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

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Abstract

This paper provides a comprehensive assessment of the impact of education finance on students' long-term outcomes and the channels through which these effects operate. For identification, the paper exploits an intergovernmental transfer reform in Norway, which generated exogenous variation in school funding based on the school-aged demographic composition at the local level in the mid-1980s. The main takeaway from the paper is that being exposed to an additional $100 per pupil revenues for education during nine years of primary and lower secondary school leads to nearly $300 in higher earnings. The effect is larger and more significant for students with low-educated parents and at the lower end of the earnings distribution, highlighting the policy's role in enhancing equality. At the municipal level, the funding shock led schools to hire more teachers and increase work hours among school employees, without affecting class size or spending composition. Notably, the benefits were greater in low-spending municipalities, where increased funding led to more teacher hiring and higher earnings gains. Results also show improvements in educational quality and marginal effects on cognitive abilities and migration. Though moderate, the effects on earnings are substantial enough for the policy to achieve an Internal Rate of Return (IRR) of almost 7%, and a Marginal Value of Public Funds ranging from 1.5 and 2.6.

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1 Introduction

Education is a crucial factor for individual and societal advancement. For individuals, it is linked for example to higher earnings [Devereux and Fan, 2011], more employment [Riddell and Song, 2011], and lower mortality [Balaj et al., 2024]. For society, it is associated with higher productivity [Kampelmann and Rycx, 2012], higher voter turnout [Sondheimer and Green, 2010], and greater social mobility [Lindley and Machin, 2012]. Given these benefits, countries are constantly trying to improve their education systems, and one of the most straightforward ways to do this is through funding. This has been the subject of considerable academic and policy debates over the last several decades, and there are still many uncertainties. Unveiling the long-term effects of school funding, particularly on earnings, is challenging due to the limited availability of data and exogenous shocks that impact students over long periods of time.

I overcome these limitations by leveraging an intergovernmental transfer reform that took place in the mid-1980s in Norway. By exploring a quasi-random shock to education funding, caused by a change in grant criteria based on the pre-reform age composition of students, I present detailed evidence of the long-term effects on individual outcomes once students are fully integrated into the labor market. By combining comprehensive municipal data with rich population-wide longitudinal data from Norwegian registers, I examine the impacts on individual outcomes such as educational attainment and earnings throughout adulthood, as well as distributional and heterogeneous effects.

Broad intergovernmental transfers (public grants) correspond to a large share of municipal expenditure on education. In 1986, one of the grant criteria, which differentiated revenue levels transferred to municipalities based on the composition of primary and lower-secondary school students, was removed. This change led local governments with a higher share of primary school students (relative to the total number of compulsory school students) to experience a relative increase in education funding. Using an event-study design, which enables me to examine the timing of the shock and its effect over time, I control for confounding shocks and include 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.

I estimate the grant shock of the reform through cross-municipality variations in the demographic distribution of students aged 7 to 12 (primary school students) compared to those aged 7 to 15 (compulsory school students). This allows me to quantify the the shock size, in order to provide an assessment of the implications of additional transfers for education on short- and long-run outcomes.

Leveraging this policy change and estimating the grant shock, I investigate the impact on students who were exposed to additional $100 in school funding, and how the municipalities used this revenues on education spending and school inputs. I show that students who were at the right age to be in compulsory school, who were living in municipalities that received higher educational funding in the year prior to the reform, showed higher educational attainment and labor income by the age of around 35 years old. These results are conditional on standard individual characteristics likely to affect later-life outcomes,

such as gender and parental education.

I also show that an additional $100 in education funding led to an increase in the number of teachers and weekly gross hours worked by school employees, as well as an increase in the number of public schools. However, the funding did not affect class size, suggesting that the additional teachers and staff were mainly used to increase instructional intensity.

In terms of magnitude, the results are consistent in all specifications. An expected increase of $100 in school funding leads to almost $300 higher yearly earnings by ages 33 to 35 for students exposed in primary school, with lower and less significant effects for those exposed in lower-secondary school. Additionally, only students exposed in primary school show significant effects on educational attainment by age 35, with an increase of about 0.03 years of study. The size of these effects on higher education degrees is similar to the range found by Jackson and Mackevicius [2023], though slightly lower, potentially influenced by intention-to-treat estimates.

Since average effects may hide substantial heterogeneity, I show that the effects on earnings are larger and more significant at the lower end of the distribution, and the effects on both labor income and educational attainment are greater for children from lower socioeconomic status families. The results also show an increasing effect on earnings over the life cycle, starting near zero at ages 26 to 27 and peaking between ages 32 to 35.

Moreover, I examine how the impact of additional educational funding varies based on municipalities' initial spending levels. By dividing the sample around the median annual per-pupil expenditure on education, the analysis reveals that low-spending municipalities responded to the funding shock by significantly increasing teacher hiring, thereby enhancing instructional quality directly. High-spending municipalities, on the other hand, allocated the additional funds differently, showing marginal increases in the number of schools and suggesting a focus on expanding educational infrastructure and a lower increase on teachers. At the individual level, students who were living in low-spending municipalities in 1986 showed a higher increase on earnings, suggest that both the proportionality of the shock and the funding allocation could be driving human capital increases.

Additionally, municipalities that were winners or losers in different magnitudes showed non-linear effects both on students and school inputs. Students in municipalities that received positive funding shocks experienced significant increases in earnings by ages 33 to 35, with larger shocks correlating to greater earnings gains. Conversely, municipalities facing negative funding shocks did not exhibit significant adverse effects on number of teachers, not on individual earnings. These findings suggest that while increased educational funding can yield meaningful long-term economic benefits, reductions within the examined range do not necessarily undermine future earnings potential.

Results indicate that about a third of the increase in earnings is mediated through effects on educational level and specialization. There is also suggestive evidence of effects on cognitive abilities. The findings reveal small but significant effects in the late twenties, suggesting that increased school funding has a modest impact on the likelihood of moving, especially among those from urban municipalities. This indicates that while educational

enhancements can slightly prompt migration, the overall effect remains limited.

Given the nationwide and detailed impacts on later-life earnings, I estimate a cost-benefit analysis of additional funding for education. The analysis computes the Internal Rate of Return (IRR) by comparing the costs—both the shock itself and its effect on additional educational attainment—against the increase in lifetime earnings from ages 28 to 60, in a back-of-the-envelope calculation. By discounting future earnings and costs to estimate their present values, I show that the future benefits surpass the initial and subsequent educational investments up to a discount rate of 6.8%. I also show that the IRR is particularly high for populations with lower parental education levels. Switching the measure to Marginal Value of Public Funds (MVPF), I find that, for a discount rate assumed between 3 and 5%, the MVPF ranges from 1.5 to 2.6. The findings support the policy's efficacy, emphasizing its potential to generate economic mobility and enhance lifetime earnings, thereby validating increased educational spending as a socially beneficial initiative.

For robustness checks, I demonstrate that, at the local level, the funding did not increase spending in any other major sector, making it unlikely that the shock was correlated with any other policy besides education. I also narrow the age brackets to isolate potential spurious correlations with specific students' demographic composition associated with the shock, and the results remain consistently statistically significant. Finally, I randomize the treatment in 100 simulations to test for potential random effect biases, finding estimates near zero.

This paper connects two contrasting strands of literature: school input interventions with mixed effects and the positive impacts of school spending reforms in the U.S. By analyzing the effects of a significant funding shock, the findings reveal that schools used the additional funds to enhance teaching resources—such as increasing the number of teachers and instructional hours—without altering the composition of capital and current expenditures. This suggests that schools and municipalities may have a better understanding of how to allocate resources efficiently than previously thought, demonstrating that even in high-spending contexts, increased funding can yield positive results when strategically deployed.

In addressing the effects of education spending, existing literature—predominantly from the U.S.—has focused on school funding formula reforms since the 1970s [Jackson and Mackevicius, 2023; Baron, 2022]. There is substantial evidence documenting the impact of education spending on various outcomes, such as test scores [Card and Payne, 2002], educational attainment [Hyman, 2017; Jackson et al., 2021], wages [Jackson et al., 2015], poverty [Lafortune et al., 2018], and intergenerational mobility [Biasi, 2023]. However, most of this literature lacks detailed information on long-term monetary outcomes, focusing instead on immediate educational achievements or using income data from surveys.

This paper contributes to the literature by presenting a consistent set of long-term impacts of increased school funding, tracking its effects on educational attainment, earnings, cognitive abilities, and migration, offering a more complete understanding of the cascading effects of educational investment. The results are also the first to show population-wide

estimates on earnings using register data, which allows me to explore both distributional impacts and investigate the policy's cost-effectiveness, particularly for students from less advantaged educational backgrounds.

Furthermore, research outside specific states in the U.S. is notably limited, often concentrating on capital expenditures, which may not be directly comparable due to differing methodological approaches [Belmonte et al., 2020; Gibbons et al., 2017; Heinesen and Graversen, 2005]. This study bridges these gaps by examining the long-term effects of increased educational funding on students' earnings into adulthood within a broader international context. Although Norway's education system is well-funded and structured differently from that of the U.S., it presents a valuable comparative analysis of how varying levels of educational investment impact long-term economic outcomes.

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 has 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]. Considering other revenue shocks for education, this paper contrasts with Brunner et al. [2022], that found that school districts used additional revenue, due to installation of wind turbines, mostly on capital spending, which led to zero effects on students' long-run outcomes. This paper finds little difference in the spending composition, with municipalities increasing both the number of teachers and the number of schools. Finally, this study contributes to the debate on the effect of school inputs on learning and long-term outcomes. While most research focuses on class size, generally finding 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].

2 Institutional Background

2.1 Educational System in Norway

Norway consistently ranks among the top countries for public education spending, with expenditures as a share of GDP increasing from nearly 6% in the 1980s to about 7% in subsequent decades. Despite a decreasing proportion of school-age children, per-student spending has remained stable at about 20% of GDP per capita, placing Norway among the top 10 countries in spending relative to this educational level.

Education in Norway is free from primary through tertiary levels. Municipalities manage primary and lower-secondary education for children aged 7 to 15, while counties handle upper-secondary education, which has an enrollment rate of around 90%. The National Ministry of Education and Research oversees higher education, where enrollment rates surged from 25% to 80% after 2000.

Norwegian schools are characterized by small sizes and low student-to-teacher ratios, enhancing individual attention, although educators generally earn less than their similarly educated peers in other sectors. Municipalities have autonomy over resource distribution, and schools have some discretion in budget and staffing decisions, but they remain under

national regulations set by the Ministry of Education and Research.

