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

Daniel Duque
Norwegian School of Economics (NHH)

August 26, 2024

Abstract

This paper investigates a range of long-term effects of increasing school funding, leveraging an intergovernmental transfer reform in Norway, which shifted grant criteria for education at the local level in the 1980s, creating a quasi-random shock in spending on primary and lower secondary education. By integrating detailed municipal and individual data, it uniquely contributes to the literature evaluating the reform's impact on educational inputs and outcomes, including cognitive abilities, educational attainment, and labor market performance up to the age of 35. The additional funding led to more teachers and higher work hours among school employees, without affecting class size, indicating a shift towards more intensive instruction. At the individual level, estimated increases on education funding positively affected educational attainment and labor income, especially for those exposed to the funding increase at a younger age. The impact increases over time, and it is more pronounced for individuals from lower socioeconomic backgrounds, besides being higher and more significant at lower points of the income distribution, highlighting its potential to mitigate socioeconomic disparities. Results also show that around a third of the impact on earnings is mediated through improvements in educational level and specialization, besides marginal effects on IQ and migration by the mid-twenties.

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

1 Introduction

Education is a crucial factor for individual and societal advancement. It is linked to improved health, higher earnings, and higher social mobility. However, universal access to quality education remains a global challenge. A pivotal element in this pursuit is funding, which significantly influences school resources and therefore learning.

However, the question of how education funding impacts student outcomes has long intrigued researchers and policymakers. Yet, understanding the long-term effects, particularly on earnings, is challenging because of limited availability of data and exogenous shocks linked to students over a long period of time. I overcome these limitations by leveraging an intergovernmental transfer reform which took place during the mid-eights in Norway. Exploring a quasi-random shock on education funding, I present detailed evidence of long-term effects on student outcomes, by the time they are fully integrated in the labour market. By combining comprehensive municipal and individual data from Norwegian registers, I examine its impacts on individual outcomes such as educational attainment and earnings throughout adulthood, besides its distributional and heterogeneous effects.

Firstly, I document that a broad intergovernmental transfers' reform increased basic education 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, which enables me to examine the timing of the shock and its effect over time, I have collected decades of municipal-level data on education, allowing me to add a wide range of controls for confounding shocks, besides municipal, year and cohort-by-year fixed-effects. The core assumption is the conditional exogeneity of the assignment variable — specifically, the ratio of primary to compulsory school students prior to the reform.

My results can be grouped into municipal-level outcomes, to explore how municipalities used the additional funding, and individual-level outcomes, to show the long-run effects on students. I show that additional $100 dollars in education funding led to an increase in teacher-per-pupil ratio and on weekly per pupil gross hours of work among school employees, as well as an increase in the number of public schools. However, the funding did not affect class size, indicating that more teachers and school staff were, on average, destined to increase instructional hours intensity.

At the individual level, I take two approaches: flexible and linear specification. The flexible approach interacts with the same variable for estimated shock size with different cohort groups. It explores 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, allowing to check for potential pre-trends.

The linear specification approach, on the other hand, interacts the estimated shock size in the municipality of residence just prior to the reform with the length of the shock (based on the years each individual was within school age from 1986 and 1991) and the age at the shock. While it assumes a linear relationship between, it also enables to get an estimate of the shock effect size that is comparable to the literature.

Controlling for various individual characteristics, such as parental education interacted with year of birth, I find that an expected increase for education funding leads to higher labor income and higher educational attainment by the age of 33-35. The size of the effect is consistently lower for students exposed at an older age, which is in line with Carneiro and Heckman [2003]. The results are consistent both in the linear specification and the flexible. The flexible approach shows no effect on cohorts who were too old to be in primary or lower-secondary school by the time of the reform.

In the linear specification approach, the estimates show that an expected additional $ 100 dollars on school funding leads to an increase of yearly earnings of $ 91 by year of exposure, but around $ 6.5 lower by each year of older age. The same pattern is found for all variables: 0.012 higher years of study by year of exposure, lowering 0.0012 for each year of age; 0.002 higher likelihood of holding a college diploma by year of exposure, lowering 0.0002 by year of age. The size of those effects is similar to the range found by Jackson and Mackevicius [2023].

The flexible approach shows that an expected increase of $ 100 dollars on school funding leads to $290 higher yearly earnings, by the age of 33-35, for students exposed in primary school, with lower and less significant effect for those exposed in lower-secondary school. Also, only those exposed at primary school show significant effects on educational attainment by the age of 35, as high as around 0.025 years of study.

Quantile regressions suggest that the effects on earnings are higher and more significant at the lower end of the distribution, and the effects on both labor income and educational attainment are higher for children from lower socio-economic status parents. Those patterns are consistent with recent literature and indicate an equality-enhancing impact of the shock. The results also show an increasing effect on earnings over lifetime, starting out from non-significant by the age of 26-27, to a peak of significant effects between 32-33 and 34-35.

Results also show that the increase in earnings mediated by around a third through the effects on educational level and specialization. Besides, some marginal effects on IQ, but less consistent between flexible and linear specification approaches.

In addition, this study also explores the impact of increased school funding on migration patterns, addressing a potential challenge for local municipalities. This aspect of the research explores the trade-off in which, while education is typically a driver of economic growth, the migration of educated individuals in search of better opportunities elsewhere can counteract these benefits at the local level. This effect is especially pronounced in settings with significant regional disparities in economic opportunities. I investigate the longitudinal effects of the school funding shock on migration at various life stages—early adulthood (21-23 years), late twenties (27-29 years), and mid-thirties (33-35 years)—to explore how increasing human capital might influence migration decisions over time. The findings reveal subtle, particularly noticeable at the ages of 27-29. The results suggest that increased school funding has a modest but statistically significant impact on the likelihood of moving, especially among those who were in urban municipalities. This indicates that while educational enhancements can slightly prompt migration, the overall effect remains limited.

For robustness checks, I show that, at the local level, the funding did not increase spending in any other big sector, making it unlikely that the shock was correlated with any other policy other than on education. I also change the the cohort groups' structure, showing the relationship with each cohort with length of the shock and age by the time of the shock, validating that the shock is stronger for cohorts exposed for longer and at a younger age. I also narrow the age brackets and results keep consistently statistically significant, showing that the demographic composition associated with the shock is not a source of bias. Finally, I randomize the treatment in 100 simulations, which finds a range of estimates around zero, showing consistency with the prior set of results.

This paper 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; Jackson et al., 2021], 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 an exogenous variation of revenue limits on the annual increase in a school district's per-pupil funding in Wisconsin, separately for operational and capital expenditures, finding that increases in operational spending have substantial positive effects on test scores, dropout rates, and post-secondary enrollment, but additional capital expenditures have little impact. However, this paper is among the first to present a consistent set of long-term impacts of increasing school funding, meticulously tracking its influence from educational attainment through to cognitive abilities and earnings, offering a more complete understanding of the cascading effects of education investment. Also, the results of this paper show that the long-term effects on students is highly dependent on the age at which students are exposed to it, with sizes varying within the found in the literature, summarized by Jackson and Mackevicius [2023]. This insight is crucial for policymakers and educators in planning and timing interventions for maximum impact.

The existing literature on the effects of education spending, predominantly from the United States, emphasizes school funding formula reforms since the 1970s [Jackson and Mackevicius, 2023; Baron, 2022]. However, international evidence, often focused on capital spending or limited in quasi-experimental designs, suggests smaller effects on educational outcomes [Belmonte et al., 2020; Gibbons et al., 2017; Heinesen and Graversen, 2005]. This contrasts with the findings of this paper, which uncovers significantly larger effects of educational spending on student outcomes.

Norway, known for its high education expenditure and a decentralized yet regulated education system [OECD, 2020], provides a unique context for examining the impact of increased education funding. Despite expectations of lower impacts from additional funding in high-spending countries [Vegas and Coffin, 2015] and reduced returns to schooling [Becker, 2009], this study finds that increased educational funding in Norway, even within its unique labor market structure [Balsvik et al., 2015; Nilsen, 2020], leads to improved outcomes for students. This is particularly true for those from lower-education backgrounds or at the lower end of the distribution. The results demonstrate positive impacts on earnings across all students affected by the funding increase, with earlier exposure leading to higher benefits. These findings align with broader literature indicating the efficacy of early human capital interventions [Carneiro and Heckman, 2003].

