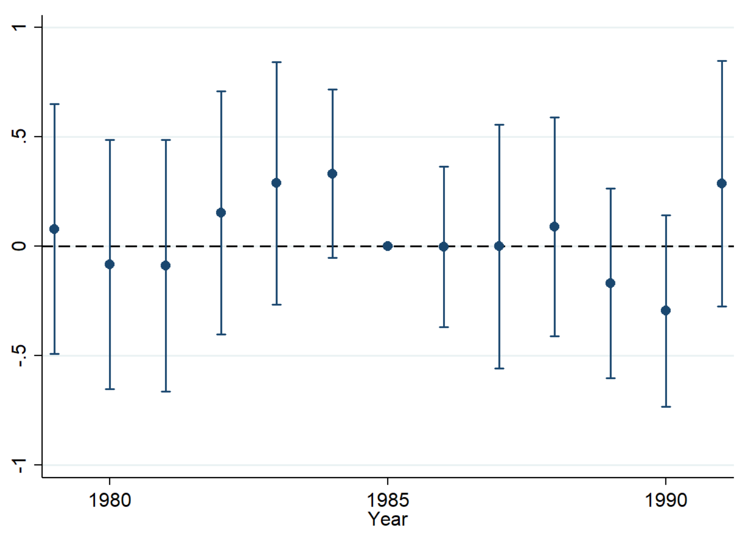
Table 6: Municipal-level regressions

VARIABLES	(1) Teaching Hours per Pupil	(2) Class Size	(3) School Staff Income	(4) Number of Schools
1982-84 x Shock	4.001	-1.685	0.130	0.339
	(4.632)	(2.087)	(0.231)	(0.512)
1986-88 x Shock	8.268*	2.727	0.0675	2.502**
	(4.278)	(3.074)	(0.327)	(1.247)
1989-91 x Shock	20.49***	-0.505	-0.187	4.692**
	(7.656)	(3.967)	(0.479)	(2.226)
Observations	3,215	4,774	3,214	4,774
R-squared	0.360	0.403	0.246	0.300
Number of Mun.	378	402	378	402

Notes: This figure shows the results from estimating Equation 3. Standard errors clustered by municipality in parentheses. Sample is 402 Norwegian municipalities that had the same borders throughout the period. Column (1) has 24 municipalities with missing data
*** p<0.01, ** p<0.05, * p<0.1

These results show that municipalities do not use additional funds to one purpose only. Revenues were channelled to both current and capital spending, not changing its composition, as Graph 4 shows. That means that this intervention diverge significantly from other ones explored in different studies in Norway in the recent literature, which focused on direct changes into school inputs [Leuven and Løkken, 2020; Borgen et al., 2022].

Figure 4: Effect on Share of Current Expenditure over Total Education Spending



Notes: This figure shows the results from estimating Equation 2 Dots represent the π_t estimates; bars represent 95% confidence intervals, clustered at the municipality level. Sample is 402 Norwegian municipalities in 1985 that had the same borders throughout the period.

4.3 Individual-level Results

As discussed in subsection 3.3.3, I test the likelihood of leaving the municipality in the following years to the reform across municipalities school additional funding. Graph 8, in the appendix, shows the regressions' point estimates and standard errors each year from 1986 to 1991. Indeed, students seem to have a lower probability of leaving municipalities receiving higher funding for education, especially in cohorts exposed at lower-secondary school. This result is in line with the literature [Gibbons and Silva, 2011; Fredriksson et al., 2016], where it has been found that parents tend to choose schools in relation to its perceived quality.

Table 7 shows regressions' results for employment and labor market income. Earnings are reported in two different forms: absolute yearly labor income (in 2011 PPP dollars) and labor income rank by cohort (year of birth).

Table 7: Individual-level regressions: Labor Market Outcomes

VARIABLES	(1) Labor Income	(2) Employment	(3) Income Rank by Cohort
Never Exposed	916.4	0.032	2,847
	(3,047)	(0.061)	(3,884)
Marginally Exposed	4,167	0.096*	5,738
	(3,201)	(0.052)	(3,612)
Exposed at Lower-	13,633***	0.051	11,017**
Secondary School	(4,382)	(0.061)	(4,274)
Exposed at Primary	12,430**	0.144**	13,144***
School	(5,343)	(0.068)	(4,420)
Observations	981,270	1,024,535	994,205
R-squared	0.262	0.009	0.215

Notes: This figure shows the results from estimating Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. *** p<0.01, ** p<0.05, * p<0.1

The interpretation of table above is that the effect is that \$1,000 of additional education resources during lower-secondary education led to a increase of yearly \$13,500 on earnings around the age of 33 and 35, which is also reflect in higher cohort labor income rank. For those exposed to the same shock during primary school, there are similar effects on earnings (around \$12,000, not statistically different from previous result), and a significant positive effect on employment and income rank.