Educational assessments in Norway begin in lower-secondary school, with high-stakes testing limited to the final years of lower-secondary and upper-secondary levels. Since 2004, national tests have been used to foster school improvement and support students needing additional help. On international benchmarks like the Programme for International Student Assessment (PISA)[1], Norway performs well in reading and mathematics across socio-economic groups. However, despite high spending, challenges remain in closing performance gaps and improving teacher salaries and professional development [OECD, 2020].

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 similar to levels seen in most developed countries with decentralized government systems [Bergvall et al., 2006]. Municipal tax revenues, on the other hand, made up 60% of municipalities' budgets.

The autonomy of municipalities in Norway was gradually reduced by the central government 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 that 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. The transfer amount was calculated based on the number of teaching hours, which were valued differently depending on the level of education (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 by the following:

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

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

[1] A triennial international survey which aims to evaluate education systems worldwide by testing the skills and knowledge of 15-year-old students.

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).

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 proposing a new intergovernmental transfer system for counties[2], and in 1982, a similar report was released for municipalities[3]. These reports served as the basis for the bills that introduced a new system in 1986, replacing most prevailing intergovernmental grants[4], creating an income-equalizing grant and three major sector grants for health, education, culture, and general purposes.

For each sector, cost matrices were constructed based on characteristics that counties and municipalities could not change over time. Associated weights were applied to these variables, providing a number of "points," which are used to distribute central administration grants to this day. The criteria and weights were developed to address the varying costs municipalities face in delivering an equal range of services in each of the three sectors.

Under this new set of rules, in the education cost matrix, no distinction was made between primary and lower-secondary education, as shown in Table 1. As a result, municipalities with a higher proportion of younger children (aged 7 to 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 the new system in 1986 would not lead to major changes in transfers to local administrations in the short term. Changes in criteria and weights were to be phased in over several years: in the first two years, the new system would be weighted at 10% and 20%, respectively, while the old system would account for the higher share. In 1988, however, the previous year's level was weighted at 80%, and the new rules were fully incorporated in 1989.

[2]NOU 1979: 44

[3]NOU 1982: 15

[4]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"

3 Data and Methodology

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 456 municipalities. This restriction ensures that I consistently classify municipalities over time.

For fiscal data, the Strukturtall for kommunenes økonomi documents are used, which are available on the Statisk Sentralbyrå (SSB) website. These documents provide detailed data on municipal per capita gross and net operating expenses since 1974. Municipal-level demographic and education-related variables, such as the number of students, schools, and teachers, are provided by the kommunedatabasen, which covers a wide range of municipality characteristics and policies since the early 1970s.

At the individual level, the sample includes all individuals born between 1964 and 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, of whom around 995,000 had a paying job.

This study explores the effect of the policy on educational attainment (in terms of years of study) and earnings at ages 33 to 35, as Haider and Solon [2006] and Böhlmark and Lindquist [2006] show that the association between lifetime returns to schooling and current earnings is strongest by the mid-30s. Since earnings increases capture the total monetary effect, I decompose the channels associated with this outcome, identifying some of the mechanisms through which education spending affects income. Higher human capital potentially translates into cognitive abilities [Ritchie and Tucker-Drob, 2018], but, until the early 2000s, no data on grades or cognitive/non-cognitive abilities was available for the entire population. Thus, I use military conscription register data at ages 18–19 for the vast majority of Norwegian-born males. During the recruitment process, most young men were required to take the General Ability Test (GAT) to evaluate their suitability for military service. The GAT consists of three speeded tests of arithmetic (30 items), word similarities (54 items), and figures (36 items). About 6-9% of the 1977-81 cohorts did not take the test due to various unrecorded reasons, such as severe physical or mental disabilities.

The GAT is similar to the AFQT and the Wechsler IQ test. Standardized component scores are 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 to each conscript. Apart from the mathematics test changing to a multiple-choice format in the early 1990s, both the test and the scoring norm remained constant throughout the period.

In addition to potential effects on cognitive abilities, the impact of education funding on earnings is also mediated by educational level and field of study. To assess this channel, I employ a predictive model focusing on the wages of individuals aged 33 to 35. The model integrates educational levels and fields of specialization, as defined by the Norwegian Central Statistical Bureau, which are listed in the appendix.

The adapted Mincerian wage equation includes cohort and municipal fixed effects. The education-specialization categories are compared to a baseline category representing only

compulsory education. Using predicted $E(Y|\text{Education-Specialization}_{k,i})$ as an outcome, I assess the effects of educational funding on earnings through educational level and field of study, further identifying the channels through which the overall policy affects income.

A third channel explored is migration choices. The literature highlights that local educational investments also affect individuals seeking better labor market opportunities [Kline and Moretti, 2014; Shapiro, 2006]. From a municipal perspective, this effect could be a partial drawback, as the migration of students who benefited from additional funding may reduce the local gains in earnings. This "brain drain" effect is particularly pronounced in settings where disparities in economic opportunities are significant across regions.

Therefore, I investigate the phenomenon 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 variables include the probability of living in a different municipality from the one where the individual resided in 1985 or living in a large city[5].

3.1 Descriptive Statistics

Education spending accounted for around 29% of municipal expenditures between 1980 and 1985, while tax revenues made up 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: This table shows author's calculations from register data generated by Statistics Norway. 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.

The table shows that 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 the total population to 12% in 1991. Although the number of students per teacher and class size decreased, along with an increase in the teaching hours proxy[6], the average number of public schools declined after 1983.

[5] Oslo, Bergen, Trondheim, and Stavanger

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

Table 3 additionally shows descriptive statistics by cohort group, with all variables fixed at ages between 33 and 35. Similar to the trends shown above, average schooling increased by over one year of study for Norwegian residents born between 1964 and 1967 compared to those born between 1980 and 1983, with a similar pattern observed in parents' educational levels. Yearly earnings, on the other hand, nearly doubled between those cohorts.

Table 3: 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 %

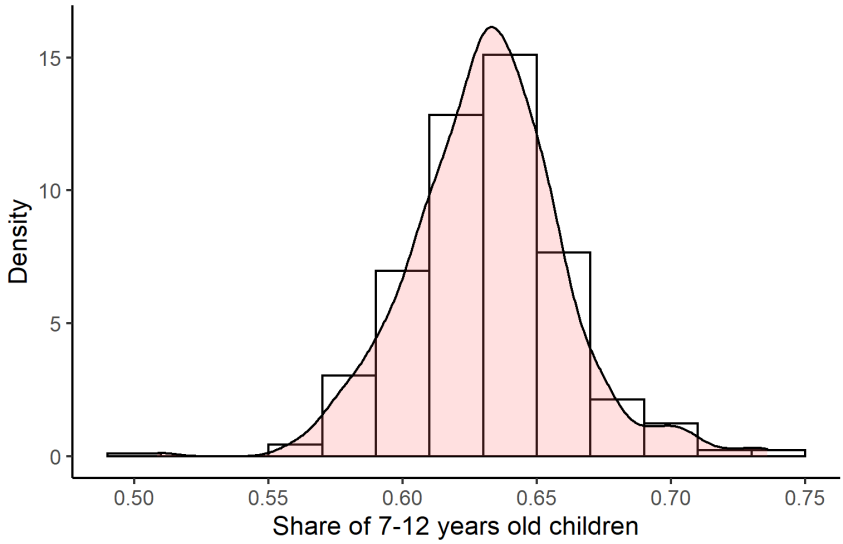
Notes: This table shows author's calculations from register data generated by Statistics Norway. Sample is restricted to students who were born between 1964 and 1983 and were living in a Norwegian Municipality in the year of 1985. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles.

3.2 Empirical Procedure

3.2.1 Estimating Shock Size

I leverage cross-municipality variation in the pre-reform share of children aged 7 to 12 over the total of children and teenagers aged 7 to 15. Figure 1 shows the share of 7-12-year-old children among those of primary and lower-secondary school age, which will be the treatment intensity variable, by municipality in 1985. The distribution shows no clear regional patterns. Most municipalities had a share between 55% and 70%, indicating a relatively small range for the treatment variable, with a standard deviation of about 0.029. However, a few municipalities exhibit more extreme shares, around either 50% or 75%.

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



Notes: This figure shows author's calculations from register data generated by Statistics Norway and tabulated by Kommunedatabasen. The share is relative to population aged between 7 and 15 years old.

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

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

where SW is the number of school weeks per year, reflecting 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 1985; and $\hat{CF}$ is the simulated unified cost factor post-reform, designed to balance the aggregate grant in the year prior to the reform.

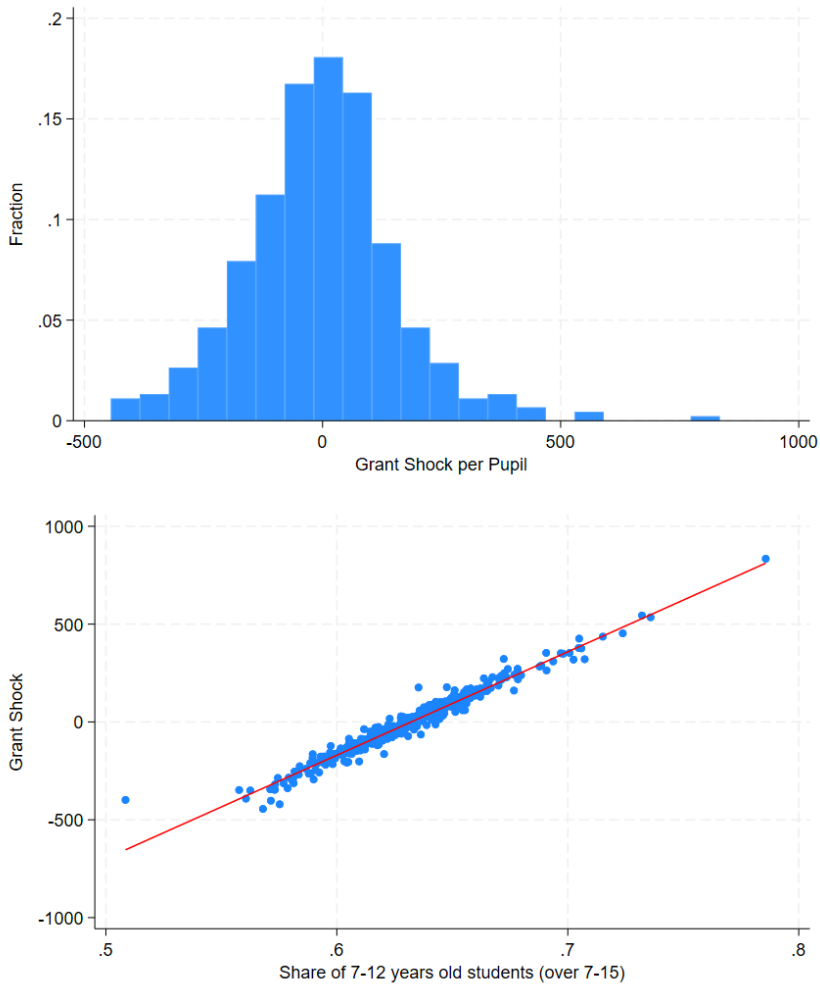
For practical application, the parameters are set as follows: the school year comprises 39 weeks; primary education involves 25.2 teaching hours per week, the maximum allowed for funding, while lower-secondary education involves 47.5 teaching hours per week. The pre-reform cost factors were \$30.2 for primary and \$34.1 for lower-secondary education, based on 1985 values. Post-reform, a unified cost factor of \$32 was established to maintain 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 would be 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.