This paper also adds to the literature examining local government responses to central government grants, offering insights into their impact on educational funding and outcomes. The literature have shown mixed results, ranging from significant crowding out to increased local spending and improved educational outcomes [Gordon, 2004; Cascio et al., 2013; Litschig and Morrison, 2013] observed positive effects on schooling, literacy rates, and poverty reduction with increased transfers in Brazil. Finally, the study also contributes to the debate on the effect of school inputs on learning and long-term outcomes. While most research focuses on class size, finding generally positive impacts [Angrist and Lavy, 1999; Fredriksson et al., 2013], evidence from Norway presents a mixed picture [Leuven and Løkken, 2020; Borgen et al., 2022]. This paper suggests that instead of direct policies targeting specific school inputs, increasing overall municipal education funding can be more effective. It reveals that municipalities tend to increase both current and capital spending without altering their composition, leading to beneficial 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.

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

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.

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.

[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.

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. 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:

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

where $Transfer_{m,t}$ represents the transfer amount to municipality m for grant size in year t, Cost $\text{Factor}_{l,t}$ represents the Cost Factor at the schooling level l in year t, $Hours_{l,m,t}$ 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, plus 10% for special education. 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 7-12) 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 1988, however, the previous year level was weighted 80%, and the new rules were fully incorporated in 1989.

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.

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.

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.

Increasing human capital through school funding may have many diverse effects on individuals, making it necessary to elaborate on the outcomes that are tested. 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 earnings, play a crucial role in individuals' well-being. Thus, it is important to primarily understand the effects of increasing education funding on those, showing also its distributional effects.

It is also important to examine the channels through which earnings increase, which can provide insight into the mechanisms through which education spending affects labor market outcomes. Higher human capital potentially converts to cognitive abilities [Ritchie and Tucker-Drob, 2018], but, by the 1980s and early 1990s, there is no available data on grades or cognitive/non-cognitive abilities for the whole population, as presented by Fredriksson et al. [2013]. Thus, 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 the 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.

To examine the impact of educational changes on earnings, this study will employ a predictive model focusing on the wages of individuals aged between 33 and 35. The model will integrate a combination of educational level and specialization categories, as defined by the Norwegian Central Statistical Bureau. Each category represents a combination of education and specialization. The wages will be compared to a baseline category representing only compulsory education.

The adapted Mincerian wage equation will include cohort and municipal fixed effects. The fixed effects will control for unobserved heterogeneity at both the cohort and municipal levels, allowing for a more precise estimation of the return to the combined education-specialization categories. The equation is specified as follows:

$$Earnings_i = \alpha + \sum_{i=1}^{K} \beta_k \cdot \text{Education-Specialization}_{k,i} + \delta_c + \mu_m + \epsilon_i \tag{2}$$

where $Earnings_i$ is the average yearly earnings for individuals aged 33 to 35, β_k represents the coefficient for the k-th Education-Specialization category, relative to the baseline of compulsory education, Education-Specialization$_{k,i}$ is vector of dummy variables that takes the value 1 if an individual i has the k-th Education-Specialization, and 0 otherwise.δ_c and μ_m represents cohort and municipal fixed effects, respectively, while ϵ_i is the error term.

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 2 shows the trends in some key variables.

Table 2: 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
1981	13897.4	0.152	0.659	7.69	10.96		18.67
1982	13010.8	0.150	0.651	7.71	10.79		18.54
1983	12519.7	0.148	0.646	7.72	10.62	4.38	18.43
1984	12297.3	0.144	0.637	7.68	10.31	4.71	18.24
1985	12525.1	0.140	0.632	7.65	9.99	4.90	18.18
1986	12213.2	0.136	0.627	7.61	9.36	5.29	17.70
1987	12394.7	0.133	0.627	7.60	8.90	5.59	17.40
1988	12128.0	0.129	0.633	7.59	8.53	5.91	17.17
1989	12226.2	0.125	0.642	7.50	8.41	6.23	17.11
1990	12249.5	0.122	0.653	7.43	8.18	6.40	16.92
1991	12523.0	0.120	0.658	7.40	7.75	6.49	16.87

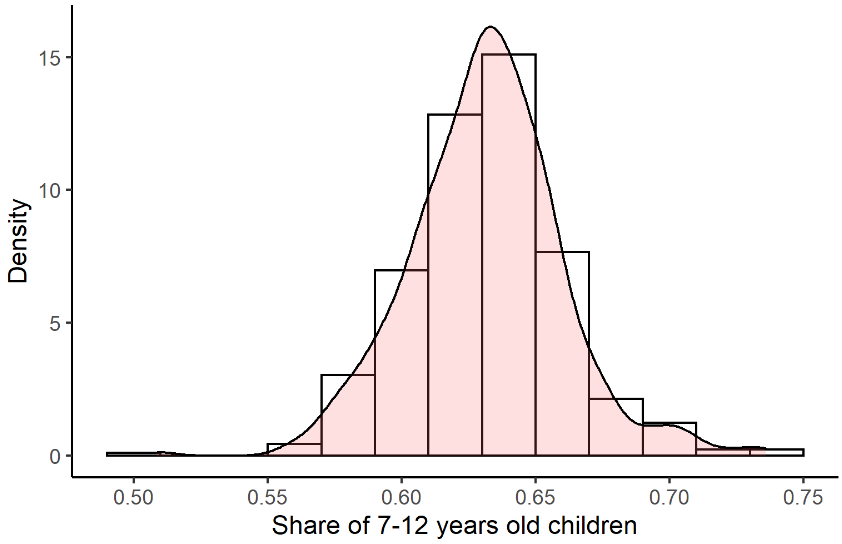
Notes: Expenditure values in 2011 PPP dollars. Teaching Hours Per Pupil Proxy defined as sum of contracted hours for employees in Primary and Lower-Secondary Schools.

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[3], the average number of public schools dropped after 1983.

Finally, figure 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 in 1985. It shows a distribution with no regional patterns. Most municipalities had a share between 55 and 70%, 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%.

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

Figure 1: 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 3 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 3: 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	18	19	20	21	22	23
1969		17	18	19	20	21	22
1970	[Baseline in Regressions]	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	Exposed at Primary School	7	8	9	10	11	12
1980		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.

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 Estimating Shock Size

Primary and lower-secondary school is compulsory in Norway, and grade retention is uncommon at those levels. Therefore, I leverage cross-municipality variation in pre-reform share of children aged between 7 and 12 years old over the total of 7-15 aged children and teenagers.

Estimating the shock size from the 1986 reform in Norway's educational grant system is achieved through the application of a detailed formula that captures the changes in the formula related to the students' demographics. This transition is quantified by comparing the pre- and post-reform scenarios, reflecting the shift in funding allocations across different levels of education - primary and lower-secondary. The formula is given by:

$$\begin{aligned} Shock_m &= SW \times \hat{CF} \times [(H_p \times sh712_m) + (H_s \times (1 - sh712_m))] \\ &\quad - [(SW \times H_p \times CF_{\text{primary}} \times sh712_m) + (SW \times H_s \times CF_{\text{secondary}} \times (1 - sh712_m))] \end{aligned}$$