In addition to examining the impact of increased education funding on labor market outcomes, it is also important to examine the effects on education outcomes, which can provide insight into the mechanisms through which education spending affects labor market outcomes. Table 8 shows regressions' results on education attainment in years of study, a dummy indicating whether it is higher than parents' and a dummy indicating the individual has college degree.

Table 8: Individual-level regressions: Education Outcomes

VARIABLES	(1) Years of Study	(2) More Education than Parents	(3) Higher Education
Never Exposed	-0.706	-0.171	-0.092
	(0.560)	(0.112)	(0.103)
Marginally Exposed	-0.134	-0.062	-0.036
	(0.518)	(0.089)	(0.102)
Exposed at Lower-Secondary School	0.470	0.062	0.118
	(0.648)	(0.109)	(0.121)
Exposed at Primary School	1.459*	0.219*	0.220
	(0.765)	(0.124)	(0.142)
Observations	1,023,285	1,023,285	1,024,535
R-squared	0.231	0.216	0.199

Notes: This figure shows the results from estimating Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. *** p<0.01, ** p<0.05, * p<0.1

There is a positive impact on educational attainment (about 1,5 year of study) for those exposed to the shock during primary school, and more likelihood of this group to have higher educational attainment than their parents (by 22 p.p.). One possible explanation for the stronger results on earnings relative to educational attainment is the sensitivity of continuous variables like earnings to changes or shocks. Also, since the educational system in Norway is highly accessible, there is a limited scope for further improvement through increased education spending.

In addition to examining the impacts on labor market and educational outcomes, it is also important to explore other potential mechanisms, such as migration. Previous studies have found that increasing human capital can lead to higher migration rates, as students seek out new opportunities and access to higher-quality education and job market opportunities in urban areas [Stark and Wang, 2002]. To test this mechanism, we will examine whether students exposed to the revenue shock for education are more likely to move to bigger cities later in life, shown in Table 9.

Table 9: Individual-level regressions: Migration Outcomes

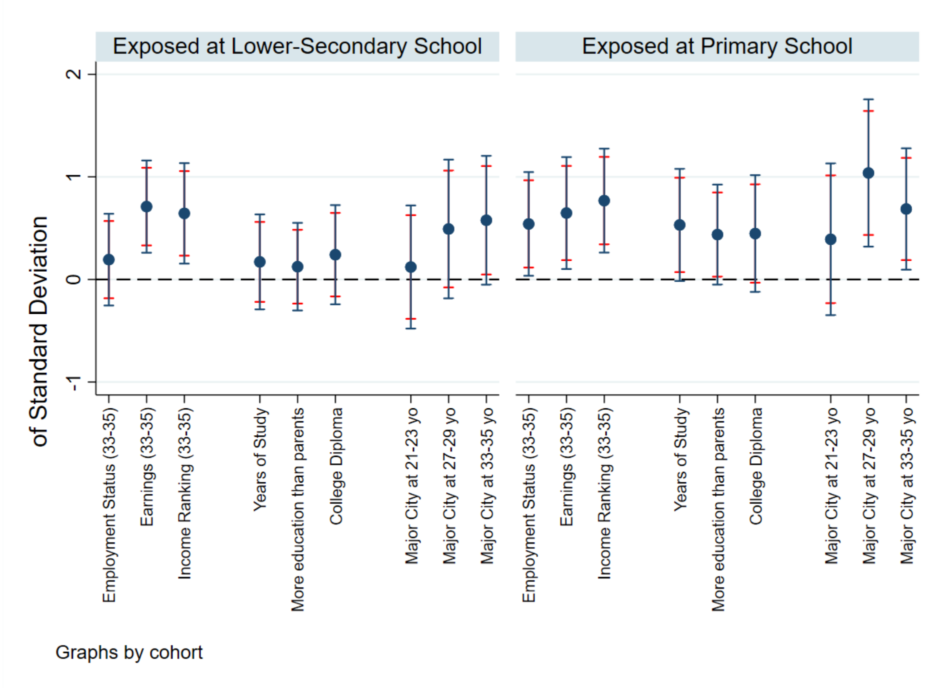
VARIABLES	(1) Large City at 21-23	(2) Large City at 27-29	(3) Large City at 33-35
Never Exposed	0.027	0.031	0.070
	(0.058)	(0.073)	(0.064)
Marginally Exposed	0.004	0.173*	0.138
	(0.063)	(0.089)	(0.086)
Exposed at Lower-Secondary School	0.027	0.177	0.195*
	(0.069)	(0.124)	(0.108)
Exposed at Primary School	0.089	0.373***	0.232**
	(0.085)	(0.131)	(0.102)
Observations	1,024,535	1,024,535	1,024,535
R-squared	0.042	0.110	0.096

Notes: This figure shows the results from estimating Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. *** p<0.01, ** p<0.05, * p<0.1

Results show only a small effect on the probability of living in a large city by the age of 21-23. On the other hand, migration to large cities at the ages of 27-29 and 33-35 years old show significant effects, especially for those exposed at primary school.