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

Figure 2: Distribution of Shock Size Estimates



Notes: This table shows author's calculations from register data generated by Statistics Norway. Expenditure values in 2011 PPP dollars. Grant shock is defined by formula 1.

The histogram displays the distribution of grant shocks 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 (out of the total of 7-15-year-olds) and the grant shock received by municipalities. The data points show a clear linear relationship: as 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 an additional yearly \$100 per pupil (in 2011 PPP dollars). This value represents around 1% of the total expenditure in 1986. In order to compare the estimates to existing literature, I will rescale the main results to \$100 per pupil. However, it is important to underscore that such a procedure assumes a linear relationship between the grant shock and its impacts, which may not fully capture the actual dynamics observed in the data.

3.2.2 Municipal-level Analysis

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

$$Y_{m,t} = \sum_{q=-1985} \pi_q(1[q=t]Shock_m) + \phi X_m^{'} + \gamma_m + \delta_t + \vartheta_{ct,t} + \epsilon_{m,t}, \tag{2}$$

where X_m is a matrix of controls for all criteria that may influence education spending, such as the pre-reform predicted annual change in the share of demographic groups and the share of children aged 7 to 15 in municipality m in year t. γ_m, δ_t, and $\vartheta_{ct,t}$ are municipal, year, and county-by-year fixed effects, which control for any changes within the same region. Since rural and central municipalities have significantly different contexts that might not be perfectly captured by covariates, there will also be fixed effects for dummies identifying the level of centrality[7] interacted with year.

By non-parametrically tracing out the full adjustment path of the treatment effect via equation (1), I can examine the reform's gradual implementation. As discussed in subsection 2.3, the variation in the underlying criteria does not lead to an immediate treatment impact. Pooling two periods of three years, I also provide a differences-in-differences analysis with phase-in and full treatment periods, for which I use the following specification:

$$Y_{m,t} = \beta_1(1[t \in 1986{-}88]Shock_m) + \beta_2(1[t \in 1989{-}91]Shock_m) + \phi X_m^{'} + \gamma_m + \delta_t + \vartheta_{ct,t} + \epsilon_{m,t} \tag{3}$$

where β_1 and β_2 express the level changes in the grouped years of 1986-88 and 1989-91, respectively. Both will reflect the reform's effects.

The main assumption underlying the identification approach is similar to that in all differences-in-differences 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 occurred. Therefore, this relative time parameter should be flat and not statistically significantly different from zero in the pre-reform period, which will be tested. 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

[7] 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.

interacted with each year. Since there is concern that the new rules might also affect other sources of central administration funding, controls for Health Sector Matrix Points will also be included. These refer to a formula introduced during the 1986 intergovernmental transfers reform, used to allocate grants for health services. This control variable is constructed using both the Health Sector grant weights and pre-1986 municipal characteristics based on demographics.

3.2.3 Individual-level analysis

I develop a similar design for individual outcomes, replacing year fixed effects with cohort fixed effects (c). I use cohort groups (g) interacted with the expected shock to estimate the effects in a flexible way. Table 4 shows the cohorts' ages 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 4: 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.

The individual-level effects are estimated using equation 4 below.

$$Y_{i,g} = \sum_{q=-1}^{3} \pi_q(1[q=g]Shock_m) + \phi X'_{m,1985} + \alpha U'_i + \gamma_m + \delta_c + \vartheta_{ct,c} + \epsilon_{i,c} \tag{4}$$

In addition to the municipal controls and fixed effects discussed earlier, the individual-level analysis will also include gender and foreigner[8] dummies, as well as the educational levels 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 3 shows clear trends in parental educational level and the share of foreigners across cohorts, these controls will be interacted with the year of birth.

The variable $Shock_m$ will be assigned based on the municipality where the individual lived in 1985, one year prior to the reform. This means the coefficients will be intention-to-treat estimates, as not all students lived in the same municipality in subsequent years. This choice addresses the potential threat of bias, as null treatment coefficients could reflect sorting-into-treatment, especially if more concerned parents moved based on where education spending or quality was 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 magnitude and direction of this response is mixed. While Houtenville and Conway [2008] provides suggestive evidence of a reduction in parental effort relative to school inputs, Datar and Mason [2008] finds very small effects (3-7% of a standard deviation) with no impact on students' achievement. Finally, Bonesrønning [2004] found no strong evidence of parental responses to different class sizes, although there is some indication that parents reduce their efforts as class sizes increase (a complementary response). The Norwegian context of heavily publicly funded education and low income inequality suggests 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, calibrated for each cohort's specific exposure, with continuous variables representing the (potential) years of exposure. Instead of simply pooling the more and less exposed cohorts, I examine how the effects of the shock vary depending on the length of time the cohort was exposed to it. The parameter estimation will be expressed in terms of the full 9 years of exposure, providing a basis for comparison across the entire implementation period.

$$Y_{i,c} = \pi Shock_{m,c} \cdot \text{Years of Exposure}_{i,c} + \phi X'_{m,1985} + \alpha U'_i + \gamma_m + \delta_c + \vartheta_{ct,c} + \epsilon_{i,c}, \tag{5}$$

where Years of Exposurei, c is the number of years for which students were school-aged after 1986, which varies from 0 to 9; $Shock_{m,c}$ is a cohort- and municipality-specific adjustment of the original shock variable ($Shock_m$), calculated to account for the phased implementation of the policy, adjusting the magnitude of the shock depending on the year of birth and capturing the gradual increase in the policy's impact. π represents the

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

coefficients of interest.

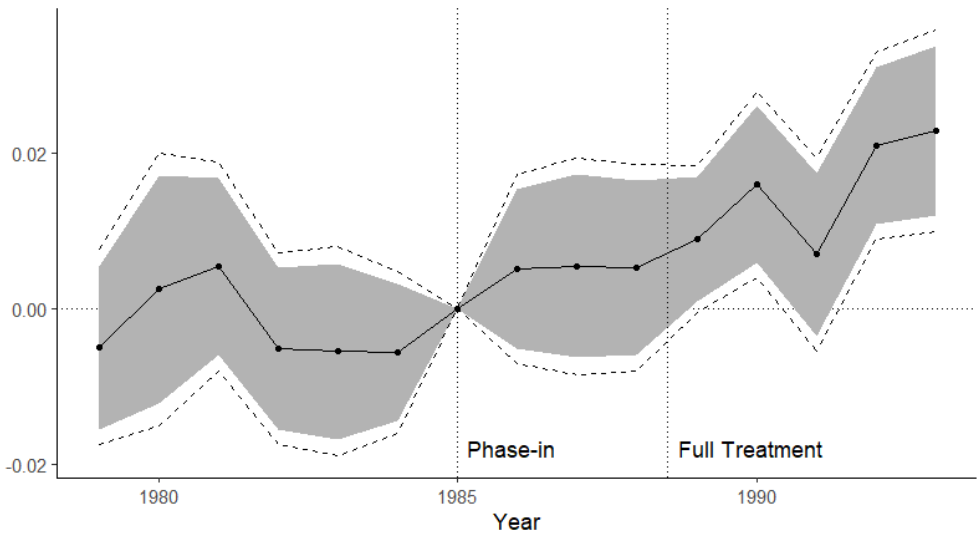
This model imposes a linear structure by interacting the calibrated school funding shock with a continuous variable representing the length of exposure. This approach allows the analysis to examine how the average effect size of the shock varies by each year of exposure. However, a limitation of this 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 increased 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, year by year, to an increase of $100 in intergovernmental transfers to education. Coefficients are mostly flat prior to the baseline year, but they increase starting in 1986 and are statistically significantly positive after 1988. This result is expected due to the gradual implementation of the reform, as discussed in subsection 2.3.

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



Notes: This figure shows the results from estimating Equation 2. Dots represent the π_q estimates; gray area represent 90% 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 noting that the effect of an additional $100 per pupil corresponds to around a 2% increase in gross expenditure. The magnitude is consistent with the shock size relative to baseline average spending.

Table 5 shows results on school inputs, with the data segmented into two periods: Phase-in (1986-88) for initial effects and Full Treatment (1989-1991) for sustained effects. There is evidence that municipalities used the additional resources to increase teaching hours, hire more teachers, and build schools—or, alternatively, to prevent them from shutting

down, as the average number of public schools dropped during this period. Interestingly, class size remained unchanged, indicating that the additional teaching hours were likely used for more tutoring or extracurricular activities.

