The Long-Run Impact of School Funding on Economic Outcomes

Daniel Duque*

February 24, 2026

Latest version here

Abstract

Do public investments in compulsory schooling translate into the same long-run economic gains for all? Using a large intergovernmental transfer reform in Norway that mechanically shifted municipal school revenues based on pre-reform student age structure, I estimate the long-run impacts of school funding on human capital, earnings, and family outcomes. First, municipalities primarily allocate additional funds hiring more teachers. Secondly, average earnings effects for students are positive but moderate: nine years of exposure to an additional $100 per pupil per year increases annual earnings by about $200 in the mid-30s, implying an internal rate of return of about 6% and a marginal value of public funds between roughly 1.2 and 2.1. However, these mean effects mask sharp heterogeneity by gender and parental background. The labor-market return is concentrated among men, whose earnings rise by about $380 per year at ages 37–40, while women show little response in own earnings. Economic gains for women are instead realized primarily through the marriage market, experiencing significant increases in partner earnings and couple per-capita income, alongside higher partnership formation. A simple model with skill-increasing labor-market discrimination and gendered norms about partner-provided consumption rationalizes why similar human-capital gains can map into gender-divergent earnings channels. Finally, returns are largest for boys from low-educated families, consistent with partial parental crowd-out of public investments among highly educated households.

JEL Classification: H75, I21, I26, I28

Keywords: Education, Intergovernmental Transfers, School Funding

*Affiliation: Masaryk University (MUNI). Contact information: daniel.duque.econ@gmail.com .
The author gratefully acknowledges financial support from the Research Council of Norway through its Centres of Excellence Scheme, FAIR project No. 262675. The author also acknowledge financial support from the AGEING-CZ project.

1 Introduction

Education is a crucial factor for individual and societal advancement. For individuals, it is linked 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. Yet, despite decades of debate, key uncertainties remain, not only about whether additional resources raise long-run outcomes, but also about for whom they do so, and through which channels. In particular, the distributional effects of education finance policies are central to current policy debates, since the same educational investment can generate very different downstream returns depending on families' private inputs and choices in adulthood.

Unveiling the long-term effects of school funding is challenging due to the limited availability of linked administrative data and the scarcity of plausibly exogenous shocks that can be followed over the life cycle. I overcome these limitations by leveraging an intergovernmental transfer reform that took place in the mid-1980s in Norway. By exploring a plausibly exogenous, mechanical variation on school 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 economic 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 use this setting not only to quantify average returns to school funding but to understand heterogeneity and mechanisms, understanding who benefits, when benefits materialize, and what local governments do with additional resources.

Broad intergovernmental transfers (public grants) represent 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 mandatory school students) to experience a relative increase in education funding. I take advantage of an event-study and a differences-in-differences design to examine the timing of the shock and its effect over time. Conditional on my controls for confounding shocks and fixed effects for municipality and cohort-by-year, my estimates are plausibly exogenous for the main assignment variable. Specifically, the ratio of primary to mandatory school students prior to the reform is main assignment variable.

Leveraging this policy change and estimating the grant shock, I investigate the impact on students who were exposed to more school funding due to the formula change, and how the municipalities used this extra transfer on education spending and school inputs. I show that students who were at the right age to be in compulsory schooling, who were living in municipalities that received higher educational funding in the year prior to the reform, showed higher educational attainment and greater labor income by the age of around 35 years old.

The paper's central contribution is thus twofold: it measures the long-run effectiveness of additional school funding, and it clarifies who it works for, and why. I show that municipalities primarily use additional resources on operational inputs, especially hiring teachers, without meaningful changes in capital spending. At the individual level, exposure to an additional $100 per pupil over nine years of compulsory schooling raises annual earnings by roughly $210 by ages 33–36, with effects that grow over the career profile. Students also increase educational attainment and shift into higher-paying fields (STEM, law, business, or medicine); predicted earnings based on education pathways account for about one-third of the total earnings effect, and cognitive abilities show a modest improvement. These patterns align with the evidence that sustained increases in school spending can yield long-run gains [Jackson and Mackevicius, 2023].

Since average effects may conceal substantial heterogeneity, I show a sharp gender asymmetry: the earnings effect is driven by men, whereas women's gains appear primarily mediated through the marriage market. Though effects on educational outcomes are enhanced for both genders, women's own earnings response is small and statistically indistinguishable from zero. However, their couple's per-capita earnings increase by more than $160 annually at ages 37–40, alongside higher partnership formation. This implies that public investments on women's human capital translate into higher success in the marriage market rather than labor market, consistent with partner matching and within-household specialization channels. Together, these heterogeneous responses imply that additional school funding is particularly effective for boys from low-education backgrounds, while women's benefits are realized largely through family formation and household income.

To rationalize these patterns, I develop a stylized model in which individuals allocate time between labor-market effort, marriage-market effort, and leisure. Human capital raises productivity in both markets, but women face labor-market discrimination that intensifies with skill (reducing the effective return to own earnings as h rises), and social norms generate a higher utility weight on partner-provided consumption for women. The model implies that an exogenous increase in human capital can lead men to realize returns primarily through own earnings, while women optimally realize returns through marriage-market channels and household resources, even when their human capital improves at least as much as men's. This framework clarifies why a school-finance reform can generate large gender differences in earnings channels without requiring gender differences in human-capital formation.

A second major heterogeneity dimension concerns parental background. I show that the impact on earnings and cognitive abilities are greater for men from low-educated families. For students with at least one college-educated parent, the effect of additional school funding on earnings is not statistically significant, suggesting that highly educated parents may partially crowd out public investments, either by compensating at baseline or by substituting private inputs when public inputs rise.

Given the broad and detailed impacts on later-life earnings, I conduct a cost-benefit analysis of increased education funding. The Internal Rate of Return (IRR) is estimated by comparing the costs—including the direct funding increase and its effect on additional educational attainment—against the increase in lifetime earnings from ages 28 to 60. By discounting future earnings and costs to estimate present values, I find that the benefits outweigh the initial and subsequent educational investments up to a discount rate of 5.8%. The IRR is higher for men only (7.5%), especially for those from low-educated families, reaching approximately 10%. Using the Marginal Value of Public Funds (MVPF) framework, I find that for a discount rate between 3% and 5%, the MVPF ranges from 1.2 to 2.1. However, for men whose parents had only compulsory schooling, the MVPF ranges from 3.3 to 5.6.

For robustness checks, I confirm that the funding increase did not lead to higher spending in other major municipal sectors, making it unlikely that the shock was correlated with any policy other than education. Additionally, I narrow the age brackets to isolate potential spurious correlations with demographic composition, and the results remain consistently significant.

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. By leveraging population registers in Norway, I provide long-run monetary outcomes, show how municipalities allocate marginal funds, and highlight heterogeneity by parental education and gender—including marriage-market mediation, thereby clarifying not only whether school funding "works," but for whom it works and why.

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 also brings gender heterogeneity to the center of the evaluation of school funding. Despite substantial convergence (and, in many contexts, reversal) in gender gaps in educational attainment, persistent disparities remain in labor-market outcomes. A growing literature argues that these gaps reflect not only human-capital accumulation but also gender norms, discrimination, occupational sorting, and the interaction between work and family decisions [Cortés et al., 2026]. Recent evidence further emphasizes that women may value human capital partly through family-related margins and the compatibility of careers with fertility, implying that the "returns" to education can operate through channels beyond own wages [Campos et al., 2026]. These insights motivate a natural question in the context of school-finance reforms: when public resources increase children's human capital, do boys and girls translate those gains into adult earnings in the same way, or do gender-specific labor- and marriage-market mechanisms generate divergent economic impacts? This paper contributes by providing robust long-term estimates of increased school funding's impact on earnings, educational paths, and cognitive abilities. Using population-wide register data, I explore both distributional effects and policy cost-effectiveness, particularly for students from disadvantaged 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 is related 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 mainly effects on operational expenditures, with municipalities increasing both the number of teachers, and no effect on capital spending.

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]. This paper shows that, while class sizes remained unchanged, teacher hiring seems to be driving the long-run positive effects on students.

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

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

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 before 1986

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:

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

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 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 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 transfer 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

[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"

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.

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 earnings at ages 33 to 36 and educational attainment, 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 only individuals' monetary output, I also investigate the outcomes mediating this economic effect.

First, the impact of education funding on earnings is also mediated by educational level and field of study. To assess this channel, I use the educational levels and 2-digits groups of degrees, as defined by the Norwegian Central Statistical Bureau, which are listed in the appendix. One of the outcomes using this classification is the likelihood of holding a tertiary education degree of STEM, law, business or medicine, wich is denoted as STEM+.

Additionally, I employ a predictive model focusing on the wages of individuals aged 33 to 36 based on all levels of educationa and their 2-digits groups of degree. 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.

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.

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	5797.5	0.152	0.659	7.69	10.96		18.67
1982	5912.5	0.150	0.651	7.71	10.79		18.54
1983	6050.4	0.148	0.646	7.72	10.62	4.38	18.43
1984	6209.3	0.144	0.637	7.68	10.31	4.71	18.24
1985	6513.6	0.140	0.632	7.65	9.99	4.90	18.18
1986	6706.3	0.136	0.627	7.61	9.36	5.29	17.70
1987	7141.6	0.133	0.627	7.60	8.90	5.59	17.40
1988	7346.4	0.129	0.633	7.59	8.53	5.91	17.17
1989	7403.8	0.125	0.642	7.50	8.41	6.23	17.11
1990	7410.1	0.122	0.653	7.43	8.18	6.40	16.92
1991	7595.4	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 1981 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[5], the average number of public schools declined after 1983.

Table 3 additionally shows descriptive statistics by cohort group, with all variables fixed at ages between 33 and 36. 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 (age 35)	12.8	13.2	13.6	13.9	14.0
Annual Earnings (from age 33 to 36)	30,046.88	32,966.44	37,677.46	41,344.68	43,160.46
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.

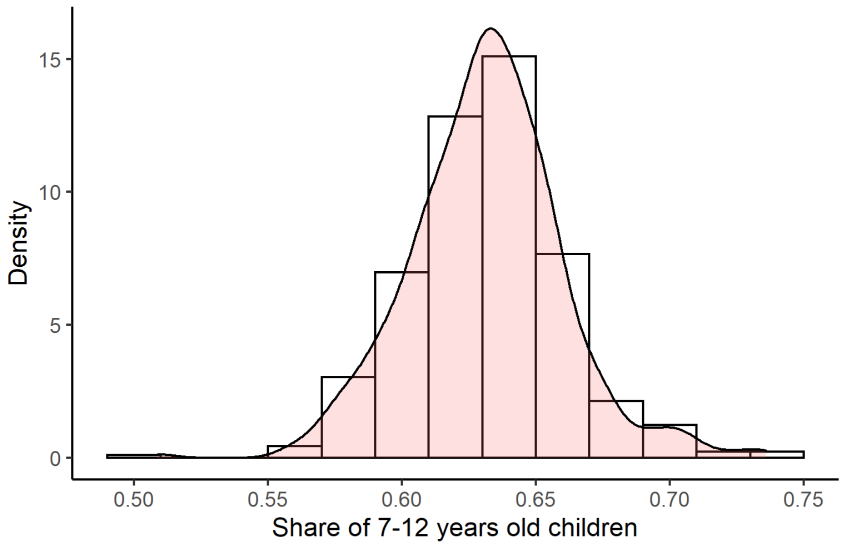
3.2 Empirical Procedure

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%.

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

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



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:

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

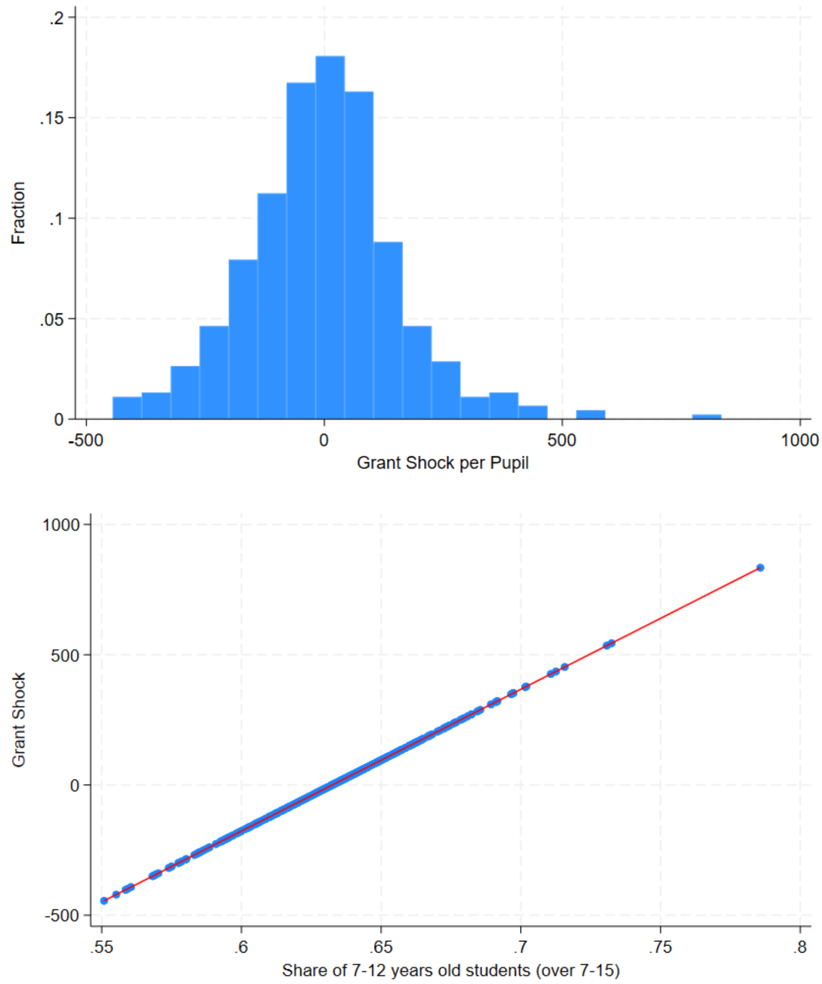
where SW is the number of school weeks per year, reflecting the annual duration of educational activities; H_p and H_s denote the weekly teaching hours for primary and lower-secondary education, respectively; CF_{primary} and $CF_{\text{secondary}}$ represent the pre-reform cost factors for each educational 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), which represents around 1.5% of the total expenditure in 1985. 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:

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

where 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. X_m is a matrix of demographic controls for all criteria that may influence education spending. 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[6] interacted with year. Additionally, I use the 1982-85 average Share of Tax Revenue (as a proportion of all revenues) and the 1981-85 average Share of Education Expenditure (as a proportion of all expenditures), which were part of the criteria for pre-reform grant distribution, both interacted with each year. Since there is 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 transfer reform, used to allocate grants for health services. This variable is constructed using both the Health Sector grant weights and pre-1986 municipal characteristics based on demographics. Finally, I add controls related to municipalities' demographics, namely: the share of children aged 7 to 15, and adults from age 21 to 64, over the total population. The overall size of the population, in log points, is also included.

By non-parametrically tracing out the full adjustment path of the treatment

[6] 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.

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 three three-years periods, I also provide a differences-in-differences analysis with phase-in and full treatment periods, for which I use the following specification:

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

where β_1 and β_2 express the level changes in the grouped years of 1986-88 and 1989-91, respectively. Both will measure the difference from the baseline years of 1983-85.

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.

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 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	Older Cohorts	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	Baseline	18	19	20	21	22	23
1969		17	18	19	20	21	22
1970		16	17	18	19	20	21
1971	Marginally exposed	15	16	17	18	19	20
1972		14	15	16	17	18	19
1973		13	14	15	16	17	18
1974		12	13	14	15	16	17
1975		11	12	13	14	15	16
1976	Exposed at Lower Secondary School	10	11	12	13	14	15
1977		9	10	11	12	13	14
1978		8	9	10	11	12	13
1979	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 primary or lower-secondary school. Children grouped into 'Older Cohorts' will be used to test for pre-trends.

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

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

In addition to the municipal controls and fixed effects discussed earlier, the individual-level analysis will also include gender and foreigner 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[7]. 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. Finally, to pick up any confounding effects with the local labor market characteristics, I also interact year of birth with labor market regions, classified by Statistics Norway, based on information about commuting flows and analogous to European Statistical Office NUTS4 level.

The variable $Shock_m$ will be assigned based on the municipality where the indi-

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

vidual 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 for most students.

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.

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

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 coefficients of interest.

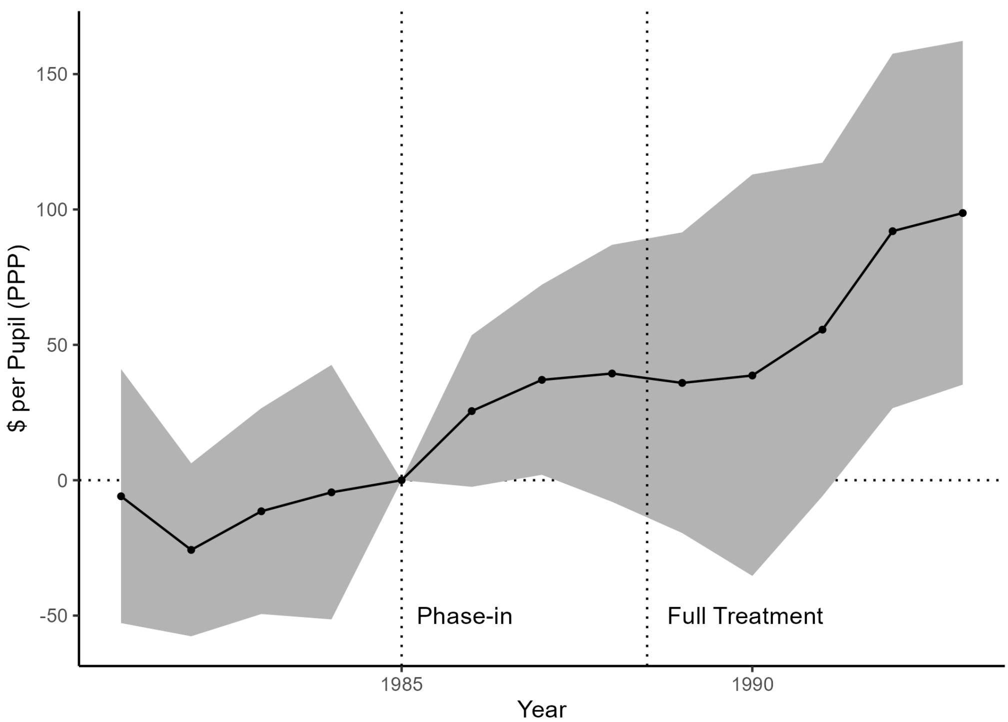
This model imposes a linear structure by interacting the calibrated school funding shock with a continuous variable representing the length of exposure. This approach allows the analysis to examine how the average effect size of the shock varies by each year of exposure. However, a limitation of this 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

Figure 3 shows the municipal response on gross operational expenditures per pupil, year by year, to an increase of $100 in intergovernmental transfers to education. Operational expenditures includes staff compensation and day-to-day supplies, such as teaching materials.

Figure 3: Effect of $ 100 higher grant on Education Gross Operational Expenditures

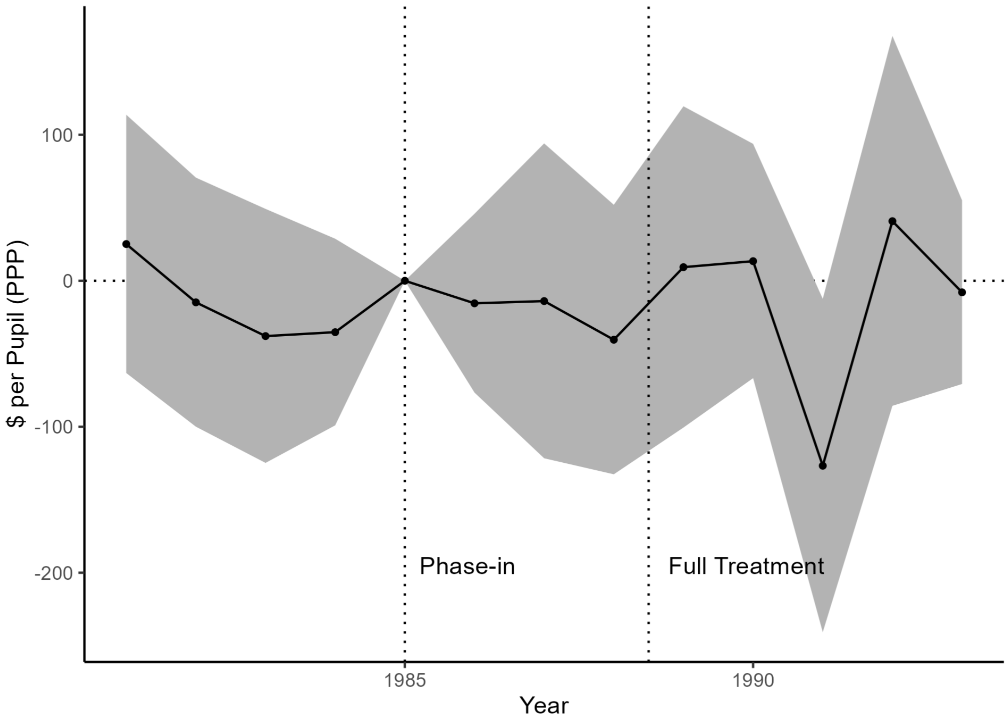


Notes: This figure shows the results from estimating Equation 2. Dots represent the π_q estimates; gray area represent 90% confidence intervals, clustered at the municipality level. Sample is 402 Norwegian municipalities in 1985 that had the same borders throughout the period.

Figure 3 shows that coefficients are mostly flat prior to the baseline year, but they increase starting in 1986, being significantly positive in the early 1990s. This result is expected due to the gradual implementation of the reform, as discussed in subsection 2.3. It is worth noting that the effect of an additional $100 per pupil on the grants corresponds, at the later period, to around $100 on expenditures. The magnitude shows the consistency between the shock size and the spending response.

Figure 4 shows the effect of additional funding on capital and maintenance expenditures. It shows that effect of additional funding is null. Thus, the shock is channeled mostly to payroll, with no response, negative or positive, on other types of expenditures.

Figure 4: Effect of $ 100 higher grant on Education Gross Capital and Maintenance Expenditures



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.

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 hire more teachers. 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) Teachers (log)	(2) Teachers per Pupil	(3) Class Size	(4) Teachers' Education	(5) Teachers' Income (ln)	(6) Number of Schools
Phase-in	0.007**	0.001	-0.0001	0.001	0.018	0.022
(1986-88)	(0.003)	(0.001)	(0.017)	(0.004)	(0.044)	(0.018)
Full Treatment	0.011***	0.002***	0.011	-0.004	-0.041	0.048
(1989-91)	(0.004)	(0.001)	(0.024)	(0.006)	(0.060)	(0.033)
Pre-Treat. Mean		0.107	17.7	14.2		7.6
Number of Mun.	378	402	402	378	378	402
Pre-trend p-value	0.259	0.199	0.018	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, from 1983 to 1991. 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.

4.2 Individual-level Results

After examining the effects of increased educational spending and how these funds were allocated, I now turn to the direct outcomes of this financial shift on the students themselves. Specifically, I investigate whether the additional funding influenced and labor market performance for those who experienced these changes during their schooling years. Table 6 presents the results of our regression analyses, employing both a flexible approach and a linear specification approach based on equations 4 and 5, respectively. I report earnings in two formats: absolute yearly labor income (in 2011 PPP dollars) and labor income rank by cohort (year of birth). Additionally, Graph 12 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 6: Individual-level regressions (aged between 33 and 36)

VARIABLES	(1) Employment Status	(2) Annual Earnings	(3) Income Rank by Cohort
Flexible Specification			
Older Cohorts (1964-67)	-0.000	62.28	0.001
	(0.001)	(67.090)	(0.001)
Marginally Exposed (1971-75)	0.001	97.54	0.002**
	(0.001)	(59.910)	(0.001)
Exposed at Lower-Secondary	0.001	134.9*	0.002
School (1976-78)	(0.001)	(76.870)	(0.001)
Exposed at Primary	0.000	249.2***	0.005***
School (1979-83)	(0.001)	(83.940)	(0.001)
Linear Specification			
9 Years of Exposure	0.000	207.0***	0.004***
	(0.001)	(75.29)	(0.001)
Pre-treatment Mean	0.948	31,561	0.504
Observations	1,024,535	1,013,950	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 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 on annual earnings of $249.6 around the age of 33 to 36, 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. On average, nine years of exposure to an additional $100 per pupil results in an increase of $207 in annual earnings.

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 little less than 1% of the pre-treatment mean, a overall moderate increase.

Building on the investigation on the long-term effects, it is important to understand the underlying processes driving the increase in earnings. I analyze the influence of school funding on education outcomes, examining how these factors contribute to the economic outcomes observed.

Table 7 displays regression results for educational attainment, the probability of holding a degree in STEM (in addition to Law, Business & Medicine), cognitive abilities (in IQ scale) and expected earnings by education path. 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 7: Potential Educational Outcomes

VARIABLES	(1) Years of Study	(2) College Diploma	(3) STEM+	(4) Expected Wage	(5) Cognitive Abilities
Flexible Specification					
Older Cohorts (1964-67)	-0.004	-0.0001	0.0003	-26.89	-0.063
	(0.009)	(0.002)	(0.001)	(30.03)	(0.087)
Marginally Exposed (1971-75)	0.001	-0.0001	-0.0001	11.15	0.019
	(0.009)	(0.002)	(0.001)	(26.97)	(0.079)
Exposed at Lower-Secondary	0.004	0.002	-0.002	-15.32	-0.062
School (1976-78)	(0.011)	(0.002)	(0.001)	(32.77)	(0.094)
Exposed at Primary	0.026**	0.004**	0.003**	86.82***	0.108
School (1979-83)	(0.012)	(0.002)	(0.001)	(33.50)	(0.086)
Linear Specification					
9 Years of Exposure	0.027***	0.005**	0.003***	93.83***	0.127*
	(0.010)	(0.002)	(0.001)	(29.47)	(0.065)
Pre-Treatment Mean	12.99	0.327	0.122	10,212.5	100.5
Observations	1,024,535	1,024,535	1,024,535	1,024,535	496,654

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. Expected wage is measured with a mincerian regression on level-specification attainment, as specified in section 3. All outcomes are measured by the age of 35, with the exception of cognitive abilities, which are measured in IQ scale by the military draft for men, from ages 18 to 19. *** p<0.01, ** p<0.05, * p<0.1

According to the linear specification approach, students exposed to 9 years of additional $100 dollars school funding have higher 0.027 years of study by the age of 35, which corresponds to an increase in 0.005 of the likelihood of holding a college degree. They also have higher a likelihood of holding a college diploma in STEM or law, business or medicine. Their educational paths lead to an expected earnings increase of about $90 in their annual earnings. Their cognitive abilities also increase by 0.13 IQ points.

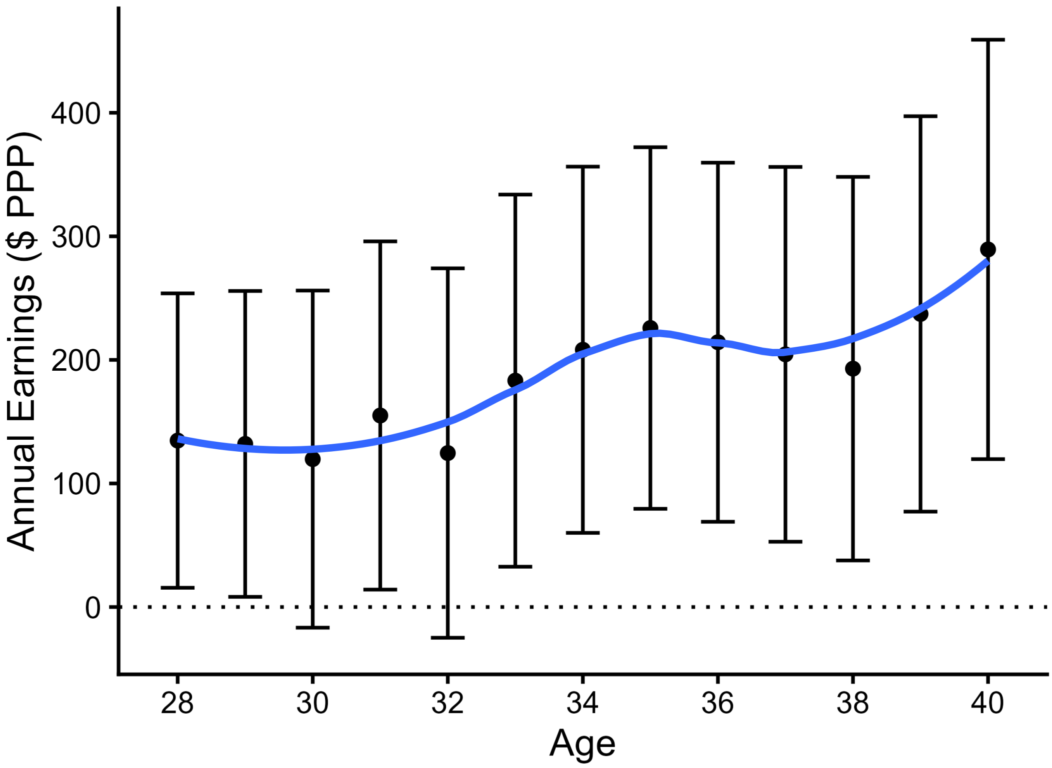
The literature, documented by Jackson and Mackevicius [2023], reports that a sustained increase of $1,000 in per-pupil school spending over four years typically results in a 0.0539 increase in the probability of obtaining a college degree, with most effects ranging between 0.05 and 0.5. My estimates indicate a proportionally lower effect. However, it is important to clarify that these are intention-to-treat estimates, as detailed in subsection 3.2.1. Additionally, a linear relationship between the funding increase and educational outcomes may not fully capture the observed data.

However, my results show that not only educational attainment increases, but students also are more likely to graduate in subjects that are better paid (STEM, law, business or medicine). Their educational paths lead to higher earnings that account to to about one third of their total increase in earnings showed in table 6.

In additional, my results also show some small effects on cognitive abilities. The effect size os consistent with the meta-analysis by Ritchie and Tucker-Drob [2018], which suggests that one additional year of study can raise cognitive abilities by as little as 1 to 5 IQ points per additional year. That is, if a linear relationship could be trusted, the funding shock leading to a full year of study would also lead to an increase of about 4 IQ points.

It is also important to understand how the effects on earnings evolve over time, drawing a path of school funding benefits throughout an individual's career. In order to elucidate the temporal dimension of these impacts, Figure 5 presents the estimated effects of higher exposure to the educational funding shock on earnings across various age groups, focusing on the linear specification approach.

Figure 5: Effect on Earnings by Age



Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 5. Standard errors 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.

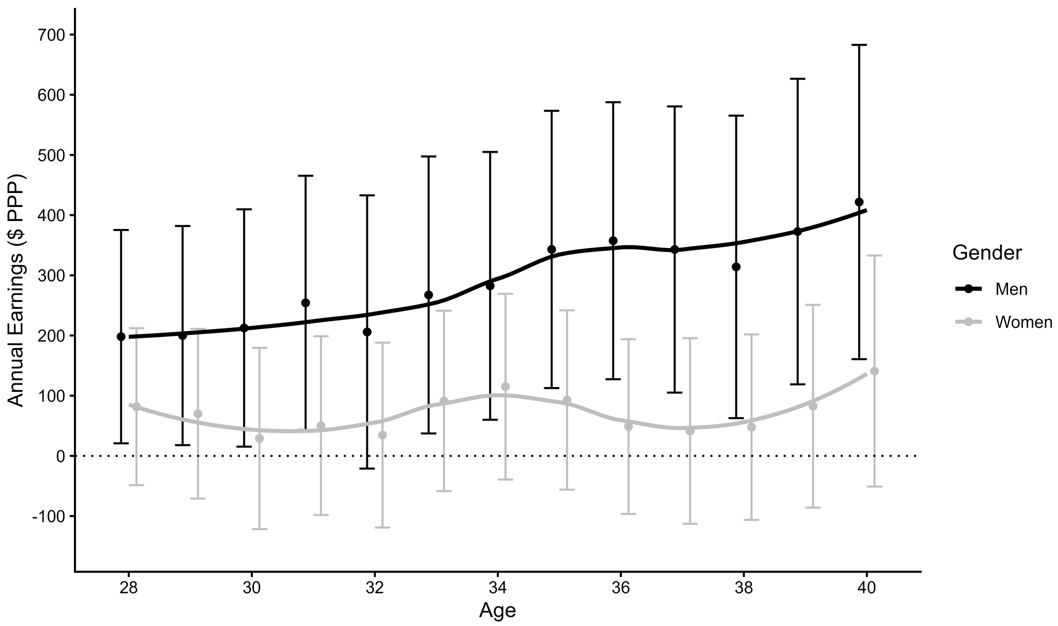
Figure shows that the effects increase with age, suggesting cumulative benefits over time. For the youngest age group, the impact of additional $100 on school funding over nine years is positive but not statistically significant. The effect becomes statistically significant and larger at older ages. The magnitude of the effect peaks by the ages of 39-40. This highlights the importance of early educational investment for long-term earnings potential.

Those results connect the micro-level outcomes of increased funding to the full economic impact. In summary, they illustrate how educational and cognitive channels of influence long-term income benefits.

4.3 Gender differences on Earnings and Family Formation

A natural heterogeneity to examine is whether additional educational funding is absorbed differently by men and women. To highlight these differences, I begin by tracing the age profile of the earnings effects separately for men and women.

Figure 6: Effect of Educational Funding on Own Annual Earnings, by Age and Gender



Notes: This figure plots estimates of the effect of nine years of exposure to an additional $100 per pupil per year on *own* annual earnings, separately by gender and by age (28–40). Dots represent point estimates and vertical bars represent 95% confidence intervals. Annual earnings are measured in dollars, adjusted for 2011 PPP (excluding the 1st and 99th percentiles). Standard errors are clustered by municipality of residence in 1985.

Figure 6 shows a striking divergence between men and women. For men, the effect is positive already by the late 20s and grows steadily through the 30s, reaching over $400 by age 40. For women, the point estimates are much smaller throughout the life cycle and are imprecisely estimated, with no confidence intervals above zero. In other words, the dynamic profile indicates that the direct labor-income gains from additional school funding are concentrated among men, while women exhibit, at most, modest changes in their own earnings even in mid-career ages.

Since the previous subsection shows no statistically significant effects of the funding shock on women's *own* earnings, a natural question is whether the school-funding increases were simply ineffective at enhancing girls' human capital. If public investments did not raise female attainment, specialization, or skills, then the absence of earnings gains would be mechanically unsurprising. To evaluate this possibility, I examine whether the same funding shock that raises men's earnings also affected women's educational trajectories and related skill measures.

Table 8 reports the effects of nine years of exposure to the funding shock on a set of human-capital outcomes, estimated separately for women and men. The outcomes again include years of schooling, completion of higher education, the probability of completing a tertiary degree in high-paying fields (STEM, law, business, or medicine; "STEM+"), and the implied *expected education premium* based on education level and field of study (as described in Section 3). Cognitive ability (IQ) is measured at military draft and is therefore available primarily for men.

Table 8: Effects of School Funding on Human Capital Outcomes, by Gender

	Years of Schooling	Higher Education	STEM+ Degree	Expected Education Premium	Cognitive Abilities
Panel A: Women					
9 Years of Exposure	0.031**	0.005**	0.004***	88.730**	—
	(0.014)	(0.002)	(0.001)	(36.300)	
Observations	499,447	499,923	499,923	499,923	—
Pre-treatment mean	13.120	0.367	0.100	9,623	—
Panel B: Men					
9 Years of Exposure	0.020	0.003	0.001	87.390**	0.127*
	(0.014)	(0.002)	(0.002)	(42.600)	(0.066)
Observations	523,838	524,612	524,612	524,612	496,654
Pre-treatment mean	12.910	0.294	0.147	10,923	100.600

Notes: Each coefficient reports the effect of nine years of exposure to an additional $100 per pupil per year (linear specification; Equation 5). Standard errors (in parentheses) are clustered by municipality of residence in 1985. Outcomes are measured by age 35, except cognitive abilities, which is measured at military draft ages 18–19 and is available primarily for men, scaled in IQ points. "Higher Education" is an indicator for holding a tertiary degree. "STEM+" indicates a tertiary degree in STEM, law, business, or medicine. "Expected Education Premium" is the predicted earnings premium based on education level and field of study as described in Section 3. Monetary units are 2011 PPP dollars. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The evidence suggests that the absence of own-earnings effects for women is not driven by a failure of school funding to raise girls' human capital. If anything, women's educational responses are at least as large as men's, and in some dimensions stronger. The funding shock significantly increases women's years of schooling and their likelihood of completing higher education and STEM+ degrees. For men, the point estimates on educational attainment are smaller and not statistically significant, although men exhibit a comparable increase in the expected education premium and a modest but significant increase in cognitive ability.

Taken together, these findings imply that school funding did improve girls' human capital, but these gains did not translate into higher earnings in the labor market on average. This divergence immediately raises an important interpretation issue: if women do not translate increased human capital into higher *own* earnings, they may nonetheless benefit through other channels that affect economic welfare, particularly those operating at the household level. A natural candidate is the marriage market. If human capital increases women's match quality or the probability of partnership formation/retention, the gains from schooling may appear more clearly in partner earnings and in household resources, even when own earnings respond weakly. This motivates complementing the individual-earnings analysis with measures of couple-level income and family formation.

To summarize these patterns at ages where individuals are well established in both the labor market and family life, Table 9 reports pooled estimates at ages 37–40, focusing on both individual earnings and couple per-capita earnings.

Table 9: Effects of Educational Funding on Earnings by Gender (Ages 37–40)

	(1)	(2)	(3)	(4)
	Own Earnings		Couple's Per Capita Earnings	
Gender	Women	Men	Women	Men
Linear Specification				
9 Years of Exposure	79.33	380.6**	163.0*	236.3**
	(101.0)	(163.9)	(96.90)	(92.38)
Observations	499,923	524,612	479,912	499,551
Pre-treatment Mean	30,971	47,519	33,546	33,648

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. *** p<0.01, ** p<0.05, * p<0.1

Consistent with the dynamic patterns in Figure 6, the pooled estimates show that the effects on own earnings are essentially driven by men. Exposure to an additional $100 per pupil per year (over nine school years) raises men's annual earnings by about $381, roughly 0.8% of the male baseline at ages 37-40. In contrast, women's own-earnings response is small and statistically indistinguishable from zero.

However, the absence of a detectable effect on women's own earnings does not imply that women do not benefit economically. Rather, their gains appear to be mediated through the marriage market and realized at the household level. When household resources are measured as the average of the woman's and her partner's earnings at ages 37–40, women's couple per-capita income increases by about $163 (significant at the 10% level), exceeding $160 annually and amounting to approximately 0.5% of the baseline. For men, couple-level earnings also rise ($236, about 0.7% of the baseline), but men's own earnings effect is larger than their couple-level effect, underscoring that the direct labor-market channel dominates for men, whereas the household channel is pivotal for women.

Taken together, the life-cycle evidence in Figure 6 and the pooled results in Table 9 motivate the remainder of this subsection: because women do not exhibit strong own-earnings gains, I investigate whether the funding shock affects women's economic outcomes through family formation, and whether these marriage-market responses help reconcile the gender gap in own-earnings effects with positive household-level gains.

Two margins move in ways consistent with marriage-market mediation. On the partnership margin, women exposed to higher school funding are more likely to have a spouse by ages 37–40, especially one with higher earnings. The estimate for having a spouse for men, however, is slightly smaller and not statistically significant, while for having one with higher earnings decreases, with a significance of 90%. This differential is exactly where one would expect couple-level earnings to pick up income effects for women—through increased partnership formation/retention and assortative matching on partners' earnings potential.

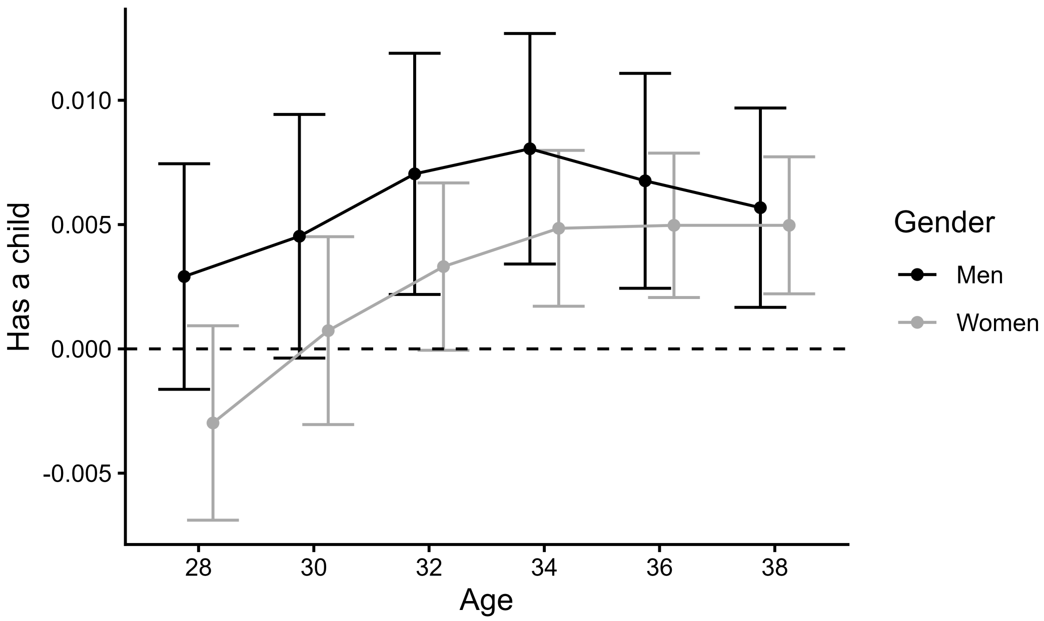
Table 10: Effects of Educational Funding on Partnership and Fertility (Ages 37–40)

Gender	(1) Having a Spouse	(2) Having a Spouse with Higher Earnings	(3) Having a Child	(4) Number of Children
Linear Specification: 9 Years of Exposure				
	0.004**	0.005**	0.005**	0.018***
Women	(0.002)	(0.002)	(0.002)	(0.005)
	0.003	-0.002*	0.006***	0.014**
Men	(0.002)	(0.001)	(0.003)	(0.007)
Pre-Treatment Mean				
Women	0.679	0.382	0.682	1.95
Men	0.640	0.0365	0.612	1.58
Observations				
Women	485,019	499,923	499,923	499,923
Men	504,622	524,612	524,612	524,612

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. *** p<0.01, ** p<0.05, * p<0.1.

On the fertility margin, both genders exhibit an increased probability of having a child by age of 37, but the timing and magnitude differ: men show a slightly larger effect, of about 1%, while women's increase is +0.007. The event-time profile in Figure 7 shows men's effects emerging in the late 20s, while women's become clearly significant in the mid-30s. This sequencing is consistent with education-induced delays and opportunity-cost dynamics for women, together with partner-matching dynamics that materialize later.

Figure 7: Effect of Educational Funding on Probability of Having a Child, by Age and Gender



Notes: This figure shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 5. Standard errors 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.

Taken together, these margins suggest the following mechanism: the funding shock raises men's earnings capacity directly and earlier, which in turn increases the probability of partnership and earlier childbearing on the male side. For women, the funding shock appears to raise household resources primarily via partner quality/earnings and partnership formation, with own earnings effects muted on average by mid-to-late 30s. In short, the household channel makes a quantitatively meaningful contribution to women's welfare even when their own earnings response is modest.

Overall, the gender heterogeneity shows direct labor-market returns for men and indirect, marriage-market–mediated returns for women. Accounting for both channels reveals broader welfare benefits of school funding than own-earnings effects alone would suggest.

The gender asymmetries documented in Tables 9 and 10 and in Figure 7 fit squarely within a growing body of work arguing that returns to human capital can be gendered, not only because men and women may face different constraints in the labor market, but also because they can respond to improved skills through different margins, including family formation and intra-household specialization. In our setting, the funding shock increases men's own earnings, while women's gains appear primarily at the household level through partner earnings.

A useful lens for interpreting these results is the framework in Cortés et al. [2026], which emphasizes that gender norms operate through multiple channels: internalized norms (preferences and beliefs tied to gender identity) and external norms (peer pressure and conformity incentives), interacting with labor-market opportunities, public policies, and discrimination. In this view, even if an education policy increases human capital similarly across genders, downstream outcomes can diverge because norms and anticipated constraints shape how individuals convert skills into labor supply, job choices, and family decisions. Importantly, the same framework highlights how norms around caregiving and motherhood can be particularly consequential for women's labor-market outcomes, consistent with evidence that child-related penalties and beliefs about working mothers are strongly associated with female labor-force participation and gender gaps across contexts.

The marriage-market channel is also closely related to recent work on education choices and gender gaps. Campos et al. [2026] show that women place relatively greater weight on balancing career and family in human-capital decisions, and that such preference differences can account for a substantial share of gender earnings gaps, underscoring that returns to education are multidimensional: individuals value not only pecuniary payoffs but also non-pecuniary attributes that affect fertility timing and work–family compatibility. Through this lens, a funding-induced increase in skills can raise women's welfare even if it does not translate one-for-one into higher own earnings, because the additional human capital can improve match quality in the marriage market, alter partnership dynamics, and relax household budget constraints through the partner's earnings.

4.4 Impact across earnings distribution and parental education

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. 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 further investigate this hypothesis, it is important to understand whether the effects of additional resources for education observed in the previous sections were experienced across students from different backgrounds.

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 11, indicate that the benefits of increased funding are predominantly observed among students from lower-SES backgrounds.

Table 11: Results by Parental Education

	(1)	(2)	(3)
Linear Specification			
Parental Education	Compulsory	Upper Secondary	Tertiary
9 Years of Exposure	438.0***	189.1**	56.10
	(165.9)	(91.29)	(134.5)
Pre-Treatment Mean	27,411.73	31,269.29	35,946.82
Relative effect	0.013	0.010	0.001
Observations	161,766	519,433	300,107

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 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 parental maximum educational attainment. *** p<0.01, ** p<0.05, * p<0.1

Results show that students with lower-educated parents drive the results. Only students with parents holding a college diploma show no significant effect, while the higher impact is found for students with parents holding only mandatory school diploma, more than double for those with at least one parent with high school diploma. This results indicates that public investments are at least partly crowded-out by more affluent parents. Parents may compensate with private resources when funding is low, whereas they may also perceive increases in public investments and reduce their own.

Importantly, this parental-education gradient is closely intertwined with the gender heterogeneity documented in Section 4.3. To make this explicit, Table 12 decomposes the results by gender. It shows that the parental-education gradient in own earnings is overwhelmingly driven by men. For those, the effect is very large for those from low-educated families ($823), declines substantially for those from upper-secondary families ($257), and is essentially zero for men from tertiary-educated households ($51). In contrast, among women the estimates are small and statistically indistinguishable from zero across all parental-education groups.

Table 12: Earnings Effects by Parental Education and Gender

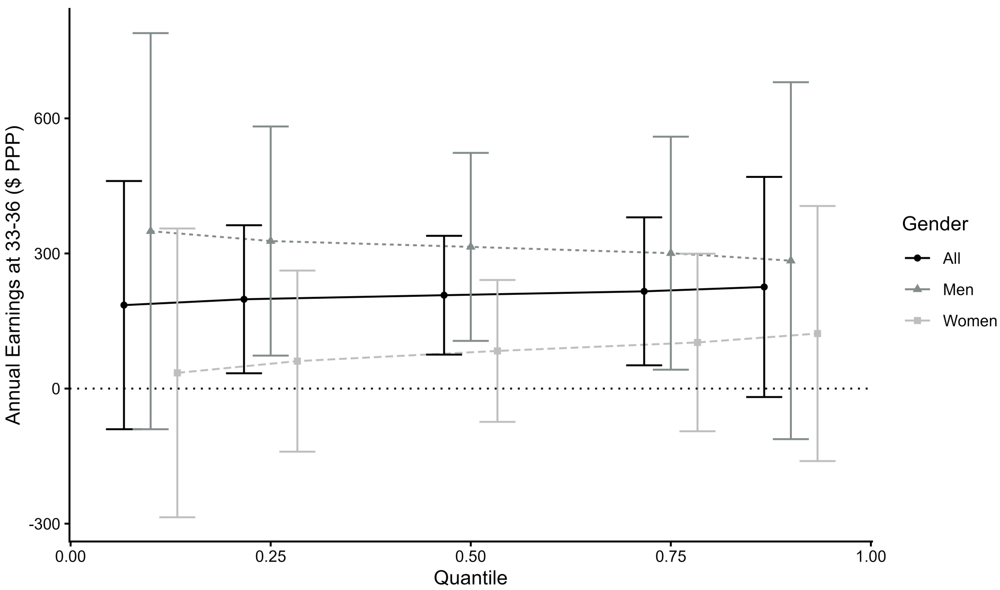
	Highest Parental Education		
	Compulsory	Upper Secondary	Tertiary
Panel A: Men			
9 Years of Exposure	822.6***	257.0*	50.83
	(291.8)	(142.7)	(209.2)
Observations	85,370	271,807	157,906
Panel B: Women			
9 Years of Exposure	22.94	98.57	80.12
	(165.3)	(96.44)	(153.0)
Observations	81,378	262,769	154,720

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 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 parental maximum educational attainment. *** p<0.01, ** p<0.05, * p<0.1

The gender and parental-education splits sharpen the interpretation of heterogeneity in the returns to public school funding. First, they reinforce the crowd-out interpretation: the marginal effect of additional public resources is strongest where baseline private investments are likely to be lowest, namely among low-educated households. Second, they show that this "high-return" margin is concentrated among men, consistent with the broader evidence that the direct labor-market channel is primarily a male channel in our data.

Norway's extensive population-wide registers also allow me to employ quantile regressions, providing new insights into how the gains from increased school funding are distributed across the earnings distribution. Quantile regression analysis, as outlined by Machado and Silva [2019], makes it possible to estimate treatment effects at different points of the conditional earnings distribution, rather than only at the mean. Focusing on the linear specification approach, I estimate quantile regressions at five points (0.1 to 0.9) not only for the full sample, but also separately by gender.

Figure 8: Quantile treatment effects on annual earnings, by gender



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. Earnings outliers excluded. Annual earnings are measured in dollars, adjusted for 2011 purchasing power parity (PPP), excluding 1st and 99th percentiles. Estimates are based on the linear specification and are reported separately for the full sample, men, and women.

In the pooled sample, the point estimates are positive across the distribution and fairly stable, with a mild upward slope from about $185 at the 10th percentile to about $226 at the 90th percentile. While these pooled results might suggest a slightly regressive pattern, the gender-specific quantile regressions reveal that the aggregate profile masks substantial heterogeneity across men and women.

For men, the pattern is instead mildly *progressive*: the estimated earnings gains are largest at the lower quantiles and gradually decline toward the upper tail of the male earnings distribution (from about $350 at the 10th percentile to about $284 at the 90th percentile). This is consistent with the earlier heterogeneity results showing that the returns to school funding are concentrated among boys from low-educated families, who are disproportionately represented in the lower part of the earnings distribution. For women, the quantile estimates are small and statistically indistinguishable from zero throughout, even though the point estimates display a regressive slope (increasing with quantiles). Taken together, these results reinforce the broader message of the gender heterogeneity analysis: the direct labor-income channel operates primarily for men, whereas women's gains are more likely to be realized through household-level mechanisms (e.g., partner earnings and partnership formation) rather than through their own earnings.

5 Model: Discrimination Increasing in Skill and Gendered Social Norms

In order to understand the difference response of the funding shock between men and women, this section presents a stylized framework in which an individual allocates time across the labor market, the marriage market, and leisure. The model is designed to capture two forces: (i) *labor-market discrimination that intensifies with human capital*, so that converting skill into own earnings becomes relatively harder for women at higher levels of h; and (ii) *gendered social norms* under which women experience less disutility from consumption being provided by a partner, modeled as $\lambda_F > \lambda_M$. Together, these features generate the key qualitative pattern: increases in human capital lead men to specialize relatively more in labor-market earnings, while women's gains are more likely to be realized through the marriage market and household resources.

5.1 Time Allocation

An agent of gender $g \in \{F, M\}$ chooses time allocations to labor-market effort w, marriage-market effort m, and leisure l:

$$w \geq 0, \quad m \geq 0, \quad l \geq 0, \qquad w + m + l = 1. \tag{6}$$

Human capital is denoted by $h > 0$, which can be interpreted as being increased by public investments in schooling.

5.2 Preferences and Consumption

Utility is Cobb–Douglas in consumption and leisure:

$$U(c, l) = c^{\alpha} l^{1-\alpha}, \qquad \alpha \in (0, 1). \tag{7}$$

Consumption is proxied by household per-capita earnings. Partner-provided resources are discounted by a gender-specific weight $\lambda_g \in [0, 1]$:

$$c_g = \frac{y_g(h, w) + \lambda_g \, \mathbb{E}[y_p \mid h, m]}{2}, \qquad \text{with } \lambda_F > \lambda_M. \tag{8}$$

The inequality $\lambda_F > \lambda_M$ captures social norms under which women place relatively higher utility weight on consumption financed by the partner (i.e., lower dependence disutility), whereas men attach a lower weight to partner-provided resources.

5.3 Earnings Technologies

Own earnings (labor market) with discrimination increasing in h. Own earnings are increasing in human capital and labor-market effort but are multiplied by a gender-specific factor $a_g(h) \in (0, 1]$ that captures discrimination and becomes more severe for women as h rises:

$$y_g(h, w) = \omega\, h\, a_g(h)\, f(w), \qquad f'(w) > 0, \;\; f''(w) < 0, \qquad \omega > 0. \tag{9}$$

I impose:

$$a_M(h) = 1, \qquad a_F'(h) < 0. \tag{10}$$

Assumption (10) implies that the gender gap in labor-market earnings increases with human capital: as h grows, women face a lower effective return to labor effort relative to men, consistent with discrimination that intensifies with skill (e.g., glass-ceiling effects, occupational barriers, or differential promotion).

Partner earnings (marriage market). Expected partner earnings are produced through marriage-market effort and are scaled by the agent's human capital:

$$\mathbb{E}[y_p \mid h, m] = \widehat{y}_p + h\, g(m), \qquad g'(m) > 0, \;\; g''(m) < 0. \tag{11}$$

This specification captures that human capital improves match quality (or partner earnings potential) and that marriage-market effort m is productive, but with diminishing returns.

5.4 Optimality Conditions

The agent chooses (w, m) to maximize $\log U = \alpha \log c_g + (1 - \alpha) \log l$, where $l = 1 - w - m$. For an interior solution, the first-order conditions are:

$$\alpha \frac{c_w}{c_g} = \frac{1 - \alpha}{l}, \tag{12}$$

$$\alpha \frac{c_m}{c_g} = \frac{1 - \alpha}{l}. \tag{13}$$

Combining (12)–(13) yields a convenient characterization: the optimal split between labor-market and marriage-market effort equates their marginal contributions to consumption,

$$c_w = c_m. \tag{14}$$

From (8)–(11),

$$c_w = \frac{1}{2}\frac{\partial y_g(h,w)}{\partial w} = \frac{1}{2}\omega h a_g(h) f'(w), \qquad c_m = \frac{1}{2}\lambda_g \frac{\partial \mathbb{E}[y_p \mid h, m]}{\partial m} = \frac{1}{2}\lambda_g h g'(m).$$

Substituting into (14) and canceling h gives the key condition:

$$\omega\, a_g(h)\, f'(w^*) \;=\; \lambda_g\, g'(m^*). \tag{15}$$

Equation (15) shows that the relative attractiveness of marriage-market effort is governed by the ratio $\lambda_g/a_g(h)$. Gendered norms ($\lambda_F > \lambda_M$) increase women's incentive to invest in the marriage market, while discrimination increasing in h (declining $a_F(h)$) further shifts women away from labor effort as human capital rises.

5.5 Comparative Statics and Interpretation

Equation (15) delivers two sharp implications.

1) Gender differences in specialization. Holding h fixed, women place greater weight on partner income ($\lambda_F > \lambda_M$), which increases the right-hand side of (15) for women and raises the optimal marriage-market effort m^* relative to labor effort w^*. In addition, since $a_M(h) = 1$ and $a_F(h) < 1$, women face a lower marginal return to labor-market effort, further shifting time allocation toward the marriage market.

2) Human-capital shocks tilt women further toward the marriage market. Because discrimination intensifies with skill ($a_F'(h) < 0$), an increase in h reduces $\omega a_F(h)$ and thus lowers the marginal return to labor effort for women, all else equal. From (15), restoring equality requires increasing m^* and/or reducing w^* for women. For men, $a_M(h) = 1$ is constant, so the labor-vs-marriage incentive does not shift with h through discrimination. Therefore, a funding-induced increase in h generates a differential response: men's gains are more likely to appear through own earnings (higher labor-market productivity), while women's gains are more likely to appear through household resources via match quality and partner earnings.

Parametric illustration. Let $f(w) = w^\gamma$ and $g(m) = m^\delta$, with $\gamma, \delta \in (0, 1)$. Then $f'(w) = \gamma w^{\gamma-1}$ and $g'(m) = \delta m^{\delta-1}$, and (15) becomes:

$$\omega a_g(h)\gamma(w^*)^{\gamma-1} = \lambda_g \delta (m^*)^{\delta-1}. \tag{16}$$

Under $\gamma = \delta$ for simplicity, the ratio of marriage-market to labor-market effort is:

$$\frac{m^*}{w^*} = \left(\frac{\lambda_g}{\omega a_g(h)}\right)^{\frac{1}{1-\gamma}}. \tag{17}$$

For men, $a_M(h) = 1$, so m^*/w^* is pinned down by λ_M and does not vary with h through discrimination. For women, $a'_F(h) < 0$ implies that m^*/w^* increases with h: as human capital rises, women optimally tilt further toward marriage-market effort.

3) Response to a school-funding shock: Δh and the induced change Δa. To connect the model more directly to the empirical setting, consider a school-funding reform that increases human capital by $\Delta h > 0$. The key feature of the model is that the reform affects women not only through the standard "higher h" channel, but also through an *endogenous* change in the discrimination factor $a_g(h)$. For men, $a_M(h) = 1$ implies

$$\Delta a_M \equiv a_M(h + \Delta h) - a_M(h) = 0.$$

For women, discrimination intensifies with skill, so $a'_F(h) < 0$ implies

$$\Delta a_F \equiv a_F(h + \Delta h) - a_F(h) \;\approx\; a'_F(h)\,\Delta h \;<\; 0.$$

Using the parametric expression in Equation (17), define the optimal specialization ratio

$$r_g(h) \equiv \frac{m_g^*(h)}{w_g^*(h)} = \left(\frac{\lambda_g}{\omega a_g(h)}\right)^{\frac{1}{1-\gamma}} \qquad (\gamma = \delta).$$

Taking logs and differentiating yields a transparent mapping from changes in discrimination to changes in specialization:

$$\Delta \ln r_g \;=\; -\frac{1}{1-\gamma}\,\Delta \ln a_g(h) \;\approx\; -\frac{1}{1-\gamma}\,\frac{\Delta a_g}{a_g(h)}. \tag{18}$$

Equation (18) implies that a reduction in $a_g(h)$ increases the marriage-market intensity $r_g(h)$. Therefore, a human-capital shock has *asymmetric* effects across genders:

$$\Delta \ln r_M = 0, \qquad \Delta \ln r_F > 0 \;\text{ since } \Delta a_F < 0.$$

In words, the school-funding shock does not change men's marriage-versus-work tilt through the discrimination channel, but it *raises* women's marriage-market tilt because the effective labor-market return becomes relatively less favorable as h increases.

To see how this translates into time allocations, note that given leisure l and the ratio $r_g = m_g^*/w_g^*$, the time constraint implies

$$w_g^* = \frac{1 - l_g^*}{1 + r_g}, \qquad m_g^* = \frac{r_g(1 - l_g^*)}{1 + r_g}.$$

Holding fixed the overall time spent outside leisure, an increase in r_g mechanically reallocates time toward the marriage market:

$$\frac{\partial w_g^*}{\partial r_g} = -\frac{1 - l_g^*}{(1 + r_g)^2} < 0, \qquad \frac{\partial m_g^*}{\partial r_g} = \frac{1 - l_g^*}{(1 + r_g)^2} > 0.$$

Thus, the funding-induced decline in $a_F(h)$ predicts $\Delta w_F^* < 0$ and $\Delta m_F^* > 0$ (conditional on total non-leisure time), while $\Delta r_M = 0$ implies no analogous reallocation for men through this channel.

Finally, these comparative statics help reconcile differential earnings responses. Women's *own* earnings,

$$y_F(h, w) = \omega\, h\, a_F(h)\, f(w),$$

are affected by three forces: the direct increase in h (positive), the decline in $a_F(h)$ (negative), and the shift away from labor effort w (negative). This makes women's own-earnings response naturally attenuated or even close to zero. By contrast, expected partner earnings,

$$\mathbb{E}[y_p \mid h, m] = \widehat{y}_p + h\, g(m),$$

benefit from both higher h and higher marriage-market effort m, implying a stronger household-income channel for women. For men, since $\Delta a_M = 0$, the standard human-capital channel dominates, leading to larger effects on own earnings.

5.6 Mapping to Empirical Patterns

The model rationalizes two empirical features observed in the data. First, when labor-market discrimination intensifies with skill, increases in human capital translate more strongly into men's own earnings than women's. Second, when social norms imply $\lambda_F > \lambda_M$, women value partner-provided consumption relatively more, strengthening the marriage-market channel. Consequently, evaluations that focus only on own earnings may understate women's welfare gains from schooling investments, while household-level measures (e.g., couple income) capture benefits operating through partner earnings and match quality.

6 Cost-benefit analysis

6.1 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:

$$Cost = \sum_{t=7}^{15} \frac{100}{(1+r)^{t-6}} + \sum_{t=16}^{22} \frac{\mathbb{E}(\Delta Educ)}{(1+r)^{t-6}}, \tag{19}$$

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[8]. 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:

$$Benefit = \sum_{t=28}^{60} \frac{\Delta Y}{(1+r)^{t-6}}, \tag{20}$$

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:

$$\sum_{t=28}^{60} \frac{\Delta Y}{(1+r_{max})^{t-6}} \geqslant \sum_{t=7}^{15} \frac{100}{(1+r_{max})^{t-6}} + \sum_{t=16}^{22} \frac{\mathbb{E}(\Delta Educ)}{(1+r_{max})^{t-6}} \tag{21}$$

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 33-36) to estimate IRR due to the strong correlation

[8] 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.

with lifetime earnings during this period.

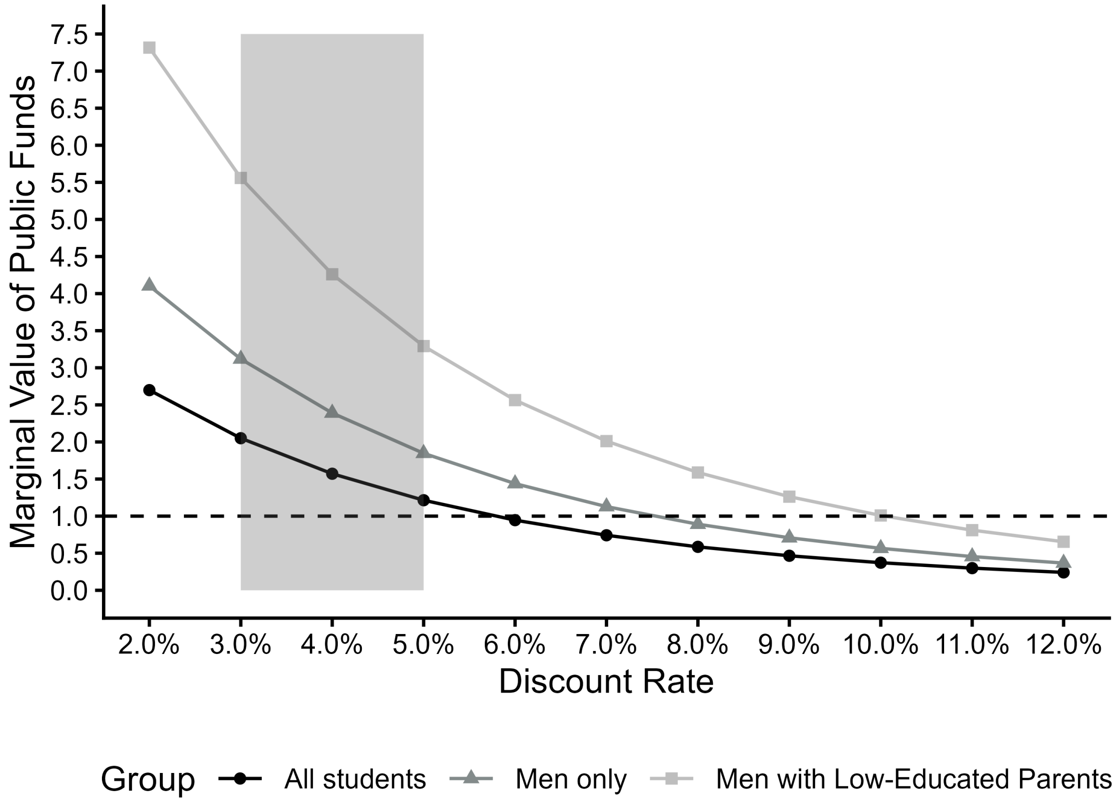
Based on these calculations, and as detailed in Table 6, the policy yields an IRR of 5.8% for the general population, increasing to 7.5% for men only and to 10% for men with low-educated parents.

6.2 Marginal Value of Public Funds

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 formula for the MVPF divides the benefits, expressed in equation 20, by the costs, expressed in equation 19.

Figure 9 shows that the MVPF ranges from 1.2 to 2.1, which means that society receives between $1.2 and $2.1 in benefits for every $1 in public investments in school funding. 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 overall moderately larger than the costs. However, when considering men only MVPF increases to a range between 1.9 and 3.1, while for men with low-educated parents, MVPF increases to a range between 3.3 and 5.6, which means that the economic returns of school funding is at least three times its costs when targeted to those students.

Figure 9: Marginal Value of Public Funds of Additional $100 for School Funding by Discount Rate and Benefited Students



Notes: This figure shows author's estimations from register data generated by Statistics Norway. The MVPF is calculated dividing the benefits of additional $100 in school funding, expressed in equation 20, by the sum of its total costs, expressed in equation 19, with estimates from equation 5. 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. Low-educated parents are defined by no parent having college diploma.

The aggregate MVPF estimates in suggest that increasing school funding can be cost-effective under standard social discount rates. However, the results imply that the cost-effectiveness of universal increases in school funding is best interpreted as moderate, rather than uniformly high.

Two forces attenuate the welfare gains when aggregating across all students. First, the effects are strongly concentrated among children from low-educated families, while they are close to zero for students with at least one college-educated parent, consistent with parental crowd-out of marginal public investments among more advantaged households. Second, the gender results indicate that women's gains are only partially captured by own earnings: women's responses operate primarily through the marriage market, raising household resources via partner earnings and partnership formation rather than increasing their individual labor income.

These patterns become clear when I compute MVPFs separately for groups whose benefits are directly reflected in own earnings. Importantly, table 13 shows that the same groups exhibiting the largest labor-market gains also display the strongest improvements in cognitive ability, suggesting that the policy's effectiveness reflects deeper human-capital accumulation. Among men overall, the reform raises annual earnings by about $315 and increases cognitive ability by 0.13 IQ points. Among men with low-educated parents, the earnings effect rises to over $560 and the cognitive effect increases to 0.20 IQ points. This alignment between earnings and cognition supports an interpretation in which additional resources raised productive skills.

Table 13: Heterogeneous Effects on Earnings and Cognitive Ability

	(1) Men only	(2) Men with low-educated parents
Panel A: Earnings (Ages 33–36)		
Earnings (USD, 2011 PPP)	314.8***	561.2***
	(115.3)	(155.5)
Observations	515,083	266,530
Pre-treatment mean	38,266	36,193
Panel B: Cognitive abilities		
IQ (standardized points)	0.127*	0.203**
	(0.066)	(0.089)
Observations	496,654	254,153
Pre-treatment mean	100.6	97.30

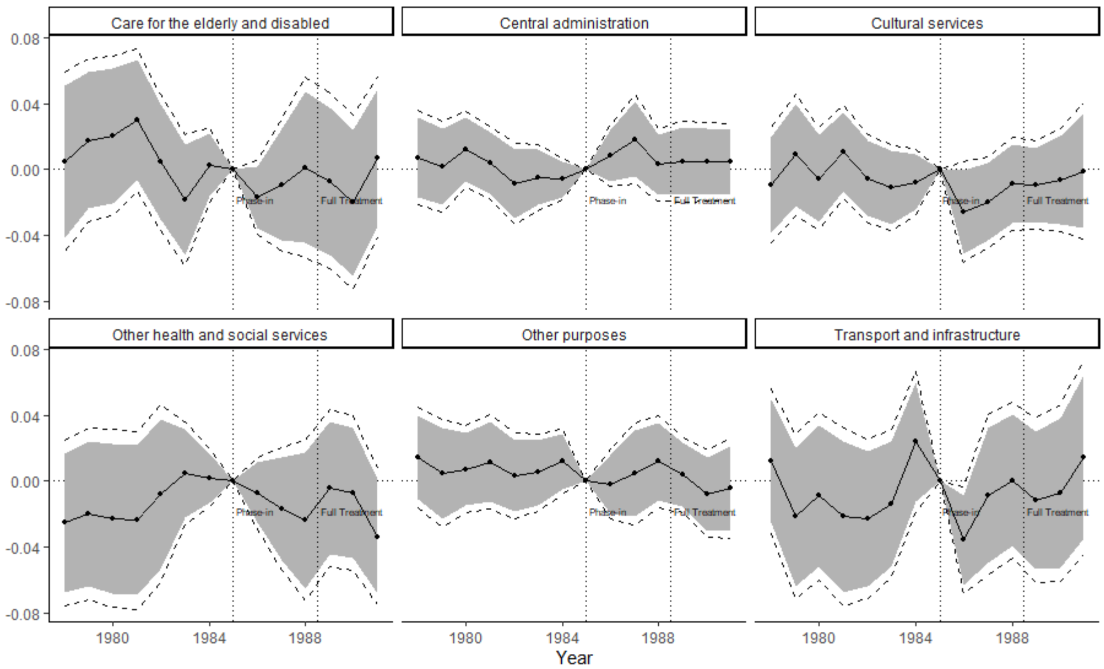
Notes: This table reports intent-to-treat estimates of nine years of exposure to an additional $100 per pupil per year. Earnings are measured in USD adjusted to 2011 PPP (excluding 1st and 99th percentiles). Cognitive ability is measured using male conscription records. Standard errors are clustered by municipality of residence in 1985. Low-educated parents are defined by neither parent having college diploma. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Taken together, the evidence suggests that the efficiency of broad school-funding expansions is positive but uneven: universal increases deliver moderate returns in the aggregate, while returns become very large when funding reaches students for whom public inputs are least likely to be crowded out and most likely to translate into labor-market productivity. From a policy perspective, this pattern strengthens the case for designing education finance systems that are more progressive in their marginal allocations, so that additional funds flow disproportionately to disadvantaged children and to the educational stages where private substitution is limited. In this sense, the headline MVPF for all students should be viewed as an average of highly heterogeneous effects, masking substantial scope for improving the cost-effectiveness of education spending through targeting.

7 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 10 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 10: 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 identification strategy 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 I 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 14: Municipal-level regressions: Different Age Brackets

Outcomes	(1) Teachers (ln)	(2) Teachers Per Pupil	(3) Teachers' Education	(4) Teachers' Income	(5) Class Size	(6) Number of Schools
			7-12 / 7-15			
Phase-in	0.308**	0.0171	-0.305	-0.0156	1.951	1.125
(1986-88)	(0.148)	(0.0254)	(0.947)	(0.204)	(2.360)	(1.008)
Full Treatment	0.490**	0.075***	0.218	-0.306	-1.371	2.358
(1989-91)	(0.207)	(0.029)	(1.478)	(0.320)	(3.059)	(1.780)
Observations	4,374	4,374	3,215	3,214	4,384	4,374
			10-12 / 10-15			
Phase-in	0.358**	0.034	0.113	0.0596	1.229	0.768
(1986-88)	(0.142)	(0.0255)	(0.847)	(0.185)	(2.047)	(0.874)
Full Treatment	0.384**	0.092***	0.889	-0.173	-1.489	1.698
(1989-91)	(0.165)	(0.026)	(1.350)	(0.296)	(2.564)	(1.526)
Observations	4,374	4,374	3,215	3,214	4,384	4,374
			11-12 / 11-14			
Phase-in	0.321**	0.049**	-0.457	0.163	0.627	0.497
(1986-88)	(0.128)	(0.025)	(0.767)	(0.203)	(1.950)	(0.701)
Full Treatment	0.281**	0.088***	0.601	-0.0470	-1.425	1.660
(1989-91)	(0.128)	(0.021)	(1.253)	(0.317)	(2.370)	(1.283)
Observations	4,374	4,374	3,215	3,214	4,384	4,374

Notes: This table shows author's estimations from register data generated by Statistics Norway. Calculations are estimates from Equation 3, using students' age composition instead of grant shock. 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

Table 15: Individual-level regressions: Different Age Brackets

Age brackets	(1) Years of Study	(2) College Diploma	(3) Employment Status	(4) Annual Earnings	(5) Income Rank by Cohort
7-12 / 7-15	0.132**	0.022**	0.008*	1,343***	0.023***
	(0.056)	(0.010)	(0.005)	(395.4)	(0.005)
Observations	1,023,285	1,024,535	1,024,535	981,306	994,205
10-12 / 10-15	0.152***	0.024***	0.002	1,214***	0.022***
	(0.049)	(0.009)	(0.004)	(370.2)	(0.005)
Observations	1,023,285	1,024,535	1,024,535	981,306	994,205
11-12 / 11-14	0.110**	0.016*	0.003	1,010***	0.020***
	(0.044)	(0.008)	(0.004)	(320.2)	(0.004)
Observations	1,023,285	1,024,535	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, using students' age composition instead of grant shock. 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 number of teachers in municipalities and various individual 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.

8 Conclusion

This study provides a comprehensive analysis of the long-term impacts of increased education funding on student outcomes, leveraging an intergovernmental transfer reform in Norway in the mid-1980s that generated plausibly exogenous variation in per-pupil resources across municipalities. On the supply side, municipalities used the additional funding primarily to expand instructional resources—most clearly through teacher hiring, while any effect on capital spending was null. This pattern suggests that local governments directed marginal funds toward intensifying instruction rather than changing the physical scale of schooling, consistent with a mechanism operating through the quality and quantity of educational services delivered.

At the individual level, the reform increased educational attainment and adult earnings, with effects mediated by educational choices and accompanied by modest gains in cognitive ability. However, the central lesson of the paper is that average effects can be misleading: the overall impacts are best characterized as moderate, since they mask substantial heterogeneity in who benefits and through which channels. In particular, the gains are concentrated among students from low-educated families, while students with at least one college-educated parent show no detectable earnings response, consistent with parental crowd-out of marginal public investments among more advantaged households. This indicates that the social returns to expanding school funding depend critically on whether the marginal public dollar reaches children for whom household inputs are most constrained.

A second source of heterogeneity concerns gender and the measurement of economic gains. The labor-market impacts are driven predominantly by men, who experience clear increases in own earnings and earlier improvements in family formation. For women, by contrast, own earnings do not respond significantly; instead, their benefits materialize primarily through the marriage market, as exposure to higher school funding increases partnership formation and raises household resources via partners' earnings and couple-level income. Put differently, a sizable share of the policy's economic payoff operates through household channels for women rather than through their individual wages.

The cost-benefit analysis reinforces this interpretation. When pooling all students, the policy's efficiency is positive but not uniformly large, reflecting both parental crowd-out at the top of the socioeconomic distribution and the fact that women's gains are not fully captured by own-earnings measures. Once the analysis focuses on the subgroups for whom benefits are most directly reflected in labor-market outcomes (men, especially from low-educated families) the estimated returns rise sharply. In that sense, the headline IRR and MVPF for the full population should be viewed as averages of highly uneven effects: the same policy can appear only moderately cost-effective in the aggregate while delivering very large returns for the disadvantaged groups it most meaningfully relaxes constraints for.

These findings have direct implications for policy design. First, they support the view that additional education spending can improve long-run outcomes even in a high-spending context like Norway, but they also show that the marginal returns depend on how funds interact with household behavior. If advantaged parents substitute away from public inputs, universal funding expansions may yield attenuated average effects relative to more progressive formulas. Second, the gender patterns underscore that welfare evaluation should look beyond own earnings and incorporate household-level outcomes when relevant. Together, these points argue for education finance systems that are not only larger but also more targeted at the margin, directing additional resources toward students and contexts where private substitution is limited and where public inputs translate into durable human-capital gains.

Finally, the paper helps reconcile two strands of prior evidence. Earlier work on school input policies in Norway often reports limited effects of marginal changes in class size or infrastructure, while U.S. evidence on school finance reforms frequently finds positive long-run impacts. By studying a sizable funding shock in Norway and documenting both how municipalities allocated the resources and which students benefited most, this paper shows that marginal funding can produce meaningful long-term gains—but that these gains are concentrated among disadvantaged children and can operate through different pathways across genders. In doing so, it advances a more nuanced view of "whether school funding works": it does, but its apparent effectiveness hinges on for whom it is evaluated and which economic margins it captures.

References

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

Balaj, M., Henson, C. A., Aronsson, A., Aravkin, A., Beck, K., Degail, C., Donadello, L., Eikemo, K., Friedman, J., Giouleka, A., et al. (2024). Effects of education on adult mortality: a global systematic review and meta-analysis. *The Lancet Public Health*.

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

Barr, A., Eggleston, J., and Smith, A. A. (2022). Investing in Infants: the Lasting Effects of Cash Transfers to New Families*. *The Quarterly Journal of Economics*, 137(4):2539–2583.

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

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

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

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

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

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

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

Brunner, E., Hoen, B., and Hyman, J. (2022). School district revenue shocks, resource allocations, and student achievement: Evidence from the universe of u.s. wind energy installations. *Journal of Public Economics*, 206:104586.

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

Campos, C., Muñoz, P., Bucarey, A., and Contreras, D. (2026). College major choice, payoffs, and gender gaps. Working Paper 34736, National Bureau of Economic Research.

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

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

Cortés, P., Hwang, J., Pan, J., and Schönberg, U. (2026). Gender norms and the labor market. Working Paper 34716, National Bureau of Economic Research.

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

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

Devereux, P. J. and Fan, W. (2011). Earnings returns to the british education expansion. *Economics of Education Review*, 30(6):1153–1166. Special Issue: Economic Returns to Education.

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

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

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

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

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

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

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

Hendren, N. and Sprung-Keyser, B. (2020). A Unified Welfare Analysis of Government Policies*. *The Quarterly Journal of Economics*, 135(3):1209–1318.

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

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

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

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

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

Kampelmann, S. and Rycx, F. (2012). The impact of educational mismatch on firm productivity: Evidence from linked panel data. *Economics of Education Review*, 31(6):918–931.

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

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

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

Lindley, J. and Machin, S. (2012). The quest for more and more education: Implications for social mobility. *Fiscal Studies*, 33(2):265–286.

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

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

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

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

Riddell, W. C. and Song, X. (2011). The impact of education on unemployment incidence and re-employment success: Evidence from the u.s. labour market. *Labour Economics*, 18(4):453–463.

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

Sondheimer, R. M. and Green, D. P. (2010). Using experiments to estimate the effects of education on voter turnout. *American Journal of Political Science*, 54(1):174–189.

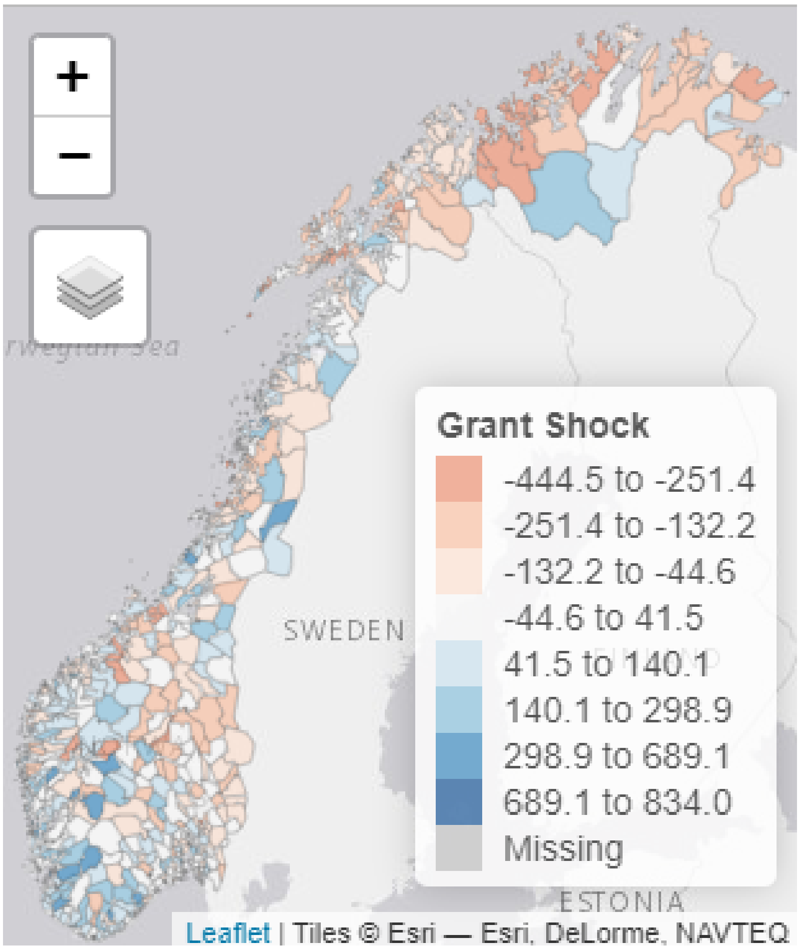
Appendices

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 11: 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

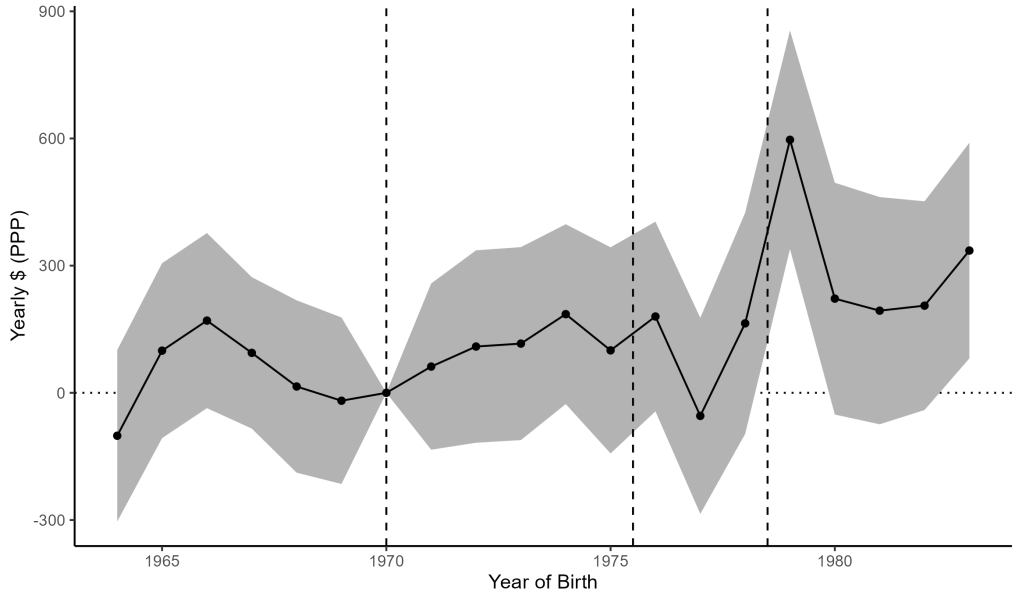
Table 16: Municipal-level regressions excluding big Cities

Outcomes	(1) Teachers (log)	(2) Teachers per Pupil	(3) Teachers' Education	(4) Teachers' Income	(5) Class Size	(6) Number of Schools
Phase-in	0.006**	0.0003	-0.005	-0.0003	0.045	0.020
(1986-88)	(0.003)	(0.0005)	(0.017)	(0.004)	(0.044)	(0.019)
Full Treatment	0.009**	0.0014***	0.005	-0.006	-0.012	0.039
(1989-91)	(0.004)	(0.0005)	(0.027)	(0.006)	(0.056)	(0.033)
Observations	4,330	4,330	3,179	3,178	4,734	4,726
Pre-Treat. Mean		0.105	14.2		18.2	7.3
Number of Mun.	398	398	374	374	398	398
Pre-trend p-value	0.613	0.252	0.457	0.803	0.040	0.337

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

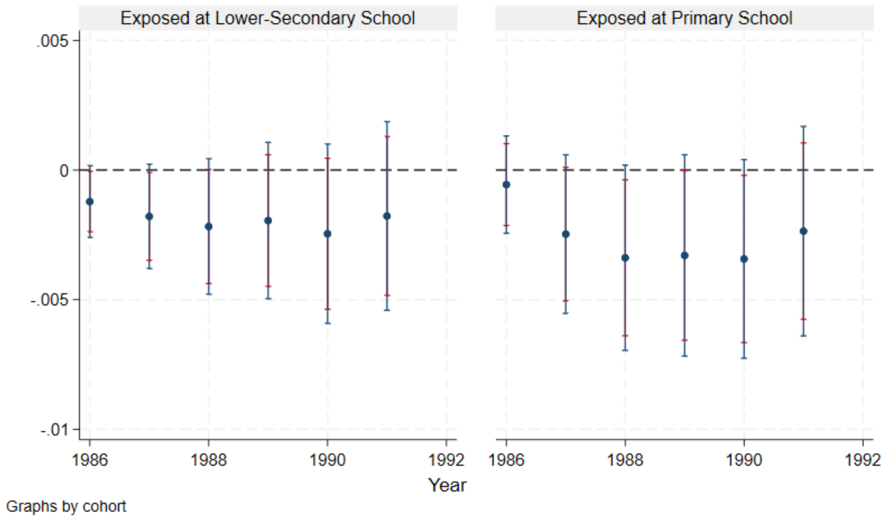
Figure 12: Effect on Earnings, by year of birth



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 13, in the appendix, shows the regressions' point estimates and standard errors each year from 1986 to 1991. Students seem to have a slightly lower probability of leaving municipalities receiving higher funding for education. 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 based on their perceived quality. However, the effect size are considerabely small, below 0.5 percentage points even for cohorts fully exposed to the shock.

Figure 13: 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.