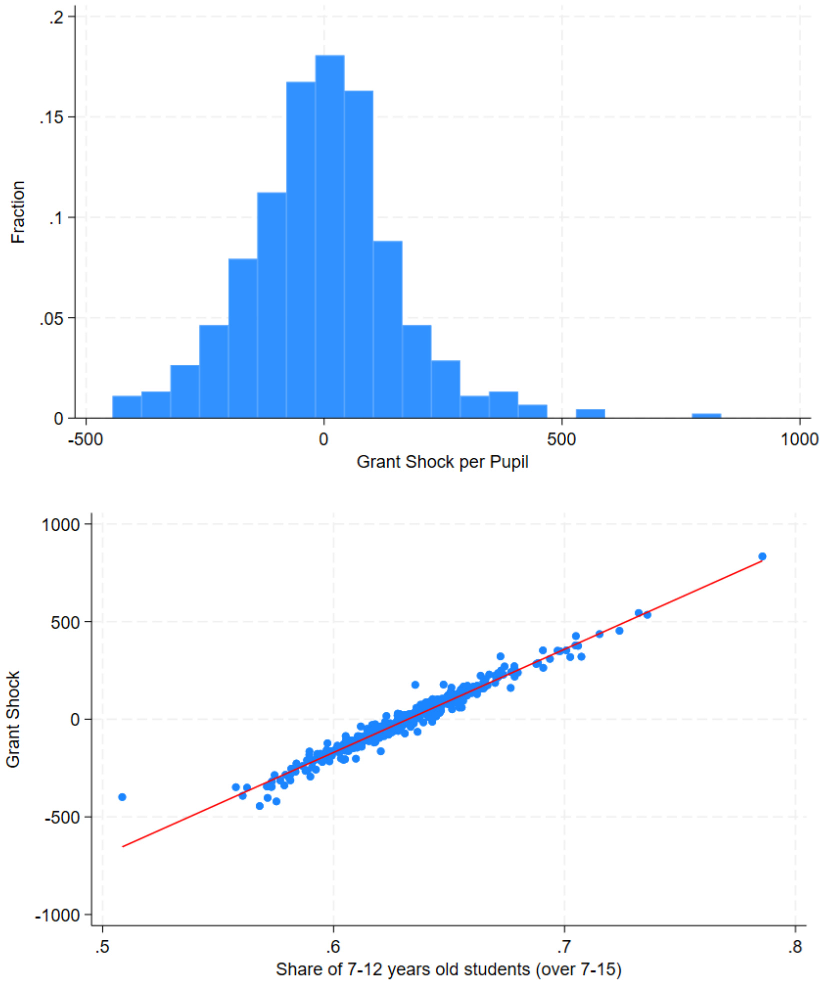
where SW is the number of school weeks per year, which reflects the annual duration of educational activities; H_p and H_s denote the weekly teaching hours for primary and lower-secondary education, respectively; CF_{primary} and $CF_{\text{secondary}}$ represent the pre-reform cost factors for each educational level, illustrating the financial parameters set by the central administration before the reform; $sh712_m$ denotes the share of students aged 7-12 in the total population of students aged 7-15 in a municipality m in the year of 1985; and, finally, $\hat{CF}$ is a simulated unified cost factor post-reform, under the to balance the aggregate grant in the year prior to the reform.

For practical application, the parameters are set as follows: the school year comprises 39 weeks; primary education involves 25.2 teaching hours per week, which was the maximum allowed for funding, while lower-secondary education involves 47.5 teaching hours per week. The pre-reform cost factors were $30.2 for primary and $34.1 for lower-secondary education, as it was the value set at 1985. Post-reform, a unified cost factor of $32 was established, aimed at maintaining the overall average spending per student across the nation. The resultant equation, incorporating these specific values, precisely quantifies the shock as the differential in grant funding attributable to the reform's implementation.

The introduction of the unified cost factor at $32 was strategically chosen to ensure that the total national grant change is zero, assuming no significant increase or decrease in overall spending. The calculated shock size reflects the redistribution of educational grants under the new rules, highlighting the differential impact on municipalities depending on their demographic composition, specifically the age distribution of their student populations.

Graphs 2 shows the estimates distribution of the education transfer amount to municipalities. All values are estimated in terms of 2011 PPP dollars per pupil.

Figure 2: Distribution of Shock Size Estimates



The histogram displays the distribution of grant shock per pupil, ranging mostly from -\$500 to \$500, with a standard deviation of \$137. The distribution is slightly right-skewed, with a few outliers receiving substantial increases. The scatter plot illustrates a positive correlation between the share of 7-12-year-old students (over the total of 7-15-year-olds) and the grant shock received by municipalities. The data points show a clear linear relationship: the proportion of younger students increases, so does the grant shock.

Given the range of the shock across municipalities, all estimates will be presented in terms of additional yearly \$ 100 per pupil (in 2011 PPP dollars). That value represents around 1% of the total expenditure in 1986. In order to compare the estimates to the literature, I will re-scale the main results to \$ 100 per pupil, but it is important to underscore that such procedure assumes a linear relationship between the grant shock and its impacts, which might not be reflective of actual dynamics that would be shown in the data.

3.3.2 Municipal-level Analysis

At the municipality-year level, I estimate models of the following form:

$$Y_{m,t} = \sum_{q=-1985} \pi_q(1[q = t]Shock_m) + \phi X_m^{'} + \gamma_m + \delta_t + \vartheta_{ct,t} + \epsilon_{m,t} \quad (3)$$

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:

$$Y_{m,t} = \beta_1(1[t \in 1986{-}88]Shock_m) + \beta_2(1[t \in 1989{-}91]Shock_m) + \phi X_m^{'} + \gamma_m + \delta_t + \vartheta_{ct,t} + \epsilon_{m,t} \quad (4)$$

where β_1 to β_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[4] 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

[4]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.

shock, as shown in equation 5.

$$Y_{i,g} = \sum_{q=-1}^{3} \pi_g (1[q = g] Shock_m) + \phi X'_{m,1985} + \alpha U'_i + \gamma_m + \delta_c + \vartheta_{ct,c} + \epsilon_{i,c} \tag{5}$$

In addition to the municipal controls and fixed effects discussed earlier, the individual-level analysis will also include gender and foreigner[5] dummies, as well as the educational level of the individual's mother and father and within family birth order, since Black et al. [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 is tested in the appendix.

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.

I also provide a linear approach to the analysis, by interacting the school funding shock with continuous variables of (potential) years of exposure. 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 to it. I specify the interaction terms in the equation 5. I include an interaction between the school funding shock and years of exposure.

$$Y_{i,c} = \pi Shock_m \cdot \text{Years of Exposure}_{i,c} + \phi X'_{m,1985} + \alpha U'_i + \gamma_m + \delta_c + \vartheta_{ct,c} + \epsilon_{i,c} \tag{6}$$

where Years of $\text{Exposure}_{i,c}$ is the number of years for which students were school-aged after 1986, which varies from 0 to 9. π is the coefficients of interest.

This model imposes a linear structure by interacting the school funding shock with a

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

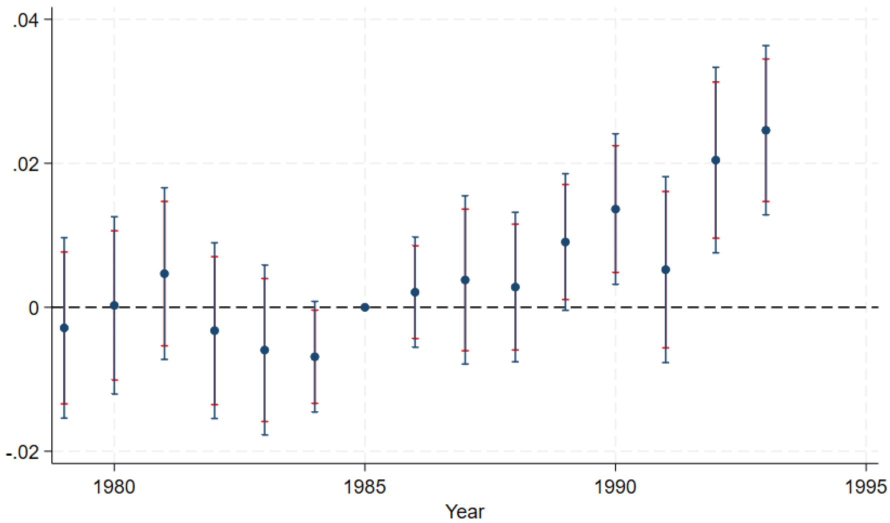
continuous variable representing the length of exposure. This approach allows the analysis to examine how the average effect size of the shock varies by each year of exposure. However, a limitation of this model is that it does not test for pre-existing trends or non-linear effects. Despite these limitations, the linear specification approach offers a valuable comparison with existing literature, allowing for an assessment of how the effects of increasing school funding in this case relate to previous findings.

4 Results

4.1 Municipal-level Results

Graph 3 shows the gross total education spending in log points response, each year, to an increase of $100 in the intergovernmental transfer to education. Coefficients are mostly flat prior to the baseline year, but they do increase from 1986 on, being statistically significantly positive after 1988 This result is expected, due to the gradual implementation of the reform, discussed in subsection 2.3.

Figure 3: Effect of $ 100 higher grant on Gross Total Education Expenditure (ln)



Notes: This figure shows the results from estimating Equation 3. Dots represent the π_t estimates; bars represent 90% and 95% confidence intervals, clustered at the municipality level. Sample is 402 Norwegian municipalities in 1985 that had the same borders throughout the period.

It is worth noticing that the effect of additional $ 100 per pupil are in between 1% and 2% increase on gross expenditure. The magnitude is in line with the shock size over the baseline average spending.

Table 5 shows results on school inputs. There is evidence that municipalities use higher resources to increase teaching hours, hiring teachers 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.

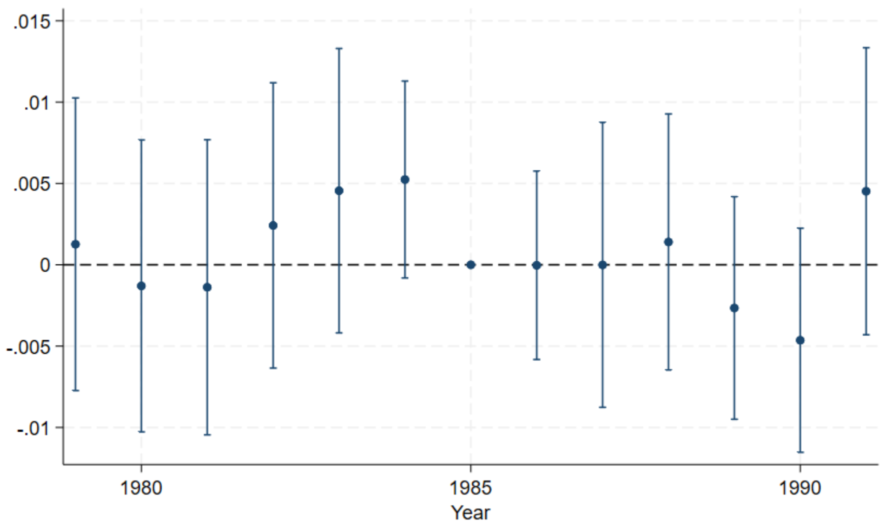
Table 5: Municipal-level regressions

Outcomes	(1) Teaching Hours per Pupil	(2) Teachers Per Pupil	(3) Class Size	(4) Teachers' Quality	(5) Teachers' Income	(6) Number of Schools
Short-Term	0.0737	0.0013***	0.0550	-0.00528	-0.000526	0.0285
(1986-88)	(0.0737)	(0.00047)	(0.0451)	(0.0169)	(0.00363)	(0.0186)
Medium-Term	0.262*	0.0015**	0.00450	0.00522	-0.00573	0.0541*
(1989-91)	(0.149)	(0.00063)	(0.0562)	(0.0262)	(0.00568)	(0.0325)
Observations	3,215	4,374	4,374	3,215	3,215	4,374
Pre-Treat. Mean	5.3	0.107	17.7	14.2	12.1	7.6
Number of Mun.	378	402	402	378	378	402

Notes: This table 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. 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 3 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.

4.2 Individual-level Results

Table 6 shows regressions' results for main educational and labor market outcomes. First, I provide the main flexible approach, and then the linear specification approach regressions, from equations 5 and 6 respectively. Earnings are reported in two different forms: absolute yearly labor income (in 2011 PPP dollars) and labor income rank by cohort (year of birth). In the appendix, Graph 7 shows the effect on earnings by year of birth, instead of cohort groups.

Table 6: Individual-level regressions

VARIABLES	(1) Years of Study	(2) Higher Education	(3) Labor Income	(4) Income Rank by Cohort
Flexible Approach				
Never Exposed	-0.011	-0.0015	31.64	.0007
	(0.009)	(0.002)	(61.04)	(.0010)
Marginally Exposed	-0.002	-0.0006	89.09	.0015*
	(0.00816)	(0.002)	(59.78)	(.0009)
Exposed at Lower-Secondary School	-0.0006	0.0004	144.97*	.0021*
	(0.011)	(0.002)	(84.15)	(.0012)
Exposed at Primary School	0.025**	0.0041*	290.15***	.0046***
	(0.011)	(0.002)	(88.07)	(.0012)
Linear Specification Approach				
Shock ·Years of Exposure	0.003**	0.0004**	27.32***	.0004***
	(0.001)	(0.0002)	(8.12)	(.0001)
9 Years of Exposure	0.027	0.004	245.88	0.0036
Pre-treatment Mean	12.99	0.328	31,168.1	.5
Pre-treatment SD	2.66	0.470	18,421.7	.29
Observations	1,023,285	1,024,535	981,306	994,205
R-squared	0.231	0.199	0.262	0.215

Notes: This table shows the results from estimating Equation 5 and 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 interpretation of the flexible approach is that the effect is that $100 of additional education resources during primary education led to a increase, around the age of 33 and 35, of 0.025 years of study and yearly $290 on earnings, which is also reflect in higher cohort labor income rank. For those exposed to the same shock during lower-secondary school, however, the estimate is considerably smaller and less significant - while for those who were only marginally exposed to the shock there is significant effect. The linear specification approach reveals a consistent pattern of effects across both earnings and education attainment. On average, an additional year of exposure of $ 100 per pupil higher funding leads to an increase of educational attainment by around 0.003. On earnings, and additional year of exposure leads to an increase of $ 27.3 on yearly earnings. Effects on college diploma and income rank by cohort are also found, by the same magnitude. Estimates are all statistically significant, reinforcing the findings of the main analysis.

The linear specification approach results are also in aligned, as expected, with the ones found in the flexible approach. By the estimates presented, nine years of exposure in primary school, would result into a sum increase of around $ 246 in earnings, similar in magnitude found in the flexible approach results. It is worth noting that, in terms of magnitude, the increase on earnings is around 1% of the pre-treatment mean - similar to the relative size of the shock, as stated in subsection 3.3.1 and showed in figure 3.

The estimates in the literature, documented by Jackson and Mackevicius [2023], assuming $1000 increase in per-pupil school spending sustained over four years, find 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, re-scaling the shock to $ 1,000, those exposed the first four degrees (ages between 7 and 10) would show an increase of their probability of getting a college diploma by 0.032. However, it is important to point out that, as stated in subsection 3.3.1, this conversion assumes a linear relationship between the size shock and its impact on the outcome, which may not be observed in the data.

4.3 Impact on earnings across distribution and age

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 7 shows results by 5 quantile points, varying from 0.1 to 0.9.

Table 7: Quantile regressions

	(1)	(2)	(3)	(4)	(5)
Quantiles	0.1	0.25	0.5	0.75	0.9
Flexible Approach					
Never Exposed	125.2	68.00	29.15	-6.018	-44.97
	(130.5)	(75.82)	(59.97)	(74.07)	(108.8)
Marginally Exposed	174.4	122.2	86.82	54.78	19.28
	(135.6)	(78.78)	(62.32)	(76.97)	(113.1)
Exposed in Lower-	182.1	159.4	144.0*	130.0	114.6
Secondary School	(171.3)	(99.55)	(78.74)	(97.26)	(142.9)
Exposed in Primary	456.8***	354.9***	285.7***	223.1**	153.7
School	(153.2)	(89.04)	(70.43)	(86.99)	(127.8)
Linear Specification Approach					
Shock ·Years of Exposure	33.75***	29.82***	27.15***	24.74***	22.06**
	(13.00)	(7.552)	(5.973)	(7.377)	(10.84)
Pre-Treatment Quantile	5,350.1	19,107.2	31,317.0	41,068.3	53,480.2
Observations	981,270	981,270	981,270	981,270	981,270

Notes: This table shows the results from estimating Equation 5 and 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. Earnings outliers excluded.
*** p<0.01, ** p<0.05, * p<0.1

The results show a positive and significant impact of the shock at lower quantiles, which diminishes and becomes non-significant as the distribution move to higher quantiles. The age interaction mirrors this trend with a negative effect, strongest and significant at the lowest quantile and waning at higher ones. These findings suggest that the educational funding shock had a varied impact on different aspects of student outcomes, with stronger effects observed at lower income, highlighting its potential for reducing inequalities.

The flexible approach also shows that estimates on earnings 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 $400 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.

Table 8, on the other hand, presents the estimated effects of an additional year exposed to the educational funding shock on earnings across different age groups.

Table 8: Earnings by age group

	(1)	(2)	(3)	(4)	(5)
Age Group	26-27	28-29	30-31	32-33	34-35
Flexible Approach					
Never Exposed	68.67	94.21*	45.75	18.38	36.14
	(53.90)	(48.98)	(57.33)	(61.04)	(63.24)
Marginally Exposed	-30.09	-15.29	36.78	62.14	112.0*
	(42.25)	(51.19)	(54.35)	(58.95)	(63.91)
Exposed at Lower-	-29.98	47.14	69.39	143.9*	154.4*
Secondary School	(56.25)	(64.91)	(73.01)	(77.09)	(86.47)
Exposed at Primary	46.66	171.6**	212.9***	263.3***	300.5***
School	(54.12)	(66.76)	(75.01)	(86.97)	(88.67)
Linear Specification Approach					
Shock ×Years of Exposure	0.276	11.83*	18.27***	26.71***	27.90***
	(5.502)	(6.055)	(6.710)	(7.935)	(8.142)
Pre-Treatment Mean	20,241.78	23,671.7	26,569.74	29,193.33	31,414.17
Observations	880,257	983,660	982,074	980,275	978,424

Notes: This table 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. Earnings outliers excluded. *** p<0.01, ** p<0.05, * p<0.1

The effects are, overall, significant and increase with age, suggesting cumulative benefits over time. For the youngest age group, the effect is positive but not statistically significant. The impact becomes statistically significant and larger at older ages.

The table indicates that the educational funding shock has a positive and increasingly significant impact on earnings as individuals age, with the magnitude of the effect peaking at ages 34-35. This highlights the importance of early educational investment for long-term earnings potential.

4.4 Channels

Table 9 shows regressions' results on IQ and Expected Earnings by Education Quality. Both flexible and linear specification approaches are shown.

Table 9: Potential Channels Outcomes

VARIABLES	(1) IQ (Men at 18-19)	(2) Education Quality
Flexible Approach		
Never Exposed	-0.0741	-44.39
	(0.0712)	(27.95)
Marginally Exposed	-0.00397	4.507
	(0.0735)	(24.74)
Exposed at Lower-Secondary School	-0.00273	-18.57
	(0.0802)	(31.49)
Exposed at Primary School	0.0787	72.01**
	(0.0746)	(32.04)
Linear Specification Approach		
Shock ×Years of Exposure	0.0435*	32.61***
	(0.0235)	(11.02)
R-squared	0.171	0.140
Observations	504,710	1,024,535

Notes: This table shows the results from estimating Equation 5 and 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

In the flexible section, for IQ, the coefficients are small and not statistically significant, suggesting little to no impact from the exposure levels. However, for education quality, the coefficients for those exposed at primary school show a significant positive impact 72.01, around 30% if the total effect on earnings.

The linear specification approach shows that an year of exposure to the shock has a positive effect for IQ, which is significant at the 10% level, and highly significant impact on Expected Earnings of $ 32.61, around 37% of the total effect on earnings.

On cognitive ability, Ritchie and Tucker-Drob [2018], in a meta-analysis, finds that increasing schooling has an effect on cognitive, but as small as 1 to 5 IQ points per additional year of education. Therefore, there should be low or no effect of the school funding shock within cohorts, given the size of the shock. The results show that, although the effects on cognitive abilities are small and imprecise, the channel of education, considering level and specialization, corresponds to around a third of the impact on earnings.

4.5 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 10 shows the results for the flexible and linear specification approaches.

Table 10: Results by Parental Education

VARIABLES	Years of Study		Labor Income	
Flexible Approach	(1)	(2)	(3)	(4)
Never Exposed	-0.007	-0.024	60.05	-38.88
	(0.010)	(0.017)	(71.33)	(124.5)
Marginally Exposed	-0.004	-0.0007	67.0	130.20
	(0.010)	(0.014)	(67.65)	(116.79)
Exposed in Lower-Secondary School	0.001	-0.005	95.90	124.61
	(0.0135)	(0.0173)	(103.95)	(123.40)
Exposed in Primary School	0.0028**	0.019	413.60***	169.95
	(0.014)	(0.0172)	(104.05)	(131.73)
Linear Specification Approach				
Shock ·Years of Exposure	0.003**	0.003	33.7***	17.87*
	(0.001)	(0.002)	(10.0)	(10.7)
Observations	524,678	498,607	508,233	473,037
Parental Education	Low	High	Low	High

Notes: This table shows the results from estimating Equation 5 and 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. 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. In the linear specification approach, an year of exposure the shock has a positive and significant effect on earnings for both groups, with the effect being slightly stronger and more significant for low educated parents. In the flexible approach, only students with low educated parents and exposed in primary school show a positive significant effect, which is higher than the estimate found for the whole sample.

The effects on educational attainment are highly significant and positive for students with low educated parents, whereas for the other group, the effect is positive but not statistically significant. In the flexible approach, similarly the previous outcome, only students with low educated parents and exposed in primary school show a positive significant effect, which is higher than the estimate found for the whole sample.

4.6 Migration Effects

Although school funding seem to have led to higher earnings in adulthood, the literature highlights that local educational investments have also effects on migration, which may be a partial drawback from a municipal perspective [Kline and Moretti, 2014; Shapiro, 2006]. Local governments often see education as a means to foster economic growth, but the migration of educated individuals seeking better opportunities elsewhere can undermine these benefits. This "brain drain" effect is particularly pronounced in settings where disparities in economic opportunities can be significant across regions.

Thus, I investigate the phenomena of "brain drain" by examining the longitudinal effects of school funding on migration across different life stages. Specifically, I focus on early adulthood (21-23 years), late twenties (27-29 years), and mid-thirties (33-35 years) to understand how increased educational opportunities influence migration decisions over time. The outcome will be either the probability of living in a different municipality from the one in 1985, or to be living in a big city[6]. Given that the lifetime migration choices differ in terms of length of time since living in a location, instead of using the estimated funding shock in the municipality a student was living by the year of 1985, I will use the municipality the student was living when she was six years old.

Table 11: Migration Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Living in Different Municipality			Living in a Big City		
Age	21-23	27-29	33-35	21-23	27-29	33-35
All Municipalities	0.000297	0.0006**	0.0005*	0.00002	0.0004**	0.0001
	(0.0002)	(0.0003)	(0.00024)	(0.0001)	(0.0002)	(0.00018)
Observations	986,962	986,962	986,962	986,962	986,962	986,962
Pre treat. Baseline	.2672	.4520	.5131	.1781	.2419	.2313
Rural Municipalities	0.0002	0.00054**	0.0004	0.0001	0.0004**	0.0001
	(0.00022)	(0.00026)	(0.00025)	(0.0001)	(0.0002)	(0.0002)
Observations	608,808	608,808	608,808	608,808	608,808	608,808
Pre treat. Baseline	.2702	.4931	.5518	.0561	.1456	.1477
Urban Municipalities	0.0011*	0.0011**	0.0017*	-0.0005*	0.0001	0.0007
	(0.0006)	(0.0005)	(0.0009)	(0.0003)	(0.0005)	(0.0005)
Observations	378,154	378,154	378,154	378,154	378,154	378,154
Pre treat. Baseline	.2626	.3873	.4524	.3696	.3931	.3624

Notes: This table 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 indicate small, but significant effects on the probability of moving, mostly

[6] Oslo, Bergen, Trondheim, and Stavanger

at the age of 27-29. This suggests that increased school funding have a limited influence on migration decisions - interestingly driven by students who lived in urban municipalities by the age of six. The small effect sizes might be attributed to cultural distinctions prevalent in Nordic countries, in which there is a widespread cultural expectation for children to become fairly independent and move out of their parental homes once they leave high school - inter-municipal migration is fairly strong, as table shows. This normative distinction may reduce the variability in migration behavior that might otherwise be influenced by differences in educational attainment or economic opportunity. As such, while an increase on human capital could be expected to drive migration in search of better opportunities in regions with stark economic disparities, in Norway, the uniformly early transition to independent living across all educational backgrounds could undermine any pronounced migration effects that would be more observable in other contexts.

5 Cost-benefit analysis

This analysis evaluates the cost-effectiveness of an educational spending policy that increases funding by \$100 per pupil annually from grades 1 to 9 (ages 7 to 15). I assess the impact of this policy on labor market outcomes from ages 28 to 60. The goal is to calculate the Internal Rate of Return (IRR), which is the maximum real discount rate at which the benefits, in terms of additional earnings, outweigh the costs.

The Cost can be expressed as a sum of discounted increases on school funding. Its present value is present below:

$$Cost = \sum_{t=7}^{15} \frac{100}{(1+r)^{t-6}} \tag{7}$$

Let ΔY represent the annual increase in earnings due to the policy, applied from age 28 to 60. The cost of the policy, C, is the total investment per pupil over the four years. The present value of the future earning increases ($Benefit$) can be expressed as:

$$Benefit = \sum_{t=28}^{60} \frac{\Delta Y}{(1+r)^{t-6}} \tag{8}$$

The policy is cost-effective if PV is greater than C, which is the present value. To find the IRR, I solve for r_{max} in the following inequality:

$$\sum_{t=28}^{60} \frac{\Delta Y}{(1+r_{max})^{t-6}} \geqslant \sum_{t=7}^{15} \frac{100}{(1+r_{max})^{t-6}} \tag{9}$$

This equation considers the discounted value of increased earnings over the working life of individuals affected by the policy, comparing it to the upfront investment to find the break-even discount rate. Considering Haider and Solon [2006] and Böhlmark and Lindquist [2006] findings that the association between the returns to schooling in lifetime and current earnings is strongest by the mid-30s, I use the estimate found for earnings between the ages of 33 and 35 in the linear specification model to find r_{max}.

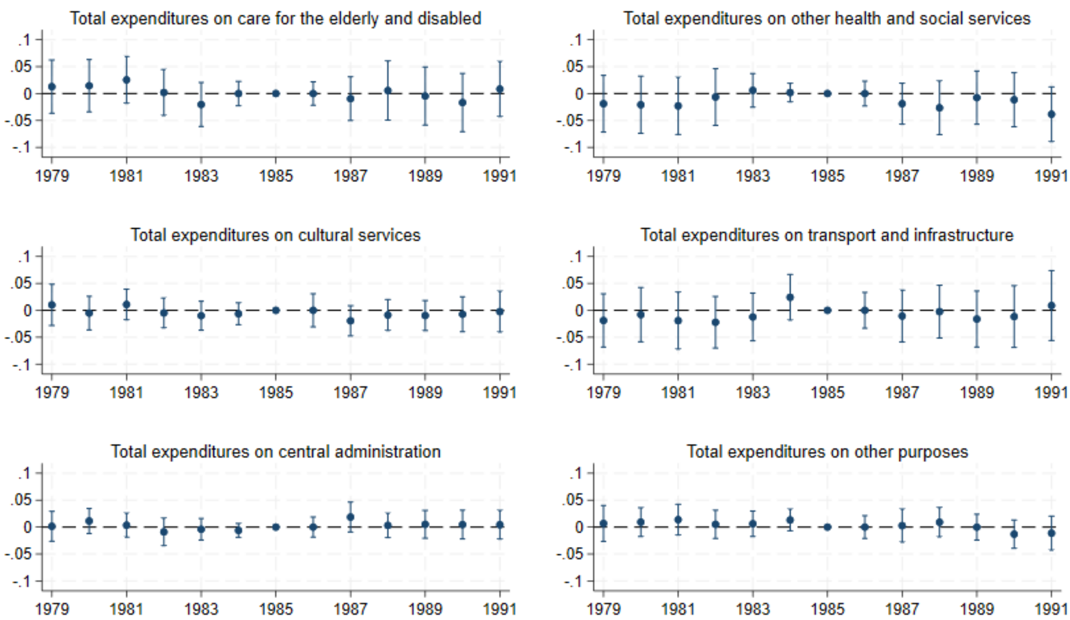
According to Table 6, nine years of exposure from the age of 7 to the shock will lead to an increase of $ 27.3 on yearly earnings. Considering this effect to be a the average shock over the life time, I calculate an IRR of 7.5% for the general population, with a lower bound of 4.5%. When considering a subpopulation of students with low-educated parents, the IRR increases to 8.5%. This indicates that the policy is particularly beneficial for this group, providing a higher return on investment. Those values are considerably higher than the ones used in the literature for cost-benefit analysis [Aakvik et al., 2005].

In summary, the updated cost-benefit analysis, using the IRR approach, validates the educational policy's effectiveness, especially for students from less advantaged backgrounds.

6 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 5 shows the same regression in Graph 3 applied to all other big sectors presented 'Strukturall for kommunenes økonomi' documents. 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.

Figure 5: Effect of $ 100 higher grant on big sectors' per capita expenditure (ln)



Notes: This figure shows the results from estimating Equation 3. 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.

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 12: 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	91.27	-0.011
			(83.39)	(0.012)
(1967-1968)	0	18.5	-50.86	0.003
			(72.99)	(0.011)
(1971-1972)	1.5	14.5	68.29	0.004
			(77.58)	(0.02)
(1973-1974)	3.5	12.5	47.54	-0.011
			(81.42)	(0.011)
(1975-1976)	5.5	10.5	124.18	-0.008
			(98.71)	(0.011)
(1977-1978)	7.5	8.5	130.29	0.007
			(103.67)	(0.013)
(1979-1980)	9	6.5	372.41***	0.029**
			(112.41)	(0.014)
(1981-1982)	9	4.5	154.79	0.019
			(109.86)	(0.013)
1983	9	3	296.98**	0.025
			(138.0)	(0.017)
Observations			927,761	967,991
R-squared			0.185	0.228

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 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.

Similarly, another concern relates to potential confounding variables associated with the demographics of the student population. The proportion of primary school-aged children (7-12 years) within the broader age group of primary and lower-secondary students (7-15 years) in each municipality serves as the main assignment variable in the analysis, however, this demographic characteristic may not be independent of other socio-economic or educational trends within the municipalities that could also influence children' outcomes over time.

To mitigate the risk of such confounding effects, a refined approach might involve narrowing the age range used to define the demographics of interest, as approached by Cabral et al. [2021]. Focusing on a more specific age cohort may potentially minimize the variability in external influences that are not directly related to the reform but are instead linked to broader age-related trends within the municipalities. This narrower demographic window could help approximate a more randomized exposure to the reform, ensuring to estimate the true effect of the reform from other concurrent developmental or policy shifts. Thus, this approach may strengthen the validity of the conclusions drawn about the reform's impact by reducing the potential overlap of unrelated socio-economic trends and educational strategies across different municipalities.

In order to further validate the results and ensure that they are indeed capturing the impact of the funding reform rather than reflecting underlying variables correlated with students' demographic composition, I implement the linear specification approach using three different demographic windows. The first is the current age range (7-12/7-15), which has already been discussed. Additionally, I will test two narrower age brackets: a six-year range (10-12/10-15) and a four-year range (11-12/11-14). By examining the effects across these varied age groups, the analysis aims to check for consistency in the impact of the funding reform. If the results remain statistically significant across all these demographic windows, it would strengthen the argument that the observed effects are indeed due to changes in funding, and not confounded by other demographic or socio-economic trends.

Table 13: Individual-level regressions: Different Age Brakets

Age brackets	(1) Years of Study	(2) Higher Education	(3) Labor Income	(4) Income Rank by Cohort
7-12 / 7-15	0.144**	0.0230**	1,487***	0.0221***
	(0.0601)	(0.0107)	(442.3)	(0.00612)
Observations	1,023,285	1,024,535	981,306	994,205
R-squared	0.231	0.199	0.193	0.184
10-12 / 11-15	0.131**	0.0215**	1,208***	0.0195***
	(0.0534)	(0.00951)	(412.7)	(0.00575)
Observations	1,023,285	1,024,535	981,306	994,205
R-squared	0.231	0.199	0.193	0.184
11-12 / 11-14	0.0976**	0.0140	1,119***	0.0191***
	(0.0467)	(0.00850)	(346.4)	(0.00485)
Observations	1,023,285	1,024,535	981,306	994,205
R-squared	0.231	0.199	0.193	0.184

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 empirical analysis across different demographic windows reveals a consistently positive and statistically significant impact of the educational reform on various socio-economic outcomes, regardless of the age bracket considered. This pattern underscores the robustness of the reform's effects, as even when the demographic window is narrowed—from the broader group of 7-12 years down to more focused groups, the estimated impacts remain positive. This consistency in outcomes across age groups strengthens the argument that the observed benefits are indeed attributable to the educational reform rather than external demographic or socio-economic factors. The findings suggest that narrower age brackets, while showing a natural decline in the magnitude of effects due to their different base, still significantly benefit from the reform. This consistency across different groups provides compelling evidence that the reform has broadly facilitated improvements in educational and economic parameters, reinforcing the effectiveness of targeted educational investments.

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 8 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.

7 Conclusion

This study provides a comprehensive analysis of the long-term impacts of increased education funding on student outcomes, leveraging a significant intergovernmental transfer reform in Norway in the mid-eighties. The research reveals that additional funding led to notable improvements in educational resources at the local level, such as higher instructional hours. However, the effects on school staff income and class size were minimal, suggesting a strategic allocation towards enhancing instructional intensity.

At the individual level, the study employs both a flexible and linear specification approach to assess the impact of increased funding on cognitive abilities, educational attainment, and labor market outcomes. The findings demonstrate that increased funding leads to higher labor income, greater educational attainment, and improved cognitive abilities, particularly for students exposed to the funding at a younger age. These effects diminish with older exposure age, aligning with literature emphasizing the effectiveness of early interventions in human capital development.

Moreover, quantile regressions and results by parental education indicate that the positive impacts of increased funding are more pronounced and significant for individuals at the lower end of the income distribution and for children from lower socio-economic backgrounds, suggesting an equality-enhancing effect of the funding increase. These results contribute significantly to the existing literature on education spending and student outcomes, challenging previous studies that suggest smaller effects of educational spending in countries with high baseline expenditure levels.

In conclusion, this paper not only provides insights into the efficacy of increased education funding in enhancing student outcomes, even in a high-spending country like Norway, but also provides evidence that local governments do respond as expected to changes in central government grants. It underscores the importance of broader increases in municipal education funding, showing that this change can yield significant benefits in student learning and long-term outcomes.

References

Aakvik, A., Vaage, K., and Salvanes, K. G. (2005). Educational attainment and family background. *German Economic Review*, 6(3):377–394.

Angrist, J. D. and Lavy, V. (1999). Using maimonides' rule to estimate the effect of class size on scholastic achievement. *The Quarterly journal of economics*, 114(2):533–575.

Balsvik, R., Jensen, S., and Salvanes, K. G. (2015). Made in china, sold in norway: Local labor market effects of an import shock. *Journal of Public Economics*, 127:137–144. The Nordic Model.

Baron, E. J. (2022). School spending and student outcomes: Evidence from revenue limit elections in wisconsin. *American Economic Journal: Economic Policy*, 14(1):1–39.

Becker, G. S. (2009). *Human capital: A theoretical and empirical analysis, with special reference to education*. University of Chicago press.

Belmonte, A., Bove, V., D'Inverno, G., and Modica, M. (2020). School infrastructure spending and educational outcomes: Evidence from the 2012 earthquake in northern italy. *Economics of Education Review*, 75:101951.

Bergvall, D., Charbit, C., Kraan, D.-J., and Merk, O. (2006). Intergovernmental transfers and decentralised public spending. *OECD Journal on Budgeting*, 5(4):111–158.

Biasi, B. (2023). School finance equalization increases intergenerational mobility. *Journal of Labor Economics*, 41(1):1–38.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2011). Older and Wiser? Birth Order and IQ of Young Men. *CESifo Economic Studies*, 57(1):103–120.

Böhlmark, A. and Lindquist, M. J. (2006). Life-cycle variations in the association between current and lifetime income: Replication and extension for sweden. *Journal of Labor Economics*, 24(4):879–896.

Bonesrønning, H. (2004). The determinants of parental effort in education production: do parents respond to changes in class size? *Economics of Education Review*, 23(1):1–9.

Borgen, N. T., Kirkebøen, L. J., Kotsadam, A., and Raaum, O. (2022). Do funds for more teachers improve student outcomes? *CESifo Working Paper*.

Cabral, M., Kim, B., Rossin-Slater, M., Schnell, M., and Schwandt, H. (2021). Trauma at school: The impacts of shootings on students' human capital and economic outcomes. Technical report, National Bureau of Economic Research.

Caetano, G. (2019). Neighborhood sorting and the value of public school quality. *Journal of Urban Economics*, 114:103193.

Card, D. and Payne, A. A. (2002). School finance reform, the distribution of school spending, and the distribution of student test scores. *Journal of public economics*, 83(1):49–82.

Carneiro, P. M. and Heckman, J. J. (2003). Human capital policy.

Cascio, E. U., Gordon, N., and Reber, S. (2013). Local responses to federal grants: Evidence from the introduction of title i in the south. *American Economic Journal: Economic Policy*, 5(3):126–59.

Coleman, J. S. (1966). Equal schools or equal students? *The Public Interest*, 4:70.

Datar, A. and Mason, B. (2008). Do reductions in class size "crowd out" parental investment in education? *Economics of Education Review*, 27(6):712–723.

Dearden, L., Ferri, J., and Meghir, C. (2002). The Effect of School Quality on Educational Attainment and Wages. *The Review of Economics and Statistics*, 84(1):1–20.

Fredriksson, P., Öckert, B., and Oosterbeek, H. (2013). Long-term effects of class size. *The Quarterly journal of economics*, 128(1):249–285.

Fredriksson, P., Öckert, B., and Oosterbeek, H. (2016). Parental responses to public investments in children: Evidence from a maximum class size rule. *Journal of Human Resources*, 51(4):832–868.

Gibbons, S., McNally, S., and Viarengo, M. (2017). Does Additional Spending Help Urban Schools? An Evaluation Using Boundary Discontinuities. *Journal of the European Economic Association*, 16(5):1618–1668.

Gibbons, S. and Silva, O. (2011). School quality, child wellbeing and parents' satisfaction. *Economics of Education Review*, 30(2):312–331.

Gordon, N. (2004). Do federal grants boost school spending? evidence from title i. *Journal of Public Economics*, 88(9):1771–1792.

Haider, S. and Solon, G. (2006). Life-cycle variation in the association between current and lifetime earnings. *American economic review*, 96(4):1308–1320.

Heinesen, E. and Graversen, B. K. (2005). The effect of school resources on educational attainment: Evidence from denmark. *Bulletin of Economic Research*, 57(2):109–143.

Houtenville, A. J. and Conway, K. S. (2008). Parental effort, school resources, and student achievement. *The Journal of Human Resources*, 43(2):437–453.

Hyman, J. (2017). Does money matter in the long run? effects of school spending on educational attainment. *American Economic Journal: Economic Policy*, 9(4):256–80.

Jackson, C. K., Johnson, R. C., and Persico, C. (2015). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. Technical report, National Bureau of Economic Research.

Jackson, C. K. and Mackevicius, C. L. (2023). What impacts can we expect from school spending policy? evidence from evaluations in the us. *American Economic Journal: Applied Economics*.

Jackson, C. K., Wigger, C., and Xiong, H. (2021). Do school spending cuts matter? evidence from the great recession. *American Economic Journal: Economic Policy*, 13(2):304–335.

Kline, P. and Moretti, E. (2014). People, places, and public policy: Some simple welfare economics of local economic development programs. *Annual Review of Economics*, 6(1):629–662.

Lafortune, J., Rothstein, J., and Schanzenbach, D. W. (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics*, 10(2):1–26.

Langørgen, A., Løkken, S., and Aaberge, R. (2013). Kommunenes økonomiske atferd 1972-2009.

Leuven, E. and Løkken, S. A. (2020). Long-term impacts of class size in compulsory school. *Journal of Human Resources*, 55(1):309–348.

Litschig, S. and Morrison, K. M. (2013). The impact of intergovernmental transfers on education outcomes and poverty reduction. *American Economic Journal: Applied Economics*, 5(4):206–240.

Machado, J. A. and Silva, J. S. (2019). Quantiles via moments. *Journal of Econometrics*, 213(1):145–173.

Nechyba, T. J. (2006). Chapter 22 income and peer quality sorting in public and private schools. volume 2 of *Handbook of the Economics of Education*, pages 1327–1368. Elsevier.

Nilsen, Ø. A. (2020). The labor market in norway: 2000-2018. *NHH Dept. of Economics Discussion Paper*, (04).

OECD (2020). Education policy outlook in norway. (20).

Ritchie, S. J. and Tucker-Drob, E. M. (2018). How much does education improve intelligence? a meta-analysis. *Psychological science*, 29(8):1358–1369.

Seland, I., Vibe, N., and Hovdhaugen, E. (2013). *Evaluering av nasjonale prøver som system*. NIFU.

Shapiro, J. M. (2006). Smart cities: Quality of life, productivity, and the growth effects of human capital. *The Review of Economics and Statistics*, 88(2):324–335.

Stanberry, L. (2013). *Randomization Test*, pages 1814–1814. Springer New York, New York, NY.

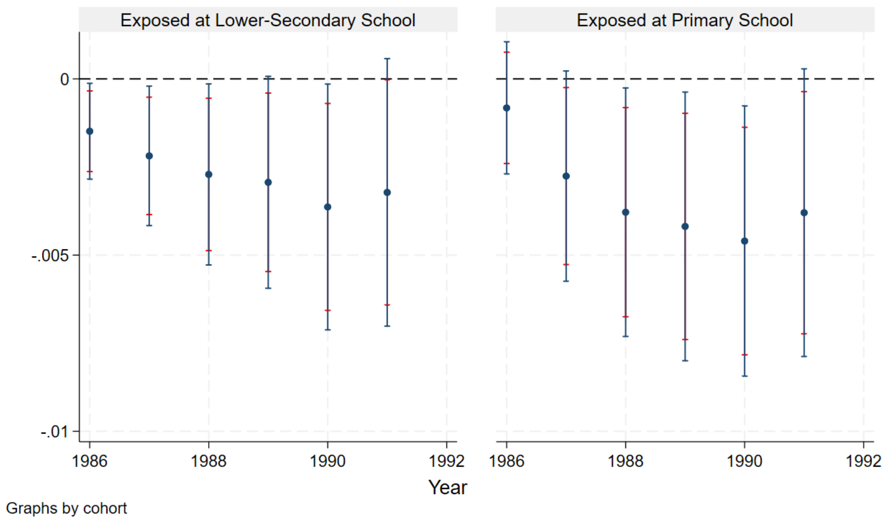
Sundet, J. M., Tambs, K., Magnus, P., and Berg, K. (1988). On the question of secular trends in the heritability of intelligence test scores: A study of norwegian twins. *Intelligence*, 12(1):47–59.

Vegas, E. and Coffin, C. (2015). When education expenditure matters: An empirical analysis of recent international data. *Comparative Education Review*, 59:289–304.

Appendices

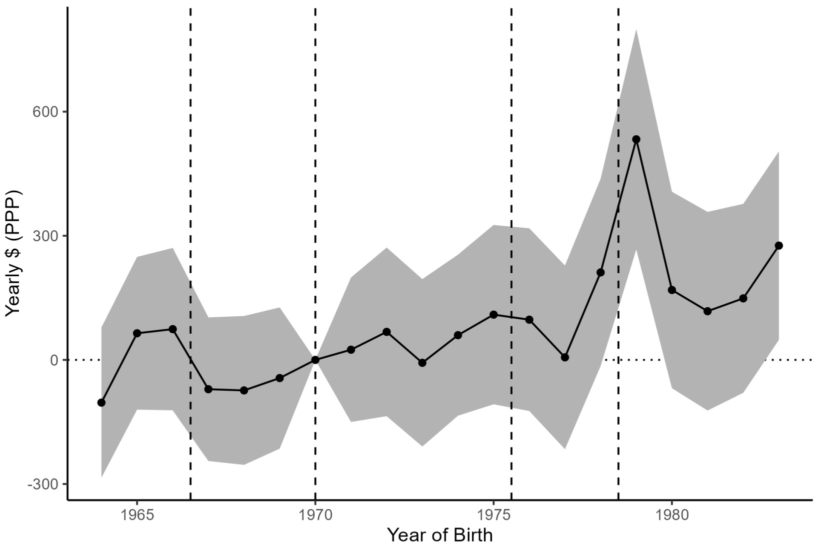
As discussed in subsection 3.3.3, I first test the likelihood of leaving the municipality in the following years to the reform across municipalities school additional funding. Graph 6, in the appendix, shows the regressions' point estimates and standard errors each year from 1986 to 1991. Indeed, students 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.

Figure 6: Effect on the Probability of Leaving the Municipality, by year



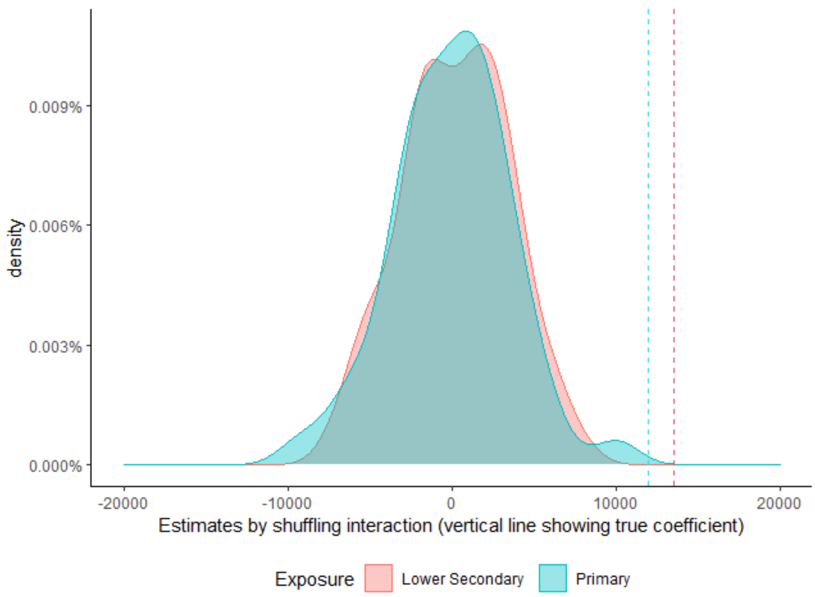
Notes: This figure shows the results from estimating Equation 6. Dots represent the π_g estimates; bars represent both 90% and 95% confidence intervals, clustered at the municipality level. Sample is individuals born between 1964 and 1983 who resided in a Norwegian municipality in 1985, which had not changed borders.

Figure 7: Effect on Earnings, by year of birth



Notes: This figure shows the results from estimating Equation 5. Instead of using cohort groups, this regression uses each year of birth. Dots represent the π_g estimates; bars represent both 90% 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 8: Point Estimates on earnings in 100 regressions after treatment random shuffling



Notes: This figure shows the results from estimating Equation 5. 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.