Table 5: Municipal-level regressions

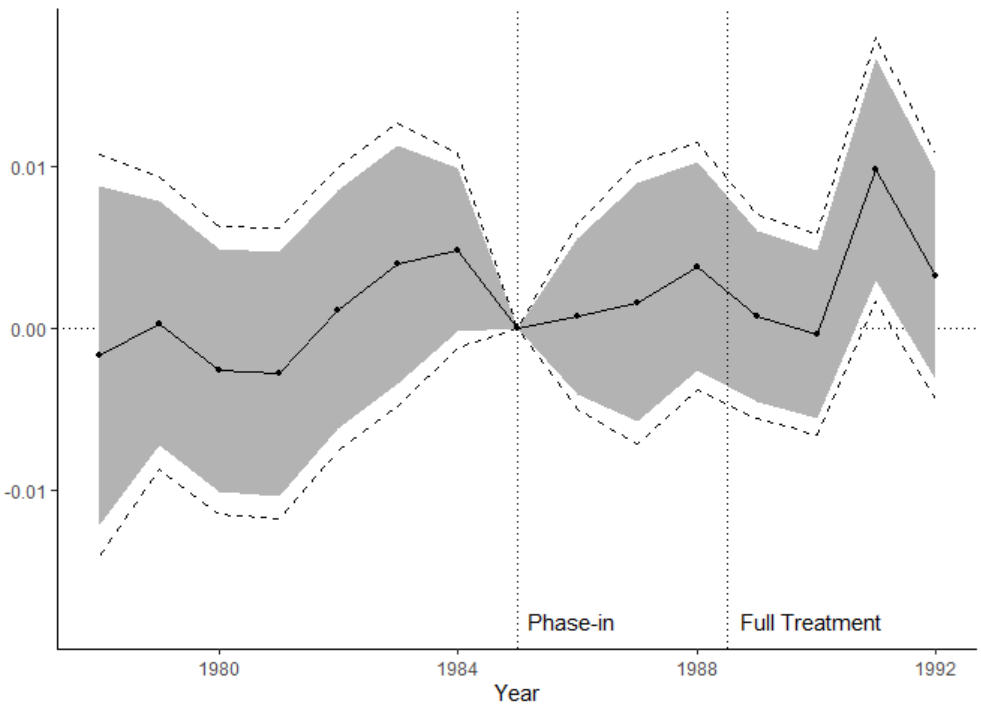
Outcomes	(1) Teaching Hours per Pupil	(2) Teachers Per Pupil	(3) Class Size	(4) Teachers' Education	(5) Teachers' Income	(6) Number of Schools
Phase-in	0.074	0.009***	0.045	-0.00528	-0.001	0.029
(1986-88)	(0.074)	(0.003)	(0.044)	(0.017)	(0.004)	(0.018)
Full Treatment	0.262*	0.013***	-0.009	0.005	-0.006	0.055*
(1989-91)	(0.149)	(0.004)	(0.057)	(0.026)	(0.006)	(0.032)
Observations	3,215	4,374	4,374	3,215	3,215	4,374
Pre-Treat. Mean	5.03	4.21	18.2	14.2	12.1	7.9
Number of Mun.	378	402	402	378	378	402
Pre-trend p-value	0.159	0.156	0.029	0.373	0.794	0.132

Notes: This table shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from 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 indicate that municipalities primarily used the additional funds to hire teachers. Excluding the four largest cities in Norway, the results remain significant for the hiring of teachers, but not for the number of schools, as shown in Table 16 in the appendix.

Graph 4 shows the effect of additional funding on the share of education spending allocated to current expenditures, which include staff compensation and day-to-day supplies, such as teaching materials. This result shows that extra funding is spent on both current and capital spending, without changing the composition. This contrasts with findings from other studies on Norwegian education policies, which focused on direct changes to 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 author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 2. Dots represent the π_q estimates; bars represent 90% confidence intervals, clustered at the municipality level. Sample is 402 Norwegian municipalities in 1985 that had the same borders throughout the period.

4.1.1 Effects by Pre-shock Level of Education Spending

Investigating the differential impact of the funding shock based on pre-reform levels of education spending is essential to understand how municipalities with varying resource constraints respond to additional funding. Municipalities with lower initial spending may prioritize different aspects of educational investment compared to those with higher spending. By splitting the sample around the median annual per-pupil expenditure of $12,000, I analyze whether the same absolute increase in funding leads to different outcomes in educational inputs across these two groups.

Table 6: Impact of Funding Shock by Pre-Reform Education Spending Levels

Outcomes	(1) Teaching Hours per Pupil	(2) Teachers (log)	(3) Class Size	(4) Teachers' Education	(5) Teachers' Income (log)	(6) Number of Schools
Low Spending						
Phase-in	0.142	0.013**	-0.090	-0.028	-0.007	-0.002
(1986-88)	(0.089)	(0.006)	(0.088)	(0.035)	(0.006)	(0.042)
Full Treatment	0.240	0.020**	-0.170	-0.027	-0.012	-0.043
(1989-91)	(0.155)	(0.009)	(0.131)	(0.059)	(0.011)	(0.059)
Observations	985	1,287	1,405	985	984	1,405
Pre-Treat. Mean	3.93	4.77	21.32	14.34	12.13	9.68
Number of Mun.	115	118	118	115	115	118
Pre-trend p-value	0.571	0.405	0.772	0.500	0.848	0.963
High Spending						
Phase-in	0.0634	0.008**	0.070	-0.002	0.001	0.023
(1986-88)	(0.088)	(0.004)	(0.050)	(0.020)	(0.004)	(0.021)
Full Treatment	0.261	0.013***	-0.004	0.013	-0.004	0.068*
(1989-91)	(0.182)	(0.005)	(0.0628)	(0.030)	(0.007)	(0.037)
Observations	2,230	3,087	3,369	2,230	2,230	3,369
Pre-Treat. Mean	5.52	3.98	16.93	14.16	12.11	7.14
Number of Mun.	263	284	284	263	263	284
Pre-trend p-value	0.108	0.232	0.0239	0.497	0.933	0.205

Notes: This table shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from 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. The sample is split by pre-reform annual per-pupil expenditure level, between above and below $12,114. *** p<0.01, ** p<0.05, * p<0.1

The results presented in the table indicate that low-spending municipalities significantly increased their teacher hiring in response to the funding shock. In contrast, high-spending municipalities also show an increase in the number of teachers, but the effect sizes are smaller. Additionally, a notable difference is observed in the "Number of Schools" variable. High-spending municipalities exhibit a marginally significant increase in the number of schools during the full treatment period (0.068*), whereas low-spending municipalities do not show a significant change. This suggests that low-spending municipalities primarily allocated the additional funds towards hiring more teachers to enhance educational quality, while high-spending municipalities may have used the funds to expand educational infrastructure by opening new schools.

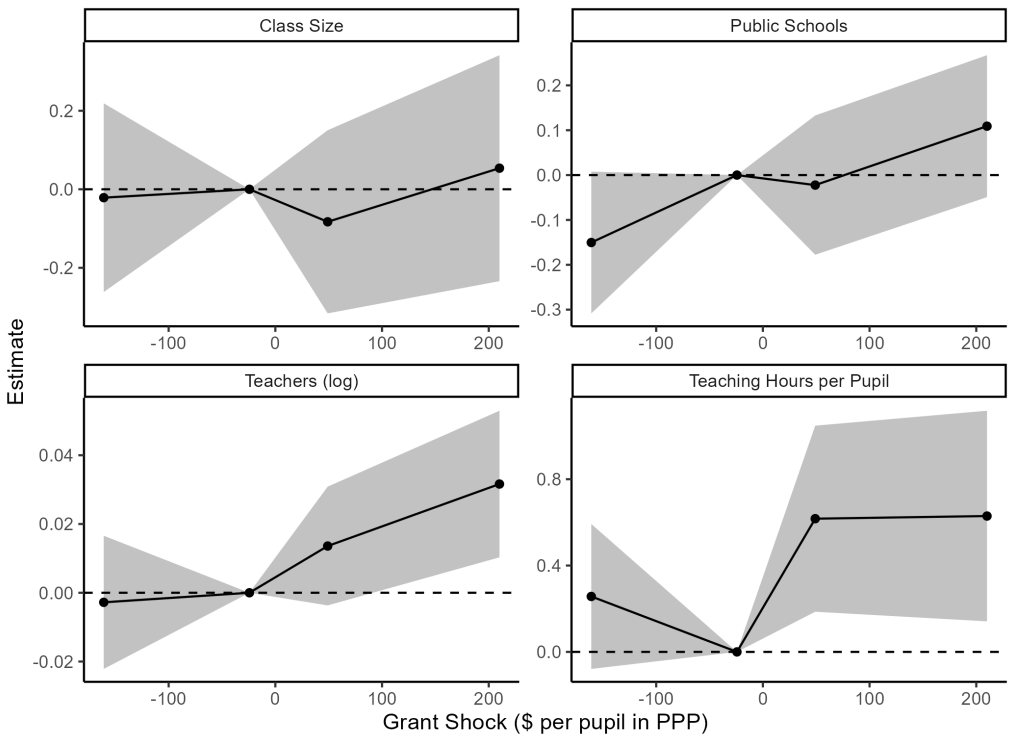
These findings highlight that the impact of additional funding is influenced by the initial level of resources. Low-spending municipalities responded to the funding shock by significantly increasing teacher hiring, which likely contributes directly to improvements in instructional quality and student support. In contrast, high-spending municipalities showed a smaller increase in teacher numbers but a marginally significant increase in the number of schools, suggesting a focus on expanding educational infrastructure. This divergence underscores the importance of considering baseline spending levels when designing and implementing fiscal policies aimed at enhancing educational outcomes.

4.1.2 Effects by Shock Size

To further explore how the size and direction of the funding shock affect municipal educational inputs, we categorize municipalities into four groups based on the magnitude of the shock they experienced: significant negative shock (less than or equal to -$50 per pupil), small negative shock (between -$50 and$0), moderate positive shock (between $0 and +$100 per pupil), and substantial positive shock (greater than +$100 per pupil). By interacting these categories with a pre/post reform dummy (before and after 1986), I assess whether the level of funding changes led to differential impacts on key municipal-level educational outcomes.

This analysis is important because municipalities experiencing negative shocks might reduce educational inputs, while those receiving positive shocks may enhance them. Understanding these differences helps in evaluating the overall effectiveness of the policy, especially in terms of how different increases in funding translate into tangible educational improvements. The results are summarized in Figure 5.

Figure 5: Non-linear Effects on Municipal Outcomes



Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from interacting size shock with pre/post dummy. Size shock categories are 'less than or equal to -$50 per pupil', 'small negative shock (between -$50 and$0)', 'between $0 and +$100 per pupil' and 'greater than +$100 per pupil', all values adjusted by 2011 PPP. Dots represent the 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 results indicate notable differences between municipalities that received negative versus positive funding shocks. Specifically, municipalities with positive shocks show significant increases in the number of teachers and teaching hours per pupil, whereas those

with negative shocks do not exhibit significant changes or show minimal adjustments.

These findings highlight that municipalities receiving positive funding shocks significantly increased their investment in educational inputs, especially on teachers. In contrast, municipalities with negative shocks did not show significant changes in any outcomes other than some weak evidence of negative effects on number of schools. No significant changes were observed in class size across any of the categories, which aligns with earlier findings.

Overall, these results underscore the differential impact of funding shocks based on their magnitude and direction. Positive shocks led to meaningful enhancements in educational inputs at the municipal level, particularly in terms of teacher hiring and instructional time. Negative shocks did not produce significant adverse effects on these inputs, which could be due to municipalities attempting to shield core educational services from budget cuts or finding alternative funding sources to mitigate the impact.

4.2 Individual-level Results

After examining the effects of increased educational spending and how these funds were allocated, we now turn to the direct outcomes of this financial shift on the students themselves. Specifically, we investigate whether the additional funding influenced educational attainment and labor market performance for those who experienced these changes during their schooling years. Table 7 presents the results of our regression analyses, employing both a flexible approach and a linear specification approach based on equations 4 and 5, respectively. We report earnings in two formats: absolute yearly labor income (in 2011 PPP dollars) and labor income rank by cohort (year of birth). Additionally, Graph 9 in the appendix visually details the earnings effects segmented by year of birth, rather than cohort groups. This analysis provides a comprehensive understanding of how increased educational investments have translated into tangible educational and economic outcomes for affected individuals.

Table 7: Individual-level regressions

VARIABLES	(1) Years of Study	(2) Higher Education	(3) Annual Earnings	(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				
9 Years of Exposure	0.029***	0.005**	260.8***	0.004***
	(0.0108)	(0.00192)	(85.49)	(0.00112)
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

Notes: This table shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 4 and 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1964 and 1983 who resided in Norwegian municipalities in 1985, which had not changed borders. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. *** p<0.01, ** p<0.05, * p<0.1

The interpretation of the flexible approach is that $100 of additional education resources during primary education led to an increase of 0.025 years of schooling and yearly earnings of $290 around the age of 33 to 35, which is also reflected in a higher cohort labor income rank. For those exposed to the same shock during lower-secondary school, the estimate is considerably smaller and less significant, while those marginally exposed to the shock show no significant effect. The linear specification approach reveals a consistent pattern of effects across both earnings and educational attainment. On average, nine years of exposure to an additional $100 per pupil results in an increase of 0.03 years in educational attainment. In terms of earnings, nine years of exposure leads to a $260.8 increase in yearly earnings. Effects on the likelihood of obtaining a college diploma and cohort income rank are of similar magnitude.

The results from the linear specification approach are consistent, as expected, with those from the flexible approach. It is worth noting that in terms of magnitude, the increase in earnings represents about 1% of the pre-treatment mean—similar to the relative size of the shock, as described in subsection 3.2.1 and shown in Figure 3.

The literature, documented by Jackson and Mackevicius [2023], reports that a sustained increase of $1,000 in per-pupil school spending over four years typically results in a 0.0539

increase in the probability of obtaining a college degree, with most effects ranging between 0.05 and 0.5. My estimates, adjusted to reflect a similar $1,000 shock, indicate that students exposed to increased funding between the ages of 7 and 10 show a 0.022 increase in the likelihood of earning a college diploma. It is important to clarify that these are intention-to-treat estimates, as detailed in subsection 3.2.1. Additionally, the effect size presumes a linear relationship between the funding increase and educational outcomes, which may not fully capture the observed data. Finally, it is worth noting that in Norway, where tertiary education is widely accessible and free, the potential for increased educational spending to enhance this outcome may be more limited than in contexts like the United States, where education costs can be a significant barrier.

4.3 Impact on earnings across distribution and age

To comprehensively understand the effects of educational funding, it is crucial to explore not just the average impacts but also how these effects are distributed across different segments of the population and evolve over time. This approach sheds light on the policy's potential to address inequalities and its long-term benefits throughout an individual's career. Norway's extensive population-wide registers allow me to employ quantile regressions and age-specific analyses, providing new insights into the distributional impacts of increased school funding and its progression as individuals age.

Quantile regression analysis, as outlined by Machado and Silva [2019], allows us to examine the effects of the funding increase across various points of the labor income distribution. Table 8 shows results by five quantile points, ranging from 0.1 to 0.9.

Table 8: Quantile regressions on annual earnings

	(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					
9 Years of Exposure	349.7***	295.4***	258.5***	225.1***	188.1*
	(134.6)	(78.23)	(61.88)	(76.42)	(112.2)
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 author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 4 and 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1964 and 1983 who resided in Norwegian municipalities in 1985, which had not changed borders. Earnings outliers excluded. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. *** 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 we move up the distribution. Such findings indicate that the additional educational funding predominantly benefits those at the lower end of the earnings distribution, potentially reducing income disparities. The flexible approach also shows that estimates on earnings are consistently higher and more significant at the lower points of the distribution, and less significant at the 0.9 quantile. The highest point estimate is found at the 0.1 quantile, showing an increase of over $400 in earnings for those exposed during primary school. Therefore, the patterns indicate that increasing school funding had an equality-enhancing effect on earnings decades later, suggesting a larger impact for low-skilled workers.

To further elucidate the temporal dimension of these impacts, Table 9 presents the estimated effects of higher exposure to the educational funding shock on earnings across various age groups.

Table 9: Annual 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-Secondary School	-29.98	47.14	69.39	143.9*	154.4*
	(56.25)	(64.91)	(73.01)	(77.09)	(86.47)
Exposed at Primary School	46.66	171.6**	212.9***	263.3***	300.5***
	(54.12)	(66.76)	(75.01)	(86.97)	(88.67)
Linear Specification Approach					
9 Years of Exposure	27.07	146.1**	188.7***	261.5***	260.7***
	(55.08)	(63.78)	(69.75)	(82.96)	(84.69)
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 author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 4 and 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1964 and 1983 who resided in Norwegian municipalities in 1985, which had not changed borders. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. *** 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 in the early 30s. This highlights the importance of early educational investment for long-term earnings potential.

4.4 Heterogeneity Analysis

4.5 Effects by Parental Education

It is important to understand whether the effects of additional resources for education observed in the previous sections were experienced equally by all types of students. Recent literature has identified a more prominent role of school investments for low-SES students [Dearden et al., 2002; Heinesen and Graversen, 2005; Belmonte et al., 2020].

To address this, the student sample was divided based on parental education levels: one subgroup consists of students whose parents do not hold an upper-secondary education degree, and another where at least one parent does. The results, shown in Table 10, indicate that the benefits of increased funding are predominantly observed among students from lower-SES backgrounds.

Table 10: Results by Parental Education

VARIABLES	Years of Study		Annual Earnings	
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-	0.001	-0.005	95.90	124.61
Secondary School	(0.0135)	(0.0173)	(103.95)	(123.40)
Exposed in Primary	0.0028**	0.019	413.60***	169.95
School	(0.014)	(0.0172)	(104.05)	(131.73)
Linear Specification Approach				
9 Years of Exposure	0.030**	0.026*	385.3***	114.9
	(0.014)	(0.015)	(107.2)	(110.2)
Parental Education	Low	High	Low	High
Pre-Treatment Mean	12.3	14.1	29,097.0	34,742.8
Observations	524,678	498,607	508,233	473,037

Notes: This table shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 4 and 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Earnings outliers excluded. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. Groups defined by upper-secondary school attainment. *** p<0.01, ** p<0.05, * p<0.1

Almost all results are driven by students with low-education parents. In the linear specification approach, one year of exposure to the shock has a positive and significant effect on earnings for both groups, with the effect being slightly stronger and more significant for students with low-education parents. In the flexible approach, only students with low-education parents who were 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-education parents, whereas for the other group, the effect is positive but not statistically significant. In the flexible approach, similar to the previous outcome, only students with low-education parents who were exposed in primary school show a positive, significant effect, which is higher than the estimate found for the whole sample.

4.6 Effects by Pre-Reform Education Spending Levels

An important consideration in evaluating the impact of the funding shock is that municipalities with different initial levels of education spending may respond differently to the same absolute change in funding. Specifically, the same shock in absolute terms may represent a different proportion of total spending for municipalities with low versus high pre-reform per-pupil education expenditure. To explore this potential heterogeneity, I split

the sample around the median of annual $12,000 per pupil and examine whether the effects of the grant shock vary between municipalities with lower and higher initial spending levels.

Table 11 summarizes the results of this analysis. The table presents the baseline mean expenditure and earnings for both groups, along with the proportional size of the shock and the estimated effect on earnings. For municipalities with pre-reform per-pupil spending below the median ("Low Spending"), the grant shock represents approximately 0.9% of their baseline expenditure. The estimated effect on earnings for individuals in these municipalities is $347, which is about 1.1% of their baseline earnings. In contrast, for municipalities with pre-reform spending above the median ("High Spending"), the shock constitutes about 0.6% of their baseline expenditure, and the estimated effect on earnings is $219, approximately 0.7% of baseline earnings.

Table 11: Effect by Pre-Shock Level of Education Spending

	Baseline Mean Expenditure	Proportional Shock	Effect on Earnings	Baseline Mean Earnings	Proportional Effect
Low Spending (n. obs.: 471,020)	10,938.8	0.9%	347.0** (163.9)	31,558.4	1.1%
High Spending (n. obs.: 510,286)	15,548.7	0.6%	218.8** (92.1)	31,219.8	0.7%

Notes: This table shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1964 and 1983 who resided in Norwegian municipalities in 1985, which had not changed borders. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. The sample is split by pre-reform annual per-pupil expenditure level, between above and below median of $12,114. *** p<0.01, ** p<0.05, * p<0.1

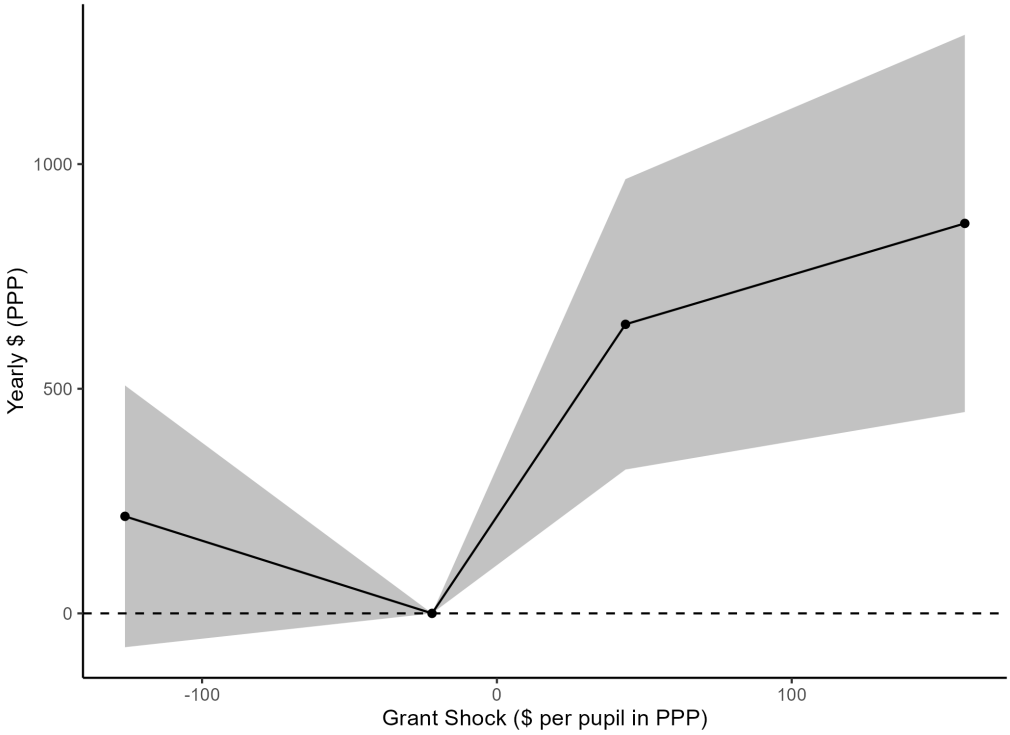
These findings suggest that the impact of the funding shock is indeed proportional to the relative size of the shock compared to the baseline expenditure. Municipalities with lower initial education spending experience a larger proportional increase in funding and, correspondingly, a larger effect on earnings. This differential impact underscores the importance of considering the initial level of resources when evaluating the effectiveness of fiscal interventions. In contexts where resources are scarce, additional funding may have a more pronounced effect on long-term outcomes, possibly due to diminishing marginal returns to education spending at higher levels of investment.

4.6.1 Effects by Shock Size

To further investigate the heterogeneity of the funding shock's impact, I categorize municipalities into four groups based on the size of the shock they received, similarly to what was done at the municipal level, in subsection 4.1.2. This categorization allows an examination of whether the magnitude and direction of the funding shock lead to differential effects on individual earnings, particularly when comparing negative and positive shocks. Understanding these nuances helps in assessing the overall effectiveness of the policy. The

results are summarized in Figure 6.

Figure 6: Impact on Individual Earnings by Shock Size



Notes: This Figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 5 by Shock Size. Dots represent the π_t estimates; gray area represent 90% confidence are clustered by municipality the students were living in 1985. Size shock categories are 'less than or equal to -$50 per pupil', 'small negative shock (between -$50 and$0)', 'between $0 and +$100 per pupil' and 'greater than +$100 per pupil', all values adjusted by 2011 PPP. Sample is restricted to individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Annual earnings are measured in dollars between ages of 33 and 35, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles.

The findings reveal a clear pattern. Students in municipalities that received positive funding shocks show significant increases in individual earnings at ages 33 to 35. Specifically, individuals in municipalities that experienced moderate positive shock showed an average earnings increase of $643, while those in municipalities that had a substantial positive shock) saw an larger increase, but in a lesser magnitude, of $868. In contrast, municipalities facing a larger negative shock did not exhibit a statistically significant change in earnings, comparing to those serving as the reference group.

These results suggest that positive funding shocks have a meaningful and statistically significant impact on long-term individual earnings, whereas negative shocks do not produce a significant negative effect. The substantial increase in earnings for individuals in municipalities with higher positive shocks underscores the marginally lower impact of additional educational funding on labor income. Conversely, the lack of significant negative effects in municipalities facing funding cuts may indicate either compensatory mechanisms at play or that reductions within the examined range do not critically undermine educational quality and long-term earnings potential.

4.7 Channels

Building on the findings regarding the school funding long-term benefits, this section explores the potential mechanisms through which these effects are realized. To understand the underlying processes driving the increase in earnings, I analyze the influence of increased educational funding on cognitive abilities and education quality, examining how these factors contribute to the economic outcomes observed.

Table 12 displays regression results for cognitive abilities (in IQ scale) and expected earnings by educational quality, employing both flexible and linear specification approaches. This analysis aims to determine the extent to which enhanced funding affects cognitive development and educational experiences, which are hypothesized to mediate the relationship between funding and earnings.

Table 12: Potential Channels Outcomes

VARIABLES	(1) Cognitive Abilities (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		
9 Years of Exposure	0.121**	85.77***
	(0.0600)	(30.51)
Pre-Treatment Mean	100.5	10,212.5
Observations	504,710	1,024,535

Notes: This table shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 4 and 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. Cognitive Abilities are measured in IQ scale. Education quality is measured with a mincerian regression on level-specification attainment, as specified in section 3. *** p<0.01, ** p<0.05, * p<0.1

Regarding cognitive abilities, the flexible approach reveals small and statistically insignificant coefficients, indicating minimal impact from increased funding exposure. Conversely, the linear specification suggests a positive, statistically significant relationship at the 5% level for additional years of funding exposure, hinting at a potential cognitive benefit from enhanced educational resources.

In terms of education quality, results from the flexible approach show a significant positive impact of $72 on expected earnings for those exposed in primary school, accounting for almost 30% of the total earnings effect. The linear specification approach records a slightly higher impact of $85.8, or about 32% of the total effect on earnings, suggesting that improvements in the quality of education play a substantial role in the observed income increases.

These findings indicate that while the effects on cognitive abilities are modest at best, the quality of education—encompassing both level and specialization—accounts for about a third of the impact on earnings. Contrasting these results with the broader literature, such as the meta-analysis by Ritchie and Tucker-Drob [2018], which suggests that increased schooling can raise cognitive abilities by as little as 1 to 5 IQ points per additional year, my results show that although the overall impact on cognitive abilities is limited, the significant improvement in educational quality and subsequent earnings enhancement underscores the value of targeted educational investments. This section thus connects the micro-level outcomes of increased funding to broader educational and economic contexts, illustrating how specific channels of influence support long-term benefits.

4.8 Migration Effects

In examining the broader implications of increased educational funding, it is essential to consider how enhanced human capital may influence migration decisions—a factor that could contribute to spatial sorting and, thus, impact earnings. Educational attainment is often linked to increased mobility, as individuals seek regions offering better economic opportunities. This subsection explores whether the additional educational funding in Norway facilitated greater inter-municipal migration, potentially driving some of the observed improvements in earnings.

To accurately capture the effects of educational funding on migration patterns, the analysis uses data from the municipality where each student resided at the age of six, rather than their location in 1985. This approach accounts for the influence of early educational experiences on subsequent mobility choices. Table 13 presents the migration results by age, indicating the probability of relocating from one municipality to another.

Table 13: 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	0.003*	0.007**	0.004*	0.0003	0.004**	0.0004
	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
Observations	986,962	986,962	986,962	986,962	986,962	986,962
Pre treat. Baseline	.2672	.4520	.5131	.1781	.2419	.2313
Rural Municipalities	0.003	0.006**	0.003	0.001*	0.004**	0.001
	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
Observations	608,808	608,808	608,808	608,808	608,808	608,808
Pre treat. Baseline	.2702	.4931	.5518	.0561	.1456	.1477
Urban Municipalities	0.012*	0.014***	0.014*	-0.004	0.002	0.005
	(0.006)	(0.005)	(0.008)	(0.003)	(0.006)	(0.006)
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 author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 4 and 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1964 and 1983 who resided in Norwegian municipalities in 1985, which had not changed borders. *** p<0.01, ** p<0.05, * p<0.1

The findings indicate small yet significant migration effects, particularly notable in the 27-29 age group, suggesting that increased school funding may have influenced the decision to move, especially among those from urban municipalities at age six. However, the impact appears limited and is particularly concentrated among individuals in their late twenties.

Several hypotheses could explain these findings. Firstly, the age-specific migration patterns may reflect life stage transitions, such as completing higher education or beginning a career, which are common in this age group and might prompt relocations. Secondly, other factors besides economic prospects may significantly shape migration decisions, such as housing markets and location amenities, potentially overshadowing the effects of educational funding. Considering the Norwegian cultural context, it is plausible that societal expectations around independence and early adulthood mobility may also play a role. In Norway, there is a general expectation that young adults will establish independent households after completing their education, which could standardize migration patterns across different educational backgrounds.

5 Cost-benefit analysis

5.1 Internal Rate of Return

This section conducts a cost-benefit analysis to assess the aggregate economic impact of increasing educational funding by \$100 per pupil annually from grades 1 to 9. The purpose of this analysis is to determine the Internal Rate of Return (IRR), which indicates the efficiency of this policy in terms of the additional earnings it generates relative to its costs. This evaluation is crucial for policymakers, as it provides a quantitative measure of the long-term value of educational investments, helping to inform decisions on future educational funding.

To evaluate this policy, the cost of the additional funding is calculated as follows:

$$Cost = \sum_{t=7}^{15} \frac{100}{(1+r)^{t-6}} + \sum_{t=16}^{22} \frac{\mathbb{E}(\Delta Educ)}{(1+r)^{t-6}}, \tag{6}$$

where t ranges from the ages of 7 to 22, capturing the period from primary to tertiary education. The term $\mathbb{E}(\Delta Educ)$ represents the expected increase in the probability of obtaining a higher education degree multiplied by the average expenditure per pupil at these levels, sourced from the World Bank database[9]. The parameter r is the discount rate used to calculate the present value of future costs.

The benefits of the policy, defined as the present value of increased future earnings ($Benefit$), are calculated using:

$$Benefit = \sum_{t=28}^{60} \frac{\Delta Y}{(1+r)^{t-6}}, \tag{7}$$

where ΔY denotes the annual increase in earnings attributed to the policy, applied from the age of 28 to 60.

The policy's cost-effectiveness is determined by comparing the present value of benefits (PV) to the calculated costs (C). The Internal Rate of Return (IRR) is the discount rate r_{max} that equates the net present value of the investment to zero:

$$\sum_{t=28}^{60} \frac{\Delta Y}{(1+r_{max})^{t-6}} \geqslant \sum_{t=7}^{15} \frac{100}{(1+r_{max})^{t-6}} + \sum_{t=16}^{22} \frac{\mathbb{E}(\Delta Educ)}{(1+r_{max})^{t-6}} \tag{8}$$

This equation balances the discounted values of future earnings against the upfront costs, identifying the break-even point for the investment. Research such as Haider and Solon [2006] and Böhlmark and Lindquist [2006] supports the use of middle-aged earnings (ages 33-35) to estimate IRR due to the strong correlation with lifetime earnings during this period.

Based on these calculations, and as detailed in Table 7, the policy yields an IRR of 6.8% for the general population, increasing to 8.5% for students from lower-educated family

[9] The spending value is the first one available from the 1990s, which is \$13,280 in secondary education and \$33,698 in tertiary education. The increased probability, estimated in Equation 5, is 0.003, significant at the 99% level.

backgrounds. This enhanced rate for disadvantaged groups underscores the policy's role in reducing educational inequities.

5.2 Marginal Value of Public Funds

I also calculate the MVPF of public school funding. The MVPF is another benefit-cost framework, which produces a common metric for the relative effectiveness of spending on different programs, assuming a certain discount rate. It compares the benefits that a policy provides to society (society's willingness to pay) to the net cost to the government of implementing it [Hendren and Sprung-Keyser, 2020]. So, instead of estimating the IRR, I discount the policy costs of benefits using a 3–5% discount rate [Barr et al., 2022].

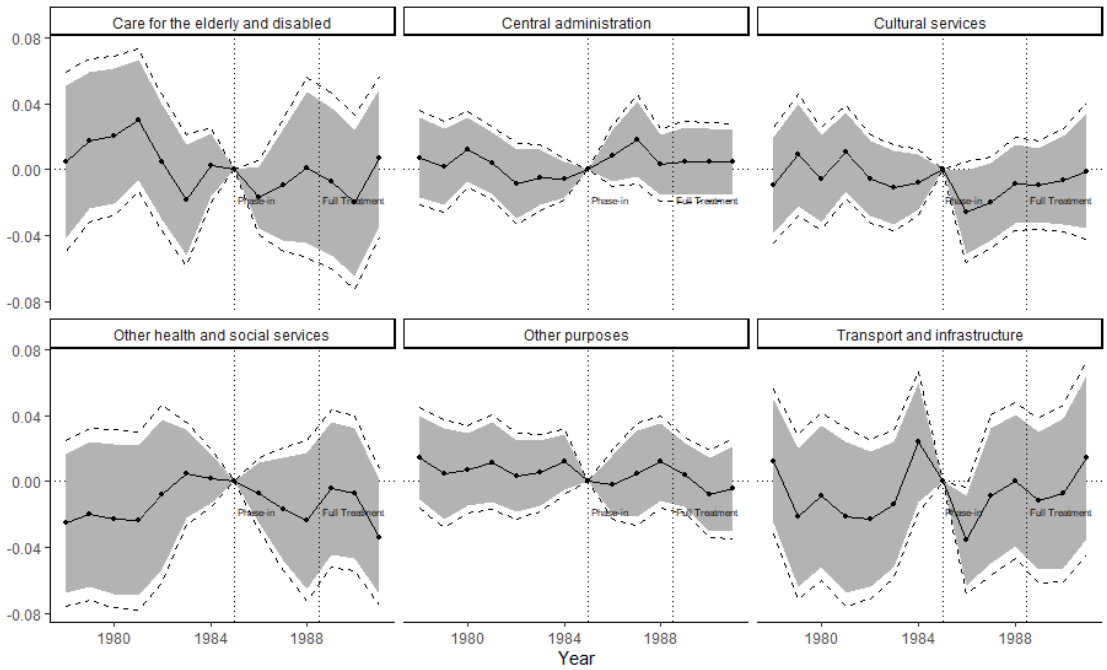
The MVPF ranges from 1.5 to 2.6, which means that society receives between $1.5 and $2.6 in benefits for every $1 in government costs. In other words, even considering only its individual labor market benefits, and under attenuated Intention-to-treat estimates, the benefits of increasing school funding are considerably larger than the costs.

In conclusion, the analysis confirms the cost-effectiveness of the educational policy, particularly highlighting its significant returns for students from less advantaged backgrounds. These findings suggest that prior to the reform, there may have been underinvestment in education, especially for low-SES groups. For policymakers, this implies that further investments in education could yield substantial economic returns, contribute to more equitable educational outcomes, and strengthen the overall educational framework. This provides a compelling case for the continuation and expansion of similar funding policies.

6 Robustness Checks

In the municipal-level analysis, I find that municipalities with a higher share of primary school-aged children in 1985 experienced increased expenditure on education after that year. However, that shock might have correlated with increases in other sectors' spending, which could mean that the individual-level analysis results are influenced by other types of policies. Figure 7 shows the same regression as in Graph 3, applied to all other major sectors presented in the 'Strukturall for kommunenes økonomi' documents. The graphs show no impact of the shock on any other major sector. Therefore, central administration school funding was indeed channeled into education by municipalities.

Figure 7: Effect of $ 100 higher grant on big sectors' per capita expenditure (log)



Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from 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.

One of the main concerns in the individual-level analysis is the structure imposed on regressions when pooling cohorts. Pooling cohorts more and less exposed to a school funding shock increases the power of the analysis, as a larger sample size is required for saturated models, such as those presented in section 3.2. However, doing so imposes a certain level of structure on the analysis, assuming a specific relationship between the cohorts and the school funding shock.

To address this potential issue, I 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 shock's impact.

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 at a very young age, it is likely that the effect of the funding shock would be more pronounced in this group.

Table 14: Individual-level regressions: Different Cohort Groups

Years of Birth	Average Years of Exposure	Average age in 1986	(1) Earnings	(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			981,306	1,023,285

Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 5. Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1965 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. *** p<0.01, ** p<0.05, * p<0.1

In this analysis, I find a significant effect for the cohort born between 1979 and 1980 and the cohort born in 1983. Despite the smaller sample size, this cohort was almost fully exposed to the school funding shock at a very young age, which contributes to the robustness of my 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 statistical 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 also 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.

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's outcomes over time.

To mitigate the risk of such confounding effects, I use a sensitivity analysis, narrowing down the age range used to define the demographics of interest. Focusing on a more specific age cohort may potentially minimize the variability in external influences not directly related to the reform but 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 that we estimate the true effect of the reform, independent of 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.

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 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 15: Individual-level regressions: Different Age Brackets

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
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
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

Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 5.Standard errors in parentheses are clustered by municipality the students were living in 1985. Sample is restricted to individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. *** 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, 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 the results were not driven by chance or by any systematic patterns in the treatment assignment.

Figure 11 reports the distribution of point estimates on earnings. Results are consistent with the original findings, with most of the coefficients remaining around zero and well 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 broad 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 teacher-to-pupil ratios and increased teaching hours. However, the effects on school staff income and class size were minimal, suggesting a strategic allocation toward enhancing instructional intensity.

At the individual level, I demonstrate a significant impact of increased funding on educational attainment and labor income, mediated by educational choices, a small increase in cognitive abilities, and migration. 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.

The cost-benefit analysis presented earlier strengthens the case for increased educational funding, demonstrating a notable Internal Rate of Return (IRR) on investments in education. This analysis is particularly revealing as it quantifies the financial returns of educational investments over the long term, showing that the benefits in terms of increased labor income significantly outweigh the costs of additional funding at a discount rate of up to 6.8%. This supports the argument that not only do educational investments improve educational and social outcomes, but they are also economically viable and offer a substantial return on public investments, particularly for disadvantaged groups, for whom the IRR rises to 8%.

Understanding the implications of these findings is crucial for policymakers in education. This research advances the literature by providing empirical evidence that increased educational spending can lead to significant improvements in student outcomes, even in contexts with already high levels of educational expenditure. It challenges the prevailing assumptions that the impact of additional educational resources diminishes in high-expenditure settings, highlighting the role of strategic funding allocations, such as enhancing teacher-to-student ratios and instructional intensity. For policymakers, the findings advocate for targeted increases in educational funding, especially in areas that directly impact instructional quality and access for low-income families, thereby promoting greater educational equity.

It is worth mentioning that previous studies on educational interventions in Norway have generally suggested that school input policies, such as changes in classroom size or direct modifications to physical infrastructure, yield limited effectiveness in enhancing student outcomes. This contrasts sharply with findings from the United States, where school spending reforms have frequently demonstrated positive impacts on student achievement, particularly in underfunded districts. These divergent results have fueled debates on the efficacy of increased educational spending across different educational systems and economic

contexts.

This paper bridges these two strands of literature by examining the effects of a significant funding shock in Norway, a country typically characterized by high baseline expenditure levels. The findings suggest that schools and municipalities may indeed be better equipped to allocate educational resources to optimize student outcomes than previously acknowledged. By showing positive results from the funding intervention in a high-spending context, this study provides compelling evidence that increased funding can effectively enhance educational outcomes, rather than merely improving school inputs.

Additionally, this study contributes to a better understanding of how educational funding impacts various demographic groups, which can guide more effective and equitable policy formulations. By demonstrating the specific benefits for students from lower socioeconomic backgrounds, the research underscores the potential of education policy as a tool for reducing inequality, helping to reduce income disparities through improved educational opportunities. Moving forward, these results can inform reforms that not only increase funding but also ensure that such investments are channeled into the most impactful areas, thereby enhancing the overall quality of education and its contributions to economic development and social cohesion.

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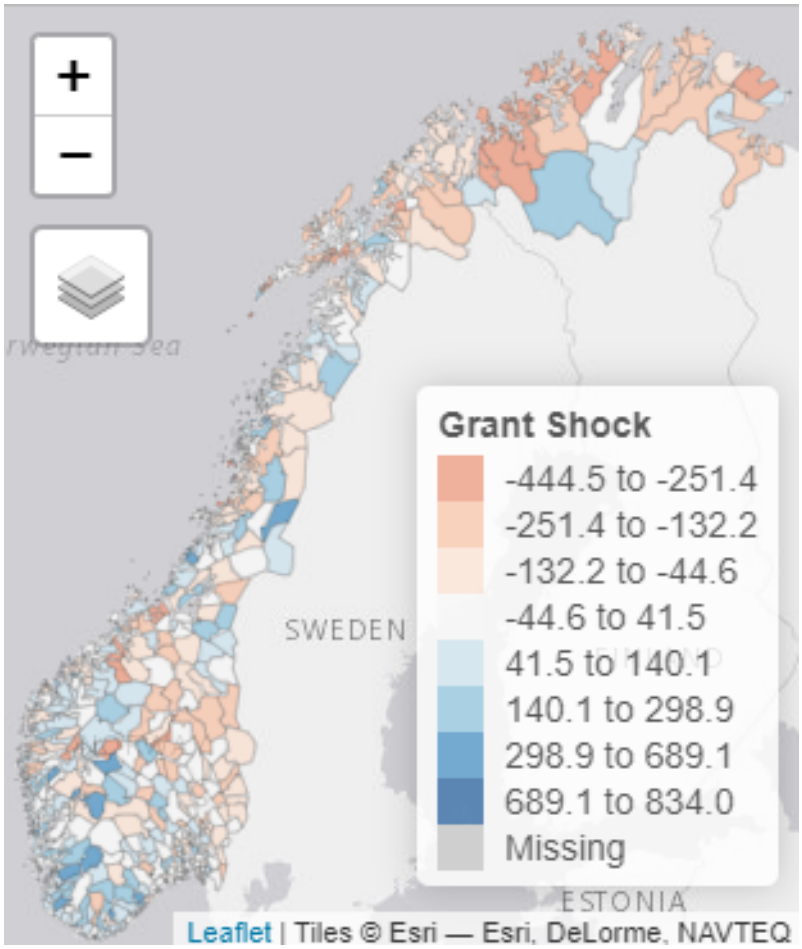
Appendices

Educational Level-Specialization Categories

1. Compulsory Education	
2. Upper Secondary School	First Year High School Diploma - Academic High School Diploma - Vocational
3. Vocational Tertiary Degree	Education Humanities and Arts Social Sciences, Business and Law Science, Mathematics and Computing Engineering, Manufacturing and Construction Agriculture and Veterinary Health and Welfare Services Unknown or General Programmes
4. College Degree	Education Humanities and Arts Social Sciences, Business and Law Science, Mathematics and Computing Engineering, Manufacturing and Construction Agriculture and Veterinary Health and Welfare Services Unknown or General Programmes
5. Master Degree	Education Humanities and Arts Social Sciences, Business and Law Science, Mathematics and Computing Engineering, Manufacturing and Construction Agriculture and Veterinary Health and Welfare Services Unknown or General Programmes
6. PhD Degree	Education Humanities and Arts Social Sciences, Business and Law Science, Mathematics and Computing Engineering, Manufacturing and Construction Agriculture and Veterinary Health and Welfare Services Unknown or General Programmes

Estimated Grant Shock

Figure 8: Estimated Grant Shock Geographical Distribution



Notes: This figure shows author's calculations from register data generated by Statistics Norway. Expenditure values in 2011 PPP dollars. Grant shock is defined by formula 1.

Municipal-level results

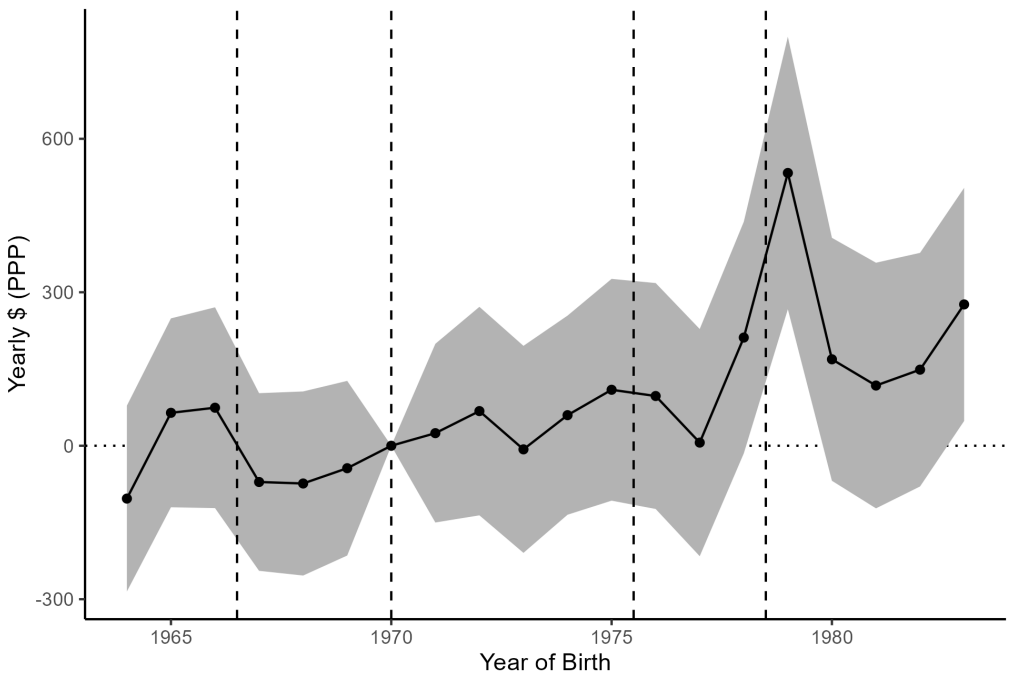
Table 16: Municipal-level regressions excluding big Cities

Outcomes	(1) Teaching Hours per Pupil	(2) Teachers (log)	(3) Class Size	(4) Teachers' Education	(5) Teachers' Income	(6) Number of Schools
Phase-in	0.073	0.009***	0.055	-0.005	-0.001	0.029
(1986-88)	(0.003)	(0.001)	(0.045)	(0.017)	(0.004)	(0.019)
Full Treatment	0.262*	0.013**	0.004	0.007	-0.006	0.053
(1989-91)	(0.149)	(0.004)	(0.056)	(0.026)	(0.006)	(0.032)
Observations	3,179	4,330	4,726	3,179	3,178	4,726
Pre-Treat. Mean	5.04	4.18	18.2	14.2	12.1	7.3
Number of Mun.	374	398	398	374	374	398
Pre-trend p-value	0.168	0.159	0.029	0.377	0.786	0.120

Notes: This table shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 3. Standard errors clustered by municipality in parentheses. Sample is 398 Norwegian municipalities that had the same borders throughout the period, excluding four biggest cities in Norway (Oslo, Bergen, Trodheim and Tromsø). Column (1) has 24 municipalities with missing data. *** p<0.01, ** p<0.05, * p<0.1

Individual-level results

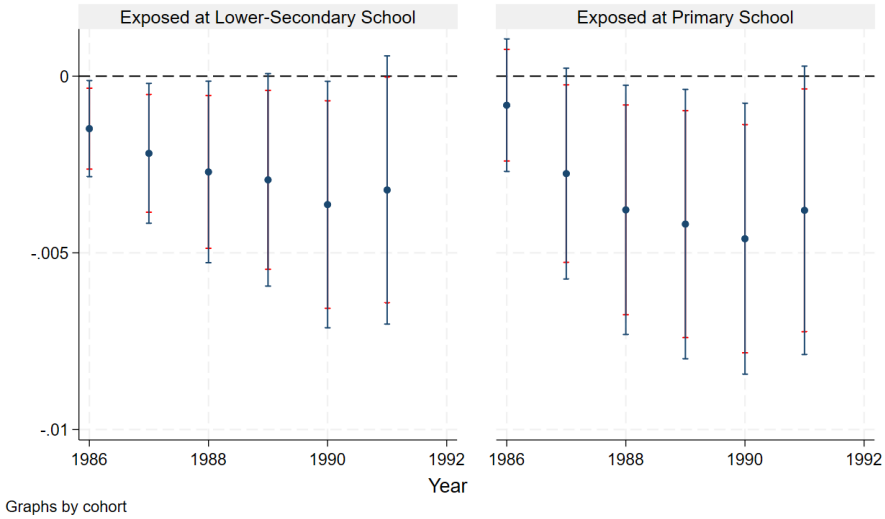
Figure 9: Effect on Earnings, by year of birth



Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 4. 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 restricted to individuals born between 1964 and 1983 who resided in a Norwegian municipality in 1985, which had not changed borders. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles.

As discussed in subsection 3.2.3, I first test the likelihood of leaving the municipality in the following years to the reform across municipalities school additional funding. Graph 10, 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.

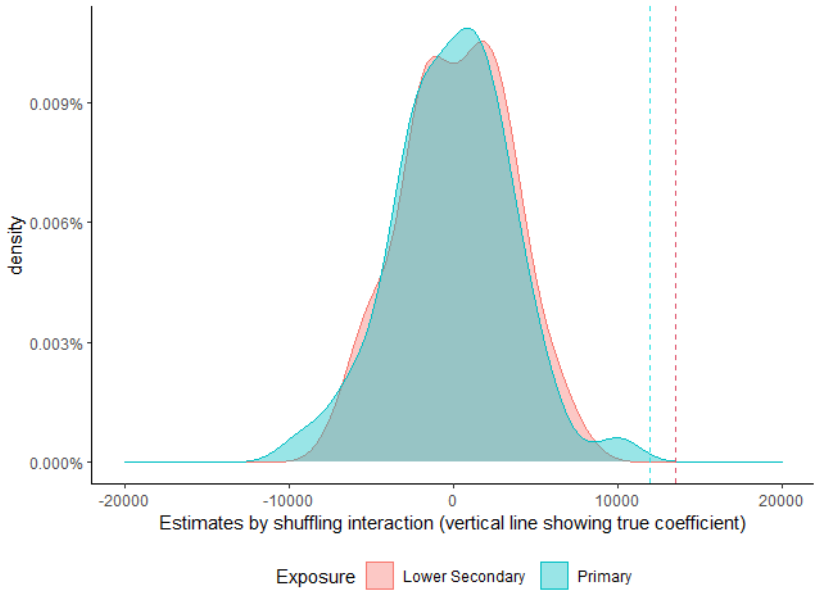
Figure 10: Effect on the Probability of Leaving the Municipality, by year



Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 5. Dots represent the π_g estimates; bars represent both 90% and 95% confidence intervals, clustered at the municipality level. Sample is restricted to individuals born between 1964 and 1983 who resided in a Norwegian municipality in 1985, which had not changed borders.

Robustness Checks

Figure 11: Point Estimates on earnings in 100 regressions after treatment random shuffling



Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 4. Dots represent the π_c estimates; treatment variable was randomly shuffled at the municipality level. Sample is restricted to individuals born between 1964 and 1983 who resided in a Norwegian municipalities in 1985, which had not changed borders. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles.