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# Long-term effects of school spending

Evidence from exiting cohort size variation

Audun Langørgen and Sturla A. Løkken

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## Abstract

This paper investigates the long-term effects of local government education spending on child outcomes, including income, educational attainment, and family formation in adulthood. We propose a novel identification strategy which exploits quasirandom variation in demographic trends when there is strong inertia in local government spending on compulsory schooling. Specifically, size of the exiting cohort that finishes compulsory schooling just before entry of the treated cohort is used as a source of exogenous variation. First, we show that exiting cohort size displays a significantly positive effect on per-pupil spending during school years of the treated cohort. Second, we argue that causal effects of school spending can be identified by utilizing exiting cohort size to instrument for school spending. In implementing this strategy, school spending is found to exhibit sizable and significant effects on income in adulthood for boys, with estimates that are relatively large for children from low- and middle-income families. By comparison, the effects of education spending are small and insignificant for girls.

**Keywords:** Education spending; School inputs; Compulsory schooling; Cohort size; Child outcomes; Local public finance

**JEL classification:** H42, H7, I2, J12, J62

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**Address:** Audun Langørgen, Statistics Norway, Research Department. E-mail: aul@ssb.no

Sturla A. Løkken, Statistics Norway, Research Department. E-mail: sal@ssb.no

# Sammendrag

Offentlig skolegang betraktes generelt som et grunnleggende og universelt verktøy for å investere i barns fremtid. Mange land har obligatorisk grunnskole for å fremme barnas læring og utvikling. Skolegang for alle barn blir ansett som essensielt for å sikre like muligheter og en produktiv arbeidsstyrke. I denne artikkelen undersøker vi effektene av kommunenes skoleutgifter per elev i grunnskolen for norske barn født i perioden 1970-1980. Vi analyserer effekten av slike utgifter på inntekt, utdanningsnivå og familiedannelse i voksen alder.

Kommunene i Norge er ansvarlige for grunnskoleutdanningen, og i den aktuelle perioden var det ni år med obligatorisk skole for barn fra 7 års alder. Basert på grunnskoledata (GSI) og kommuneregnskap fra perioden 1977-1996 har vi konstruert et mål for offentlige skoleutgifter per elev etter kommune, år og fødselskohort. Dette gjør at vi kan undersøke endringer i barns eksponering mot endringer i offentlig skoleutgifter per elev over tid.

En utfordring med observasjonsstudier er at variasjon i utgifter kan henge sammen med ulike bakgrunnsfaktorer, som for eksempel familie- og nabolagskjennetegn. Vi benytter oss av endringer i størrelsen til avgangskullene som slutter i grunnskolen som en kilde til kvasi-randomisert variasjon i utgift per elev for skolestarterne. Denne metoden bygger på ideen om at store elevkull vil binde opp ekstra ressurser mens de er i skolen, men at slike ressurser blir frigjort når elevene slutter på skolen. Utgiftene per elev blir derfor relativt høye etter at et stort avgangskull har sluttet, og vi finner at dette gir en varig fordel for skolestarterne.

Studien viser at gutter som opplever økt ressursbruk i grunnskolen får høyere inntekt som voksne. Videre tyder resultatene på at gutter som mottar økt ressursbruk også fullfører flere år med utdanning og har større sannsynlighet for å stifte familie i voksen alder. Effektene er størst for barn fra lavinntektsfamilier. Vi finner imidlertid ingen signifikante effekter av skoleutgifter for jenter. Denne studien bidrar til å belyse hvordan offentlig pengebruk på grunnskoler påvirker barnas utfall som voksne og hvem som klarer seg bedre når grunnskoleutgiftene øker.

# 1 Introduction

Public education spending is widely considered as a basic and universal instrument for investing in the future of children. Most countries rely on compulsory education systems to advance the education of all children during formative stages of development, which is regarded as essential for equality of opportunity and a productive labor force. Funding this system has meant that education accounts for a substantial proportion of government spending (OECD, 2020; World Bank, 2017). These facts have motivated a large empirical literature which analyzes the effectiveness of education spending in improving child outcomes.

There is growing evidence in the literature that increases in school spending *can* improve child outcomes. Yet, the estimated school spending impacts vary by context and display considerable diversity. Thus, more research is warranted to address a number of key questions: How are outcomes in adulthood affected by interventions that increase spending per child in compulsory education? Does such policies improve the outcomes of children from disadvantaged families? What kind of spending interventions and type of school inputs are likely to improve child outcomes? To address these questions, this paper investigates the long-term effects of per-pupil spending during compulsory schooling for Norwegian children in the 1970-1980 birth cohorts. For children in those cohorts, we analyze the impact of per-pupil spending on income, educational attainment and family formation in adulthood.

Local governments in Norway are responsible for primary and middle schools, offering mandatory education to children from age 7 to 15. Based on enrollment and accounting data for the period 1977-1996, we construct measures of per-pupil spending by municipality, year and cohort. Per-pupil spending by cohort is defined as an average over the nine years of compulsory schooling. In other words, we observe children's exposure to changes in per-pupil spending across cohorts who spent their childhood in a given municipality. Children are recipients of compulsory school spending during mandatory school-age years, and not during non-school-age years. Hence, differential exposure follows from the interaction between changes in per-pupil spending and the timing of children's school enrollment.

A major challenge in observational studies is that variation in spending is confounded with other factors, such as unobserved family and neighborhood characteristics. An ideal experiment in our setting could be to randomly drop money on some municipalities but not others (Jackson, 2020). In the absence of such experiment, we exploit that the treatment variable of interest normalizes spending by enrollment. Therefore, random shocks in enrollment will arguably provide exogenous variation in per-pupil spending. This idea is similar to Hoxby (2000), who utilizes natural population variation in children's own-cohort size by grade, school and district to instrument for class size.

Yet, our approach differs from Hoxby's by relying on random variation in size of the cohort that graduates and exits from compulsory schooling just before entry of the treated cohort. When the school-exiting cohort is relatively large (small), we show that there is an immediate and persistent increase (decrease) in per-pupil spending. Thus, size of the nine years older school-exiting cohort displays a significantly positive effect on per-pupil spending during school years of the school-entering cohort.

This source of variation is stemming from inertias in the adjustment of education spending, which likely result from features of the institutional setting. Specifically, turnover costs, job switching barriers, bureaucratic inertia and interest group politics may explain the strong persistence of spending levels. By contrast, enrollment is discontinuous at the transition between school years, because the compulsory education system dictates the dates of entry and exit in the years of age 7 and 16, respectively. As a result, the combination of persistent spending levels and discrete jumps in enrollment is found to generate substantial variation in per-pupil spending across cohorts and municipalities.

To address issues with correlated trends, sorting and reverse causality, we propose that school-exiting cohort size can be used as an instrumental variable for per-pupil spending. Next, we employ this instrument in a two-stage least squares (TSLS) model. We show that graduating cohort size is relevant as a predictor of whether students in compulsory schooling will inherit a relatively large or small pool of resources from the school-exiting cohort. When controlling for demographic trends in cohort sizes by age, we argue that the instrument is exogenous. The exclusion restriction requires that school-exiting cohort size will affect outcomes of treated children in the school-entering cohort solely by changing per-pupil spending during school years of the treated cohort.

Estimation of the TSLS model is based on extensive register data for individuals and families in combination with detailed municipal enrollment and accounting data. The empirical results show that males who were exposed to higher per-pupil spending during compulsory schooling receive higher incomes in adulthood. The estimated income elasticity for males is around 0.7, which implies that on average, increasing per-pupil spending by 10 percent in all school-age years increases adult incomes of males by 7 percent. Furthermore, the results suggest that males who receive more school spending attain more years of schooling and higher likelihood of family formation. We do not find any significant school spending effects for females.

This paper is related to three strands of the literature. First, we contribute to the literature that analyzes the impacts of school spending on child outcomes. To identify causal effects, the recent empirical research emphasizes design-based methods that exploit quasi-random sources of variation in school spending. Several studies utilize variation from school finance reforms (Jackson et al., 2016; Lafortune et al., 2018; Brunner et al., 2020) or quirks and discontinuities in school funding formulas (Kreisman and Steinberg, 2019; Gigliotti and Sorensen, 2018; Hyman, 2017; Gibbons et al., 2017; Johnson, 2015;

Cascio et al., 2013). Various approaches rely on exogenous shocks to the revenues of school districts or local governments which provide compulsory education (Hægeland et al., 2012; Jackson et al., 2021; Brunner et al., 2022). Other studies exploit revenue limit referendums through which passage of property tax increases are approved or defeated by a narrow majority of voters (Abott et al., 2020; Baron, 2022).

Beyond different empirical methods, mixed conclusions are likely to emerge from studies that vary by contextual factors, including contrasts between places, time periods, educational systems, types of spending, and sources of spending variation. Since the empirical evidence comes from settings with different bundles of contextual factors, it is challenging to disentangle the relative importance of such factors. On top of that, in many settings, it can be difficult to find sources of spending variation that are plausibly exogenous. For instance, many countries have not implemented school finance reforms or revenue limit referendums, which means that identification methods are specific to the US and cannot be applied everywhere. To overcome such difficulties, it might be helpful to extend the repertoire of credible research designs, which is pursued in this paper.

Second, our study is related to a literature on how public interventions may improve children's long-term outcomes and increase intergenerational mobility. The associated theory deals with human capital formation, investments in schooling and other inputs in child development (Becker and Tomes Becker and Tomes (1979), Becker and Tomes (1986); Solon, 1999, Solon (2004); Heckman and Cunha, 2007; Del Boca et al., 2014). A subset of the empirical literature is concerned with estimating causal effects of investments in the production of human capital (e.g. Biasi (2023)).

Third, our approach is connected to the public finance literature which studies local public spending behavior and the distribution of resources to districts and individual recipients (Aaberge et al., 2019). Prior work has demonstrated that institutional knowledge and thorough understanding of the funding, allocation and targeting of school spending is important for credible research designs aimed at estimating the effects of school spending. For example, public programs intended to increase school spending on disadvantaged students may instead be spent on advantaged students or tax reductions by local authorities (Gordon, 2004; Cascio et al., 2013; Hyman, 2017; Brunner et al., 2020; Fischer, 2023). Thus, local public allocation processes can be a crucial component of the mechanisms that mediate school spending impacts. That is clearly relevant in our setting, since we make use of results from analyzing the dynamics of per-pupil spending to propose a novel instrument for school spending.

A limitation of this study is that we observe per-pupil spending at the municipal level, and not at the school level, grade level or individual level. While such data limitations are common in research on school finance, the reliance on district-level averages may mask disproportionality in the allocation of spending to children from different family backgrounds. Consequently, there are two different channels that may generate heterogeneous impacts of per-pupil spending on child outcomes (Jackson and Mackevicius, 2021). Larger school spending impacts for low-income than high-income students may reflect that (i) the spending allocation is progressive (favoring low-income students), and/or (ii) the responsiveness of child outcomes to school spending is larger among low-income than high-income students.

To examine heterogeneous effects by family background, we rank family backgrounds by income and partition the sample in three groups with low, middle and high parental income. Estimation results indicate that school spending impacts are relatively large for males from low- and middle-income families. Next, we provide indirect evidence that heterogeneous effects by parental income groups are not mediated through a progressive allocation of the instrument-induced spending variation. Hence, the results suggest that the marginal returns of school spending are relatively large for males from low- and middle-income families.

Since the estimated school spending impacts may vary by different types of school inputs, we utilize a breakdown of spending on wage cost and other operating expenses. On average, wage cost account for about 80 percent of total operating expenses. The exiting cohort size instrument is found to display a significantly positive effect on wage cost, whereas other operating expenses are unaffected by the instrument. Thus, it appears that adjustment inertia is associated with inertia in the hiring, firing and mobility of teachers. Moreover, we show that school investments and class size are unaffected by the instrument. Taken together, the evidence suggests that our school-spending impacts are first and foremost mediated through changes in the student-teacher ratio that are unrelated to class size. The emerging picture is consistent with positive effects of teacher aides, pull-out teaching and small-group instruction, which finds support in Andersen et al. (2020) and Bonesrønning et al. (2022).

A policy implication is that school performance can be improved through reforms that reduce student-teacher ratios. The Norwegian government implemented two such reforms in 2012 and 2015. Reiling et al. (2021) show that the 2015 reform was undermined by leakage of funds to non-intended purposes, suggesting that strong enforcement mechanisms are necessary to direct the use of earmarked grants. Borgen et al. (2022) find that the 2012 reform reduced student-teacher ratios, but they did not find any effect on student performance. Since the 2012 reform was transitory, it might have incentivized schools and teachers differently from a setting with persistent changes in student-teacher ratios. Moreover, in analyzing a relatively recent reform, they diverge form our study by considering short- and medium-term rather than long-term effects of school resources.

When there are strong impediments to re-allocation of teachers, further questions arise regarding the scope for better targeting of school resources to achieve efficiency or equity goals. Since teacher mobility is associated with turnover costs and disruption costs, future research may explore organizational and regulatory interventions that improve the balance between teacher mobility and stability of the school environment.

The remainder of the paper is structured as follows. We describe institutional details in Section 2, and present the data and sample in Section 3. Section 4 analyzes local government spending behavior and resource allocation. Section 5 discusses the empirical strategy for identifying causal effects. Section 6 offers evidence on how long-term child outcomes are affected by changes in education spending, and Section 7 concludes.

# 2 Institutional background

## 2.1 Local public finance

In Norway, local governments are responsible for providing compulsory education through primary schools and middle schools (lower secondary schools). Municipalities are multi-purpose jurisdictions with responsibility for an array of public services.[1] Local government spending is funded through a centralized system of financing which includes tax revenues, intergovernmental block grants and matching grants.[2] Income taxes and natural resource taxes (e.g. hydropower plants) are used as sources of local tax revenues, but the tax bases as well as the tax rates are determined by the central government. The centralized system of financing has meant that there is limited local government discretion over taxes and total incomes. Instead, local governments are required to adjust spending to their incomes in the long run, since persistent budget deficits are prohibited. However, local governments have considerable discretion in the allocation of spending, where compulsory education is financed almost exclusively out of unconditional funding.

Compulsory education, child care and long-term care account for more than 49 percent of local government spending in 1996. The target group for compulsory education is restricted to children aged 7—15 years. By comparison, only preschool children are eligible for child care, whereas long-term care is targeted mostly to elderly people. Hence, the majority of local government spending is targeted to different demographic groups defined by age. Since the central government has enacted numerous service standards and entitlement legislation, there are mandatory programmatic spending components (expenditure needs) which increase with the population proportion of the relevant targeted age groups. Beyond expenditure needs, local governments are responsible for making priorities between different service sectors and demographic groups for a given budget constraint. As

[1] Municipal services include preschool education and care (for children aged 1-6), long-term care (for elderly and disabled), health care (provided by general practitioners), social assistance, child welfare, culture and infrastructure. On average, compulsory education spending constitutes 23.4 percent of total municipal expenditure in 1996.

[2] The grant system allocates intergovernmental transfers to local governments based on transparent and operational criteria using on official statistics. Population size and demographic composition by age are important determinants of the allocation of grants. Moreover, there are grants aimed at supporting small municipalities, rural areas and regions in Northern Norway.

a result, education spending per child varies across municipalities due to different fiscal capacities (determined by the central government) and/or different spending priorities across service sectors (determined by local governments).

## 2.2 Compulsory school system

Compulsory education is provided free of charge in public schools which are owned and funded by local governments.[3] Children are required to attend compulsory schooling over a period of 9 years. The school year is divided in two terms, where the first semester starts in August and the second semester starts in January and ends in June. According to administrative regulation, school entry is stipulated in August in the year of age 7. Compulsory schooling ends in June in the year of age 16, and includes six years in primary school and three years in middle school. The year and date of school entry is determined by the education system rather than parental choice, whilst grade retention or acceleration is almost never applied.[4]

Education is organized in schools, grades and classes. A grade includes children born in the same calendar year, and is completed during a school year. Students are assigned to a given class through the school year and through multiple grades. School catchment areas are strictly enforced. Consequently, parental school choice for a given residence is not allowed, but can be obtained by moving between municipalities or between school catchment areas within a municipality.

Despite the tradition of decentralized decision making combined with substantial fiscal disparities, local governments are supposed to provide uniform standards of education to all children. A unified National Curriculum was established i 1974 with detailed prescription of content and methods. Driven by egalitarian objectives, a related development in the 1970s was phasing out of special schools and prohibition of permanent ability tracking within a school. Instead children with diverse abilities and backgrounds are integrated in the same classroom. At the same time, the National Curriculum stipulates that each child be entitled to individually adapted teaching, and the Norwegian School Law requires that supplemental instruction is provided to students with special needs or poor language proficiency.

In August 1997, school starting age was reduced from 7 to 6 and the number of years in compulsory schooling were extended from 9 to 10. Since the 1980 cohort graduated from compulsory schooling in June 1996, none of the cohorts in this study were affected by the reform in 1997.

[3]Compulsory education provided by publicly funded private schools account for less than 3 percent of total enrollment.

[4]Exceptions from the stipulated school starting age is rarely practiced, and when permitted, it is mostly for pupils born in December or January.

## 2.3 Institutional constraints and allocation of resources

The allocation of teachers to students is affected by the organization of schools and by various regulatory and contractual constraints. According to the Norwegian School Law, only certified teachers can be employed in permanent positions. When no certified teachers apply for a vacant position, a non-certified teacher can be employed for up to one school year. Teacher jobs are specific to physical schools and typically limited to a certain educational stage (e.g. primary school versus middle school) with corresponding qualification requirements. Once hired, employment protections restrict the re-allocation of teachers between schools. Thus, there are barriers to job switching which imply that teachers can be locked-in as employees at a given school within a municipality. Although turnover rates among teachers are moderate, retirement and optional job switching will offer municipalities some flexibility to re-allocate employment (Falch and Strøm, 2005).

The teachers union must be informed prior to every hiring decision, and can intervene to prevent re-allocation of teachers across schools. There is a high degree of unionization among teachers, where the teachers union participates in a centralized national system of wage bargaining. Teacher salaries follow a lock-step schedule based on level of experience and education. Wages are completely determined through central bargaining, which concerns wage growth and changes to the salary schedule. Hence, the resulting wage structure is rather rigid and compressed (Bonesrønning et al., 2005).

To reduce children's traveling distances, relatively small schools are often located in rural areas with a dispersed population. Middle schools enroll more children per grade, since they normally recruit children from larger school catchments areas. Schools are subject to strict maximum class size rules at the school-by-grade level. Maximum class-size is 30 according to national regulation.[5] Consequently, class size varies with enrollment by school, grade and municipality.

Frequent rotation of teachers or classmates may expose children to disruption costs. Thus, to facilitate a stable and secure environment, children and teachers often follow a given class through multiple grades. Some teachers and assistants rotate more among classes to allow for specialization in subjects and re-allocation of resources. Nevertheless, smaller classes may acquire reduced student-teacher ratios through the lock-in of teachers on classrooms. Since teachers are employed at specific schools and classrooms, there is normally a discontinuity in the assignment of teachers to students at the transition from primary to middle school, and, naturally, when children exit from the last grade of middle school. While teachers can be employed at a given school for their entire working life, children are locked-in through mandatory enrollment during ages 7-15. Hence, compulsory schooling stipulates the specific dates when children's barriers to entry and exit are turned on/off for different birth cohorts. As a result, the lock-in of children and teachers on

[5]From 1986, the maximum class size rule was changed to 28 for primary schools.

classrooms disappears when children exit from compulsory schooling. From that moment, there is freedom to re-allocate resources that have been targeted to the exiting cohort.

# 3 Data sources and descriptive statistics

Our analysis is based on annual spending data from the local government accounts and enrollment data from the compulsory schooling database. These municipal level data are combined with individual administrative register data on family characteristics, municipality of residence, and children's long-run outcomes that are maintained by Statistics Norway.

## 3.1 Spending and enrollment data

Yearly spending on compulsory education is measured in local government accounts, which are reported to Statistics Norway by every Norwegian municipality. Detailed accounting guidelines for local government are prescribed through central government regulation, which are designed to minimize misreporting and to secure transparent and consistent accounting practices across municipalities. According to law, local government accounts are verified and accepted by local government councils and moreover by certified public accountants. Spending is itemized by input type as well as functions of government in local government accounts. In this study, education spending is defined by operating expenses, out of which the majority goes to salaries of teachers and other personnel. Other components are teaching material, equipment and maintenance of school facilities.

Information on municipal spending is supplemented by data on the number of students and classes by grade, year and municipality. The source of this information is the Compulsory Schooling Database ("Grunnskolenes informasjonssystem"), maintained by the Norwegian Directorate of Education. The data provide information on the number of classes and enrollment by grade, school and municipality.

Per-pupil spending is obtained by combining annual accounting data with enrollment data, whereby spending is standardized relative to the number of students in a municipality in a given year. Figure 1 shows the evolution of percentiles in the distribution of school spending per pupil across municipalities. The median value has increased from NOK 36.7K per student in 1977 to NOK 52.6K per student in 1996. There is considerable dispersion in per-pupil spending across local governments. In 1996, for example, spending per child differs by NOK 37.4K between a municipality at the 90th percentile as compared to a municipality at the 10th percentile.[6]

[6]In Appendix A, we define a measure of average log per-pupil spending during all school-age years of a given cohort in a given municipality. Since the nine years of compulsory schooling are overlapping with ten fiscal years, the first and the tenth fiscal year in school are each weighted 50 percent, while the eight years in between are given full weight. Figures D1 and D2 display substantial variation in the growth

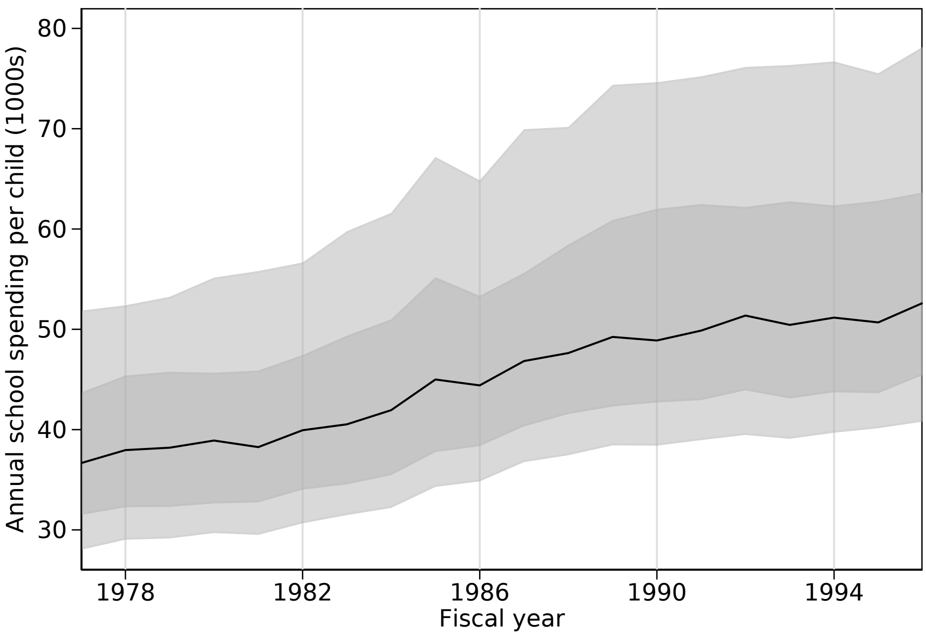



**Figure 1.** Evolution of school spending per pupil

*Note*: This figure displays percentiles in the yearly distributions of operational school spending per pupil across local governments over the period 1977-1996. This period covers all years when the 1970 to 1980 birth cohorts attended compulsory schooling (years 1977–1986 for the 1970 cohort, and 1987–1996 for the 1980 cohort). The median level of spending is represented by the black line, the inner/darker shaded gray area represents the interquartile range (25th–75th percentile), whilst the outer/lighter shaded gray area represents the interdecile range (10th–90th percentile). Expenditures are adjusted for inflation by the Consumer Price Index, and expressed in thousands of 2009 Norwegian kroner (NOK).

## 3.2 Administrative register data

For measuring individual and family characteristics, municipality of residence, and children's long-run outcomes, we make use of several administrative registers that we can link through unique individual identifiers. The panel data sets include tax registers, social security registers and education registers for the population of Norwegian residents. For each individual, the databases provide extensive demographic and socioeconomic information. The individual identifiers allow us to match children to their parents and siblings, and to match spouses to one another. Exact geographical identifiers permit the assignment of individuals to municipalities of residence in each year, including all years of childhood for children born in 1970 or later.

As our measure of incomes of the child generation, we make use of the after-tax income reported to the Norwegian Tax Authority, which consists of labor earnings (from wages and self-employment), and public cash transfers (such as unemployment benefits, short-term sickness benefits and parental leave benefits) net of tax payments. Incomes are measured over different time periods depending on birth year of the child generation. We define *individual income* as the income of the persons in adulthood, measured as the average inflation-adjusted after-tax income over a period of ten years during ages 27–36.

rates of per-pupil spending across cohorts, both within and between municipalities.

*Parental income* is defined by the (inflation-adjusted) sum of pension-qualifying income of both parents in the 6 preschool years before the onset of compulsory education. The extent of maternal labor market participation is measured based on the mother's income history during the child's preschool years. For more details on variable definitions, see Appendix A.

Educational institutions report educational attainment of individual students directly to Statistics Norway. Educational attainment of the child generation is measured as the number of *years of schooling* completed in adulthood at age 34, and *high school completion* is defined by having attained at least 12 years of schooling.

## 3.3 Sample selection and descriptive statistics

Our sample is based on all individuals who are born as Norwegian citizens in the period 1970-1980. We observe the municipality of residence of children (or their mothers) from 1970 and onwards. The 1980 cohort is the last which is unaffected by the reduction of school starting age and expansion of mandatory schooling in 1997. As of 1996, there are 435 municipalities in Norway with an average of 10,056 residents. However, we exclude municipalities that were involved in amalgamations during the period 1977-1996. This restriction reduces the sample size by 27 percent. Next, we exclude students who do not comply with the standard compulsory school progression and children who are enrolled in a few private schools that are approved as substitutes for public schools during mandatory school years. Finally, we exclude individuals with missing data during the treatment or outcome period, such as children born abroad, children with unknown father or mother, and individuals who die or emigrate before age 34. Taken together, these restrictions remove 35 percent of individuals born between 1970 and 1980 from the sample, which yields a baseline sample of 409,380 children.

Municipality of birth is often misreported, especially for the older cohorts. To remedy this, we instead use the mother's registered municipality of residency as a proxy. Thus, each child's municipality of origin is defined as the mother's municipality of residence in the birth year of the individual. During childhood years, we use the actual registered residency of children to determine whether they move, and timing and destination of movements by municipality.

Table 1 presents summary statistics for all individuals in our baseline sample (1), as well as for the subsamples of children with low (2) versus high (3) parental income during preschool years (ages 1-6). Panel A reports the mean sample value of different child outcomes as adults. Intergenerational persistence of socio-economic status (SES) is demonstrated by relatively low income and educational attainment in adulthood of individuals with low parental income, whereas the likelihood of parenthood is relatively high among individuals with low SES background.

**Table 1.** Sample means of selected characteristics

| | All | Parental income | |
|---|---|---|---|
| | | Low | High |
| *A. Child outcomes* | (1) | (2) | (3) |
| Individual income | 216 425 | 205 958 | 226 892 |
| Low individual income ($\leq 25pct.$) | 0.25 | 0.28 | 0.22 |
| High school completed | 0.81 | 0.76 | 0.86 |
| Years of schooling | 14.0 | 13.5 | 14.5 |
| Single | 0.28 | 0.29 | 0.28 |
| Parenthood | 0.72 | 0.73 | 0.71 |
| *B. Resources and enrollment* | | | |
| Spending per student (ages 7-15) | 40 847 | 41 589 | 40 099 |
| Wage cost per student (ages 7-15) | 32 456 | 32 846 | 32 066 |
| Childcare coverage (ages 4-6) | 0.31 | 0.29 | 0.33 |
| Enrollment ratio | 0.98 | 0.982 | 0.976 |
| *C. Family characteristics* | | | |
| Boy indicator | 0.51 | 0.51 | 0.51 |
| Parental income (preschool years) | 338 539 | 247 601 | 429 478 |
| Mother's years of schooling | 11.1 | 10.5 | 11.7 |
| Father's years of schooling | 11.6 | 10.7 | 12.5 |
| Mother not working (preschool years) | 0.66 | 0.83 | 0.49 |
| Parents married | 0.91 | 0.89 | 0.93 |
| Age of mother (at birth) | 26.3 | 25.3 | 27.3 |
| Age of father (at birth) | 29.2 | 28.5 | 29.9 |
| Immigrant background | 0.011 | 0.008 | 0.013 |
| *D. Family migration* | | | |
| Cross-municipality mover (age 1–15) | 0.25 | 0.24 | 0.27 |
| Cross-municipality mover (preschool years) | 0.20 | 0.18 | 0.21 |
| Municipalities | 385 | 385 | 385 |
| Observations | 409 380 | 204 690 | 204 690 |

*Note:* This table presents summary statistics for (1) all children in the analysis, as well as subsamples consisting of individuals from families with parental income (2) below and (3) above the median. Panel A reports the sample means of our outcome variables. Panel B reports municipality-of-birth characteristics including school spending and wage cost per student, on average, during ages 7-15. Childcare coverage is defined by the average participation rate during ages 4-6 of children belonging to different cohorts. Enrollment ratio is defined by enrollment in first grade into public primary schools, measured as a proportion of the total number of children who are aged 7 at end-year. Panel C reports mean values of selected family characteristics of the children in each sample, and Panel D displays indicators for family movement during childhood. Monetary amounts are adjusted for inflation by the Norwegian Consumer Price Index, and expressed in 2009 NOK. See Appendix A for variable definitions.

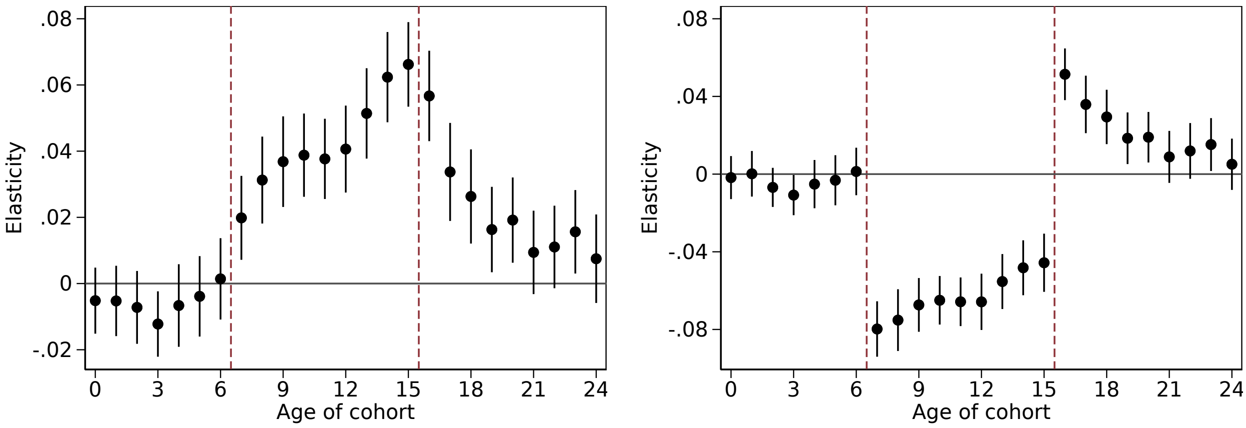
Panel B presents mean values of spending and enrollment variables measured at the level of municipality-by-cohort. Across cohorts in our sample, mean annual school spending per student during school years is NOK 41.6K (40.1K) for individuals with low (high) parental income. In our sample, the total average participation rate in childcare during ages 4-6 is 31 percent, and relatively high among individuals with high parental income. Around 98 percent of children enter public primary schools in the year of their 7th birthday.

Panel C presents mean values of selected family characteristics measured during early childhood years. Sample composition on boys and girls is balanced on high-SES versus low-SES families. Children with low parental income are more broadly associated with low-SES characteristics: They have parents with less schooling, lower proportion of married parents, lower maternal labor-market participation, and younger parents, in comparison to children with high parental income. Around 1 percent of children in the sample have immigrant background. In Panel D we report indicators of family movement across municipality borders during childhood years and preschool years, respectively. We find that the majority of family movements occur during preschool years, and high-income families display higher mobility than low-income families.

# 4 Local government spending behavior

Prior work has demonstrated that there is significant inertia in the response of local government spending to demographic shocks and income shocks (Borge et al., 1995; Aaberge et al., 2019). Various mechanisms may explain sluggish adjustment of spending. First, in producing public services, local governments have to deal with capacity constraints, regulatory and contractual constraints, employment protection (e.g. when hiring or firing teachers), labor market frictions, and bureaucratic inertia, whereby responses to shocks are delayed. Second, since teacher mobility imposes turnover costs and disruption costs on schools and students, such costs can be reduced through teacher retention (Gibbons et al., 2021; Sorensen and Ladd, 2020; Ronfeldt et al., 2013). Third, analogous to consumption smoothing in life-cycle models, local governments are allowed to use transitory budget deficits and surpluses to smooth service production over the business cycle. Fourth, models of interest group politics suggest that interest groups in favor of the status quo are able to resist the re-allocation of spending between service sectors and target groups, which may reduce the responsiveness of spending to changes in age composition (Poterba, 1997; Borge and Rattsø, 1995).[7] Fifth, when local governments make use of backward-looking spending allocation rules (e.g. when budgeting is based on last year's enrollment),

[7]Lobbying by teachers unions in favor of school spending may contribute to the so-called flypaper effect, whereby money "sticks where it hits" (Brunner et al., 2020). In our context with little tax discretion, re-allocation of spending across service sectors is the potential source of crowd-out rather than tax relief for local residents.




**(a)** Yearly school spending

**(b)** Yearly school spending per pupil

**Figure 2.** Response of yearly school spending to changes in cohort size

*Note:* This figure shows the responsiveness of yearly school spending ($a$) and yearly school spending per pupil ($b$) with respect to cohort size by age. Cohort size variables are measured at end-year, and are defined by log number of children by municipality and age of cohort. The figures display partial elasticity estimates that are obtained from OLS regressions of log school spending (per pupil in $b$) on log cohort sizes. The model includes municipality and year fixed effects, and controls for total local government income and lagged income changes as specified in Equation (9) in Appendix B. The cohorts between the vertical (dashed) lines are the students aged 7–15, who are enrolled in compulsory schooling at end-year. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality level.

the resulting spending adjustment is lagging behind changes in demographic composition (Gigliotti and Sorensen, 2018).

## 4.1 Response of yearly school spending to demographic shocks

Presence of inertia in local government resource allocation implies that per-pupil spending is affected by changes in enrollment. Moreover, changes in enrollment is determined by relative sizes of the cohorts that enter and exit from compulsory schooling. To examine inertia in the spending adjustment, we specify a model of local government yearly education spending in Appendix B. This model includes year and municipality fixed effects, and accounts for dynamic responses of local governments to demographic shocks and income shocks.

Based on regression results from this model, Panel $a$ of Figure 2 display elasticity estimates of log yearly school spending on log cohort sizes by age. We find that education spending responds quite slowly to changes in enrollment. Following entry of a large birth cohort in compulsory schooling, there is a gradual increase in school spending during the nine years of enrollment. Moreover, education spending responds slowly to exit of a large birth cohort from compulsory schooling. As a result, education spending is significantly affected by size of cohorts which have recently graduated.

Next, we employ per-pupil spending as dependent variable in the model of local government education spending, whereby yearly education spending is normalized by enrollment. Panel $b$ of Figure 2 report elasticity estimates of log yearly per-pupil spending on log cohort sizes by age. The significantly negative estimates for the school-age cohorts reflect that the spending increase is insufficient to offset the enrollment increase due to entry of a large cohort. Conditional on other cohort sizes, there is a discrete jump in per-pupil spending when a large cohort finishes compulsory schooling. Since the spending adjustment is slow, this jump follows mechanically from the fact that the graduating cohort is abruptly excluded from enrollment. The increase in per-pupil spending fades away rather slowly, as indicated by sustained improvement over several years up to age 20 of the graduated cohort. Appendix B provides an in-depth discussion of the relationship between municipal education spending, enrollment, cohort sizes and local government income.[8]

## 4.2 Response of spending during school-age years

To further investigate how school spending is affected by changes in enrollment, we formulate a model of spending adjustment where spending during all school-age years is used as dependent variable. To establish a measure of average school spending across school-age years, we employ the relationship $t = c + a$ between time, cohort and age to project yearly per-pupil spending onto the cohort and age dimensions. Consistent to the institutional setting, this method exploits that children aged 7-15 are enrolled in compulsory education, whereas other cohorts are excluded. Thus, our cohort treatment variable is defined by the average log spending per pupil over the nine years corresponding to mandatory school age (For details, see Appendix A). Variation in treatment (within municipalities) comes from the non-overlap between the periods when different cohorts of children attended schooling, and from changes in per-pupil spending over time. Let $u_{mc}$ denote *per-pupil spending*, defined by average log spending per pupil in municipality $m$ over the nine years when cohort $c$ attended compulsory schooling. The model of per-pupil spending is expressed by:

$$u_{mc} = \mu_m + \delta_c + \sum_{a=0}^{A} \tau_a n_{mca} + x'_{mc}\theta + \epsilon_{mc}, \tag{1}$$

where $\mu_m$ and $\kappa_c$ denotes a full set of fixed effects for municipality and cohort, and $n_{mca}$ is log number of people in municipality $m$ belonging to the birth cohort which turns $a$ years old in the year of age 7 of cohort $c$.[9] Thus, the parameter $\tau_a$ captures the partial elasticity of age $a$ cohort size on per-pupil spending.[10] The vector $x_{mc}$ includes controls

[8]We show that operational education spending is quite income-inelastic. Hence, we do not control for municipal incomes in the model of spending during all school-age years.

[9]Cohort size by age is measured at end-year in the year of school entry of cohort $c$. Thus, cohort size is defined at different points in time relative to different cohorts, where $n_{mc7}$ denotes own-cohort size of the treated cohort at age 7.

[10]Notice that $u_{mc} = \tilde{u}_{mc} - s_{mc}$, where $\tilde{u}_{mc}$ is average log spending, which we term *school spending*, and $s_{mc}$ is average log enrollment, which we term *enrollment* for brevity. Averages are taken over the nine years when cohort $c$ attended compulsory schooling in municipality $m$. A decomposition of (1) is obtained by separately regressing spending and enrollment during school-age years on the right-hand-side

for demographic characteristics related to cohort $c$ in municipality $m$.[11]

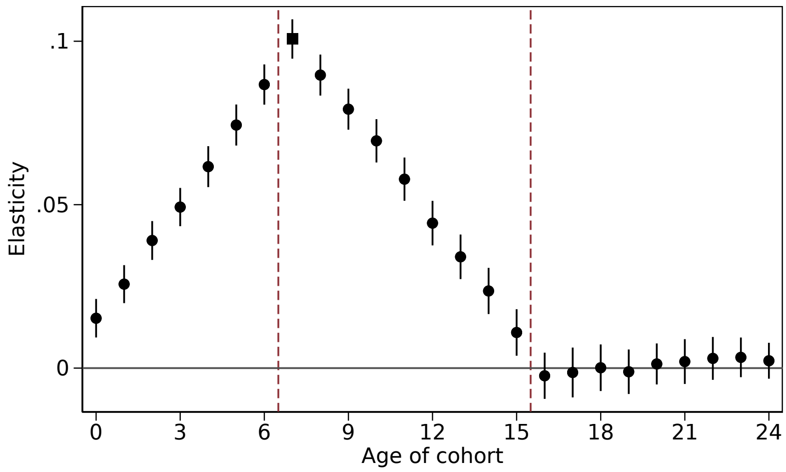
Figure 3 reports the responsiveness of spending and enrollment during school-age years with respect to cohort sizes by age, based on the estimation results of Equation (1). The top panel ($a$) displays elasticity estimates when enrollment ($s_{mc}$) is used as substitute for the dependent variable in Equation (1), where $s_{mc}$ is defined by average log enrollment across all school-age years. There is a pyramid contour for the elasticity estimates of enrollment with respect to cohort size by age. The estimated apex of elasticities on the treated cohort (age 7) reflects the inverse relationship between cohort-distance and duration of the overlapping period when two different cohorts are enrolled. On the intensive margin, the greatest degree of overlap is obtained for the two adjacent cohorts (at age 8 and 6), which are one year older or younger than the treated cohort. On the extensive margin, there is no overlapping period of schooling with the cohorts that are born at least nine years earlier or later than the treated cohort.

The middle panel ($b$) of Figure 3 shows a rather different picture for the elasticity estimates of school spending ($\tilde{u}_{mc}$) with respect to cohort size by age. Here, the dependent variable is *not* standardized by enrollment. If total spending were adjusted rapidly to compensate for changes in total enrollment, Panel $b$ would have displayed a coefficient profile similar to Panel $a$. Instead, there is a plateau of high spending elasticities for cohorts aged 7—16, which indicates a strongly asymmetric impact from sizes of older versus younger cohorts. Older cohorts (born before the treated cohort) display relatively large estimates for the effects of cohort size on school spending in comparison to younger cohorts (born after the treated cohort). Although the cohorts aged 16—21 have completed compulsory schooling at the time when 7-year-old children enter school, those older cohorts exhibit significantly positive effects on spending dosage for the treated cohort. This configuration of estimates is clearly driven by the sluggish spending adjustment that we find in yearly spending data (Appendix B).

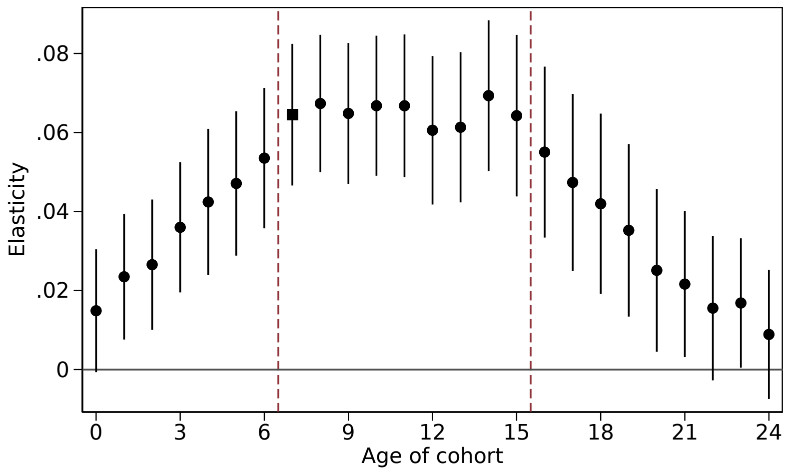
Finally, Panel $c$ of Figure 3 shows elasticity estimates of per-pupil spending ($u_{mc}$) with respect to cohort size by age. Reflecting the difference between estimates in the first two panels, there is a zigsag-shaped coefficient profile. For the treated and adjacent cohorts, the enrollment effect is dominating compared to the spending effect. Hence, elasticity estimates are significantly negative for cohorts aged 4–8. The reason is that increased enrollment crowds out spending per student through competition over resources during many years of schooling. Conversely, elasticity estimates are significantly positive for cohorts aged 13—21, for which there is little or no enrollment overlap with the treated cohort. A shorter period of overlapping enrollment reduces exposure to competition for school resources. However, the entrenchment of school spending is rather persistent, since

expression.

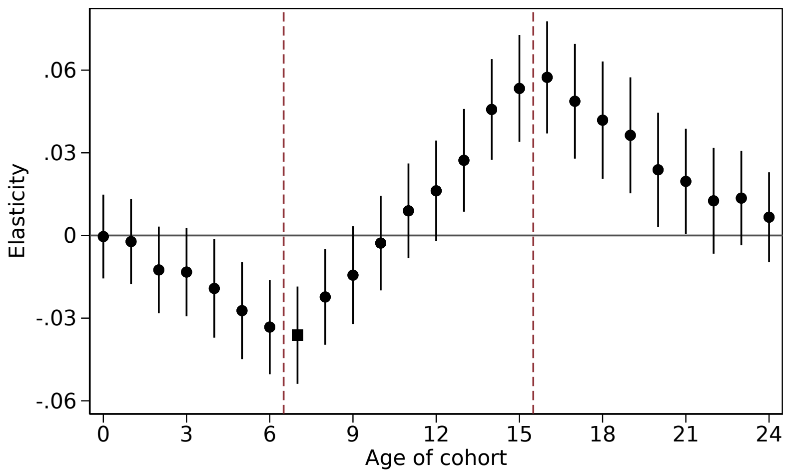
[11] When using $A = 24$, we control for the size of five-year age groups above age 24. Moreover, to ensure that parameter estimates are unconfunded by trends in population size, we control for log municipal population size in different childhood years by age.




**(a)** Enrollment




**(b)** School spending




**(c)** Per-pupil spending

**Figure 3.** Response of spending and enrollment during school-age years

*Note:* These figures show the responsiveness of (a) enrollment, (b) school spending, and (c) per-pupil spending with respect to cohort size by age. Dependent variables are measures of average log enrollment and average log spending (per pupil) across all mandatory school-age years of the treated cohort (square marker). Elasticity estimates are obtained from OLS regressions of the model in Eq. 1. The vertical (dashed) lines encompass the students aged 7–15, who are enrolled in compulsory schooling when the treated cohort (age 7) attends first grade. The markers represent point estimates of partial elasticities, and the vertical spikes represent 95-percent confidence intervals. Standard errors are clustered at the municipality-of-birth level.

the reversal process of extra spending (cost savings) on a relatively large (small) cohort only begins when the cohort has finished school. Consequently, the greatest and most significant elasticity estimates are found for the cohorts which exit compulsory schooling around the time of school entry of the treated cohort.

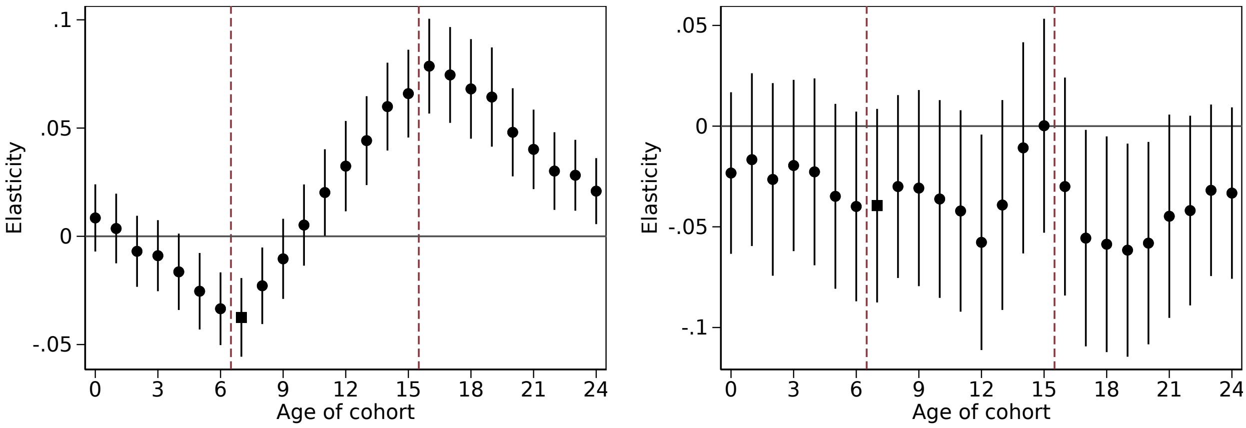
## 4.3 Allocation of inputs

The municipal accounting data in our study allows for a breakdown of spending on salaries and other operating expenses. Salaries mainly consist of teacher wages, but also include wages of other employees such as administrative staff. Spending on other inputs include expenses paid for semi-durables such as teaching materials, equipment and school maintenance. On average, other inputs account for about 20 percent of total operating expenses. Since the wage scale of teachers is rather compressed, it is clear that most of the variation in wage cost per pupil is associated with variation in the student-teacher ratio.

Figure 4 shows elasticity estimates of wage cost per pupil and other spending per pupil with respect to cohort size by age.[12] The results for wage cost display a zigzag-shaped coefficient profile. In comparison to the results for total spending in Table 3, elasticity estimates are even larger and more significant, not least for older cohorts which graduated before entry of the treated cohort. These findings suggest that stickiness of spending is associated with job switching barriers and turnover costs. By contrast, the results for spending on other inputs show elasticity estimates that are mostly insignificant and negative. If anything, other spending is adjusted to offset changes in labor cost per child, perhaps to smooth fluctuations in total spending per child. When recently graduated cohorts (age 17—20) are large, overspending on salaries may crowd out some spending on teaching materials and maintenance during school years of the treated cohort.

Appendix C reports results from analyzing the impact of cohort sizes on class size experienced by the treated cohorts during years of compulsory schooling. The results show that there is competition over classrooms (staffed with teachers) between children in the treated cohort and adjacent cohorts of children. As a results, children belonging to a large cohort and/or children with larger adjacent cohorts are exposed to larger class sizes during years of compulsory schooling. By contrast, the elasticity estimate of class size of the school-starting cohort with respect to school-exiting cohort size is small and insignificant.

[12]Similarly as for total school spending, log spending on wages and other inputs is averaged over the 9 years corresponding to mandatory school age.




**(a)** Wage costs per pupil

**(b)** Other spending per pupil

**Figure 4.** Response of wage cost and other spending during school-age years

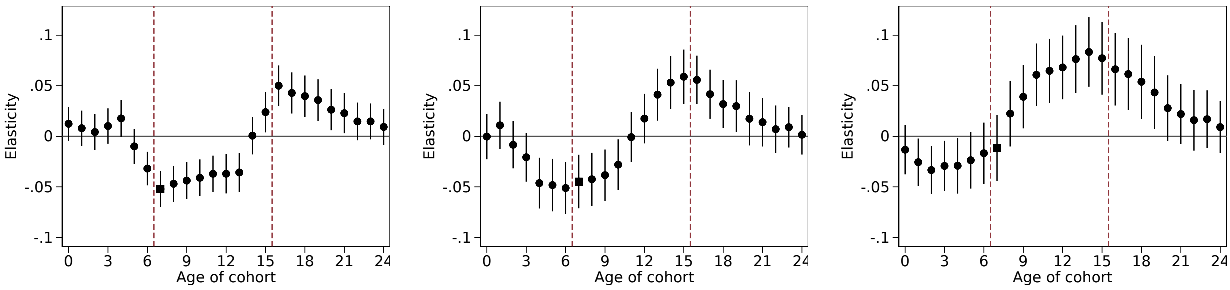
*Note:* These figures show the responsiveness of wage costs and other operating expenditure per pupil with respect to cohort size by age. Estimates represent results from OLS regressions of the model in Eq. 1. Dependent variables are measures of average log wage cost per pupil ($a$) and average log other operating expenditure per pupil ($b$) across all mandatory school-age years of the treated cohort (square marker). The cohorts between the vertical (dashed) lines are the students aged 7–15, who are enrolled in compulsory schooling when the treated cohort (age 7) attends first grade. The markers represent point estimates of partial elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.

## 4.4 Per-pupil spending during educational stages

So far, our aggregation method ignores that changes in per-pupil spending may occur during different stages of compulsory schooling. Consistent to the institutional setting, we distinguish between the following educational stages; (i) the first three grades in primary school (ages 7-9), (ii) the last three grades in primary school (ages 10-12), and (iii) the three grades in middle school (ages 13-15). Let $u_{mc}^{7-9}$ denote average log spending per pupil measured over ages 7-9, and define $u_{mc}^{10-12}$ and $u_{mc}^{13-15}$ by corresponding measures over ages 10-12 and 13-15.[13] Next, these variables are employed to estimate the model of spending per pupil separately for three different educational stages.

Figure 5 shows elasticity estimates of per-pupil spending by educational stages with respect to cohort size by age. Negative cohort size elasticities are found for cohorts which are enrolled in compulsory schooling during a given educational stage, reflecting that spending adjustments do not fully offset changes in enrollment. The elasticities for cohorts not enrolled during a stage are around zero for younger cohorts and significantly positive for older cohorts up to age 19. Positive cross-cohort size elasticities extend to more cohorts as they finish compulsory schooling during the progression of the treated cohort. Looking at sub-figures *a*–*c* in sequence, the elasticity profiles resemble a rising tide that successively covers more cohorts that are closer in age to the treated cohort. Intuitively, when a particularly large cohort exits from compulsory schooling, there is a discrete and immediate jump in per-pupil spending for those children who remain in school. Because

[13]In accordance with our general procedure, the measures are adjusted to account for the fact that three subsequent school years are overlapping with four calendar years.




**(a)** Grades 1-3 **(b)** Grades 4-6 **(c)** Grades 7-9

**Figure 5.** Response of per-pupil spending by educational stages

*Note:* These figures show the responsiveness of average school spending per student, during three stages of compulsory schooling, with respect to cohort size by age. Estimates represent results from OLS regressions of the model in Eq. 1. Dependent variables are average log spending per child measured separately across grades 1-3 (ages 7-9), grades 4-6 (ages 10-12) and grades 7-9 (ages 13-15). The cohorts between the vertical (dashed) lines are the students aged 7–15, who are enrolled in compulsory schooling when the treated cohort (age 7) attends first grade. The markers represent point estimates of partial elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.

of discreteness of school exit and entry combined with stickiness of school spending, there are discontinuities and shocks in per-pupil spending at the transition between school years. Of particular interest, we find that size of the cohort (age 16) that graduates just before school entry of the treated cohort (age 7) displays partial elasticities on per-pupil spending that are significantly positive, where point estimates are around 0.06 and slightly increasing over the course of stages.

# 5 Identification strategy

In this section, we describe our empirical strategy for identifying the causal effects of school spending on children's long-run outcomes. Any direct association between outcomes and spending might be endogenous for several reasons. Under egalitarian objectives, the allocation of school resources determined by teachers, schools and local governments is compensatory towards children with low ability and achievement. When more spending is targeted to children with low potential outcomes, there is a reverse causality problem and OLS estimates of the effects of school spending are biased downwards. On the other hand, sorting of families by socioeconomic status may generate positive associations between child ability and school spending. For instance, affluent neighborhoods may generate more municipal tax revenues than poor neighborhoods, and parents who invest more in their children may choose to reside in areas with high per-pupil spending. Note, however, that the relationship between targeting and sorting is mixed at the municipal level in Norway. Due to several fiscal disparities, relatively high spending levels are found in less central regions as well as in the most affluent suburban areas with a high income tax base.

## 5.1 Instrumental variable model

To identify the causal effects of school spending, we exploit school-exiting cohort size as a source of exogenous variation. Hence, exiting cohort size is used as an instrumental variable (IV) to correct for selection bias. This strategy is implemented in a two-stage least squares (TSLS) model with fixed effects, which controls flexibly for the sizes of cohorts other than the exiting cohort. The model is expressed by:

$$y_{imc} = \eta_m + \kappa_c + \beta \cdot u_{imc} + n'_{mc(-z)}\gamma_{(-z)} + x'_{imc}\lambda + \varepsilon_{imc} \tag{2}$$

$$u_{imc} = \mu_m + \delta_c + \tau_z \cdot z_{mc} + n'_{mc(-z)}\tau_{(-z)} + x'_{imc}\theta + \epsilon_{mc} \tag{3}$$

where $y_{imc}$ is the outcome variable for individual $i$ in birth cohort $c$ born in municipality $m$. Average log per-pupil spending over the school-age years of individual $i$ born in municipality $m$ and cohort $c$ is denoted $u_{imc}$, which we call *residence-based per-pupil spending*. Parameters $\eta_m$ and $\mu_m$ account for municipality fixed effects, whereas $\kappa_c$ and $\delta_c$ are cohort fixed effects. The instrument $z_{mc}$ is defined by log cohort size of the cohort (age 16) that is exiting compulsory schooling at the time of entry of the treated cohort (age 7). To distinguish between instrument and covariates, cohort sizes of other cohorts than the school-exiting cohort are contained in the vector $n_{mc(-z)}$.[14] Parameter vectors $\gamma_{(-z)}$ and $\tau_{(-z)}$ account for the corresponding cohort size effects, where the instrument cohort is excluded. The vector $x_{imc}$ includes a full set of indicator variables for number of siblings and birth order of child $i$. These covariates are included to alleviate any concern that family size and birth order is correlated with the exiting cohort instrument.[15] Stochastic error terms are denoted $\varepsilon_{imc}$ and $\epsilon_{mc}$, respectively. Parameters of interest are the second-stage treatment effect $\beta$, and moreover $\tau_z$ which captures the first-stage effect of the covariate-adjusted instrument on school spending.

Notice that, for families who move during childhood, the school spending treatment variable $u_{imc}$ adds up per-pupil spending across different municipalities according to where the child resided in each year (see Appendix A). For children who spend their childhood in the municipality of birth, the residence-based per-pupil spending equals the per-pupil spending associated with that municipality ( $u_{imc} = u_{mc}$). For movers, $u_{mc}$ represents the average school spending they would have received if they never moved. Children who move during compulsory years of schooling receive some years of treatment from their municipality of origin, whereas those who move before school starting age solely receive treatment from their municipality of destination.

[14] The cohort size vector $n_{mc(-z)}$ includes (log) number of residents by municipality and birth cohort from age 0 to 24, except that the exiting cohort (age 16) is omitted. Moreover, the vector includes (log) cohort sizes of five-year age groups for ages 25–29, 30–34, ..., 80–89, age 90 and above. Cohort sizes are measured at the time of school entry of the treated cohort (age 7).

[15] Specifically, it appears that higher birth order increases the probability of having siblings in the exiting cohort, which creates a positive association between birth order and the instrument.

As a robustness check, we estimate an alternative version of the baseline TSLS model in Equations (2)– (3) where the treatment variable is the spending provided in the municipality of birth, $u_{mc}$, instead of the residence-based school spending obtained through family movement. Since all students are then assigned the treatment associated with their municipality of origin, this specification identifies the intention-to-treat (ITT) effects of school spending on child outcomes. By contrast, the estimand in the baseline model identifies the local average treatment effect (LATE) for the students actually exposed to instrument-induced spending variation in the municipality of origin. Family movement is considered to reduce the rate of compliance with the instrument, since the instrument is defined by school-exiting cohort size in the municipality of origin. Therefore, children who move are not (fully) exposed to the instrument. For identification, we assume that changes in per-pupil spending acquired through family movement are unrelated to the instrument.[16]

## 5.2 Instrument validity

We now discuss validity and threats to our identifying assumptions, including relevance, independence, exclusion and monotonicity, which are standard assumptions in applications of instrumental variable methods (Angrist et al., 1996; Angrist and Imbens, 1995). When the standard assumptions are valid, the TSLS estimate will capture an average causal effect for individuals in the complier population (LATE). In our setting, compliers are children who receive more school spending when the exiting cohort is relatively large.

***Relevance***

The relevance assumption requires that the instrument must be correlated with the treatment. Figure 3 in the previous chapter demonstrates relevance, since the exiting cohort size shows a significantly positive effect on per-pupil spending during school-age years of the treated cohort. Moreover, Figure 5 displays significantly positive and rather stable effects of the instrument on per-pupil spending through different stages of compulsory schooling. There is a sizable and persistent change in per-pupil spending when a particularly large (or small) cohort exits from compulsory schooling. Since this effect slowly fades away, optimal timing of exposure to the instrument is obtained by the school-entering cohort which replaces the school-exiting cohort in the population of enrolled students.

Differential relevance of the instrument is a concern if there is heterogeneity in the allocation of the instrument-induced spending variation across children with different ability or socioeconomic status (SES). This could mean that the instrument is relevant for a subset of children who are targeted through the allocation process (e.g. low-SES children). We cannot directly evaluate this hypothesis, because municipal accounting data

[16] We provide evidence supporting this assumption in Table 3.

do not report the detailed allocation of school resources to individual children. However, since classmate composition is heterogeneous and teachers provide a mixture of joint and individually adapted instruction, it appears plausible that the instrument is generally relevant for different subgroups.

Another concern is that the lock-in of teachers on schools and classrooms in combination with crowd-out effects might lead to disproportional allocation of instrument-induced spending variation across grades and cohorts of children. In Appendix (C), we present evidence that the disproportional allocation of classrooms is first and foremost driven by variation in treated own-cohort size and sizes of adjacent cohorts. By contrast, average class size within the treated cohort is uncorrelated to our exiting cohort instrument. When controlling for non-instrument cohort sizes, our identifying spending variation does not come from disproportionality due to lock-in of teachers on classrooms. Furthermore, since the enrollment periods of the exiting and the treated cohort do not overlap, there is never direct crowd-out due to resource competition between those two cohorts.

If there were lock-in of resources on primary schools versus middle schools, it appears plausible that our instrument is relevant also during different educational stages. The reason is that resource availability during stages is positively related to our instrument regardless of whether spending inertia operates through lock-in on school units or lock-in on compulsory schooling organized as a department of local government service production. In the former case, richness or scarcity of resources are left behind first in primary schools and later in middle schools when the exiting cohort progresses through the school stages. In the latter case, resources that are locked-in on the instrument cohort are more freely re-allocated across stages at the time of school entry of the treated cohort. In summary, the institutional setting and indirect evidence indicates that the treated cohort does indeed benefit (suffer) from an increase (decrease) in per-pupil spending when the 9 years older exiting cohort is particularly large (small).

***Conditional independence***

The conditional independence assumption requires that children's potential outcomes are independent of the covariate-adjusted instrument. This means that, conditional on covariates, the instrument must be as-good-as randomly assigned. A threat to identification is that our instrument might be associated with underlying demographic trends that are correlated with children's long-run outcomes. To address this concern, we control flexibly for the cohort sizes of all other cohorts than the exiting cohort, and moreover for family size, children's birth order, municipality and cohort fixed effects. This allows us to purge the exiting cohort instrument for trends in cohort size by municipality, while retaining the high-frequent differences in cohort size that are plausibly exogenous. By exploiting random population variation, our method is similar in spirit to Hoxby (2000), except that we employ the exiting cohort size as instrument rather than own-cohort size of the treated cohort.

Although the independence assumption is not directly testable, a testable implication is that the instrument is uncorrelated with predetermined characteristics which affect children's future outcomes. A common way to assess this implication is by investigating whether the instrument passes a balancing test. Table 2 shows the results of such balancing test, where we estimate OLS regressions of children's outcomes and size of the instrument cohort on predetermined characteristics, including parental income, education, age at birth, immigrant background, marital status, and mother's labor market participation. Columns 1–3 show the coefficients from regressions of long-run outcome variables on family characteristics and gender, conditional on the vector of controls in our baseline specification. Most of these coefficients are statistically significant both individually and jointly as indicated by the F-tests. In Column 4, we show the coefficients from the same regressions with the instrument as the dependent variable. Here, barely any coefficients are statistically significant and they are not jointly significant at conventional levels. It is reassuring to find that the instrument is uncorrelated with predetermined family characteristics that are strong predictors of the outcomes of interest. Since the regressions in 2 is estimated on the pooled sample of boys and girls, we report separate balancing checks for boys (Table D1) and girls (Table D2) in the Appendix. We find that F-tests for the effects of family characteristics on the instrument variable are jointly insignificant both for boys ($p = .28$) and girls ($p = .42$).

Exogeneity of the instrument might be violated if families were able to manipulate the instrument-induced spending variation through their choices. For instance, families may move prior or subsequent to a child's school entry to acquire a more favorable exposure to exiting cohort size. To respond strategically before school entry, families would need to understand the identifying mechanism and moreover obtain information regarding geographical variation in exiting cohort size. Subsequent to school entry, movement decisions might be affected when observing scarcity or abundance of school resources in their original location or in potential destinations. We examine whether movement across municipalities is a source of endogenous sorting in Table 3. An indicator for family movement during preschool years and school-age years is regressed on the instrument and all controls in our main specification (Eq.3). The estimated coefficients for the instrument is reported in Column 1 for boys and girls. We find that the effects of exiting cohort size on family movement are statistically insignificant. Still, one might worry that the instrument affects the choice of destination for mover families. Column 2 shows that the instrument has no discernible effect on the residence-based school spending received by those who move before school-starting age. The effect of the instrument on residence-based school spending for individuals who move at any time during childhood is reported in Column 3. As expected, due to some exposure to schooling in the municipality of birth, the effect on all movers is positive, but quite small and statistically insignificant at conventional levels.

**Table 2.** Balancing tests

| | Log-income (1) | Years of schooling (2) | Single (3) | Instrument (4) |
|---|---|---|---|---|
| Parental log-income | 0.080*** | 0.56*** | -0.023*** | 0.00058 |
| | (0.0042) | (0.016) | (0.0031) | (0.00036) |
| Mothers' years of schooling | 0.0063*** | 0.22*** | -0.00069 | -0.000093 |
| | (0.00080) | (0.0042) | (0.00051) | (0.000093) |
| Fathers' years of schooling | 0.0056*** | 0.19*** | -0.00082* | -0.000011 |
| | (0.00041) | (0.0024) | (0.00047) | (0.000065) |
| Mother not working | -0.015*** | -0.29*** | 0.0081*** | -0.000055 |
| | (0.0037) | (0.024) | (0.0025) | (0.00041) |
| Age of father | -0.0019*** | 0.0022 | 0.0035*** | -0.000064* |
| | (0.00027) | (0.0015) | (0.00020) | (0.000033) |
| Age of mother | 0.0015*** | 0.082*** | 0.000083 | -0.000017 |
| | (0.00033) | (0.0021) | (0.00024) | (0.000047) |
| Immigrant | -0.073*** | -0.10*** | 0.033*** | -0.00050 |
| | (0.013) | (0.033) | (0.0067) | (0.0010) |
| Boy | 0.19*** | -0.62*** | 0.090*** | -0.00021 |
| | (0.0073) | (0.020) | (0.0020) | (0.00024) |
| Parents married | 0.072*** | 0.61*** | -0.091*** | -0.00096 |
| | (0.018) | (0.11) | (0.018) | (0.0034) |
| Parents cohabiting | 0.0048 | -0.028 | -0.025 | -0.0030 |
| | (0.020) | (0.12) | (0.019) | (0.00357) |
| Joint F-test | 306 | 2515 | 468 | 1.24 |
| Joint p-value | 0 | 0 | 0 | 0.24 |
| N Schools | 385 | 385 | 385 | 385 |
| N Individuals | 409 380 | 409 380 | 409 380 | 409 380 |

*Note:* This table reports coefficient estimates for regressions of family characteristics on various outcomes (Columns 1–3) and school-exiting cohort size (Column 4). The estimation sample includes male and female students. Each column reports estimation results from separate two-way fixed effects regressions that control for all covariates in our baseline TSLS specification. The F-test and p-value for joint significance is reported in the bottom of the table and includes all covariates in the table in addition to unreported coefficients for dummies of zero income, missing parental education, and other types of parental households. Standard errors (in parentheses) are clustered at the municipality-of-birth level. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

**Table 3.** Tests for endogenous responses of families and local governments

| | Family migration (ages 1-15) | School spending (mover age 1--6) | School spending (mover age 1--15) | Enrollment ratio (1st grade) | Childcare coverage (ages 4–6) |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **A. Boys** | | | | | |
| Instrument | 0.016 | 0.0074 | 0.015 | -0.0065 | -0.0032 |
| (exiting cohort size) | (0.012) | (0.012) | (0.011) | (0.0053) | (0.018) |
| N schools | 385 | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 41 163 | 52 260 | 209 570 | 209 570 |
| **B. Girls** | | | | | |
| Instrument | -0.0038 | -0.0014 | 0.0038 | -0.0069 | -0.0011 |
| (exiting cohort size) | (0.013) | (0.013) | (0.011) | (0.0052) | (0.019) |
| N schools | 385 | 385 | 385 | 385 | 385 |
| N individuals | 198 810 | 39 196 | 50 389 | 198 810 | 198 810 |

*Note:* This table reports estimation results for the effects of the instrument on five potentially endogenous variables, separately for boys (panel A) and girls (panel B). Making use of our baseline model specification, Column 1 shows effects of the instrument on family movement during childhood years before age 16. Column 2 reports effects of the instrument on school spending in the sample of students whose families move in preschool years (age 1–6), whereas Column 3 shows effects in the sample of all students who move before age 16. Column 4 reports effects of the instrument on the first grade enrollment ratio in the child's municipality of birth, where the enrollment ratio equals enrollment in first grade into public primary schools as a proportion of cohort size at the time of school entry. Finally, Column 5 displays estimated effects of the instrument on childcare coverage of the treated cohort. Childcare coverage is measured by the average participation rate in childcare from age 4 to 6 by cohort and municipality. Standard errors (in parentheses) are clustered at the municipality-of-birth level.

Among students who stay in their municipality of birth, there is a small minority that enters private primary schools which are approved by the government. A concern is that the choice between private and public schools could be affected by our instrument. Another margin of choice is available through non-compliance with the rule of mandatory school entry in the year of age 7. There is 1.2 percent who enter compulsory schooling one year too early, whereas those who start one year too late account for 2.1 percent. To account for potential selection bias, we consider the enrollment ratio, which is defined by enrollment in first grade into public primary schools relative to the population of children that is stipulated to enter compulsory schooling according to the rule of progression. Column 4 of Table 3 shows that for boys and girls alike, the estimated effects of the instrument on the enrollment ratio are small and statistically insignificant, indicating that private school choice and school starting age is uncorrelated with the instrument. Finally, Column 5 demonstrates that the instrument does not affect participation in municipal childcare during preschool years of the children in our sample. In summary, the evidence supports random assignment in balancing tests and moreover when investigating potential sources of endogenous selection through strategic behavior.

***Exclusion***

The exclusion restriction requires that the causal effect of the instrument is mediated through no other channel that differs from the first stage. In our setting, this means that changes in exiting cohort size solely affects children's outcomes by changing the level of school spending per student during school-age years.

Hoxby (2000) argues that class size may change as a consequence of random variation in the size of subsequent cohorts enrolled in a school. By utilizing variation in own-cohort size of the treated cohort to instrument for class size, she finds no significant effect of class size on student achievement. However, when analyzing children's long-run outcomes, the own-cohort size and adjacent-cohort sizes might also affect outcomes through competition over jobs at the time of labor market entry. Suppose that local labor market entrants compete over a given pool of available jobs. Then it is unfavorable to be member of a large cohort, since increased inflow of job seekers makes it harder to get attractive first job offers (Welch, 1979; Berger, 1985; Morin, 2015; Moffat and Roth, 2016). Similarly, cohort crowding effects may occur when students compete for admission into different fields of study in high schools and colleges (Bound and Turner, 2007). Against this, Reiling (2016) finds that beneficial effects of being part of a large cohort more than offset any adverse effects of reduced educational resources. Thus, various mechanisms might invalidate the exclusion restriction when instrumenting school spending with own-cohort size of the treated cohort.

On the other hand, since the exiting cohort finishes education and enters the labor market about nine years earlier than our treated cohort, there is obviously little competition over field of study or job market careers among members of those two cohorts. Moreover, since we include a vector of controls for birth cohort sizes by age, cohort and municipality, our covariate-adjusted instrument is by construction orthogonal to own and adjacent cohort sizes. Hence, our empirical strategy is designed to rule out violations of the exclusion restriction due to cohort-crowding effects.

A potential threat to identification is that the instrument-induced spending variation might be correlated with differences in expenditure on other public services provided by local governments. For a fixed local government budget, a high level of education spending may crowd out spending on other local public services (Baicker and Gordon, 2006). In particular, local government provision of subsidized child care services to preschoolers is a municipal service that is targeted to families with children. During the 1970s and 1980s, childcare coverage varied considerably across municipalities (Havnes and Mogstad, 2011). Children were enrolled in child care mostly from ages 4-6, which corresponds to the period when the exiting cohort attended lower secondary school (grade 7–9). In Column 5 of Table 3, we consider the effect of the exiting cohort instrument on municipal childcare participation during preschool years of the treated cohort, conditional on our main specification controls. There are small and insignificant effects of instrument cohort size on childcare participation among treated children. This finding indicates that childcare participation is irrelevant as an alternative channel for the effect of our instrument on children's outcomes. Moreover, Figure B3 in Appendix B suggests that effects of the instrument on children's outcomes are not transmitted through changes in school investments.

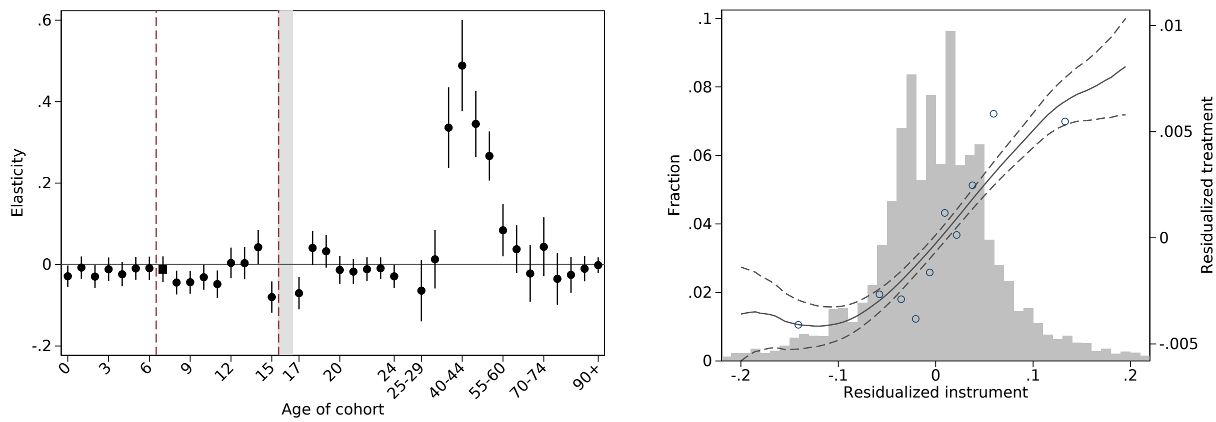
***Monotonicity***

When accounting for heterogeneous effects across individuals, we invoke a monotonicity assumption to identify causal effects for the compliers. The monotonicity assumption requires that for any value of the instrument, counterfactually increasing (decreasing) the instrument cannot result in a decrease (increase) in the treatment dosage. In our study, this means that school spending per child during school-age years will not decrease if the exiting cohort size is increased, and vice versa. For instance, if low school spending induced by the instrument stimulate moving families to choose destinations with higher levels of school spending, this would violate the monotonicity assumption and the estimated effect could no longer be interpreted as LATE. However, as we demonstrated in the first three columns of Table 3, neither the decision to move, nor the level of spending received by those who move, are correlated with the instrument. The monotonicity assumption cannot be directly evaluated, but in Chapter 4 we discuss the possible mechanisms underlying the first stage of the instrument. We argue that the non-overlapping enrollment periods of the treated and instrument cohorts rule out any direct crowd-out effects on spending between those two cohorts. Moreover, violation of monotonicity through targeting means that some children are treated as inferior in the allocation of spending, which contradicts the egalitarian objectives of the compulsory school system.

## 5.3 Covariate-adjusted instrument

Since we assume that exiting cohort size is conditionally exogenous, one might worry that the relationship between the instrument and covariates is misspecified in our TSLS model. To investigate this concern, Figure 6 provides a graphical representation of parameter estimates and residuals from the regression of exiting cohort size on covariates. Based on these results, we consider whether (i) the regression of the instrument on covariates produces meaningful results, and (ii) the distribution of residuals is well-behaved and displays substantial variation.

The left-most panel $(a)$ of Figure 6 shows estimates of partial elasticities in the log-linear regression of exiting cohort size on the sizes of other cohorts by age. A full set of fixed effects for municipality, cohort, birth order, and family size are controlled for in the regression. Hence, the regression model for exiting cohort size is equivalent to the first-




**(a)** Regression of exiting cohort size on other cohort sizes

**(b)** Residualized instrument

**Figure 6.** Regression of instrument on covariates

*Note:* These figures show the results from a modified first-stage regression where the specification is identical to Eq. (3), except here the instrument (exiting cohort size, age 16) is the dependent variable. The left-most panel (a) plots the partial elasticities of school-exiting cohort size with respect to cohort sizes of other age groups. The vertical gray bar indicates that exiting cohort size is omitted as regressor when used as dependent variable. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes (male sample). Standard errors are clustered at the municipality-of-birth level. The right-most panel (b) shows a histogram of the residuals from the same regression. Additionally, we illustrate the first-stage relationship between the residualized instrument and the (similarly) residualized treatment variable in two different ways. First, the hollow circles represent the mean residualized treatment across 10 equally sized bins of the ranked residualized instrument. Second, the solid black line shows the slope of a local linear regression of the residualized treatment on the residualized instrument (the dashed black lines represent the 95 percent CI) using default Epanechnikov kernel with 0.08 bandwidth. We utilize all observations to generate the binned means and the local linear regression, but for visibility reasons we limit the x-axis to display residuals between -0.2 and 0.2 which equals 97.5 percent of the observations.

stage specification, except that log exiting cohort size is omitted as predictor variable when treated as dependent variable. We find that cohort size elasticity estimates do not differ significantly from zero for the youngest cohorts (ages 0-7), for young adults (ages 20-34) and for elderly cohorts (ages 60 and above). A plausible explanation is that those cohorts rarely contain parents or siblings of individuals in the exiting cohort (age 16). By contrast, cohort-size elasticity estimates are sizable and significantly positive for middle-aged adults (ages 35-54), who are in the typical life-cycle stage for having children in the school-exiting cohort. Moreover, there are significantly negative elasticity estimates for primary adjacent cohorts (ages 15 and 17) and positive estimates for secondary adjacent cohorts (ages 14 and 18). Natural population variation may produce this pattern, because siblings are seldom born in consecutive calendar years. It is more common that the age-difference of siblings is around two years. In summary, we find that presence of family relationships among members of different cohorts generates significant cohort-size correlations due to family structure.

The right-most panel (*b*) of Figure 6 displays a histogram of the residuals from the regression of exiting cohort size on covariates. These residuals represent the covariate-adjusted instrument, or residualized instrument, which we exploit as our source of identifying variation. By construction, the residualized instrument is uncorrelated with covariates. The distribution of residuals is nearly symmetric around zero with standard deviation 0.075. Thus, the presented evidence supports our claim that estimation results from the demographic nuisance model are meaningful and well-behaved.

Blandhol et al. (2022) show that "covariate richness" is important for ensuring that TSLS has a causal interpretation. Covariate richness requires that the predicted values obtained from the regression of instrument on covariates accurately reproduce the conditional mean function of the instrument. In other words, the specification has to be sufficiently flexible to capture the functional form of the relationship between instrument and covariates. Consequently, we perform Ramsey's (1969) RESET test for the regression of instrument on covariates. The $p$-value from this test is 0.92. Thus, it is reassuring to find that the RESET test fails to reject the null hypothesis that the specification has rich covariates.

# 6 Empirical results

This section presents estimation results of the effects of compulsory school spending on children's long-run outcomes using the TSLS setup from Equations (2)– (3) in the previous section.

## 6.1 Long-run effects on income

First, we return to the question about relevance of the instrument. Panel A of Table 4 shows the first-stage estimates, which are statistically significant at conventional levels. At the bottom of the table we supply the F-value of the test for instrument significance.[17] These results support our claim that the instrument satisfies the relevance assumption. The point estimates imply that when school-exiting cohort size increases by 10 percent, there is an increase in per-pupil spending during school-age years that exceeds 0.4 percent.

The reduced-form regression model is obtained by substituting the first-stage expression from Equation (3) into the second stage Equation(2). The reduced-form results in Panel B of Table 4 display estimates of the direct elasticity of the instrument on individual income. The income variable for the child generation is measured over ages 27-36 and accounts for individual disposable income, which includes after-tax earnings, capital incomes and cash transfers. We find that males benefit significantly from exposure to a larger school-exiting cohort, whereas estimates are insignificant for females. The point estimates for males implies that an increase by 10 percent in school-exiting cohort size will increase individual income by 0.29 percent, and reduce the risk of having low income by 0.23 percentage points.

[17]The instrument is statistically significant at all traditional levels and above the Stock and Yogo (2005) critical value for maximum 5-percent relative bias.

**Table 4.** Effects of school spending on individual income in adulthood

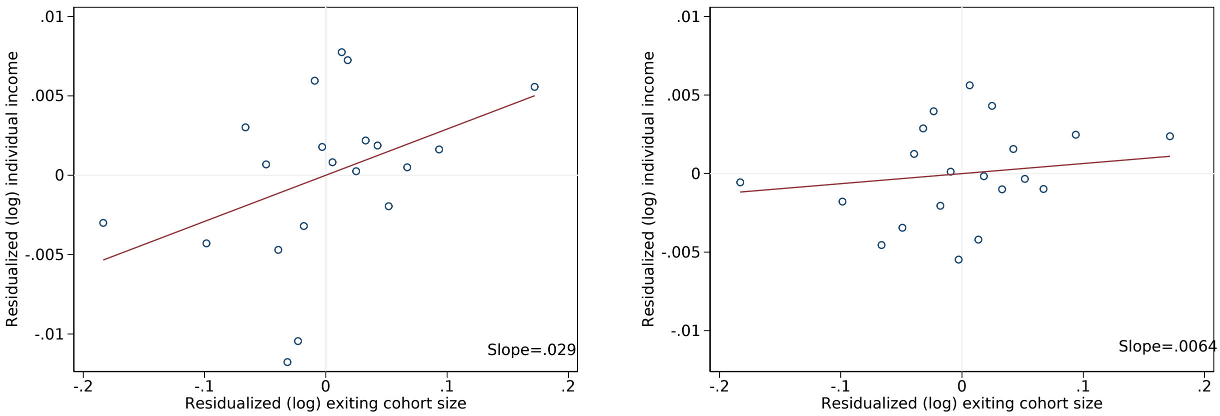
| | Log-income | | Low income ($\leq 25pct$) | |
|---|---|---|---|---|
| | Males | Females | Males | Females |
| | (1) | (2) | (3) | (4) |
| **A. First stage:** | | | | |
| Exiting cohort size | 0.041*** | 0.047*** | 0.041*** | 0.047*** |
| | (0.0075) | (0.0080) | (0.0075) | (0.0080) |
| **B. Reduced form:** | | | | |
| Exiting cohort size | 0.029** | 0.0064 | -0.023** | -0.013 |
| | (0.013) | (0.012) | (0.011) | (0.015) |
| **C. TSLS:** | | | | |
| School spending | 0.71** | 0.14 | -0.55** | -0.27 |
| | (0.32) | (0.25) | (0.27) | (0.31) |
| Joint F-test (FS) | 30.1 | 33.6 | 30.1 | 33.6 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 199 810 | 209 570 | 199 810 |

*Note:* This table reports estimation results for the effects of average log school spending during compulsory schooling on (i) log average income, and (ii) indicator for low income during ages 27-36. Estimation is done separately for males and females. Panel A displays the first-stage effects of the school-exiting cohort instrument on school spending. Panel B shows the reduced-form effects of the instrument on the outcome, and Panel C reports TSLS estimates of school spending on the outcome. As instrument for school spending we utilize the cohort size (by municipality) of the students who graduated from compulsory schooling just before entry of the treated cohort of students. An F-test for instrument significance is reported in the bottom of the table. Standard errors (in parentheses) are clustered at the municipality-of-birth level. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

Panel C presents TSLS estimates of compulsory school spending on log income and the low-income indicator. The effect estimates of school spending on income in adulthood differ by gender, and are not significant for females. For males, the elasticity estimate on individual earnings is 0.71 and statistically significant at the 5 percent level. This implies that a 10 percent increase in per-pupil spending in all school-age years will, on average, increase adult incomes by 7.1 percent. While this point estimate is large, it is in line with findings in the recent literature that employs plausibly exogenous sources of variation in school spending, such as Jackson et al. (2016). Moreover, for males, Column 3 displays a significantly negative effect of school spending on the low-income indicator, which indicates substantial school spending impacts among individuals in the lower part of the income distribution.

Increases in human capital might improve adult outcomes through assortative mating. To probe this mechanism, we consider the impact of school spending on family income in adulthood. Table D3 in Appendix D displays similar results for family income as for individual income. Elasticity estimates are somewhat smaller for the family income of males. Thus, the results do not indicate that assortative mating plays an important role in mediating long-term effects of changes in compulsory school spending.

By applying the Frisch-Waugh-Lovell Theorem, we create a graphical representation of




**(a)** Males **(b)** Females

**Figure 7.** Binned scatter plots of reduced-form relationship

*Note:* These figures are binned scatter plots of the relationship between residualized individual income and residualized instrument. Figure (a) and (b) report the reduced-form relationship in the male and female sample, respectively. Computation of the residualized instrument is explained in Section 5.3 and Figure 6. We perform analogous regressions of log individual income on covariates to partial out residuals for the outcome variable. The bins represent vingtile groups for the residualized instrument. Plotted values are group means of the residualized instrument on the horizontal axis versus group means of the residualized individual income in adulthood. Lines represent OLS regression estimates for residualized individual income on the residualized instrument. Slopes of the regression lines show the responsiveness of individual income in adulthood with respect to school-exiting cohort size, and correspond to the reduced-form elasticity estimates for males and females in Table 4.

the reduced-form estimates in Figure 7, separately for (a) males and (b) females. Plotted circles represent binned group means of residualized individual income versus residualized instrument. Lines represent OLS regression estimates for the residualized outcome variable on the residualized instrument. The relatively steep slope of the regression line for males corresponds to the sizable reduced form estimate found in Column 1 of Table 4. In contrast, the regression line for females is much flatter, reflecting the small and insignificant estimate found in Column 2.

In a similar fashion, appendix Figure D3 provides a graphical representation of the first-stage relationship, suggesting that this relationship is reasonably represented by a linear functional form. Additionally, panel (*b*) of Figure 6 shows a local linear regression of the residualized treatment on the residualized instrument. This is a flexible analogue to the first-stage relationship in Equation (3) and shows that school spending is monotonically increasing and close to linear in the exiting cohort size.

## 6.2 Educational attainment and family formation

In this section we examine whether school spending affects educational attainment and family formation in adulthood. Panel A of Table 5 shows the TSLS estimates of compulsory school spending on educational outcomes. Columns 1 and 3 suggest that compulsory school spending yields positive effects for males on high school completion and years of schooling. The semi-elasticity estimate for males' years of schooling is statistically significant at the 10 percent level. The coefficient indicates that when school spending increases by 10 percent in all school-age years, educational attainment increases by 1/3 of a year. For females, on the other hand, the estimated effects in Columns 2 and 4 are relatively small and statistically insignificant.

In Panel B of Table 5, we consider the effects of school spending on family formation in adulthood. Columns 5 and 6 show TSLS estimates when an indicator for single status in adulthood is used as outcome. Here, the effect for males is negative and statistically significant at the 10 percent level. The coefficient indicates that when school spending increases by 10 percent in all school-age years, the likelihood of single status decreases by 0.069 percentage points. Columns 7 and 8 show TSLS estimates for the effects of school spending on an indicator for parenthood. The point estimates are statistically insignificant, but larger for males than for females. Taken together, Table 5 suggests that males who receive more school spending obtain more years of schooling and higher probability of mating in adulthood.

When looking at different long-run outcomes, we find that the males generally respond more strongly to the instrument-induced shock than females. While these results are consistent with other findings in the literature, this paper does not aspire to explain *why* the school spending impacts are larger for males than for females.

## 6.3 Heterogeneous effects

So far, the estimation results show that the effects of education spending are heterogeneous by gender. In this section, we investigate whether the effects of education spending on children's outcomes differ by family background. We may distinguish between heterogeneous effects that occur in the first stage and the second stage of the IV model. In the second stage, the marginal productivity of education spending depends on educational production functions that are likely to differ between disadvantaged and advantaged students. In the first stage, resources are not necessarily equally distributed per student since local government school spending can be allocated according to compensatory (or reinforcing) criteria. Hence, the targeting of spending variation might disproportionally cater to the needs of disadvantaged (or advantaged) students. When resources become more plentiful, there is increased capacity for individually adjusted teaching, for instance through employment of teacher aides or pull-out teaching for children with special needs. On the other hand, it appears plausible that joint teaching in the classroom is better adjusted to the needs of the median student than to the tails in the ability distribution. Consequently, changes in per-pupil spending are potentially accompanied by changes in the mixture of teaching practices and availability of resources targeted to individually adjusted teaching.

To study heterogeneous effects across the income pecking order, we group children (within cohorts) in terciles by parental family income. Table 6 shows the effects of school

**Table 5.** Effects of school spending on educational attainment and family formation

**A. Educational attainment:**

| | High school completed | | Years of schooling | |
|---|---|---|---|---|
| | Male | Female | Male | Female |
| | (1) | (2) | (3) | (4) |
| School spending | 0.26 | 0.13 | 3.30* | 0.0029 |
| | (0.30) | (0.25) | (1.83) | (1.72) |
| Joint F-test (FS) | 303.13 | 33.86 | 303.13 | 33.86 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 199 810 | 209 570 | 199 810 |

**B. Family formation:**

| | Single | | Parenthood | |
|---|---|---|---|---|
| | Male | Female | Male | Female |
| | (5) | (6) | (7) | (8) |
| School spending | -0.69* | 0.33 | 0.54 | 0.065 |
| | (0.37) | (0.28) | (0.36) | (0.27) |
| Joint F-test (FS) | 30.1 | 33.6 | 30.1 | 33.6 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 199 810 | 209 570 | 199 810 |

*Note:* This table reports TSLS estimation results for the effects of average log school spending during compulsory schooling on high-school completion (1–2), years of schooling (3–4), indicator for single status (5–6), and indicator for parenthood (7–8). Estimation is done separately for males and females. Panel A displays estimates of school spending impacts on educational attainment, and Panel B shows estimates of school spending impacts on family formation outcomes. An F-test for instrument significance is included for all regressions. Standard errors (in parentheses) are clustered at the municipality-of-birth level. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

spending on individual log-income and a low-income indicator. The IV estimates are obtained by performing TSLS regressions seperately by gender and tercile groups in the parental income distribution. We find that the effects of school spending are relatively large for males with background from low- or middle-income families. For males growing up in low-income families, the effect of school spending on log-income in adulthood is significantly positive (and significantly negative for the low-income indicator). The estimated effects are smaller and not significant for males from high income families, and moreover for females in general, regardless of sosioeconomic background. Table D5 in the Appendix report heterogeneous effects of school spending on family income of the child generation in adulthood, which display a similar pattern as for individual income.

Table D6 shows the heterogeneous effects on other outcomes in subsamples by low, middle and high parental income. Similarly as for income, we find relatively strong responses to school spending for males who grow up in low- and middle-income families. They experience a reduced likelihood of being single in adulthood and obtain more years

**Table 6.** Heterogeneous effects of school spending on individual income, by parental income group

| | Individual income | | Low income ($\leq 25pct$) | |
|---|---|---|---|---|
| | Male | Female | Male | Female |
| | (1) | (2) | (3) | (4) |
| School spending: | | | | |
| Low income parents | 0.72** | 0.14 | -0.60** | -0.29 |
| | (0.31) | (0.25) | (0.27) | (0.31) |
| Middle income parents | 0.60** | 0.11 | -0.45* | -0.22 |
| | (0.30) | (0.24) | (0.26) | (0.30) |
| High income parents | 0.31 | 0.13 | -0.24 | -0.33 |
| | (0.45) | (0.31) | (0.42) | (0.38) |
| Joint F-test (low FS) | 22.6 | 24.2 | 22.6 | 24.2 |
| Joint F-test (mid FS) | 15.0 | 17.0 | 15.0 | 17.0 |
| Joint F-test (high FS) | 13.7 | 19.1 | 13.7 | 19.1 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 199 810 | 209 570 | 199 810 |

*Note:* This table reports TSLS estimates of the effects of school spending during compulsory school on adult earnings for children with low, medium and high parental income backgrounds. The families of the children are defined as low, medium or high income if the parental income was at the bottom (0-33 percentiles), middle (34-66 percentiles) or top (67-100 percentiles) third of ranked incomes during the preschool years when the child was between 0 and 6 years old. The first two columns show the TSLS estimates of the effect of school spending on individual income between ages 27–36 for males (1) and females (2). The last two columns show the TSLS estimates of the effect of school spending on an indicator for low income for males (3) and females (4). Standard errors (in parentheses) are clustered at the municipality-of-birth level. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

of schooling.

As discussed in Jackson and Mackevicius (2021), heterogeneous effects may reflect differences in spending changes experienced across income groups. Yet, we cannot evaluate this hypothesis directly. The reason is that we observe yearly education spending at the municipality level, but not at the individual level. Measures of average per-pupil spending by municipality and year may mask disproportonality in the allocation of spending to children from different family backgrounds.

Instead, we provide an indirect test of differential targeting in the spending allocation across income groups. This test is based on the following hypothesis: If there is progressivity in the spending allocation by parental income group, the effect of school-exiting cohort size on per-pupil spending will increase with the proportion of low-income students within the school-exiting cohort. The rationale for this hypothesis is that children who are favored through the allocation of school resources will leave behind more resources that are available for re-allocation at the time of school exit.

To test for differential targeting, children in exiting cohorts are grouped by terciles of *their* parental income distributions. We calculate the proportions of low-, middle- and high-income students within school-exiting cohorts by municipality. Next, the dynamic adjustment model (Eq. 9 in Appendix B) is modified by including interactions between school-exiting cohort size and proportions of graduating children by income group. The estimation results from this modified model are displayed in Figure B4. The results suggest that there is a slightly progressive allocation of the instrument-induced spending variation. We fail to reject the null hypothesis ($p = 0.70$) that the effects on per-pupil spending are equal by income groups within the school-exiting cohort.[18] Hence, the indirect test for disproportional spending allocation suggests that our reported heterogeneous effects are reflecting differences in the marginal response of outcomes to spending changes.

### 6.4 Robustness checks

Our empirical strategy is designed to purge the exiting cohort instrument for correlated trends in cohort sizes within municipalities. Hoxby (2000) employs a different method to ensure that correlated demographic trends are removed when own-cohort size is used to identify class-size effects. First, she estimates deterministic trends by regressing log enrollment for each grade and district on a polynomial function of birth cohort. Second, the residual variation from this regression is used as instrument for class size.

Our empirical methods should not be vulnerable to correlated demographic trends since a) the control vector includes cohort size by age, and b) our instrument exploits cohort size variation from a cohort that is nine years older and therefore not adjacent to the treated cohort. Yet, as a sensitivity exercise, we replicate Hoxby's regression methods to remove the deterministic component of exiting cohort size. To this end, we estimate deterministic trends by regressing log cohort size of the exiting cohort (age 16) for each municipality on a polynomial function of birth cohort.[19] The residuals are interpreted as de-trended shocks to the exiting cohort size. Next, we estimate versions of the TSLS model where observed exiting cohort size is replaced by the de-trended variation in exiting cohort size, whilst our main specification is otherwise unchanged.

Table D7 in the Appendix displays results from TSLS estimations based on de-trended instrument for exiting cohort size. We employ four different auxiliary regression models that we use to de-trend exiting cohort size. The alternative specifications account for municipality-specific polynomial trends in log exiting cohort size: Linear, quadratic, cubic

[18] In similar indirect test, we find no evidence that the allocation of school spending is disproportional based on gender.

[19] Consistent to our general procedure, cohort size of the exiting cohort is measured in the year of school entry (age 7) of the treated cohort. This is equal to the year of school exit (age 16) of the exiting cohort. The trend in exiting cohort size is estimated for the sample of treated cohorts that were born in the period 1968-1982, whilst the exiting cohorts were born in 1959-1973. In accounting for the effects of class-size on grade-specific achievement, Hoxby (2000) estimates trends that are specific for each grade and district. In considering long-run outcomes, we estimate trends in exiting cohort size for each municipality. Thus, in our setting, the residual variation that is used as instrument is measured prior to treatment and does not vary by grade.

and quartic. For males, the point estimates for the effect of school spending on individual income are significantly positive and somewhat larger than in the baseline model specification. As in the baseline, TSLS estimates for females do not differ significantly from zero.

Next, we provide robustness checks which extend the baseline specification with other control variables that might introduce confounding variation into our model. The results from these alternative specifications are presented in Appendix Table D8. The first set of additional controls is constituted by the family characteristics that are included in the balancing check in Table 2. The second set of controls accounts for regional trends (linear cohort trends by region). Across alternative model specifications, our TSLS estimates are sizable and significantly positive for males, but insignificant for females. Thus, our main findings are robust when including the extended control vectors.

Finally, we compare our baseline local average treatment effect estimates to the intention-to-treat effect estimates from using per-pupil spending ($u_{mc}$) by municipality and cohort as the treatment variable instead of residence-based per-pupil spending ($u_{imc}$). The ITT effects are presented in Table D4 and can be interpreted as the effect of assigning treatment at the municipality level. Since the reduced form (RF) model does not depend on the treatment variable, RF estimates are unaffected by re-definition of the treatment. Yet, the first-stage estimates are almost 20 percent larger for boys and 11 percent larger for girls compared to our baseline estimates in Table 4. These differences reflect that students who move receive spending that is unrelated to the instrument (see Table 3). Consequently, the estimates of the effect of school spending are proportionally smaller than the LATE estimates in our baseline results. The difference in estimates is smaller than the 25 percent of students who move before the end of compulsory schooling. Notice, however, that students who move subsequent to school entry will be partly treated, which implies that 25 percent is an upper bound on the difference between the estimates.

Although the LATE effects show the average treatment effects for the students who actually receive treatment, the ITT estimates may be more policy relevant in certain contexts. Policy makers at the national level should be interested in the LATE, whereas local authorities might be more interested in the ITT effect of increases in school spending at the municipal level.

# 7 Summary and discussion

This paper examines the long-term impacts of local government spending on compulsory schooling. We estimate the effects of school spending on children's income, educational attainment and family formation in adulthood. As source of identifying variation, we rely on differences in population size of the cohorts that exit from compulsory schooling just before entry of children in the treated cohort.

For males, the estimation results display substantial long-term benefits from increases in per-pupil spending. The estimated elasticity of male income with respect to per-pupil spending is around 0.7. This central estimate is of similar magnitude as found in Jackson et al. (2016). However, the results differ in that elasticity estimates for females are small and insignificant in our sample.

When investigating different margins of local government adjustment, we find that instructional spending and staffing are strongly affected by the instrument due to stickiness of employment. In contrast, other operational spending, capital expenditure, number of classrooms and class size are unaffected by the instrument. Consequently, our source of spending variation comes close to isolating the effects of changed staffing per pupil while holding other school inputs and class size constant. For example, local governmens may increase staffing per pupil by increasing the number of teachers and assistants per classroom. Our results suggest that such policies yield beneficial long-term effects, in particular for males from low- and middle-income backgrounds.

In the Norwegian context, existing research tends to report small effects of school resources on child outcomes (Borgen et al., 2022; Leuven and Løkken, 2020; Falch et al., 2017; Leuven et al., 2008). In order to reconcile our findings with the literature, we offer several explanations. First, many quasi-experimental studies do not isolate changes in staffing per pupil from changes in other inputs and changes in class size. For example, whilst maximum class-size rules affect class size and teacher density for the treated cohort, school administrators may compensate for such inequity through the allocation of teachers, assistants and other resources across classrooms. As a result, the estimated effects on child outcomes may account for class size changes that are partly neutralized through input substitution. Second, while our paper estimates the local average treatment effect for the compliers, many studies estimate intention-to-treat effects which are typically smaller, as we also demonstrate in this study. Third, there are some studies which find significant effects of school resources in Norway, although typically on short-term outcomes or for specific target groups (Bonesrønning et al., 2022; Iversen and Bonesrønning, 2013; Hægeland et al., 2012).

School-exiting cohort size is found to exhibit positive effects on per-pupil spending that are statistically significant and rather stable through different stages of compulsory schooling. Hence, the instrument-induced variation is informative about persistent changes in per-pupil spending that continue over nine years of schooling. When implementing policies that provide funding for extra teachers, such kind of intervention may stimulate teacher mobility, which can mediate adverse effects on child outcomes (Gibbons et al., 2021; Sorensen and Ladd, 2020; Ronfeldt et al., 2013). In particular, transition and disruption costs may occur in the earlier stages of an intervention. However, it appears unlikely that our study conflates the effects of extra teachers with adverse effects due to teacher mobility. The reason is that we exploit enrollment shocks rather than employment shocks to instrument for school spending. Consequently, our school spending impacts can be interpreted as long-term effects along several dimensions: The effects capture the response of children's long-run outcomes, in response to a persistent change in staffing per pupil throughout compulsory school years, where effects are identified under conditions of negligible transition costs and disruption costs.

The identification strategy developed in this paper can potentially be applied more widely. Inertia in public sector spending allocation is not a phenomenon particular to the Norwegian setting. Furthermore, compulsory school systems in other countries incorporate specific regulations of entry and exit. Since causal effects of education spending depend on institutions and other contextual factors, comparable estimates from other settings might broaden our understanding of the impacts of school spending on child outcomes.

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# A Variable definitions

## Treatment variable - local government education spending

Annual accounting data combined with education register data allow us to measure education spending per student, where yearly education spending is standardized relative to the number of children who are enrolled in compulsory schooling within each municipality. Hence, log *yearly per-pupil spending* is defined by

$$u_{m,t} = \tilde{u}_{m,t} - s_{m,t}, \tag{4}$$

where $\tilde{u}_{m,t}$ is log *yearly school spending* in municipality $m$ in year $t$, and $s_{m,t}$ is log *enrollment* in compulsory schooling in municipality $m$ in year $t$. Since enrollment is mandatory, and enrollment in year $t$ is measured in October/November, the number of children enrolled corresponds closely to total size of the cohorts which are in mandatory school age at end-year.

Next, we consider how to transform log yearly per-pupil spending ($u_{m,t}$) to average log per-pupil spending over nine years of compulsory schooling. To this end, we make use of the relationship $t = c + a$ between time, cohort and age to project yearly per-pupil spending onto the cohort and age dimensions. Moreover, since the calendar year (and fiscal year) is overlapping with two school years, school-year log spending ($\tilde{u}_{mca}$) is defined as follows:

$$\tilde{u}_{mca} = \frac{1}{2}\left(\tilde{u}_{m,c+a} + \tilde{u}_{m,c+a+1}\right), \tag{5}$$

where $\tilde{u}_{m,c+a}$ is log school spending in municipality $m$ in the fiscal year when children in birth cohort $c$ are $a$ years old at end-year. Thus, we make use of average log spending in the two fiscal years which overlap with the school year. Accordingly, school-year log spending per pupil ($u_{mca}$) is defined by:

$$u_{mca} = \tilde{u}_{mca} - s_{mca}, \tag{6}$$

where $s_{mca} = s_{m,c+a}$ denotes log enrollment in municipality $m$ in the school year when children in birth cohort $c$ are $a$ years old at end-year. Since children attend compulsory schooling from age 7 to 15, it follows that average log per-pupil spending over nine years of compulsory schooling ($u_{mc}$) is given by:

$$u_{mc} = \frac{1}{9}\sum_{a=7}^{15} u_{mca} = \frac{1}{9}\sum_{a=7}^{15} \tilde{u}_{mca} - \frac{1}{9}\sum_{a=7}^{15} s_{mca} = \tilde{u}_{mc} - s_{mc}. \tag{7}$$

For brevity, we use the label *per-pupil spending* when referring to the variable $u_{mc}$. The second equality is obtained by inserting from (6) in (7), while the third equality defines averages over time, where $\tilde{u}_{mc}$ denotes *school spending*, or average log spending in municipality $m$ assigned to cohort $c$. Average log enrollment over the nine years when cohort $c$ attended compulsory schooling in municipality $m$ is denoted $s_{mc}$.

For children who grow up in municipality $m$, we consider $u_{mca}$ as an appropriate measure of per-pupil spending during grade $a-6$. However, to account for families who move between municipalities, we define $u_{imca}$ by log per-pupil spending in the origin or destination municipality where child $i$ resided in the school year of age a (grade $a-6$). Consistent to equation 5, spending per pupil in the origin and destination municipalities are each weighted 50 percent in a fiscal year of relocation. Notice that municipality index $m$ denotes the municipality of origin where the child is born. Finally, we accumulate log per-pupil spending over time and across municipalities according to where the child resided in each year:

$$u_{imc} = \frac{1}{9}\sum_{a=7}^{15} u_{imca}, \tag{8}$$

where $u_{imc}$ measures the average log per-pupil spending in the municipalities where child $i$ actually resided during different school years. When referring to $u_{imc}$, we use the label *residence-based per-pupil spending* or just *per-pupil spending* for brevity. This variable is employed as a treatment variable that accounts for variation in exposure to per-pupil spending due to family movement during childhood.

## Outcome variables

*Individual income*: Individual income is defined as the average CPI-adjusted after-tax income of the treated individual, measured over a period of 10 years in adulthood during ages 27-36. After-tax income, collected from the Norwegian tax registries, includes wages and salaries, self-employment income, property income, taxable capital income, and transfers received, minus tax payments.

*Low individual income:* The indicator for low individual income equals 1 for individuals below the 25th percentile of the individual income distribution within each cohort, and 0 otherwise.

*Household income:* Household income is defined as the sum of individual incomes of the treated person and the spouse or cohabiting partner living in the same household at age 34. Individual income of the partner is measured over the same period of 10 years as the individual income of the treated person. When comparing couples and single households, we account for economies of scale in consumption by adjusting household income of couples by an equivalence scale equal to 1.5.

*Low household income:* The indicator for low household income equals 1 for individuals below the 25th percentile of the household income distribution within each cohort, and 0 otherwise.

*High school degree:* Individuals are defined as high school graduates when they have completed high school in adulthood at age 34, based on levels of completed education in the education registry.
*Years of schooling.* Years of schooling is defined as the stipulated length (in years) of the completed education of individuals in adulthood at age 34. Levels of completed education are reported in the education registry.
*Parenthood.* Individuals are defined as parents when they have offspring in adulthood at age 34, based on family information in the Norwegian population registry.
*Single.* The indicator for single households equals 1 for individuals who live alone without a married or cohabiting partner, and 0 otherwise. Based on family information in the Norwegian population registry, the indicator is measured in adulthood at age 34.

## Control variables

*Parental income.* This variable is constructed in a similar way as household income of the treated person in adulthood. We sum up the CPI-adjusted income of both parents, measured over a period of 6 years in childhood during preschool years (ages 1-6) of the treated person. Here, we make use of pension-qualifying income from the Norwegian tax registry, because this income definition is available further back in time. Pension-qualifying income includes wages, salaries and work-related public insurance benefits.
*Parental income rank.* When partitioning the sample by parental income, the ranking of parental family incomes is performed within each birth cohort of children in the sample.
*Individual specific controls.* Based on linked administrative data, we create child-specific control variables from the population registries, such as *birth cohort*, *municipality of residence*, and *gender*. We also collect unique person identifiers for the parents, the *age of both parents* at the child's birth, and the child's total number of *siblings* and *birth order* among them.
*Family specific controls.* We create control variables for having an *immigrant background*, defined as having one or both parents who were not born as Norwegian citizens. We construct a variable for each *parent's years of education*, defined similarly as for the child, but measured in the year before the child begins primary school (age 6). Furthermore, an indicator for missing education data is used, as adult education information was sourced from the 1970s census and frequently incomplete, particularly for children in older cohorts. We also account for the *parents' marital status* (married, cohabiting, single parent, or other) and the proportion of preschool years the *mother is not working.* In this context, the yearly indicator for not working is defined by having income below the substantial gainful amount (G) that is used as a threshold for accumulation of pension points in the Norwegian pension system.
*Municipality specific controls.* In addition to school spending per child, we also create

variables such as average *childcare coverage* in the last preschool years (age 4–6). For a given municipality, childcare coverage is defined as the average proportion of children in the cohort that participate in childcare services during ages 4-6. The *enrollment ratio* is the proportion of students enrolled in first grade of public primary schools relative to the number of resident children in the municipality who are 7-years-old at end-year.

# B Analysis of spending adjustment on yearly accounting data

In this Appendix, we develop an empirical model of local government education spending that allows for inertia in the response to shocks. The model is estimated on yearly panel data, where the dependent variables are education spending, enrollment and per-pupil spending by year and municipality.

## B.1 Model of local government education spending

In line with previous evidence, local government education spending is specified as a function of the size of different age groups and municipal incomes. Let $u_{m,t}$ denote log education spending per student in municipality $m$ in year $t$. The model of yearly education spending is expressed by:

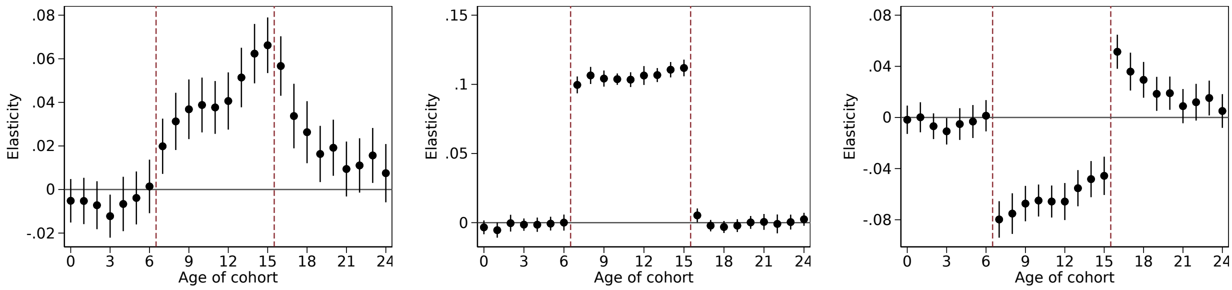
$$u_{m,t} = \xi_m + \vartheta_t + \sum_{a=0}^{A} \phi_a n_{m,t,a} + x_{m,t}\psi + \sum_{j=0}^{J-1} \omega_j \Delta v_{m,t-j} + \omega_J v_{m,t-J} + \epsilon_{m,t}, \qquad (9)$$

where $\xi_m$ and $\vartheta_t$ denotes a full set of fixed effects for municipality and year, and $n_{m,t,a}$ is log number of people in municipality $m$ belonging to the birth cohort which turns $a$ years old in year $t$. Hence, the parameter $\phi_a$ captures the partial elasticity of age $a$ cohort size on education spending per child.[20] When using $A = 24$, we include the size of five-year age groups from age 25-29 to 90 years and above in a vector of controls ($x_{m,t}$) for observable characteristics. Furthermore, the model accounts for spending dynamics by incorporating a finite distributed lag in relative income changes. Log total income of municipality $m$ in year $t$ is denoted $v_{m,t}$, whilst the difference operator $\Delta$ is used to express relative income changes from the previous year. Since the last lag ($J$) is accounted for in levels of log income, the parameters $\omega_j$ are cumulative income elasticities measured $j$ periods after an income shock ($j = 0, 1, .., J$), where $\omega_J$ is the long-run elasticity.

## B.2 Estimation results

Figure B1a displays estimates for the partial elasticities of yearly education spending ($\tilde{u}_{m,t}$) with respect to cohort size by age. Most of the variation in enrollment numbers comes from variation between cohorts (across age) rather than within cohort (across years). Therefore, we may interpret these estimates as partial dynamic effects on spending at

[20]Notice that $u_{m,t} = \tilde{u}_{m,t} - s_{m,t}$, where $\tilde{u}_{m,t}$ is log education spending and $s_{m,t}$ is log total enrollment in compulsory schooling in municipality $m$ in year $t$. Consequently, a decomposition of (9) is obtained by separately regressing log spending and log enrollment on the right-hand-side expression.




**(a)** Yearly school spending

**(b)** Yearly enrollment in compulsory schooling

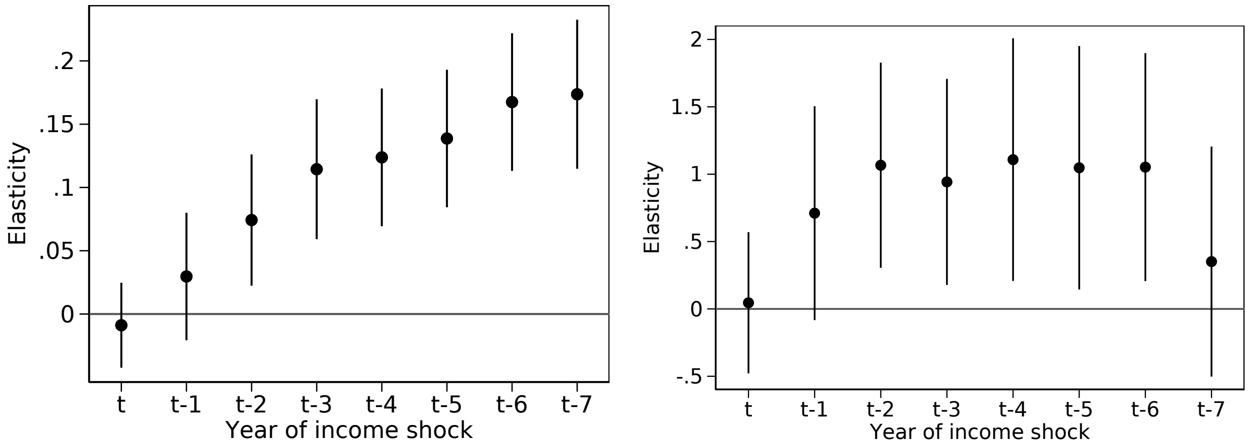
**(c)** Yearly per-pupil spending

**Figure B1.** Response of yearly school spending and enrollment to changes in cohort size
*Note:* These figures show the responsiveness of yearly school spending (a) enrollment (b) and per-pupil spending (c) with respect to cohort size by age. Cohort size variables are measured at end-year, and are defined by log number of children by municipality and age of cohort. Figures display partial elasticity estimates that are obtained from OLS regressions of the model of local government education spending. The model includes municipality and year fixed effects, and controls for total local government income and lagged income changes. The cohorts between the vertical (dashed) lines are the students aged 7–15, who are enrolled in compulsory schooling at end-year. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality level.

different ages obtained from following a cohort that includes 1 percent more students than other cohorts. This interpretation indicates that spending responds quite slowly to the entry of a relatively large cohort in school. In the year of age 7, when the cohort is entering the first grade, the spending response is less than one third of the accumulated response in the year of age 15. Likewise, education spending responds rather slowly to changes in exiting cohort size. In the year of age 16, when the cohort is exiting compulsory education, the spending increase is almost as high as one year earlier. Later on, while the cohort-size effect on spending is fading out, there is a spending increase that is rather persistent by age. As opposed to years following school exit, the effects of cohort size on school spending are small and mostly insignificant during preschool years.

Figure B1b shows the elasticity estimates of yearly enrollment ($s_{m,t}$) with respect to cohort size by age. Total enrollment is indeed closely related to the number of children in each birth cohort from age 7 to 15. Partial elasticities around 0.11 imply that when the sizes of all 9 cohorts in mandatory school age increase by 1 percent, total enrollment will increase by (nearly) 1 percent.

Figure B1c displays elasticity estimates of yearly education spending per student ($u_{m,t}$) with respect to cohort size by age. The significantly negative estimates for the school-age cohorts reflect that the spending increase is insufficient to offset the enrollment increase due to entry of a large cohort. A positive enrollment shock makes schools more crowded, which means that per-pupil spending is literally crowded out for a (nearly) fixed budget. This "crowd-out" effect is attenuated as children move through the grades, because spending is slowly adjusted to compensate for cohort size. From age 15 to 16, there is a sizable jump from negative to positive impact of cohort size on spending per student. This jump is resulting from sluggish spending adjustment combined with the exit of children from compulsory schools in the year of age 16, as required by the school system. Mechanically,




**(a)** Yearly per-pupil spending **(b)** Yearly school investments per pupil

**Figure B2.** Response of per-pupil spending and school investments to municipal income changes

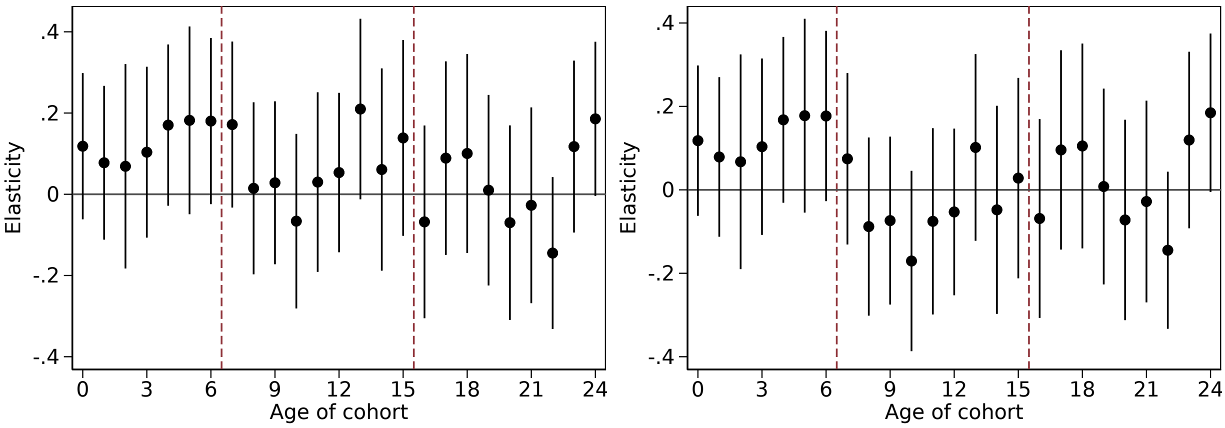
*Note:* These figures show the responsiveness of yearly per-pupil spending (a) and school investments per pupil (b) to changes in local government income. Figures display partial elasticity estimates of dynamic income effects that are obtained from OLS regressions of the model of local government education spending. The full regression specification is described in Equation (9). Standard errors are clustered at the municipality level

the exiting cohort is excluded in the denominator of spending per student, whilst (nominator) spending has become elevated to accommodate the expenditure needs of a large cohort. Consequently, there is an instantaneous reversal of the crowd-out effect which is reinforced by a lock-in effect on school resources.

Panel a of Figure B2 reports estimates for dynamic income elasticities, which reflect the response of yearly operational per-pupil spending to local government income shocks. This establishes that operational school spending responds slowly to shocks in income as well as enrollment. Our estimate for the long-run income elasticity is equal to 0.17, which means that local government demand for operational school spending is quite income-inelastic even in the long run.

Apart from operational school spending, local governments may affect children's outcomes through capital spending and school facility investments. Capital spending on compulsory schooling are reported in municipal accounting data. To investigate how school investments respond to shocks in enrollment and municipal income, we estimate versions of Equation (9) with yearly school investments instead of operational school spending on the left-hand side. Figure B3 shows that the estimated elasticities of school investments with respect to cohort size by age do not differ significantly from zero. On the other hand, Figure B2 shows that dynamic income elasticities for school investments are around 1 in the medium term, which is high in comparison to income elasticities of operational school spending. Thus, school investments respond relatively strongly to municipal income shocks, whereas responses to enrollment shocks are not discernible.

Finally, we consider whether changes in school spending are differentially targeted to children based on parental income or by gender. Since we do not observe the allocation




**(a)** Yearly school investments



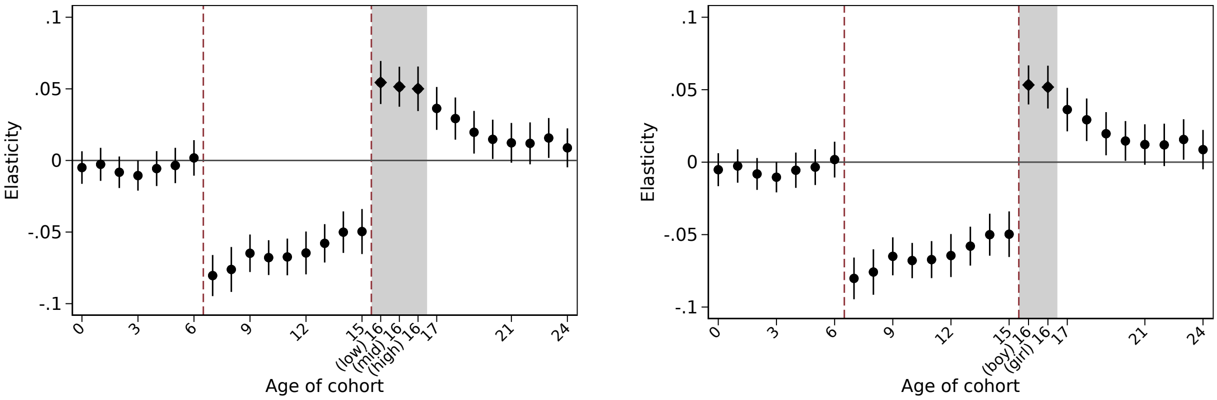
**(b)** Yearly school investments per pupil

**Figure B3.** Response of yearly school investments to changes in cohort size

*Note:* These figures show the responsiveness of yearly school investments (a) and yearly school investments per pupil (b) with respect to cohort size by age. Cohort size variables are measured at end-year, and are defined by log number of children by municipality and age of cohort. Figures display partial elasticity estimates that are obtained from OLS regressions of the model of local government education spending, making use of school investments as dependent variable. The circles represent the point estimates of the elasticities and the 95-percent confidence interval is shown by the vertical spikes. Sub-figure B3a shows the elasticities of school investments, whereas sub-figure B3b shows the elasticities school investments per pupil with respect to cohort size by age. The full regression specification is described in Equation (9). Standard errors are clustered at the municipality level.

of education spending to schools, grades or individual children, we exploit *size* and *composition* of the school-exiting cohort (age 16) to perform an indirect test for differential targeting. To this end, we modify the model of yearly per-pupil spending by including interactions between *size* of the school-exiting cohort and *proportions* of school-exiting children by tercile groups (low/medium/high) in the parental income distribution. If the allocation of spending is favoring low-SES (high-SES) children, we expect that the increase in per-pupil spending is larger when the school-exiting cohort includes a larger proportion of children with background from low-SES (high-SES) families. The rationale for this hypothesis is that children who are favored through differential targeting of school resources will leave behind more resources that are available for re-allocation at the time of school exit. Similarly, by including interactions between *size* of the school-exiting cohort and the *proportions* of boys and girls in the exiting cohort, we can investigate whether gender is a source of differential targeting.

Figure B4 displays estimation results from the modified models. The results in the left-most panel (a) suggest that children with low-income backgrounds are slightly favored through the local government allocation of school spending. We fail to reject the null hypothesis ($p = 0.70$) that the elasticities of per-pupil spending are equal with respect to income groups within the school-exiting cohort. Thus, elasticity estimates for the instrument with interactions suggest that the allocation of spending is nearly proportional to the number of children in different parental income groups. Similarly, the results in the right-most panel (b) indicate that boys receive slightly more resources, but again the differences are small and we cannot reject the null hypothesis ($p = .67$) of equal allocation




**(a)** Parental income groups of exititing cohort **(b)** Gender composition of exiting cohort

**Figure B4.** Heterogeneity in the response of per-pupil spending to exiting cohort size

*Note:* This figure shows the responsiveness of yearly per-pupil spending with respect to cohort size by age, similarly as in Figure B1c. The model is modified by including interactions between size of the school-exiting cohort (age 16) and proportions of school-exiting children by parental income groups (a) and the gender composition of the exiting cohort (b). In panel (a), we group graduating children in tercile groups based on *their* parental income, measured during ages 1-6 of the child, and interact the exiting cohort size with the proportions of children by tercile groups within the exiting cohort. Similarly, in panel (b), we interact the exiting cohort size with the proportions of boys end girls in the exiting cohort. The elasticity estimates for the interactions terms between size and composition of the school-exiting cohort are represented by black diamond markers within the shaded gray areas. We are unable to reject the null hypothesis that these parameters are equal ($p = .70$ for parental income terciles and $p = .67$ for gender). The cohorts between the vertical (dashed) lines are the students aged 7–15, who are enrolled in compulsory schooling at end-year. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality level.

of per-pupil spending by gender.

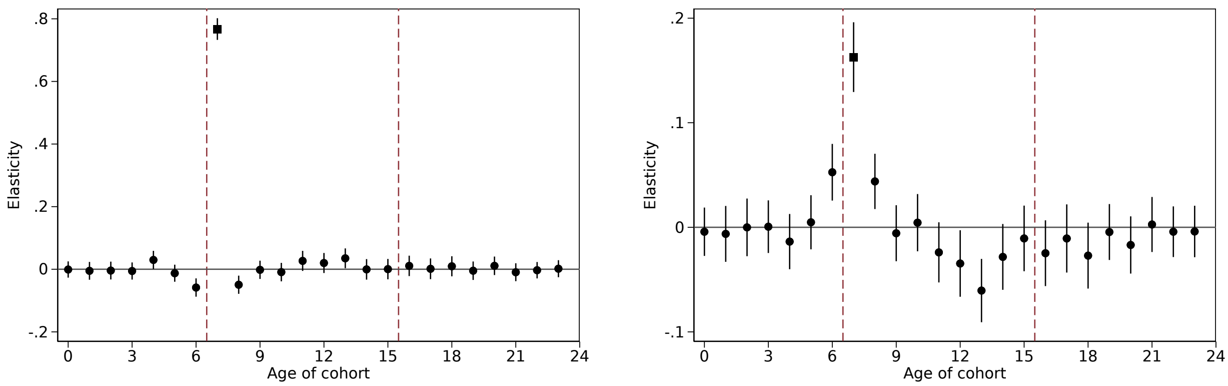
# C Classrooms and class size

In this Appendix, we analyze the relationship between number of classrooms, class size, and cohort sizes measured by log number of children in different cohorts. In comparison to local government accounting data, enrollment and classroom data are more disaggregated. Thus, we observe number of classrooms, enrollment and class size at the grade level by municipality and year.

The allocation of teachers to grades and cohorts is positively associated with the allocation of classrooms. The reason is that each classroom has to employ at least one teacher during instruction time. However, the allocation of classrooms is affected by other constraints than the employment of teachers. In the short run, the number of available classrooms is restricted by the physical capacity of school facilities. Furthermore, classrooms are allocated to fulfill the maximum class-size rule which forbids more than 30 children per class. The latter constraint implies that the allocation of classrooms is affected by cohort sizes in different grades of compulsory schooling. To demonstrate this, we estimate models for the average log number of classes per cohort and average log class size per cohort. Class size is defined by number of students per class within grade-municipality-year cells. Moreover, by averaging over school-age years and grades, we obtain precise measures of classes and students per cohort within cohort-by-municipality cells.

Figure C1a shows estimates for the effects of cohort sizes on average log number of classes per cohort. The elasticity of own-cohort size (age 7) on the number of classes is below 0.8. Hence, more classrooms are allocated to larger cohorts, but larger cohorts are not fully compensated through the allocation of classrooms. There is competition with adjacent cohorts (age 6 and 8) over classrooms. Figure C1b shows that classrooms are more crowded and average class size increases with cohort sizes of the treated and adjacent cohorts. The own-cohort size elasticity on average class size is 0.16, whereas cross-cohort size elasticities of adjacent cohorts are around 0.05. Sizes of graduated cohorts (age 16 and above) display insignificant effects on number of classes and class size for the treated cohort.

Our study of per-pupil spending relies on municipal accounting data by fiscal year. Similarly as in other studies, we do not observe the allocation of school spending to different grades or cohorts. Since we employ cohorts of children as treatment and comparison groups, we would ideally measure annual spending per student at the level of grade-by-municipality. Despite the egalitarian objectives of the compulsory school system, per-pupil spending is not necessarily equalized across grades. First, some pull-out teaching and extra resources is devoted to compensatory treatment for children with special needs. Second, distortions in per-pupil spending may arise when teachers are assigned




**(a)** Number of classes

**(b)** Class size

**Figure C1.** Effects of cohort sizes on number of classes and class size

*Note*: These figures show the responsiveness of average number of classes and class size with respect to cohort size by age. Dependent variables are measures of average log number of classes and average log class size (students per classroom) across all mandatory school-age years, within the treated cohort (square marker). Cohort size variables are defined by log number of children by municipality and cohort, measured at the time of school entry of the treated cohort. Figures display partial elasticity estimates that are obtained from OLS regressions of (a) average log number of classes and (b) average log class size on log cohort sizes. The model is based on cohort-panel data and includes municipality and cohort fixed effects, and controls for log municipal population size in different childhood years by age. The cohorts between the vertical (dashed) lines are the students aged 7–15, who are enrolled in compulsory schooling when the treated cohort (age 7) attends first grade. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.

to classrooms and average class sizes differ across grades. Third, organizational rigidities and job switching costs imply that teachers can be locked-in on schools (or educational stages). Fourth, per-pupil spending may differ across grades due to differences in educational practices. In comparison to primary schools, middle schools typically lump together more students per classroom whilst also providing more hours of instruction per week.

To obtain some indirect evidence regarding the allocation of spending by grade, Figure C2 shows model estimates for the effects of cohort sizes on log number of classes by grade. The results show that there is competition over classrooms (staffed with teachers) between children in the treated cohort and adjacent cohorts of children. As a results, children belonging to a large cohort and/or children with larger adjacent cohorts are exposed to larger class sizes during years of compulsory schooling. Due to recapture of classrooms, class sizes during middle school is significantly affected by sizes of older cohorts (age 11-14) who recently completed middle school. In all grades, there are insignificant effects of school-exiting cohort size (age 16) on class size of the treated cohort (age 7).

In 1st grade, the treated cohort is exposed to competition over classrooms from older adjacent cohorts (age 8 and 9). Later in 2nd and 3rd grade, competition from younger cohorts (age 6 and 5) starts at the time of their school entry. Somewhat smaller crowd-out effects of adjacent cohorts persist through 4th and 5th grade. In 6th grade, there are positive effects of cohort sizes of older adjacent cohorts (age 8 and 9). This finding suggests that 6th graders benefit from recapture of classrooms (and resources) that are abundant when large cohorts of children recently finished primary school.

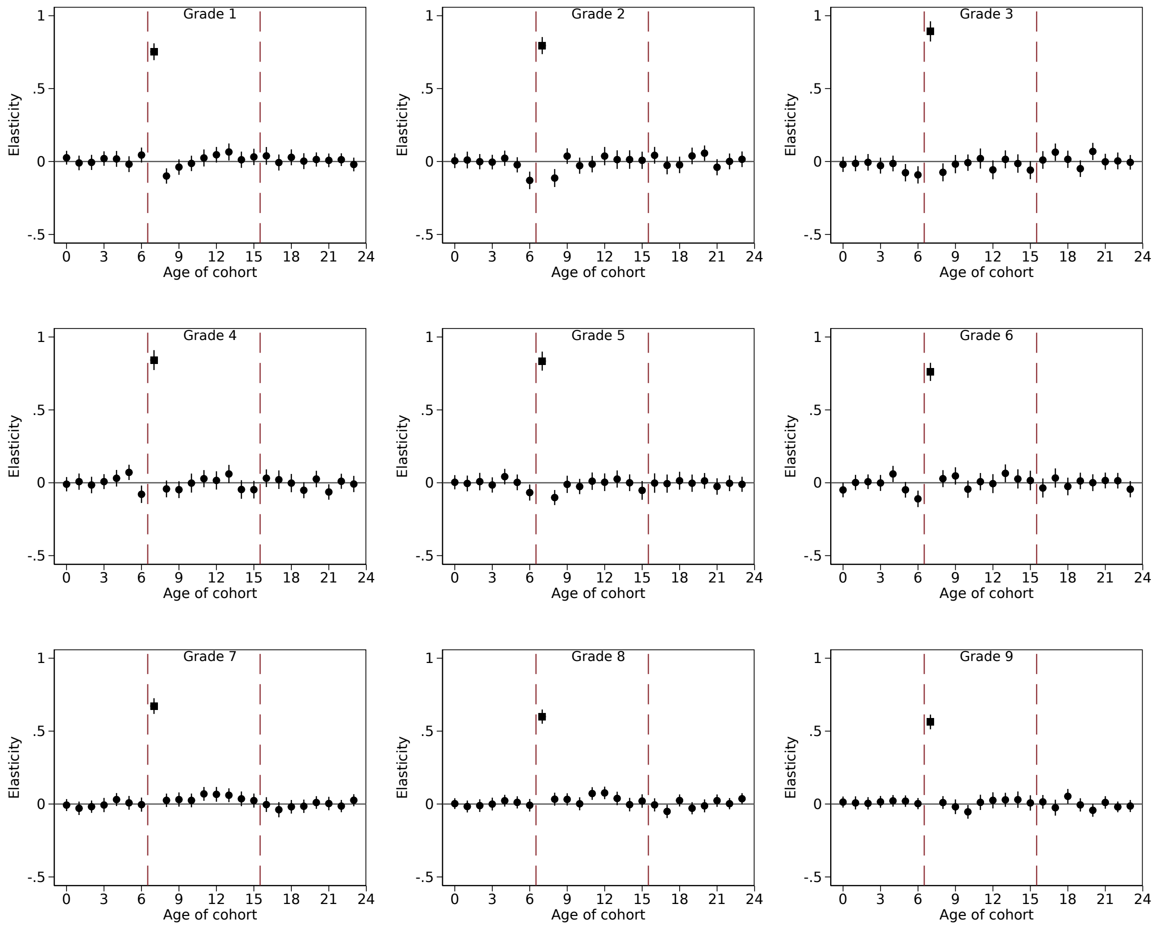
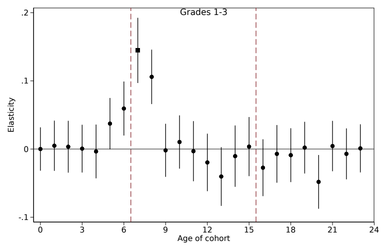
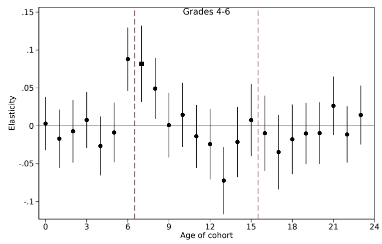



**Figure C2.** Response of number of classes by grade to changes in cohort size

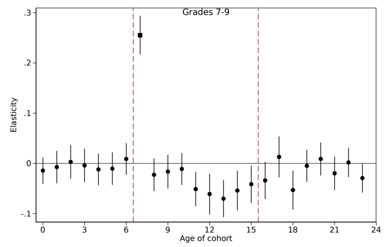
*Note:* These figures shows elasticity estimates of number of classes by grade with respect to cohort size by age. The full regression is specified in Equation 1, except that number of classes by grade is used as dependent variable. Sub-figures report results from separate regressions by grades 1-9. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.




**(a)** Grades 1–3.




**(b)** Grades 4–6.




**(c)** Grades 7–9.

**Figure C3.** Response of class size by educational stages to changes in cohort size
*Note:* These figures shows elasticity estimates of class size by educational stages with respect to cohort size by age. Dependent variables are log average class size in (a) grades 1–3, (b) grades 4–6, and (c) grades 7–9. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.

In middle school, crowd-out effects of adjacent cohorts are insignificant, whereas the own-cohort size elasticity on number of classrooms is smaller than in primary school. However, students gain more classrooms when there are large cohorts of children (age 11-14) who recently completed middle school. A plausible explanation is that 7th - 9th graders are in position to inherit classrooms (with teachers) that earlier were occupied by older cohorts. Notice that there are insignificant effects for the cohort (age 16) which is exiting compulsory schooling just before entry of the treated cohort (age 7). Thus, disproportional benefits from recapture of classrooms appear to fade away in the medium term.

There is some evidence that children in 6th grade and 9th grade capture more classrooms when there are relatively large cohorts in lower grades. Lock-in effects are less persistent when targeting classrooms to children in the last grade of a school. This might help to smooth school resources over time and moreover to front-load opportunity for re-allocation of classrooms.

Figure C3 shows model estimates for the effects of cohort sizes on log class size by grade. Class size is measured by the number of students per classroom within grade and cohort. Average class size is found to increase with own-cohort size and the size of adjacent cohorts, whereas class size of the treated cohort is uncorrelated with size of the school-exiting cohort (conditional on covariates).

Figure C4 reports histograms for the changes in class sizes in the transition between grades. Changes are most pronounced in the transition from primary school to middle school (from 6th to 7th grade), when class sizes are found to increase for the majority of students.

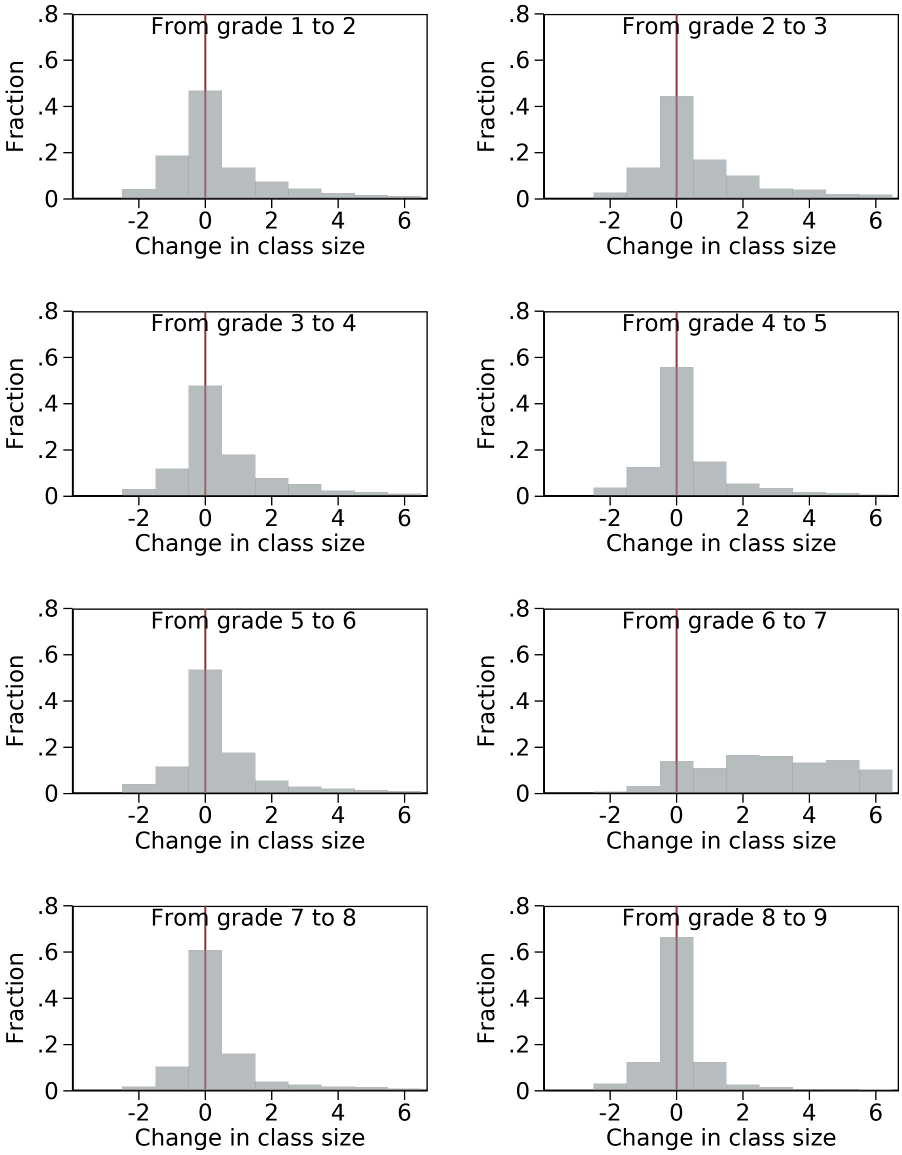



**Figure C4.** The average difference in class size between grades within municipalities.
*Note:* This figure shows the distribution of change in average class size in the transition between grades within municipalities.

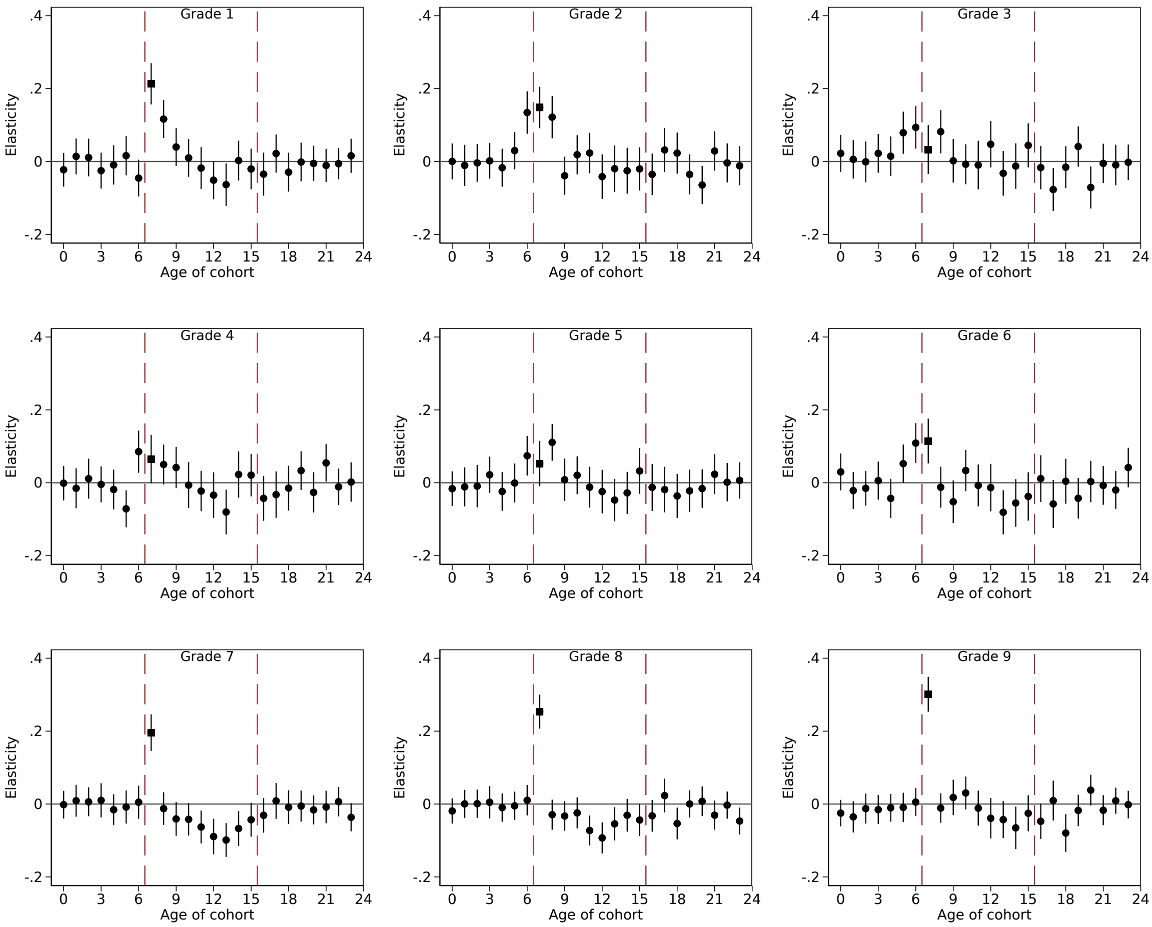



**Figure C5.** The response of class size by grade to changes in cohort size

*Note:* These figures shows elasticity estimates of class size by grade with respect to cohort size by age. The full regression is specified in Equation 1, except that class size by grade is used as dependent variable. Sub-figures report results from separate regressions by grades 1-9. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.

# D Supplementary results: figures and tables

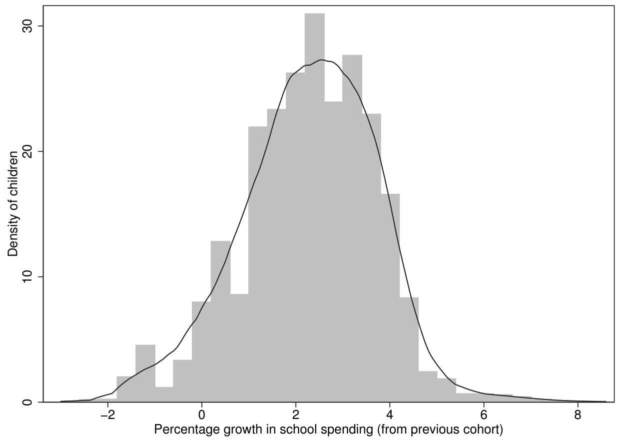



**Figure D1.** Growth in per-pupil spending relative to preceding cohort

*Note:* This figure displays a histogram (and kernel estimates) of the distribution across children of growth in per-pupil spending from a cohort to the next (pooled averages over the cohorts 1970-1980). Growth is measured by the cohort difference in average log per-pupil spending over the relevant years of compulsory schooling, where spending has been adjusted for inflation by the Norwegian Consumer Price Index. The municipalities are weighted by the total number of children in our baseline sample. The distribution is centered around 2-3 percent spending growth, where the cohort difference for most children is between -2 and 5 percent.

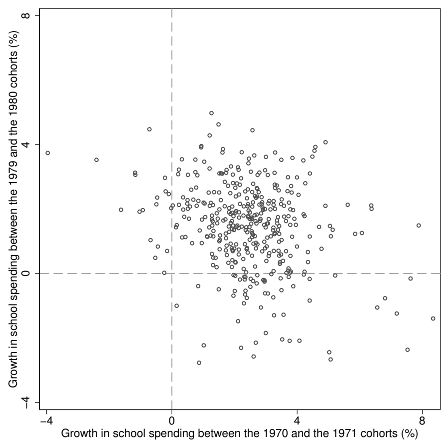



**Figure D2.** Growth in per-pupil spending for cohorts of 1970/71 vs. 1979/80

*Note:* This figure shows a municipality scatter plot of the growth in per-pupil spending for the 1970/71 cohorts versus the 1979/80 cohorts. Growth is measured by the cohort difference in average log per-pupil spending over the relevant years of compulsory schooling, where spending has been adjusted for inflation by the Norwegian Consumer Price Index. The 1970/71 cohorts and the 1979/80 cohorts represent the two first and the two last cohorts in our baseline sample. The scatter plot represents spending growth rates within the former versus the latter pair of cohorts. At this cohort-distance, spending growth rates display a slightly negative serial correlation (equal to -0.27). Broadly speaking, this means that local government spending growth rates are almost uncorrelated in earlier as compared to later years of the period 1977-1996.

**Table D1.** Balancing tests (boys)

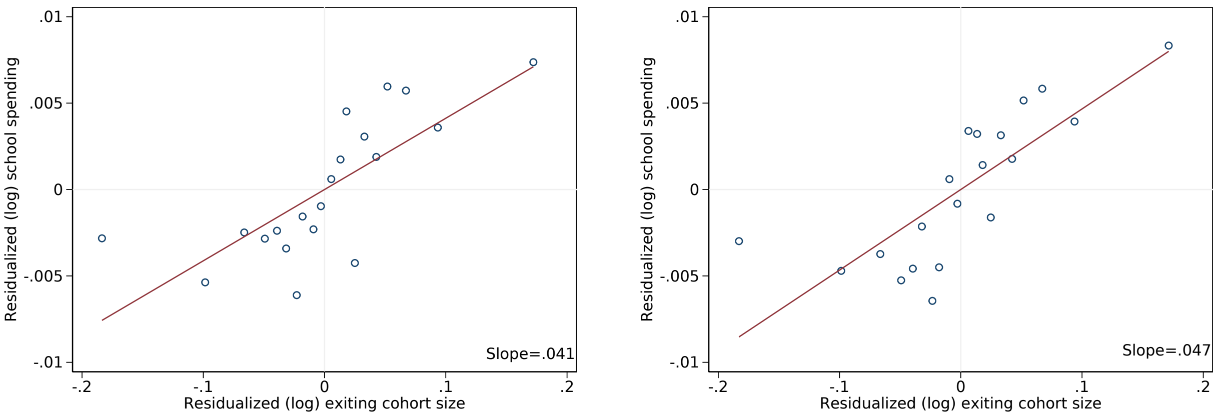
| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Log-earnings | Years of schooling | Single | Instrument |
| Family income (log) | 0.097*** | 0.57*** | -0.028*** | 0.00040 |
| | (0.0062) | (0.021) | (0.0034) | (0.00049) |
| Mothers' years of schooling | 0.0050*** | 0.22*** | -0.0015** | -0.0000046 |
| | (0.0010) | (0.0051) | (0.00067) | (0.00012) |
| Fathers' years of schooling | 0.0038*** | 0.19*** | -0.0018*** | -0.0000039 |
| | (0.00072) | (0.0031) | (0.00065) | (0.000095) |
| Mother not working | 0.0058 | -0.24*** | 0.010*** | -0.000066 |
| | (0.0041) | (0.029) | (0.0034) | (0.00052) |
| Age of father | -0.0029*** | 0.0015 | 0.0044*** | -0.000063 |
| | (0.00039) | (0.0014) | (0.00027) | (0.000043) |
| Age of mother | 0.0018*** | 0.079*** | 0.00021 | 0.000020 |
| | (0.00048) | (0.0022) | (0.00034) | (0.000064) |
| Immigrant | -0.088*** | -0.064 | 0.040*** | -0.0014 |
| | (0.017) | (0.048) | (0.0091) | (0.0014) |
| Parents married | 0.14*** | 0.74*** | -0.081*** | 0.00052 |
| | (0.031) | (0.17) | (0.023) | (0.0048) |
| Parents cohabiting | 0.030 | 0.14 | -0.0064 | -0.0025 |
| | (0.034) | (0.18) | (0.026) | (0.0050) |
| Joint F-test | 117 | 1497 | 75 | 1.19 |
| Joint p-value | 0 | 0 | 0 | 0.28 |
| N Schools | 385 | 385 | 385 | 385 |
| N Individuals | 209 570 | 209 570 | 209 570 | 209 570 |

*Note:* This table reports balancing tests for male students on various outcomes (Columns 1–3) and our instrument (Column 4) with respect to family characteristics. Each column reports a separate regressions based on our baseline specification. The F-test for joint significance is reported in the bottom of the table and includes all covariates in the table in addition to unreported coefficients for dummies of zero income, missing parental education, and other types of parental households. Standard errors (in parentheses) are clustered at the level of municipality of origin. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

**Table D2.** Balancing tests (girls)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Log-earnings | Years of schooling | Single | Instrument |
| Family income (log) | 0.062*** | 0.54*** | -0.018*** | 0.00085* |
| | (0.0037) | (0.020) | (0.0037) | (0.00049) |
| Mothers' years of schooling | 0.0080*** | 0.23*** | 0.00022 | -0.00020 |
| | (0.0012) | (0.0054) | (0.00077) | (0.00012) |
| Fathers' years of schooling | 0.0074*** | 0.19*** | 0.00020 | -0.000028 |
| | (0.00057) | (0.0036) | (0.00060) | (0.000089) |
| Mother not working | -0.036*** | -0.34*** | 0.0059* | -0.000017 |
| | (0.0042) | (0.024) | (0.0031) | (0.00053) |
| Age of father | -0.00089*** | 0.0030 | 0.0026*** | -0.000061 |
| | (0.00031) | (0.0022) | (0.00031) | (0.000050) |
| Age of mother | 0.00103*** | 0.0847*** | -0.000019 | -0.000062 |
| | (0.00033) | (0.0027) | (0.00031) | (0.000063) |
| Immigrant | -0.057*** | -0.15*** | 0.025** | 0.00053 |
| | (0.017) | (0.049) | (0.0099) | (0.0014) |
| Parents married | 0.011 | 0.46*** | -0.097*** | -0.0021 |
| | (0.020) | (0.14) | (0.027) | (0.0047) |
| Parents cohabiting | -0.017 | -0.22 | -0.040 | -0.0030 |
| | (0.021) | (0.13) | (0.026) | (0.0048) |
| Joint F-test | 154 | 1080 | 48 | 1.037 |
| Joint p-value | 0 | 0 | 0 | 0.42 |
| N Schools | 385 | 385 | 385 | 385 |
| N Individuals | 199 810 | 199 810 | 199 810 | 199 810 |

*Note:* This table reports balancing tests for female students on various outcomes (Columns 1–3) and our instrument (Column 4) with respect to family characteristics. Each column reports a separate regressions based on our baseline specification. The F-test for joint significance is reported in the bottom of the table and includes all covariates in the table in addition to unreported coefficients for dummies of zero income, missing parental education, and other types of parental households. Standard errors (in parentheses) are clustered at the level of municipality of origin. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.




**(a)** Males **(b)** Females

**Figure D3.** Binned scatter plots of first-stage relationships

*Note:* These figures are binned scatter plots of the relationship between residualized school spending and residualized instrument. Figure (a) and (b) report the first-stage relationship in the male and female sample, respectively. Computation of the residualized instrument is explained in Section 5.3 and Figure 6. We perform analogous regressions of log avarage school spending on covariates to partial out residuals for the treatment variable. The bins represent vingtile groups for the residualized instrument. Plotted values are group means of the residualized instrument on the horizontal axis versus group means of the residualized school spending. Lines represent OLS regression estimates for residualized school spending on the residualized instrument. Slopes of the regression lines show the responsiveness of school spending with respect to school-exiting cohort size, and correspond to the first-stage elasticity estimates for males and females in Table 4.

**Table D3.** Effects of school spending on family income in adulthood

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Family income (log) | | Low family income ($\leq 25pct$) | |
| | Males | Females | Males | Females |
| **A. First stage:** | | | | |
| Exiting cohort, age 16 | 0.041*** | 0.047*** | 0.041*** | 0.047*** |
| | (0.0075) | (0.0080) | (0.0075) | (0.0080) |
| **B. Reduced form:** | | | | |
| Exiting cohort, age 16 | 0.027** | -0.0073 | -0.021 | 0.0095 |
| | (0.013) | (0.013) | (0.014) | (0.013) |
| **C. 2SLS:** | | | | |
| School spending | 0.658** | -0.16 | -0.51 | 0.21 |
| | (0.31) | (0.27) | (0.33) | (0.28) |
| Joint F-test (FS) | 30.1 | 33.6 | 30.1 | 33.6 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209 566 | 199 797 | 209 566 | 199 797 |

*Note:* This table reports the effects of average (log) education spending during compulsory schooling on (log) average income during ages 27-36, and a low-income indicator, separately for males and females. The table reports the first stage effect of the instrument on school spending in panel A, reduced form effects of the instrument on the outcome in panel B, and two-stage least squares estimate of school spending on the outcome in panel C, as well as an F-test for instrument relevance. The first two columns (1–2) show the results for (log) family income measured when the children are 27-36 years old. The last two columns (3–4) show the results for a low family income indicator, if average family income is in the lower quartile of the cohort distribution. Average education spending is instrumented by the cohort size of the students leaving compulsory school as the treated students enrolls in first grade. Standard errors (in parentheses) are clustered at the level of municipality of origin. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

**Table D4.** ITT effects of school spending in municipality of birth on individual income in adulthood

| | Log-income | | Low income ($\leq 25pct$) | |
|---|---|---|---|---|
| | Males | Females | Males | Females |
| | (1) | (2) | (3) | (4) |
| **A. First stage:** | | | | |
| Exiting cohort size | 0.051*** | 0.053*** | 0.051*** | 0.053*** |
| | (0.0089) | (0.0092) | (0.0089) | (0.0092) |
| **B. Reduced form:** | | | | |
| Exiting cohort size | 0.029** | 0.0064 | -0.023** | -0.013 |
| | (0.013) | (0.012) | (0.011) | (0.015) |
| **C. TSLS:** | | | | |
| School spending | 0.57** | 0.12 | -0.44** | -0.24 |
| | (0.25) | (0.22) | (0.21) | (0.27) |
| Joint F-test (FS) | 33.3 | 33.8 | 33.3 | 33.8 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 199 810 | 209 570 | 199 810 |

*Note:* This table reports intention to treat estimation results for the effects of average log school spending during compulsory schooling associated with the municipality of birth on (i) log average income, and (ii) indicator for low income during ages 27-36. Estimation is done separately for males and females. Panel A displays the first-stage effects of the school-exiting cohort instrument on school spending. Panel B shows the reduced-form effects of the instrument on the outcome, and Panel C reports TSLS estimates of school spending on the outcome. As instrument for school spending we utilize the cohort size (by municipality) of the students who graduated from compulsory schooling just before entry of the treated cohort of students. An F-test for instrument significance is reported in the bottom of the table. Standard errors (in parentheses) are clustered at the municipality-of-birth level. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

**Table D5.** Heterogeneous effects of school spending on family income, by parental income group

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Family income | | Low family income ($\leq 25pct$) | |
| | Male | Female | Male | Female |
| School spending: | | | | |
| Low income parents | 0.671** | -0.14 | -0.519 | 0.17 |
| | (0.30) | (0.27) | (0.33) | (0.29) |
| Middle income parents | 0.549* | -0.22 | -0.44 | 0.27 |
| | (0.29) | (0.25) | (0.31) | (0.26) |
| High income parents | 0.31 | -0.36 | -0.22 | 0.26 |
| | (0.43) | (0.33) | (0.43) | (0.32) |
| Joint F-test (low FS) | 22.6 | 24.2 | 22.6 | 24.2 |
| Joint F-test (mid FS) | 15.0 | 17.0 | 15.0 | 17.0 |
| Joint F-test (high FS) | 13.7 | 19.1 | 13.7 | 19.1 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 199 810 | 209 570 | 199 810 |

*Note:* This table reports the effects of school spending during compulsory school on adult earnings for children with low, medium and high parental income backgrounds. The families of the children are defined as low, medium and high income if the parental income was at the bottom (0-33 percentiles), middle (34-66 percentiles) or top (67-100 percentiles) third of ranked incomes during the preschool years when the child was between 0 and 6 years old. The first two columns show the 2SLS estimates of the effect of school spending on individual income between ages 27–36 for males (1) and females (2). The last two columns show the 2SLS estimates of the effect of school spending on an indicator for low income for males (3) and females (4). Standard errors (in parentheses) are clustered at the level of municipality of birth. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

**Table D6.** Heterogeneous effects of school spending on educational attainment and family formation, by parental income group

**A. Educational attainment:**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | High school completed | | Years of schooling | |
| | Male | Female | Male | Female |
| Low income parents | 0.25 | 0.15 | 2.67 | -0.11 |
| | (0.30) | (0.27) | (1.85) | (1.76) |
| Middle income parents | 0.15 | 0.046 | 2.90* | -0.34 |
| | (0.29) | (0.25) | (1.76) | (1.67) |
| High income parents | -0.019 | -0.33 | 2.26 | -1.90 |
| | (0.45) | (0.56) | (2.09) | (2.26) |
| Joint F-test (low FS) | 22.6 | 24.2 | 22.6 | 24.2 |
| Joint F-test (mid FS) | 15.0 | 17.0 | 15.0 | 17.0 |
| Joint F-test (high FS) | 13.7 | 19.1 | 13.7 | 19.1 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209570 | 199810 | 209570 | 199810 |

**B. Family formation:**

| | (5) | (6) | (7) | (8) |
|---|---|---|---|---|
| | Single | | Parenthood | |
| | Male | Female | Male | Female |
| Low income parents | -0.715* | 0.31 | 0.54 | 0.04 |
| | (0.37) | (0.28) | (0.36) | (0.27) |
| Middle income parents | -0.604* | 0.40 | 0.54 | 0.12 |
| | (0.35) | (0.27) | (0.35) | (0.26) |
| High income parents | -0.65 | 0.62 | 0.64 | 0.05 |
| | (0.41) | (0.39) | (0.43) | (0.31) |
| Joint F-test (low FS) | 22.6 | 24.2 | 22.6 | 24.2 |
| Joint F-test (mid FS) | 15.0 | 17.0 | 15.0 | 17.0 |
| Joint F-test (high FS) | 13.7 | 19.1 | 13.7 | 19.1 |
| Municipalities | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 199 810 | 209 570 | 199 810 |

*Note:* This table reports the effects of school spending on income in adulthood for students with low, medium and high income parents. The families of the children are defined as low, medium and high income if the parental income was at the bottom (0-33 percentiles), middle (34-66 percentiles) or top (67-100 percentiles) third of ranked incomes during the preschool years when the child was between 0 and 6 years old. Panel A shows the heterogeneous 2SLS estimates on high school graduation by age 34 (columns 1–2) and years of schooling attained by age 34 (columns 3–4). Panel B shows the heterogeneous 2SLS estimates on an indicator for being single at age 34 (columns 5–6) and an indicator for having children by age 34 (columns 7–8). The F-test for joint instrument significance is included for all first stage regressions. Standard errors (in parentheses) are clustered at the level of municipality of birth. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

**Table D7.** Effects of school spending on individual income when instrument is de-trended

| | (1) Baseline | (2) Linear | (3) Quadratic | (4) Cubic | (5) Quartic |
|---|---|---|---|---|---|
| **A. Males** | | | | | |
| School spending | 0.71** | 0.79** | 0.82** | 0.86** | 0.80** |
| | (0.32) | (0.35) | (0.35) | (0.34) | (0.34) |
| Joint F-test (FS) | 30.1 | 31.3 | 27.3 | 34.1 | 30.5 |
| Municipalities | 385 | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 209 570 | 209 570 | 209 570 | 209 570 |
| **B. Females** | | | | | |
| School spending | 0.14 | 0.19 | 0.13 | 0.19 | 0.24 |
| | (0.25) | (0.29) | (0.27) | (0.26) | (0.26) |
| Joint F-test (FS) | 33.6 | 32.0 | 30.7 | 33.6 | 33.3 |
| Municipalities | 385 | 385 | 385 | 385 | 385 |
| Observations | 199 810 | 199 810 | 199 810 | 199 810 | 199 810 |

*Note:* This table reports the effects of school spending on individual income in adulthood under different specifications of the trend in school-exiting cohort size. The first column repeats our baseline estimate from Table 4. In the rest of the columns, the instrument is de-trended by regressing out a polynomial trend of n-th order in school-exiting cohort size. We successively include higher order polynomial trends: Linear (Column 2), quadratic (Column 3), cubic (Column 4), and quartic (Column 5). Panel A shows the TSLS estimates for males, and Panel B shows TSLS estimates for females. School-exiting cohort size by municipality and cohort is defined by log number of children aged 16 at end-year. The F-test for instrument significance is included for all regressions. Standard errors (in parentheses) are clustered at the municipality-of-birth level. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

**Table D8.** Effects of school spending on individual income when including additional controls

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **A. Males** | | | | | |
| School spending | 0.705** | 0.577* | 0.711** | 0.577** | 0.674** |
| | (0.32) | (0.30) | (0.30) | (0.29) | (0.33) |
| Joint F-test (FS) | 30.1 | 30.2 | 35.6 | 36.2 | 30.8 |
| Municipalities | 385 | 385 | 385 | 385 | 385 |
| Observations | 209 570 | 209 570 | 209 570 | 209 570 | 209 570 |
| **B. Females** | | | | | |
| School spending | 0.14 | 0.12 | 0.09 | 0.07 | 0.05 |
| | (0.25) | (0.25) | (0.24) | (0.24) | (0.26) |
| Joint F-test (FS) | 33.6 | 33.7 | 40.2 | 40.7 | 35.2 |
| Municipalities | 385 | 385 | 385 | 385 | 385 |
| Observations | 199 810 | 199 810 | 199 810 | 199 810 | 199 810 |
| **Specification:** | | | | | |
| Family characteristics | | ✓ | | ✓ | ✓ |
| Regional trends | | | ✓ | ✓ | ✓ |
| Baseline instrument | ✓ | ✓ | ✓ | ✓ | |
| De-trended instrument | | | | | ✓ |

*Note:* This table reports the effects of school spending on individual income in adulthood (age 27-36) when controlling for alternative and extended sets of covariates. Panel A shows the TSLS estimates for males, and Panel B shows the TSLS results for females. For reference, Column 1 shows our baseline effect estimates from Table 4. In Column 2 we control for family characteristics by including all covariates tested in the balancing check in Table 2. Column 3 shows the results when including linear cohort trends by region, based on Norway's five main regions (North-, Middle-, West-, South- and East-Norway). Column 4 controls for both family characteristics and regional trends. Finally, in Column 5 we show the results from using the quadratically de-trended instrument (Column 3 in Table D7) while controlling for both family characteristics and regional trends. The F-test for instrument significance is reported for all regressions. Standard errors (in parentheses) are clustered at the municipality-of-birth level. Point estimates marked ***, ** and * are statistically significant at the 1, 5 and 10 percent levels.

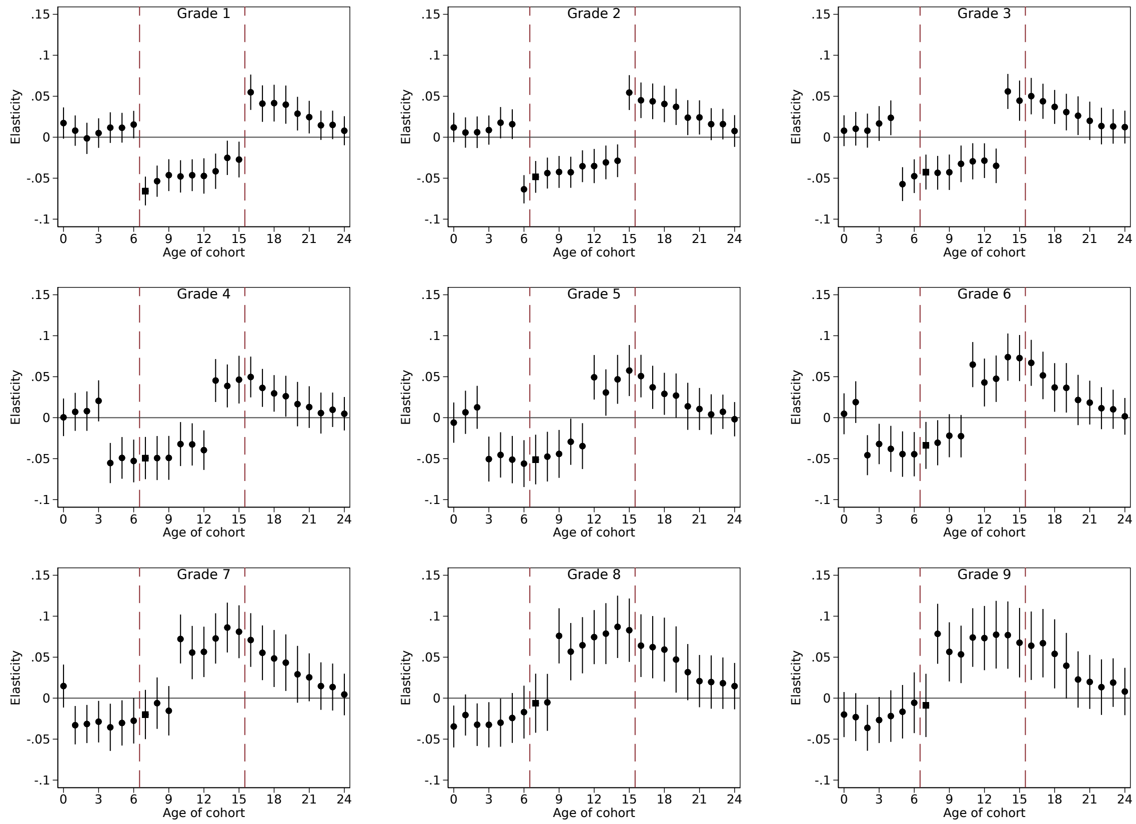



**Figure D4.** The effects of cohort size by age on single grade average education spending

*Note:* This figure shows the effects of (log) cohort size by age on single grade average (log) education spending. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.

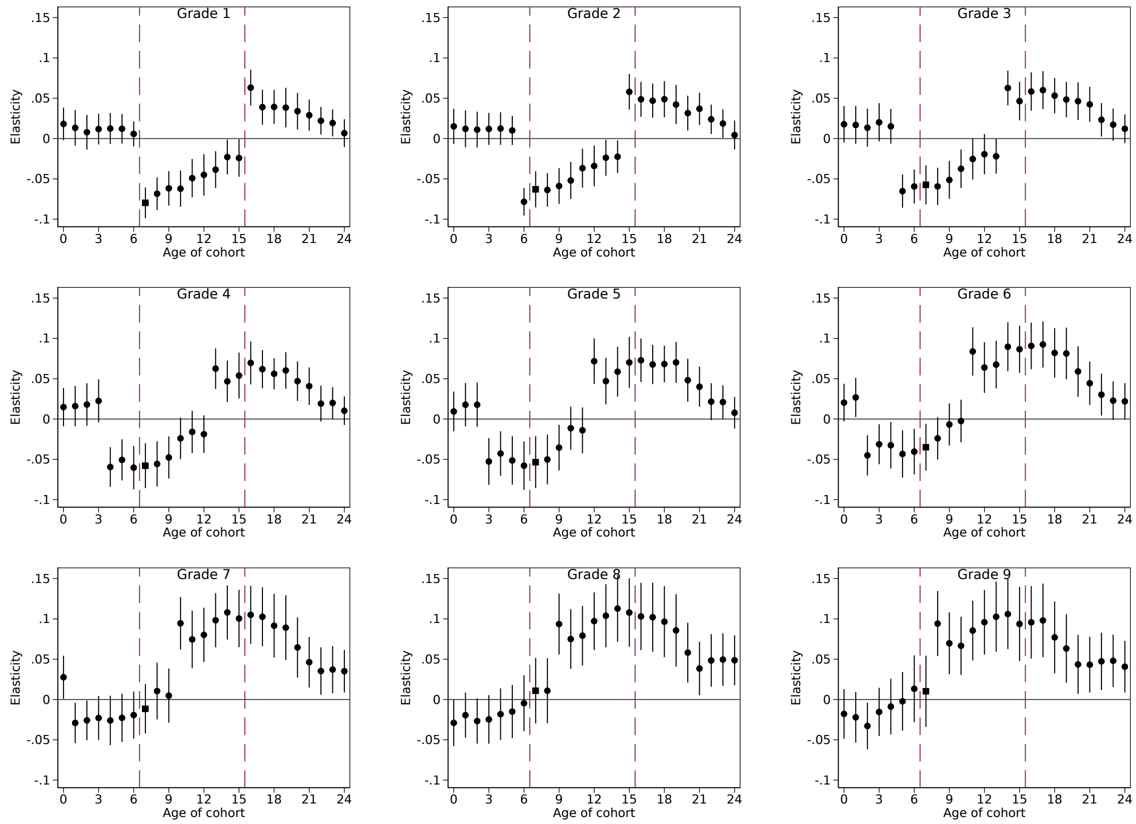



**Figure D5.** The effects of cohort size by age on single grade average wage costs

*Note:* This figure shows the effects of (log) cohort size by age on single grade average (log) wage costs. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.

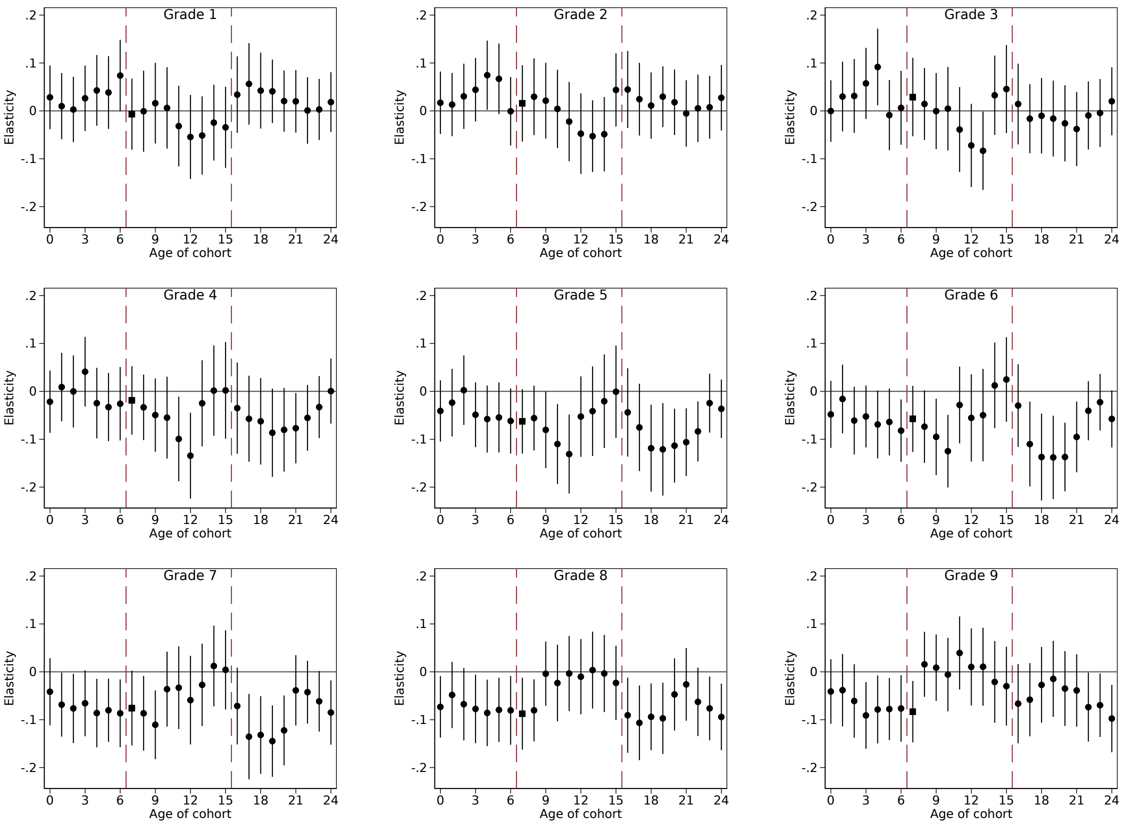



**Figure D6.** The effects of cohort size by age on single grade average other spending

*Note:* This figure shows the effects of (log) cohort size by age on single grade average (log) other spending. The markers represent point estimates of elasticities, and 95-percent confidence intervals are shown by vertical spikes. Standard errors are clustered at the municipality-of-birth level.