Graph 5 presents the results in terms of standard deviation. It shows that effects vary between 0.5 and 1 SD. Also, in general, impacts are higher for those exposed to the shock in Primary School, in line with Carneiro and Heckman [2003].

Figure 5: Effect of $ 1000 of Expected Education Revenue Shock on Main Variables, in Terms of Standard Deviation



Notes: This figure shows the results from Table 7 to 9. Dots represent the π_g estimates in terms of Standard Deviation; bars represent both 90% and 95% confidence intervals, clustered at the municipality level. Sample is individuals born between 1964 through 1983 who resided in a Norwegian municipality in 1985. Earnings outliers excluded.

Finally, I estimate regressions from equation 6. Table 10 presents the estimates interacting the municipal shock with length of exposure (based on expected compulsory school age) and age at the year of 1985.

Table 10: Individual-level regressions: by years of exposure and age

VARIABLES	(1) Labor Income	(2) Years of Study	(3) College Diploma
Shock ×Years of Exposure	4,341***	0.641***	0.105***
	(1,590)	(0.199)	(0.035)
Shock ×Years of Exposure×	-319.9*	-0.063***	-0.010***
Age at 1985	(170.1)	(0.021)	(0.004)
Observations	981,270	1,023,285	1,024,535
R-squared	0.262	0.231	0.198

Notes: This figure shows the results from estimating Equation 6. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. *** p<0.01, ** p<0.05, * p<0.1

The results reveal a consistent pattern of effects across both earnings and education attainment. Table indicates that the effect of the school funding shock on both outcomes is increasing with time of exposure and decreasing with age at the time of the shock. On average, an additional year of exposure leads to higher earnings by around $4,300, but about $320 lower for students one year older on average. On educational attainment, and additional year of exposure leads to higher 0.64 years of study, but 0.06 lower for students one year older on average, while the probability of holding a college diploma increases about 0.11 by year of exposure, but 0.01 lower according to age of exposure. Estimates are all statistically significant, reinforcing the findings of the main analysis.

The estimates in the literature, documented by Jackson and Mackevicius [2023], assume $1000 increase in per-pupil school spending sustained over four years, finding an average effect of 0.0539 over educational attainment (probability of college degree), ranging between 0.05 and 0.5 ninety percent of the time. My estimates indicate that this impact largely varies according to the age of exposure. Those exposed to the shock in the first four degrees (ages between 7 and 10) increase their probability of getting a college diploma by 0.171. However, for those exposed in the last four degrees (11 to 15 years old), the effect is as small as 0.003, and not statistically significant.

On earnings, on the other hand, those exposed to the shock in the first four degrees (ages between 7 and 10) increase their labor income by $9690 at the age of 33-35. However, for those exposed in the last four degrees (11 to 15 years old), the effect is as small as $3290, and not statistically significant. For it to be significant at the 95% level, the student should be exposed at maximum at 5th degree to 8th (ages 10 to 14), increasing their earnings by $4570 at age of 33-35.

4.4 Heterogeneity Analysis

It is important to understand whether the effects of additional resources to education observed in the previous section were experienced equally by all types of students. Since the recent literature has identified a more prominent role of school investments on low-SES students [Dearden et al., 2002; Heinesen and Graversen, 2005; Belmonte et al., 2020], I split the sample by parental educational level, with a subsample of children with parents with no higher secondary education degree and another with at least one parent holding it. Table 11 shows the results.

Table 11: Results by Parental Education

VARIABLES	Years of Study		Labor Income	
	(1)	(2)	(3)	(4)
Never Exposed	-0.442	-1.537	2,355	-2,567
	(0.646)	(1.097)	(3,536)	(6,222)
Marginally Exposed	-0.251	-0.045	2,700	7,053
	(0.640)	(0.905)	(3,570)	(6,238)
Exposed in Lower-Secondary School	0.418	0.342	10,087*	15,624**
	(0.802)	(1.044)	(5,374)	(6,290)
Exposed in Primary School	1.806*	0.975	22,373***	4,508
	(0.951)	(1.127)	(6,749)	(7,798)
Observations	524,678	498,607	508,233	473,037
R-squared	0.095	0.169	0.256	0.217
Parental Education	Low	High	Low	High

Notes: This figure shows the results from estimating Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Earnings outliers excluded. Groups defined by upper-secondary school attainment. *** p<0.01, ** p<0.05, * p<0.1

As one may see, almost all results are driven by low education parents' students, including on years of study, which are significant for those exposed to the shock in primary school. For those, earnings also increase on about yearly $22,000, the largest effect that was found, over 1 standard deviation in size.

Further investigating the school funding increase distributional impacts, I also use of quantile regressions, based on Machado and Silva [2019], to examine the effects of the policy across different points of labor income distribution. Table 12 shows results by 5 quantile points, varying from 0.1 to 0.9.

Table 12: Quantile regressions on Earnings

Quantile	(1) 0.1	(2) 0.25	(3) 0.5	(4) 0.75	(5) 0.9
Never Exposed	6,109	2,935	794.8	-1,176	-3,358
	(6,448)	(3,749)	(2,994)	(3,725)	(5,481)
Marginally Exposed	9,033	6,059	4,053	2,207	161.8
	(7,079)	(4,116)	(3,287)	(4,089)	(6,018)
Exposed at Lower-	17,718**	15,221***	13,537***	11,987**	10,270
Secondary School	(8,765)	(5,096)	(4,070)	(5,063)	(7,451)
Exposed at Primary	21,685**	16,028***	12,213***	8,699	4,809
School	(9,512)	(5,531)	(4,417)	(5,495)	(8,086)
Baseline	5,026.10	18,579.21	30,057.26	42,216.26	56,020.91
Observations	981,270	981,270	981,270	981,270	981,270

Notes: This figure shows the results from estimating Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Earnings outliers excluded.
*** p<0.01, ** p<0.05, * p<0.1

Results show that estimates are consistently higher and more significant on lower points of the distribution, and not significant at all at the quantile 0.9. The higher point estimate is found on quantile 0.1, showing an increase of over $20 thousands on earnings for those exposed at primary school. Therefore, patterns indicate that increasing school funding had a equality-enhancing effect on earnings decades latter, suggesting a stronger impact for low-skilled workers.

Finally, as discussed in the introduction, Cascio et al. [2013] finds that municipalities that are more able to crowd-out to higher external education funding, lowering their own investments, show lower or no actual increases on school spending, not improving students outcomes. I investigate that hypothesis splitting the sample by centrality, from rural to central[7]. Rural municipalities spend the most on education per pupil, but, having lower per capita tax revenues (over 10p.p. lower share of total revenues), they show low capacity to use their own funds for different purposes, since they are more depending on central administration grants.

Table 13: Education and Economic Characteristics by Municipality Type in 1985

	Per Pupil Educ. Spending	Share of Tax Revenues	Share of Educ. Federal Funding
Rural	6890.9	.414	0.525
Neither	5452.2	.538	0.426
Central	6031.0	.584	0.321

Obs: Expenditure in 2011 PPP dollars

In 1980, rural municipalities were 53% of total, while 17% were considered central. Table 14 shows the effects of the shock on the outcomes for which it was found an statistically significant impact on aggregate.

[7]Municipalities were grouped into 3, from rural (levels 1 and 2) to central (level 7)

Table 14: Results by Municipality Type - Municipal-Level

VARIABLES	Educ. Spending (ln)			Teaching hours Per Pupil			Public Schools		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1982-84 x	-0.0725	-0.169	-0.349	6.670	-2.507	-8.805	0.449	-2.263	3.538
Shock	(0.170)	(0.325)	(0.461)	(5.685)	(3.042)	(7.191)	(0.565)	(1.634)	(3.614)
1986-88 x	0.381	-0.110	1.695*	10.03*	4.534	2.421	3.238**	-4.524	2.493
Shock	(0.289)	(0.537)	(1.008)	(5.328)	(4.732)	(6.065)	(1.367)	(4.248)	(4.209)
1989-91 x	0.902**	0.180	1.631	23.10**	4.381	-12.19	4.713**	3.803	4.243
Shock	(0.366)	(0.662)	(1.298)	(9.570)	(7.468)	(12.81)	(2.355)	(10.29)	(6.220)
Observations	2,703	1,218	883	1,716	874	625	2,683	1,204	887
R-squared	0.968	0.983	0.967	0.390	0.561	0.507	0.285	0.297	0.626
Number of Mun.	226	102	74	204	101	73	226	102	74
Group	Rural	Neither	Central	Rural	Neither	Central	Rural	Neither	Central

Notes: This figure shows the results from estimating Equation 4. Standard errors clustered by municipality in parentheses. Sample is 402 Norwegian municipalities that had the same borders throughout the period. Columns (4)-(6) have 24 municipalities with missing data. Groups defined by centrality measure from Statistics Norway, which varies from 1 to 7 (1-2: rural; 3-6: neither; 7: central). *** p<0.01, ** p<0.05, * p<0.1

The results are driven mostly by rural municipalities, for which there are significant effects on all outcomes. More central municipalities seem to crowd-out the additional resources.

Such as for the municipal-level regressions, I will split the individual-level sample by type of municipality the students were in 1985, from rural to central. Focusing on schooling and earnings, table 15 shows the results.

Table 15: Results by Municipality Type - Individual-Level

VARIABLES	Years of Study			Labor Income		
	(1)	(2)	(3)	(4)	(5)	(6)
Never Exposed	-1.134	0.918	0.573	2,429	418.4	7,077
	(0.772)	(1.365)	(1.300)	(3,764)	(7,317)	(8,473)
Marginally Exposed	-0.549	0.989	-0.789	5,881	4,279	-1,520
	(0.726)	(1.083)	(1.268)	(4,461)	(6,740)	(7,413)
Exposed at Lower-	-0.228	2.098	-1.510	12,472**	14,443*	12,021
Secondary School	(0.944)	(1.362)	(1.339)	(6,244)	(7,449)	(10,595)
Exposed at Primary	0.741	2.317	0.175	12,118*	11,273	9,896
School	(1.038)	(1.866)	(1.644)	(6,772)	(10,378)	(14,491)
Observations	227,265	315,740	480,280	219,650	303,681	457,939
R-squared	0.204	0.216	0.253	0.295	0.267	0.247
Group	Rural	Neither	Central	Rural	Neither	Central

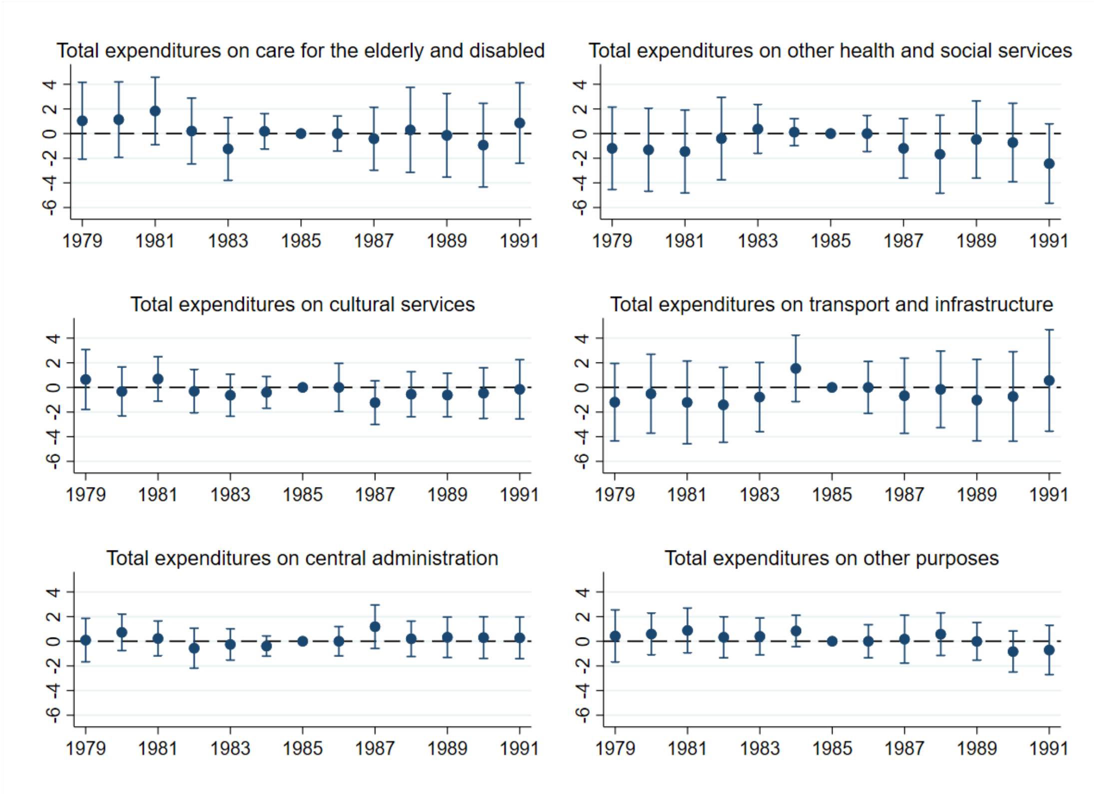
Notes: This figure shows the results from estimating Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Earnings outliers excluded. Groups defined by centrality measure from Statistics Norway, which varies from 1 to 7 (1-2: rural; 3-6: neither; 7: central). *** p<0.01, ** p<0.05, * p<0.1

The results show more consistently significant effects on earnings for those who were in rural municipalities. Considering that this subsample has the smallest sample size (around 220,000), the results suggest that they are driven by the policies which took place only in those municipalities, as shown in the previous section.

5 Robustness Checks

In the municipal-level analysis, I find that municipalities with higher share of primary school aged children in 1985 experienced higher expenditure on education after that year. However, that shock might correlated with an increases in other sectors' spending. That would mean that individual-level analysis could be due to other types of policies. Figure 6 shows the same regression in Graph 3 applied to all other big sectors presented 'Strukturall for kommunenes økonomi' documents.

Figure 6: Effect of \$ 1000 higher grant on big sectors' per capita expenditure (ln)



Notes: This figure shows the results from estimating Equation 2. Dots represent the π_t estimates; bars represent 95% confidence intervals, clustered at the municipality level. Sample is 402 Norwegian municipalities in 1985 that had the same borders throughout the period.

The Graphs show no impact of the shock on any other big sector. Central administration school funding, thus, was indeed channeled into education by municipalities.

One of the main concerns in the individual-level analysis, however, is the structure imposed to regressions when pooling cohorts. When pooling cohorts more and less exposed to a school funding shock, I increase the power of the analysis, as a larger sample size are required for saturated models, such as presented in section 3.3. However, by doing so, a certain level of structure is imposed in the analysis, making an assumption on the relationship between the cohorts and the school funding shock.

Thus, in this section, I will pool only two years of birth cohorts, reducing the level of imposed structure in the analysis, and allowing for a more nuanced examination of the impact of the shock. However, this also reduces the power of the analysis, as the smaller sample size results in less precise estimates of the impact of the shock.

It is important to consider the trade-off between having a smaller sample size and the degree to which the cohort was exposed to the school funding shock. While smaller sample sizes mean less power to detect an effect, if the cohort was almost fully exposed to the shock and at a very young age, it is likely that the effect of the funding shock would be more pronounced in this group.

Table 16: Individual-level regressions: Different Cohort Groups

Years of Birth	Average Years of Exposure	Average age in 1986	(1) Labor Income	(2) Years of Study
(1965-1966)	0	20.5	3,179	-0.690
			(4,052)	(0.734)
(1967-1968)	0	18.5	-4,573	0.182
			(3,633)	(0.668)
(1971-1972)	1.5	14.5	1,930	0.285
			(4,166)	(0.631)
(1973-1974)	3.5	12.5	649.8	-0.699
			(4,291)	(0.680)
(1975-1976)	5.5	10.5	5,793	-0.502
			(5,343)	(0.753)
(1977-1978)	6	8.5	6,030	0.459
			(5,796)	(0.837)
(1979-1980)	5.5	6.5	19,238***	1.823**
			(6,481)	(0.868)
(1981-1982)	3.5	4.5	8,019	1.233
			(6,389)	(0.886)
Observations			886,382	924,508
R-squared			0.248	0.228

Notes: This figure shows the results from estimating Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is individuals born between 1965 and 1982 who resided in a Norwegian municipalities in 1985, which had not changed borders. *** p<0.01, ** p<0.05, * p<0.1

In this analysis, I find a significant effect in cohort born between 1979 and 1980. Despite the smaller sample size, this cohort was almost fully exposed to the school funding shock at a very young age, which likely contributed to the robustness of our results. This highlights the importance of considering the timing and intensity of exposure when evaluating the impact of school funding shocks. The smaller sample size of this cohort may have limited our overall power, but the high degree of exposure to the shock in this group allowed us to draw more confident conclusions about its effect. These findings contribute to the growing body of evidence on the importance of early childhood education and the long-term benefits of school funding interventions.

It is worth mentioning that I find significant effects on both earnings and years of study for the same cohort. This consistent pattern of results highlights the robustness of the findings and supports the conclusion that the school funding shock had a lasting impact on this cohort.

I also run some placebo tests, using the main specification of the shock effect on variables that should not be affected by it. I take advantage of military conscription register data at age 18–19 years old for the vast majority of Norwegian-born males. During the recruitment process, most young men had to take this test, called General Ability Test (GAT), to evaluate their suitability for military service. The GAT is based on three speeded tests of arithmetic (30 items), word similarities (54 items), and figures (36 items). About 6-9% of the 1977-81 cohorts didn't take the test due to various unrecorded reasons, such as severe physical or mental disabilities. The GAT is similar to the Wechsler IQ test and Raven Progressive Matrices test. Test-retest reliabilities were .84, .72, and .90 [Sundet et al., 1988]. Component scores were standardized, summed, and reported on a 1-9 'stanine' scale, where category 5 represents an average IQ of 100 and one stanine unit equals a difference of 7.5 IQ points.

Following convention, I calculate the IQ score from the aggregate stanine score given each conscript. Apart from the mathematics test changing to multiple-choice format in the beginning of the 1990s, both the test and the scoring norm were constant throughout the period.

Walhovd et al. [2016] shows that, even though general cognitive ability declines with age, there is a high stability in individuals' cognitive ability relative to their same-age peers. Ritchie and Tucker-Drob [2018], in a meta-analysis, finds that increasing schooling might have an effect on cognitive, but as small as 1 to 5 IQ points per additional year of education, which would not be captured by the GAT scale. Therefore, there should be no effect of the school funding shock within cohorts. I also test the effect of the shock on earnings controlling for either ability or ability and height - which should not affect the results.

The table reports the coefficients for each exposure level and their respective standard errors. The bottom part of the table includes information on the fixed effects used in each model and whether IQ and Height were included as control variables.

Table 17: Individual-level regressions: Placebo tests

VARIABLES	(1) IQ	(2) Height	(3) Earnings	(4) Earnings	(5) Earnings
Never Exposed	-4.960	2.317	-1,234	153.8	18.40
	(4.514)	(2.083)	(4,646)	(4,431)	(4,451)
Marginally Exposed	0.521	2.260	6,481	6,266	6,243
	(4.675)	(1.950)	(5,092)	(5,131)	(5,167)
Exposed at Lower-Secondary School	0.782	3.207	21,535***	20,960***	20,743***
	(4.527)	(2.395)	(6,929)	(6,896)	(6,860)
Exposed at Primary School	3.613	2.587	15,448**	14,352*	14,268*
	(5.181)	(2.383)	(7,652)	(7,541)	(7,542)
Fixed Effects					
Regular	Y	Y	Y	Y	Y
IQ	N	N	N	Y	Y
Height	N	N	N	N	Y
Observations	504,710	515,222	480,216	480,216	478,048
R-squared	0.167	0.085	0.185	0.218	0.221

Notes: This figure shows the results from estimating Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is male individuals born between 1964 and 1983 who took the GAT test and resided in a Norwegian municipalities in 1985, which had not changed borders. *** p<0.01, ** p<0.05, * p<0.1

Finally, in order to assess the robustness of our findings, I conduct a randomization test, as described in Stanberry [2013], in which I randomly shuffled the treatment assignment 100 times. The purpose of this test is to ensure that results were not driven by chance or by any systematic patterns in the treatment assignment.

Figure 9 reports point estimates distribution on earnings. Results are consistent with original findings, with most of the coefficients remaining around zero, and way below the actual treatment estimates.

6 Conclusion

Basic education is a sector that has seen increasing public spending in most countries, but the literature on the effects of quasi-exogenous interventions on its funding has been mostly limited to the United States, where it has relied on court-ordered changes in state funding formula. There has also been a gap in the literature on the effects of education spending on students' later labor outcomes.

This paper fills this gap by exploring the short and long-run effects of an intergovernmental transfers reform on municipalities' education funding, school inputs, and ultimately, students' outcomes in adulthood. Using a rich set of fixed effects and controls and an event-study design, relying on the pre-reform share of primary school students, the reform provided an exogenous shock to municipal education revenue, leading to a significant effect on schools' budgets. The student-level panel data allows for the examination of the effects of spending at each educational level on students' outcomes later in life, when they were up to 35 years old.

The results show that the additional resources for education led to an increase in municipal spending in that sector, driven mostly by rural municipalities, as other administrations seem to offset this intergovernmental transfer, which was also found in another Norwegian policy in the 2000s [Reiling et al., 2021]. The higher spending resulted in more teachers, teaching hours, and schools in those municipalities.

At the individual level, students living in municipalities that experienced a higher revenue shock for education showed higher mobility, higher educational attainment, and a considerable increase in earnings, between 0.5 and 1 standard deviation, with higher and more significant impacts on lower end in labor income distribution. These latter effects were concentrated among those living in rural municipalities and with under-educated parents, demonstrating the additional education funding's prominent role in benefiting low-SES students.

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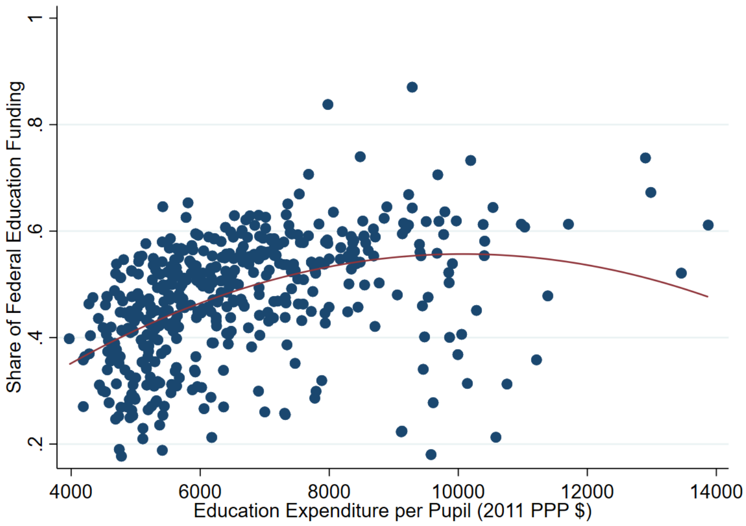
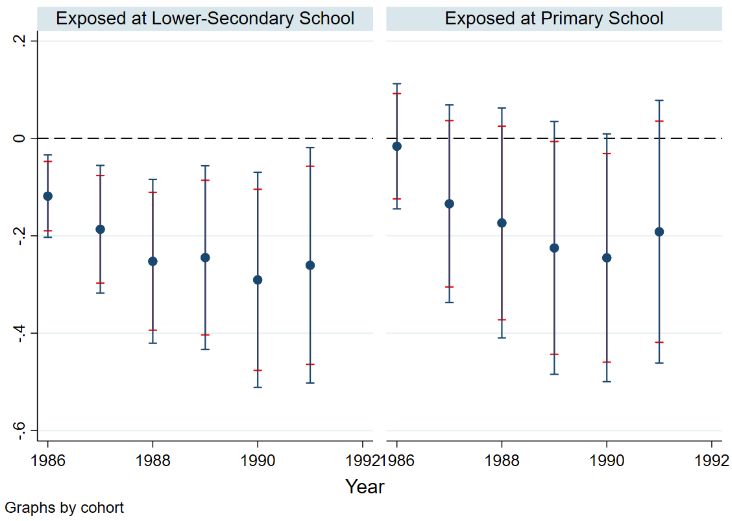
Appendices

Figure 7: Municipal education spending and central administration funding share in 1985



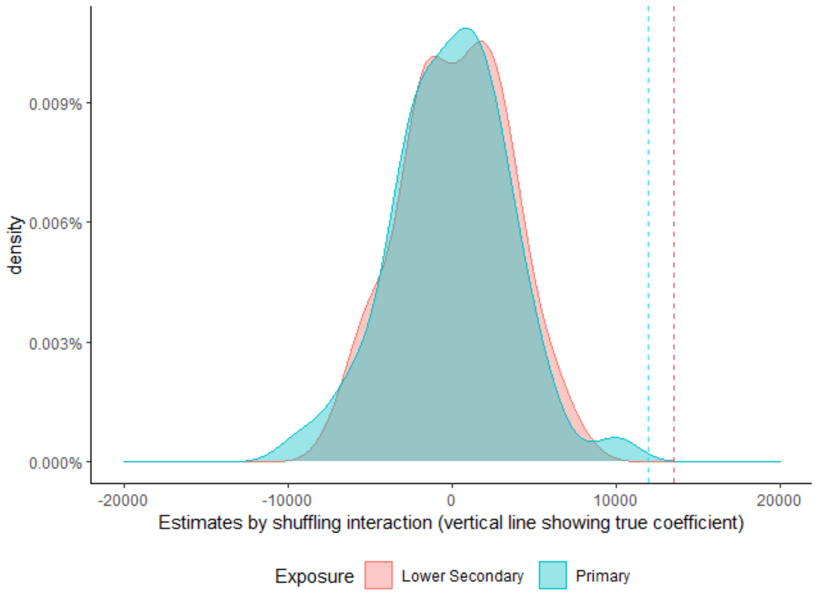
Sources: 1985 Strukturtall for kommunenes økonomi

Figure 8: Effect on the Probability of Leaving the Municipality, by year



Notes: This figure shows the results from estimating Equation 5. Dots represent the π_g estimates; bars represent both 90% and 95% confidence intervals, clustered at the municipality level. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipality in 1985, which had not changed borders.

Figure 9: Point Estimates on earnings in 100 regressions after treatment random shuffling



Notes: This figure shows the results from estimating Equation 4. Dots represent the π_c estimates; treatment variable was randomly shuffled at the municipality level. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders.