ROCKWOOL Foundation Berlin
Institute for the Economy and the Future of Work (RFBerlin)

DISCUSSION PAPER SERIES 133/26

The Demand for Private Schools and Its Impact on School Segregation and Student Outcomes

Jacob Arendt, Anders Holm

www.rfberlin.com MAY 2026

The Demand for Private Schools and Its Impact on School Segregation and Student Outcomes

Authors

Jacob Arendt, Anders Holm

Reference

JEL Codes: I21; I24; J15; R28

Keywords: Private schools; socio-economic and ethnic segregation; student achievement

Recommended Citation: Jacob Arendt, Anders Holm (2026): The Demand for Private Schools and Its Impact on School Segregation and Student Outcomes. RFBerlin Discussion Paper No. 133/26

Access

Papers can be downloaded free of charge from the RFBerlin website: https://www.rfberlin.com/discussion-papers

Discussion Papers of RFBerlin are indexed on RePEc: https://ideas.repec.org/s/crm/wpaper.html

Imprint

RFBerlin
ROCKWOOL Foundation Berlin –
Institute for the Economy
and the Future of Work

Gormannstrasse 22, 10119 Berlin
Tel: +49 (0) 151 143 444 67
E-mail: info@rfberlin.com
Web: www.rfberlin.com

The Demand for Private Schools and Its Impact on School Segregation and Student Outcomes

JACOB NIELSEN ARENDT† AND ANDERS HOLM*

Abstract

This study examines the impact of private school attendance on segregation and student achievement in compulsory school in Denmark. We show that increased private school attendance is driven by students from high socio-economic groups. Leveraging variation across municipalities, grade and calendar years and instrumental variables based on private school openings, we find that higher private school enrollment is associated with higher segregation of disadvantaged children. From event study models of the private school openings and a mover design that controls for student parental background, peer parental background, past achievement and non-cognitive scores, we find small achievement effects of private school attendance.

Keywords: Private schools; socio-economic and ethnic segregation; student achievement

JEL Codes: I21; I24; J15; R28

† Corresponding author: The Rockwool Foundation, Ny Kongensgade 6, 1472 Copenhagen, Denmark (email: jar@rff.dk),

* Western University, Department of Sociology, Room 5306, 1151 Richmond Street, London, ON N6A 5C2, Canada (email: aholm@uwo.ca).

Funding: The project was funded by the Rockwool Foundation, Copenhagen, Denmark (grant 1248).

I. Introduction

Many countries have expanded school choice options to combat segregation and improve school efficiency (Boeskens 2016; OECD 2019). Recent expansions include public schools that operate independently of catchment areas, such as charter and magnet schools in the United States, independent schools in Sweden, academies and free schools in the United Kingdom, and private schools in Denmark. Despite their popularity, our understanding of how such expansions in school choice affect student segregation and educational outcomes remains limited. Recent literature reviews indicate that the theoretical predictions are ambiguous and that the empirical evidence is mixed (Urquiola 2016; Egalité and Wolf 2016; Epple and Romanov 2017), thereby warranting further empirical investigation.

This study examines the case of Denmark, where private school enrollment has increased from less than 8 percent in the 1980s to 20 percent in 2021. Danish private schools share important similarities with publicly funded independent schools in other countries, as they are non-profit organizations primarily financed by the government (receiving approximately 76 percent of the average public school funding) and are not subject to catchment area restrictions. We investigate whether the expansion of private schools in Denmark has contributed to increased student segregation and widening achievement gaps.

All analyses are based on administrative data covering the full population of Danish students in compulsory schooling from 2007 to 2021. A key contribution of this study is the use of complementary identification strategies to examine effects on both segregation and achievement. Although these strategies rely on different identifying assumptions, they yield consistent results.

To examine whether private school enrollment affects the segregation of disadvantaged students across schools, our first approach employs a fixed effects model that exploits differential changes in private school enrollment and segregation across

municipalities, grade levels, and years. This model was also used in Monarrez et al. (2022). The results show that higher private school enrollment is positively associated with increased segregation of students from households with both low and high parental educational attainment, students from non-Western backgrounds, and students from low-income households. The magnitude of these associations is comparable to that reported in previous studies (Monarrez et al. 2022; Irmert et al. 2023).

Our second approach more directly addresses potential bias arising from reverse causality - namely, that segregation itself may induce flight into private schools (Card and Rothstein 2007; Rangvid 2010). We exploit the opening of private schools in a student's local residential area as an instrument for local private school enrollment shares. More than 150 new private schools opened during the study period, corresponding to an increase of approximately 30 percent. We argue that these openings were partly driven by major public school reforms, which generated increased demand for alternative schooling options across municipalities and neighborhoods with varying socio-economic characteristics. We show that private school openings are unrelated to local student composition, and the instrumental variable estimates corroborate the findings from the fixed effects model.

In the second part of the paper, we employ two distinct approaches to examine the effects of private school attendance on student outcomes. First, we implement a mover design that estimates private school effects by comparing students who transfer from public to private schools with students who transfer between public schools. In doing so, we control for differences in parental background, prior achievement, non-cognitive skills, and both pre- and post-transfer peer group characteristics. The mover design indicates that sorting of students with more highly educated parents and stronger prior achievement explains a substantial share of the observed achievement gap. However, changes in peer group composition following school transfers account for a similarly large share. The remaining "pure" school effect is small but statistically significant, with private school attendance leading to modest increases in exam scores and a higher

likelihood of enrolling in the academic track in upper secondary education, rather than the vocational track.

To assess the robustness of these findings to unobserved selection into private schools, we exploit the quasi-random variation generated by local private school openings - again linked to national reforms - and estimate an event study model. This model compares students who complete compulsory schooling before and after the establishment of a private school in their local area with students in areas without such openings. The event study results confirm the findings from the mover design, showing small but statistically significant effects on school-leaving examination scores, and positive - though statistically insignificant - effects on educational enrollment within two years of completing compulsory schooling.

Overall, the findings suggest that the expansion of private schools in Denmark has increased student segregation across schools, while only modestly widening achievement inequalities, primarily through peer group effects.

The study contributes to two strands of the literature: First, research on the effects of school choice on segregation has historically relied on evidence from limited geographic areas or non-causal designs (Gill et al. 2007; Ritter et al. 2007; Bifulco et al. 2009). More recent studies have used national data and policy-driven expansions in school choice to identify effects, finding positive associations between school choice and segregation in Sweden (Böhlmark et al. 2016), across OECD countries (Irmert et al. 2023), and in the United States following charter school expansion (Marcotte and Dalane 2019; Monarrez et al. 2022). Among these, only Monarrez et al. (2022) explicitly accounts for sorting across regions by combining fixed effects and instrumental variable strategies, as in the present study. A key result from our analysis is that, despite examining a context that differs substantially from the United States, the estimated associations are remarkably similar in magnitude.

Second, while many studies find limited average effects of expanded school choice on student achievement (e.g., Cullen et al. 2005, 2006; Clark 2010; see also reviews by

Egalité and Wolf 2016; Epple, Romano, and Urquiola 2017), some evidence points to larger positive effects for specific subgroups, such as Black students (e.g., Wolf et al. 2013; Chingos and Peterson 2015). At the same time, a growing body of research finds substantial positive effects in particular institutional settings, including U.S. charter schools (e.g., Angrist et al. 2010; Abdulkadiroğlu et al. 2011, 2017; Setren 2019; Cohodes et al. 2021) and U.K. academies (Eyles and Machin 2019; Neri and Pasini 2023; Bertoni et al. 2023). We contribute to this literature by examining the Danish case within a Scandinavian school system embedded in a comprehensive welfare state that emphasizes equal opportunity throughout the education system.

II. Institutional setting

Compulsory education in Denmark spans ten years, from ’grade 0’ (similar to kindergarten) through ’grade 9’, with children beginning school in the year they turn six. All children have a guaranteed place in their local public school based on their residential address, with these schools funded and operated by municipal governments. Unlike in the US, where school funding often depends on local property taxes and house prices, funding per student in Denmark is determined primarily by local governmental priorities. The national government supports municipalities whose demographic composition is associated with weaker tax bases, to ensure reasonably equal funding opportunities across municipalities, and many municipalities provide higher funding to more disadvantaged schools.

Private schools in Denmark are non-profit institutions that have served as an alternative option to public schools since the 1850s. Private schools include both schools with a religious or alternative pedagogical or learning approach, as well as more traditional academic-oriented focus. Most private schools are relatively small with small class sizes, and the schools follow collective agreements and pay teachers similar wages as in public schools. The private schools are free to choose their independent curriculum,

but students are subject to the same national learning goals and national exams as public schools which are administered by the government.

The private schools do not serve a particular catchment area and are, therefore, not required to admit students who live nearby or in the same municipality. On the contrary, private schools are allowed by law to select their students without justification. This is primarily to allow schools to select students that fit the school profile (e.g. supporting a specific religious or pedagogical profile), but it also allows schools to cream skim their students to keep costs low and exam performance high[1]. The private schools are subsidized by the national government, and the subsidy has varied between 71% and 76% of the average costs of students in public schools. Parents therefore pay a tuition fee that typically varies between USD 200-500 per month.

III. Data and descriptive statistics

We use administrative Danish register data covering all students in compulsory school (grades 0 through 9) from 2007 to 2021. We observe the school attended and whether it is a private or public school. We treat schools at distinct addresses as separate units to obtain measures of the dispersion of minority groups across geographical areas. We exclude students attending special needs schools (roughly 5%) and students attending boarding school in grades 8 and 9 (approximately 6%).

We examine segregation of students based on parents' educational attainment, income, and ethnic background (see the next section for a description of the segregation measures). These variables are obtained from the person registry and from registers collected by education institutions and tax authorities. We measure families with low educational attainment (neither parent has completed high school) and families with high educational attainment (either parent has a master's degree). Low and high income

[1] Exam performance is published each year for all schools, public and private: https://uddannelsesstatistik.dk/Pages/Topics/13.aspx

is defined as whether equivalized disposable family falls below the median divided by 1.5 or above the median times 1.5. We consider an ethnic minority group consisting of students with a non-Western background.[2]

Student achievement is measured using scores in the compulsory school examinations in grade 9 in subjects Math and Danish. In part of the analysis, we use previous achievement tests and non-cognitive scores as controls. Students are tested bi-annually from grade 2 to 8 in Danish and in grade 3 and 6 in Math. These achievement tests are highly correlated with the final examination scores in grade 9[3]. The non-cognitive scores are obtained from national well-being surveys, conducted at every grade level. The achievement tests and well-being surveys are compulsory in public schools, but not in private schools. Two different sets of well-being surveys are utilized; A teacher-assisted survey from grade 0 to 3 and a student survey from grade 4 to 9. We follow Houmark et al. (2024) and use the well-being survey for grades 4 to 9 to construct five composite scores, three of which – conscientiousness, agreeableness, and emotional stability – resemble personality traits from the Big-Five personality test (Costa and McCrae 1985). The last two composite scores are school related and are referred to as subjective school achievement and a school satisfaction score.

We measure educational enrollment in high school or vocational training that follows the completion of compulsory schooling in Denmark. Because it is common to participate in a voluntary 10th grade, we measure educational enrollment within 2 years of completing the 9th grade. Enrollment as an outcome has been thought of as capturing a broader set of skills, including non-cognitive outcomes (Wolfe et al. 2013; Chingos and Peterson 2015). We standardize exam, achievement and well-being scores, within years to account for outcome-specific trends such as grade inflation.

[2] Western countries are defined by Statistics Denmark as the European Union, the European Economic Area, and the four Anglo-Saxon countries: England, the United States, Canada, Australia, and New Zealand.

[3] The correlation coefficient between scores achieved in the tests in grade 6 and in the final exams in grade 9 is approximately 0.6 for Danish and 0.7 for math (Skov and Flarup 2020).

Table 1. Descriptive statistics for public and private schools, 2007-2021.

	Public schools (1)	Private schools (2)	All (3)
Age	10.54	10.76	10.58
Girl	49%	51%	49%
School size (students)	514	315	481
Father's years of education	14.47	15.15	14.58
Mothers's years of education	14.32	14.90	14.41
Equivalized disposable family income (USD)	246839	288425	253695
Parent's low educational attainment	6.6%	3.4%	6.1%
Parent's high educational attainment	18.6%	28.0%	20.1%
Low income	16.9%	13.5%	16.4%
High income	11.6%	18.1%	12.7%
Non-Western origin	8.1%	6.8%	7.9%
Number of schools (2007)	1552	489	2041
Number of schools (2021)	1172	536	1708
Observations	7,549,900	1,490,577	9,040,477

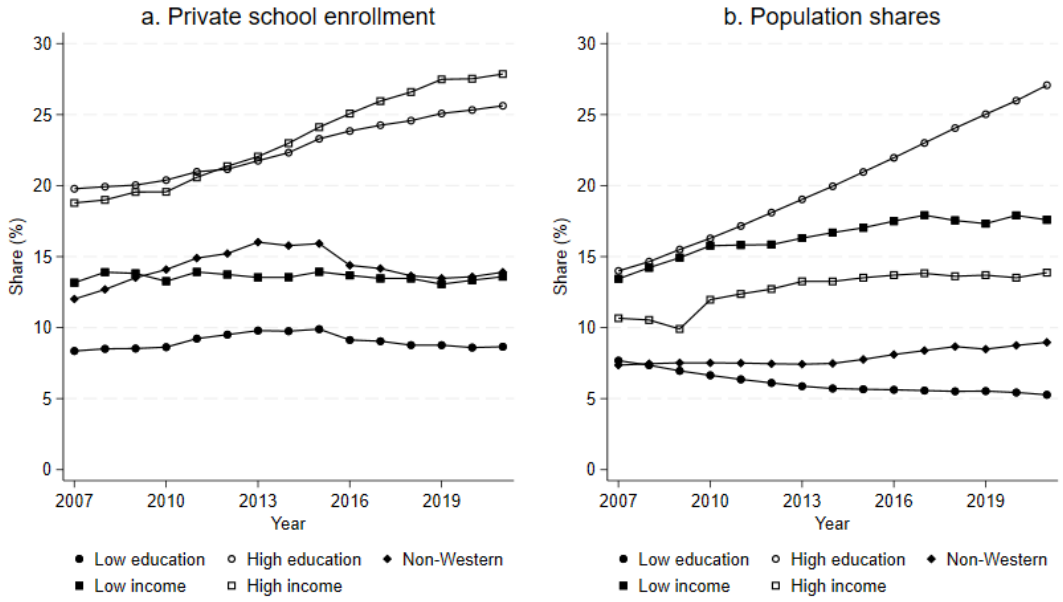
Notes: Parent's low educational attainment is defined as both parents having compulsory education as their highest educational attainment. Parent's high educational attainment is defined as at least one parent having completed a master's degree (long-cycle). Equivalized disposable family income is created with the modified OECD equivalence scale. Low and high income are defined as equivalized disposable family income being below 66% or above 150% of the median income. Being of Western origin is defined as being from the EU and EEA countries, USA, Canada, Australia, New Zealand.

A. Descriptive statistics

From the number of students reported in the bottom row of Table 1, we find that 16.5% of students are enrolled in a private school across the entire period. On average, students in private schools have parents who have higher educational attainment and higher income, and who are more often of Danish origin. While not all differences are large, the fraction of students with at least one highly educated parent is 50% larger in

private schools than in public schools (28% vs. 19%). The table also reveals that while private schools have fewer students than public schools (school size being 315 vs. 514), the number of private schools has increased from 489 in 2007 to 536 in 2021, while the number of public schools has decreased. Therefore, in 2021 private schools made up 32% of all schools as opposed to 24% in 2007.

Figure 1. Private school enrollment and population shares among subgroups.



Notes: Low education attainment is defined as neither parent having any education beyond compulsory schooling. High education is defined as at least one parent having a master's degree. Low income is defined as disposable family income being below 66% of the median and high income when it is above 1.5 times the median. Non-Western origin is defined as not being from a Western country, see the data section for details.

The mean level of private school enrollment masks a significant development over time, where private school enrollment rate has increased from 13% to 19%, mainly driven by students from a high socio-economic background. This is shown in panel a) of Figure 1. While enrollment for students with low income has remained stable around 8%, it has increased from 19% to 27% for students of highly educated parents

and slightly more for students with high-income parents. Panel b) shows that part of the increase in private school enrollment is likely to be driven by the large increase in the share of students from a high parental educational background. We provide a more detailed analysis of the determinants of private school enrollment in Appendix B by means of an Oaxaca-Blinder decomposition. It shows that socio-economic differences related to education and family income explain up to 40% of the increase in private school enrollment.

IV. Methods

A. A fixed effect model for segregation

We are concerned with the dispersion of minority or disadvantaged students *across schools* (public and private) within a local area and chose the Gini index as the primary measure of segregation because it has desirable properties (James and Taeuber 1985; Massey and Denton 1985; Frankel and Volij 2011)[4]. We measure segregation at the municipal level because municipalities administer the public schools and students are guaranteed a place at a public school within their municipality. The roughly 2,000 schools (cf. Table 1) are dispersed across 98 municipalities with an average population size of 60,000 people. We explore sensitivity to the use of different segregation measures, aggregation areas and explore segregation across different minority groups.

To examine the impact of private school enrollment shares on segregation, we start by regressing measures of segregation (S) at a given grade level across schools within

[4] For instance, the Gini index is composition and size invariant, meaning that segregation is unaffected if the fraction of the minority group within each school or the total population is scaled equally across schools. This is, for instance, not the case for the variance ratio used in Monarezz et al. (2022). This is important because the size of the minority groups changes considerably across local areas and over the period of observation. The Gini also fulfils the transfer principle and the Lorenz criterion. The transfer principle states that segregation decreases when a minority student moves from a school with a large fraction of minority students to a school with fewer. The Lorenz criterion states that segregation is higher if the underlying Lorenz curve is dominating, James & Taeuber (1985). The transfer principle is, for instance, not fulfilled by another commonly used segregation measure, the dissimilarity index.

municipalities on the fraction of students in private schools at similar levels of aggregation (*P*):

$$S_{gtm} = \alpha_{gt} + \gamma_{gm} + \eta_{tm} + \beta P_{gtm} + \pi_2 X_{gtm} + \epsilon_{gtm} \quad (1)$$

where *m* is the municipality in which the school is located, *g* the grade level and *t* the calendar year. The fixed effects η_{tm} account for time specific municipality effects, capturing if some municipalities have become more segregated over time than others, which may correlate with private school enrollment. Likewise, γ_{gm} captures grade-specific municipality effects capturing, say, if segregation is higher in lower secondary school levels in some municipalities than others. Further, α_{gt} capture time-varying grade level effects, capturing if segregation has increased more at some grade levels and ϵ_{gtm} is an idiosyncratic error term. X_{gtm} includes the size of the local student population, the size of the minority group, and the number of schools in the area, to provide robustness towards the sensitivity of the segregation index to changes in these variables. We also test whether the relationship is driven by *residential* segregation by including a measure of local segregation across parishes of the students residing in the municipality (as opposed to across schools) as a control. There are roughly 6 parishes per municipality. The model is estimated with standard errors clustered at the municipal level.

B. A mover design for student outcomes

Students in private schools may have higher achievement scores either because of sorting of better students into private schools, because of better peer groups or because of private school effects. The Danish private school system potentially generates effects via all channels. Our empirical strategy seeks to disentangle student sorting and peer group effects from the school effect.

The school effect is a combined effect of teacher sorting and effects arising e.g. from a specific pedagogical focus and smaller schools. We have no information that allows

separation of teacher and school effects, and since teacher salaries are uniform across public and private schools, teacher sorting only arises if teachers self-select to private schools for non-pecuniary reasons.

To estimate the private school effect, we use a mover design, where we compare students in public schools who transfer to a private school with students who transfer to another public school. This avoids comparison of students who chose a public or a private school at school start and instead compares students who are all attending a public school at some point. To control for selection into private schools *among* the students transferring school, we utilize administrative data on parental background as well as data available only for students in public schools on past achievement, non-cognitive skills and well-being in the years prior to moving (see the data section).

To differentiate effects of student sorting from peer group effects, we separately control for pre- and post-transfer parental background of peers. While the post-transfer peer-group effect is not due to the private schools per se, it is part of the value added to students transferring to private schools.

While this is a selection-on-observables-strategy, it controls for several channels of selection known to be important for student outcomes, not included in many standard analyses of student achievement. To the extent that private school students are selected positively on additional unobserved characteristics, as they are on observed characteristics (cf. Table 1), this method likely provides an upper bound for the true effect.

C. Private school opening designs

We supplement each of the methods described above by designs that only utilizes variation in private school attendance stemming from private school openings. We define a private school opening as the event that a private school with positive

enrollment is observed for the first time in the postal district where the student lives[5]. More than 150 new private schools opened from 2007 to 2021, which is 7% of the total number of schools and 30% of the number of private schools in Denmark in 2007 (see Figure A.1). We argue that many private school openings were triggered by public school reforms that generated an unforeseen rise in private school options at the local level. One reform in 2012 had an objective to limit special needs education in public schooling due to rising costs. While the reform succeeded holding students with special needs in normal classes, it came at a cost of the well-being of these students (Nielsen & Rangvid 2016). Another reform in 2013 extended the length of the school day in public schooling and a decree simultaneously forced teachers to work more hours without compensating pay (Act no. 409, 2013). This reform took a large toll on many teachers and students with (seemingly) no positive effects on overall achievement or student well-being (Nielsen et al.2020; Jensen et al. 2020). These reforms may have triggered a demand for private schooling, either for students on the margin for special needs education, or because it worsened the class environment in public schools through increased disruptions. To fully account for the impact of private school openings it is important that public school closures took place at an increased pace in the early years of the period, particularly in rural areas (Bækgaard 2010). This triggered private school openings as substitutes of the closed public schools in rural areas and these new schools are not expected to have the same impact on segregation and student outcomes as other private school openings. Appendix Figure A.1 shows the frequency of private school openings in rural and urban areas and compares it to public school closures. It shows that in rural areas, private school openings are more frequent in years immediately following years with many public school closures, whereas the same pattern is not present in in urban areas. In the following we describe how the private school openings

[5] Throughout our study period, there are between 550 and 580 postal districts where a school is located. We merge very small postal districts in the city of Copenhagen.

are used as an instrumental variable in segregation models and in an event-design to examine student outcomes.

C.1 Instrumental variable estimation for segregation

The estimates of β from model (1) could be biased due to reverse causality if flows to private schools are driven by high segregation among public schools, i.e., an uneven distribution of the minority groups within public schools. To address such reverse causality, we use enrollment in newly opened private schools as an instrumental variable for the total private student share. To allow for some time for adjustment, we lag enrollment shares in newly opened private schools by two years, i.e., $P^{n}_{g-2t-2m}$ (where superscript 'n' stands for newly opened), as an instrumental variable (IV) for total private school enrollment at that grade level in the current year, P_{gtm}. The IV estimate identifies a local average treatment effect (LATE) under four assumptions: independence, exclusion, monotonicity and a valid first-stage (Angrist and Imbens 1995). The independence restriction requires that it is close to random if an opening occurs for students at a given grade level. Monotonicity requires that private school enrollment does not decrease when a private school opens. This could happen, for instance if private schools are downsizing, e.g. due to increasing rents in cities. The exclusion restriction requires that student enrollment in new private schools in year t-2 does not affect segregation in year t beyond its impact on total private student enrollment in year t. For instance, opening of new private schools should not increase student flows between public schools or to and from the municipality. The assumptions are discussed and examined further below. Under these assumptions, the estimated effect identifies an effect for the complier group of students, who are admitted to private school, only when a new school opens.

C.2 Event-study design for student outcomes

We supplement the mover-design for student outcomes with an event-study design that compares student cohorts residing in the same postal district, *d*, who completed compulsory schooling before and after a private school opened in the district, with cohorts who completed compulsory school across the same calendar years in districts without a private school opening. Recent studies have pointed out potential problems with such a staggered roll-out design (e.g. Callaway and Sant'Anna 2021; Sun and Abraham 2021; see Roth et al. 2023 for a review of the more general literature). The main problem is to avoid using previously treated as controls for later treated. To tackle this, we stack all event year cohorts on top of each other (Cengiz et al. 2019). The effects are therefore estimated in the area-and time-by-event-cohort specific fixed effect model:

$$y_{dtc} = \alpha_{dc} + \gamma_{tc} + \sum_{\kappa=-4,\neq-2}^{5} \beta^{\kappa} I(c - t = \kappa) + \epsilon_{dtc} \quad (2)$$

where y_{dtc} is the mean student outcome (e.g., the compulsory national examination grade) in postal district *d* at time *t,* and where a private school opening occurred at time c. As in the segregation analysis, we choose a reference period two periods prior to the opening to allow for a short anticipation effect. β^{κ} is the effect of a private school opening κ periods after the opening, and it has a causal interpretation under the assumption of parallel trends in districts with and without an opening.

The parallel trend assumption is tested by the estimates of pre-opening differences, and by examining residential student mobility and compositional changes. Such pre-testing for parallel trends can provide additional bias. We apply a method from Rambachan and Roth (2023) that tests how large deviances of the pre-treatment

differences are allowed in post-treatment periods, for post-treatment results to become insignificant[6].

V. Results

A. Private school attendance and segregation

We focus on segregation based on parental educational attainment but compare the results to findings for segregation of other groups below (parental income and non-Western background). The results from estimates of model (2) are shown in table 2 for segregation of children whose parents have high or low educational attainment.

From column 1 we find that private school enrollment is positively related both to segregation of children from families with high and to children from families with low educational backgrounds. Including the full fixed effects structure from model (2) in column 2 raises the coefficient for segregation of students whose parents have low educational attainment, whereas it is left unaltered for students of highly educated parents. This means that within area and grade variation in segregation of students from families of low educational attainment is more closely related to private school enrollment than between areas and grades. The association for students from families with low educational attainment drops again when controlling for the observed control variables (size of the local area and the minority groups) in column (3) but – perhaps more surprisingly – is only slightly reduced when controlling for segregation across parish of residence (rather than schools) within the municipality (column 4).

[6] Estimates are obtained using the stata command honestdid. We use the method Rambanach and Roth (2023) referred to as the ARP-method (short for Andrews, Roth and Pakes 2023).

Table 2. Relationship between local private school shares and local segregation of students by parental education level.

	Outcome: School segregation			
	(1)	(2)	(3)	(4)
Segregation by low parental education	0.336***	0.439***	0.376***	0.352***
	(0.012)	(0.065)	(0.058)	(0.054)
Segregation by high parental education	0.162***	0.170***	0.145**	0.153***
	(0.010)	(0.063)	(0.058)	(0.049)
Pairwise area, grade and time FE	No	Yes	Yes	Yes
Control variables	No	No	Yes	Yes
Local residential segregation	No	No	No	Yes
Observations	14700	14700	14700	14700

Notes: Results from population weighted regressions of municipal segregation across schools on private school share (model 2 described above). Low parental education is a dummy taking value one if neither parent has educational attainment beyond compulsory schooling. High parental education is a dummy taking value one if at least one parent has a master's degree. Fixed effects include grade-by-time, grade-by-area and time-by-area fixed effects. Controls include number of schools, number of students, and the size of the 'minority' group (students whose parents have low or high educational attainment). Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

According to the estimate in column 4, this increase is associated with an increase in segregation by 0.035 Gini points for every 10 percentage-points increase in the private school share (0.352 x 0.1). To provide a benchmark of the size of the effect, consider the increase in the private school student share observed from 2007 to 2021 of roughly 6 percentage points (from 13% to 19%), this corresponds to an increase in segregation of 5.4% relative to the local initial levels of segregation. Another way to illustrate the size of the relationship is to calculate the associated elasticity, which is 16% for segregation of students whose parents have low educational attainment and 10% for segregation of students whose parents are highly educated.

A.1 Robustness: Measures of segregation

We explored the robustness of the relationship between segregation and private school enrollment with respect to the way segregation is measured, in two ways. First, we

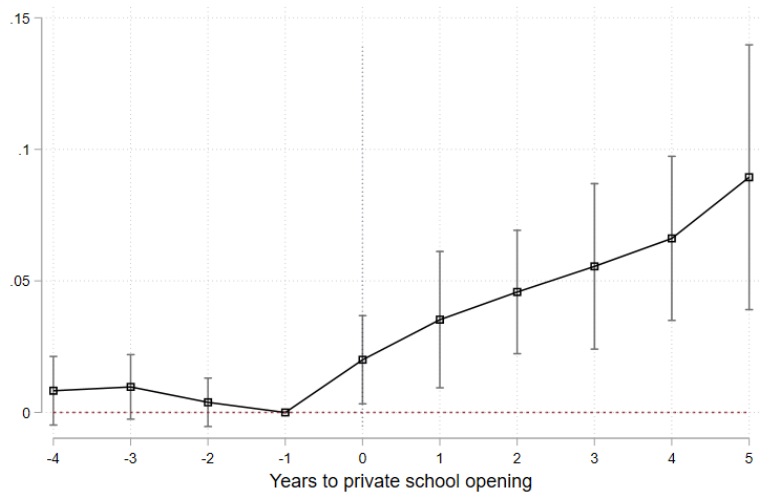
examine if we find similar results for other dimensions of segregation than by parental education. Private school enrollment is also associated with higher segregation of students from a non-Western background and low parental disposable income, but not for high income, see Table A.1. Second, we examined whether the results are sensitive towards the use of different segregation indices and areas of aggregation (Table A.2). The alternative areas of aggregation are described in more detail in Appendix C. These results confirm that the positive relationships between private student shares and segregation are robust across student dimensions, segregation measures, areas, and subsamples.

A.2 Instrumental variables

To examine bias from reverse causality, Table 3 presents IV variable estimates of model (1) using private school enrollment in newly opened private schools as instrument. For the IV-estimates to be valid, we need the exclusion, monotonicity and independence assumptions to be fulfilled.

To provide support for the independence assumption, Table A.3 shows that neither student parental background characteristics nor lagged segregation predict private school openings at a given grade level. This supports the expectation that private school openings were partly due to external factors and occurred across socio-economic areas. The assumption is further supported by estimates from the event-study (model (2)) of the effect of private school openings on private school enrollment, shown in Figure 2. Since model 2 is based on cohort-comparisons, it shows the effect on private school enrollment in 9^{th} grade in years before and after an opening. It shows no evidence of pre-treatment differences and a large increase in private school enrollment in post-treatment years. The increasing trend after the opening therefore simply reflects that student cohorts are more likely to enroll in private schools at lower than at higher grade levels when a new school opens.

Figure 2. Event study estimates of the effect of a private school opening on private school attendance.



Notes: Estimates of the effect of private school openings on private school enrollment in 9th grade in a postal district from event study model (2) before and after the first observed opening. The control group are students from similar cohorts in never treated districts. Vertical bars are 95%-confidence intervals based on robust standard errors, clustered at the cohort-by- municipal level.

Table 3 reports the results from the IV model. It shows that the first-stage effect is large, significant and positive, with an F-value of 29.9. The coefficient of 0.197 tells us that a new private school raises absorbs 1 in 10 of all students at a given grade level in the municipality, total private school enrollment at that grade level in the municipality increases by 1.97 percentage points.

The IV-estimates confirm a positive relationship between private student shares and local segregation of students with different socio-economic and ethnic characteristics[7]. Three of the five IV-estimates are significant on a 10% level. The estimated effect size corresponds to an increase of 0.05 Gini points in segregation of low students with low educated parents (0.06*0.789) for the observed change in private school enrollment of 6 percentage points over the period. The IV-estimates therefore support that the

[7] The results are qualitatively the same if the opening is only lagged one year, as opposed to two years.

association between local segregation and private school shares is not driven by selection.

Table 3. Instrumental variable estimates of the relationship between local private school shares and local segregation of students by parental education level.

	Segregation by:				
	Low education	High education	Low income	High income	Non-Western
	(1)	(2)	(3)	(4)	(5)
Private	0.789	1.075**	1.068*	0.671	1.721***
	(0.626)	(0.445)	(0.559)	(0.663)	(0.533)
First-stage:	0.197***	F-value:	29.945		
	(0.036)				

Notes: Results from instrumental variables estimations of population weighted regressions of local segregation on private school share, measured by school municipality (model 1 described above). Local segregation is measured by the Gini index for segregation of students with different parental characteristics: Low education is a dummy taking value one if neither parent has educational attainment beyond compulsory schooling. Parent's high educational attainment is a dummy taking value one if one parent has a master's degree. All models control for the following fixed effects: grade-by-time, grade-by-area and time-by-area fixed effects. Controls include number of schools, number of students, and the size of the group (students whose parents have low or high educational attainment). Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

As is often the case, the IV-estimates are larger than the FE-estimates. Under a constant effect assumption, it implies that the unobserved propensity for segregation is negatively correlated with private school shares. This is at odds with findings of "white flight" (Card et al. 2007; Rangvid 2010) but could occur if new private schools open in areas where segregation would otherwise be declining, e.g. due to improvement of local public schools. Under effect heterogeneity, the larger IV estimates may arise when new private schools generate more segregation than existing ones, e.g., if school choice is more restricted for high-SES families in areas where private schools open. Both scenarios could also indicate violations of independence and exclusion restrictions.

While we cannot rule out these scenarios, we stress that the IV- and FE estimates are not significantly different, and their difference could also reflect larger uncertainty of the IV method.

We also explored results by urbanicity and found similar effects in rural and urban municipalities (municipalities where the largest city has more than 100.000 inhabitants), see Appendix Table A.4.

B. Private school attendance and student achievement

This section examines if private school enrollment impact student outcomes, using the mover-design and the event-study model. Table 4 presents the estimated differences in outcomes between students in private and public schools, gradually controlling for different types of selection into private school. Because of the positive selection of students into private schools on all observable characteristics (see Table 1), these are most likely upper bounds of the 'pure' private school effect.

We start by showing the OLS estimates of the difference between students in private and public schools in the entire sample. Column 1 shows that the within municipality and year difference between public and private schools 9th-grade examination scores in the Danish language exams is 0.253 of a within-cohort standard deviation, and 0.308 for scores in Math. The association decreases to about half the size when controlling for own parental background and pre-transfer peer background (column 2)[8].

[8] The controls are parental education and income, gender, age, non-Western origin, co-habiting parents, municipality dummies, and if experienced a public school closure, and the pre-transfer school-cohort peer means of the same variables.

Table 4. OLS estimates of private versus public school differences in student outcomes.

	Sample:						
	All		Mover design				
Student outcomes:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Danish	0.253***	0.097***	0.293***	0.100***	0.045***	0.029***	0.056**
	(0.005)	(0.004)	(0.013)	(0.012)	(0.010)	(0.010)	(0.029)
Math	0.308***	0.168***	0.333***	0.154***	0.113***	0.089***	0.046
	(0.005)	(0.004)	(0.013)	(0.013)	(0.011)	(0.011)	(0.030)
Enrollment in high school	0.103***	0.0535***	0.113***	0.050***	0.034***	0.023***	0.036**
	(0.002)	(0.002)	(0.006)	(0.006)	(0.005)	(0.006)	(0.014)
Vocational training	-0.072***	-0.041***	-0.084***	-0.041***	-0.031***	-0.023***	-0.036***
	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)	(0.006)	(0.013)
High school or vocational training	0.035***	0.015***	0.036***	0.014***	0.008**	0.005	0.005
	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
Observations	301334	301334	28478	28478	28478	28478	5224
Parental and peer background	NO	YES	NO	YES	YES	YES	YES
Past achievement	NO	NO	NO	NO	YES	YES	YES
Post transfer peer background	NO	NO	NO	NO	NO	YES	NO
Past achievement changes	NO	NO	NO	NO	NO	NO	YES

Notes: Results from regressions of student outcomes on a dummy for private school enrollment for all students (columns 1-2) and for students transferring from public schools to private schools relative to transfers to public schools (columns 3-7). All models control for calendar year, municipal dummies, and the grade at which the school transfer was made (grades 4-8), and condition on non-missing on all controls. The parental and peer background includes parental education and income, gender, age, and indicators of non-Western origin, municipality, and if experienced a public school closure, as well as school cohort means pre-transfer means of the same variables. The achievement tests are the national tests in Danish (grades 2, 4, or 6), the personality tests are five different summary test scores from the national well-being survey. Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

There is also a very large difference in high school enrollment which is 10 percentage points higher in private schools, but it decreases to 5.3 percentage points when controlling for student parental and peer background characteristics. This occurs at the expense of a drop in the fraction of students who enroll in vocational training (and therefore not by a drop in non-enrollment), and the differences between private and public schools in either high school or vocational training is only 1.5 percentage point.

Column 3-7 shows the results from the mover design that compares students who transfer from a public school to a private school versus to another public school. To control for differences in achievement and personality traits prior to the transfer, these estimates only include students in at least 4th grade, where such controls are observed.

Columns 3 and 4 repeat the exercise from columns 1 and 2 in this smaller sample of movers and show that results are close to being indistinguishable from the national representative sample: Raw differences are large but are reduced by more than 50% when controlling for individual and pre-transfer peer parental background.

Columns 5-7 control for past achievement, non-cognitive scores and post-transfer peer characteristics. Appendix Table A.5 shows that past achievement differs substantially between students transferring to private and public schools: While the standardized test score in Danish subjects is 0.1 for students transferring to other public schools (with means for all public school students being zero), it is 0.3 for students transferring to private schools[9]. The difference in past achievement between students who transfer to public and private schools, while all students are still in public school, is therefore two-thirds of the size as the difference in 9th grade exam scores of all public and private school students (Table 4, column 1). The differences in non-cognitive scores are smaller: The score on conscientiousness is slightly higher for students transferring to private schools in the years prior to the transfer, but alike for agreeableness and subjective achievement. Students who transfer to private schools,

[9] Math is not included because it is only tested in grade 3 and 6.

however, score worse on emotional stability and experience a larger drop in school satisfaction during the years prior to the transfer.

Column 5 shows that the differences in student outcomes in 9th grade and beyond narrow further to roughly a third of the raw differences when controlling for these differences in student achievement test scores, non-cognitive skills and school satisfaction in the year prior to the school transfer. The effect on exam scores is reduced to 0.045 of a standard deviation in the Danish exam and 0.113 in Math, and the effect on the fraction who enroll in high school is reduced to 3.4 percentage point, almost entirely accounted for by a similar drop in vocational training. Except for Math scores, these differences are relatively small, and substantially smaller than the standard regression estimates in columns 1-4.

Column 6 controls for post-school transfer socio-economic peer group composition to explore how much of the differences are explained by better peers in private schools. This reduces differences in Danish and Math exam test scores to 0.029 and 0.089 of a standard deviation and wipes out the effect on overall educational enrollment (enrollment in either high school or vocational training). This illustrates – subject to the caveat that these are not causal peer effects - that the sorting into schools with better peers matter for student achievement gapsb between public and private schools.

Finally, column 7 controls for *changes* in past test and non-cognitive scores, i.e., accounting for the observed reduction in well-being and subjective achievement for students transferring to private schools. This reduces the sample size substantially but also reduces the size of the difference in Math test scores.[10]

[10] In our mover design, we define students who switch from a public to a private school as the treated group, and students who switch between public schools as the comparison group. We argue that moves typically reflect two broad mechanisms. First, some moves are driven by residential relocation, which often entails changing municipalities and re-optimizing school choice. Second, other moves reflect dissatisfaction with the current public school, prompting families to seek an alternative. To better isolate moves plausibly driven by dissatisfaction rather than relocation, we re-estimate the mover models restricting the sample to students who move within the same municipality. Under this restriction, the estimated effects are larger, though not statistically

We cannot conclude from columns 4-7 which set of controls that explain more of achievement gaps, as it depends on the order of which controls are included. Appendix Table A.6 shows the results from the mover-design when control sets are included separately. It shows that past achievement and non-cognitive scores (column 4) and post-transfer peer background (column 3) reduces achievement gaps by roughly the same amount as parental background (column 1), whereas pre-transfer peer background (column 2) have a more limited impact on the difference between outcomes for students in public and private schools.

We finally explore effect heterogeneity, to cast light on the findings reported in many studies that disadvantaged students may benefit more from school choice. Table A.7 shows results for different groups of students who generally perform below average (boys, students with low educated parents, having a Non-Western background), and Table A.8 shows results for groups of students who perform above average. The tables show slightly larger differences for lower performing groups in Danish exam scores and enrollment in high school, and smaller and insignificant effects for higher performing groups. The effects for groups of students with an average above or below mean levels are, however, not significantly different from each other, and the difference in Math test scores are alike across groups (e.g. boys vs. girls).

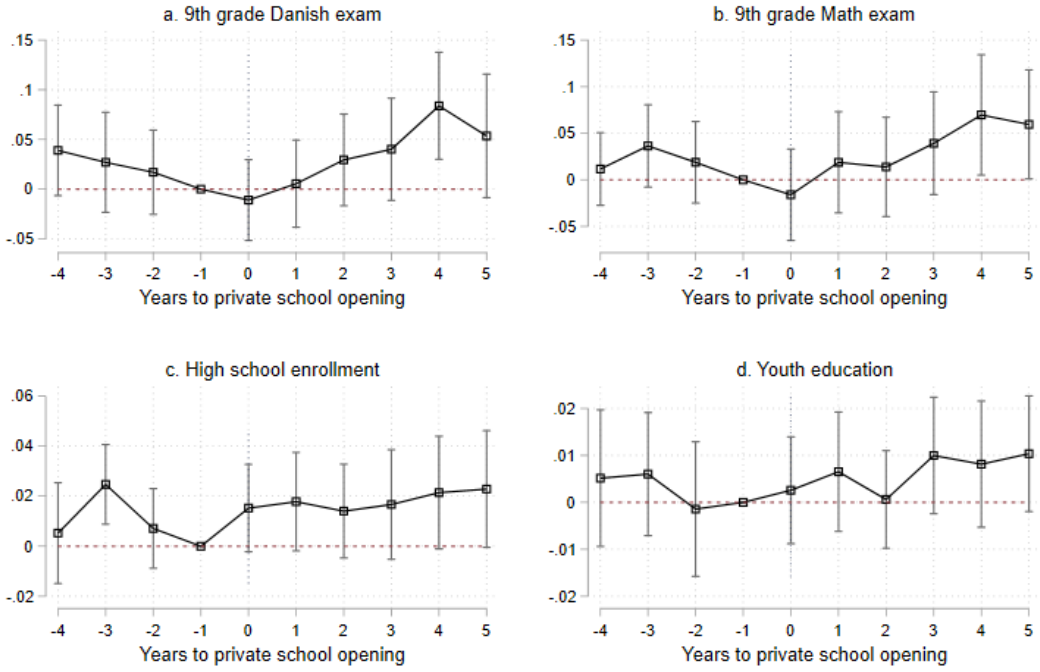
B.1 Event-study estimates of the effect on student outcomes

To examine the robustness of the mover design to selection on unobserved covariates, the estimates from the event study model (2) of effects of private school openings on

different from the baseline estimates. This pattern is consistent with the idea that municipality-to-municipality movers are a more heterogeneous group whose outcomes reflect many concurrent changes (new neighbourhood, peer environment, commuting distance, family circumstances), which can dilute or confound the estimated school-sector effect. By contrast, within-municipality moves are more likely to represent an active response to school fit or perceived quality, so the treatment contrast is "cleaner" and the estimated effect increases.

student outcomes are shown in Figure 3. Panel a) and b) in Figure 3 shows no significant differences in 9th grade examination scores in Danish and Math for cohorts attending exams prior to the private school opening, but significantly positive effects four to five periods after the opening, where cohorts have had a longer period to attend new private schools.

Figure 3. Event study estimates of the effect of a private school opening on student achievement.



Notes: Estimates of the effect of private school openings in a postal district from event study model (2) before and after the first observed opening. The control group are students from similar cohorts in never treated districts. The "9th-grade exam GPA" is the standardized grade point average of the final national examination scores in Danish and math. The figure shows the difference in cohort mean outcomes for students before and after the first observed private school opening in the district, compared with students in districts without a private school opening. Vertical bars are 95%-confidence intervals based on robust standard errors, clustered at the cohort times municipal level.

The pooled effects in all post-opening periods are significant at a 5% significance level. An opening induces an increase in exam score of slightly more than 0.05 of a standard deviation five years after the opening in both Math and Danish (the pooled effect over all post-opening years is 0.03).

The limited effect size on test scores is likely due to the effect being an intent-to-treat effect and may also explain why we see an even more limited effect on high school enrollment and enrollment in any youth education (high school or vocational training) in panel c) and d). The latter effects are only significant on a 10% significance level. Appendix Figure A.2a-b reveals that the positive effects on exam scores are driven by students residing in urban districts and that for these students, there is also a significant and positive effect on high school enrollment after four years. No significant effects are found for students residing in rural areas, where many private schools replaced closed public schools.

Since we are comparing different cohorts, the effects could also be driven by changes in cohort composition, for instance because a private school opening attracts new families to an area. This is assumed away by the exclusion restriction. We test this indirectly in Appendix Figure A.3, that shows no significant changes in student cohort composition nor in student mobility across parishes or local public schools, before or after the private school opening.

B.2 Sensitivity to pre-treatment parallel trend testing

We test whether the event-study estimates are biased due to pre-testing for parallel trends using the methodology in Rambachan and Roth (2023). We focus on the results 4 periods after the opening, which is the period where we find the largest significant effects. Table A.9 shows that when the post-treatment period difference is 0.2 times the pre-treatment (in absence of treatment), there is still a significant effect on a 10%-level, whereas it becomes insignificant if the post-treatment difference is larger than that.

Again, the effect is more robust in urban areas. Here, the effect on 9th grade exam test scores is still significant when post-treatment differences are as 0.6 times pre-treatment differences. Nevertheless, the results show that relatively small deviances from parallel trends assumption leave the effects insignificant. Caution should therefore be taken when interpreting these effects.

VI. Discussion

In this study, we find that the surge in private school attendance in Denmark is partly driven by the increasing share of students with parents from high socio-economic backgrounds. We further provide evidence of a robust, albeit relatively modest, association between private school enrollment rates and the segregation of disadvantaged students in Denmark. While students in private schools perform substantially better than those in public schools, we show that this difference is largely explained by positive selection into private schools.

We employ complementary empirical approaches to examine the effects on both segregation and student achievement. To analyze segregation, we use both a fixed effects model and an instrumental variables approach. The fixed effects model exploits variation across municipalities, grade levels, and time, thereby accounting for spurious correlations that may arise, for example, if private schools systematically locate in particular areas, if trends in housing prices jointly affect segregation and private school attendance, or if municipalities organize schooling differently across grade levels. The instrumental variables approach, in contrast, leverages variation in private school attendance induced by school openings triggered by national school reforms (cf. Section IV.C), and is better suited to address reverse causality, for instance arising from "white flight." The two strategies are thus complementary, and both support the conclusion that higher private school attendance increases social and ethnic segregation.

The fixed effects estimates suggest that the 6 percentage point increase in private school enrollment observed between 2007 and 2021 lead, for instance, to a 5.4 percent increase in school segregation among children whose parents have low levels of educational attainment. These estimates are close in magnitude to those found for ethnic segregation in relation to charter school expansion in the United States (Monarrez et al. 2022) and are robust across a wide range of sensitivity analyses. It is therefore noteworthy that the relationship between school choice expansion and segregation appears similar across very different institutional contexts. While this finding may be surprising, it is consistent with evidence documenting substantial socio-economic sorting across neighborhoods and school districts even within the relatively egalitarian Danish setting (Landersø and Heckman 2016; Gul et al. 2021; Eshaghnia, Heckman and Razavi 2023; Gandil and Bjerre-Nielsen 2024).

To examine the effects of private school attendance on student outcomes, we first implement a mover design in which we compare students who transfer from a public school to either a private school or another public school. The analysis controls for parental background, prior achievement, non-cognitive skills, school satisfaction and both pre- and post-transfer peer group characteristics. Controlling for these variables reduces private-public achievement gaps to around a third of the observed difference and around a quarter to a third of the controlled differences are due to better peer compositions in private schools. The difference in the 9th grade math exam score is, for instance, reduced from 0.33 to 0.09 of a standard deviation. As this is a selection-on-observables strategy, it likely provides upper bounds on the causal effects of attending a private school. We therefore complement this approach with an event study design that exploits variation in exposure to private school openings, thereby accounting for unobserved selection into areas with greater private school availability under a parallel trends assumption. While standard tests do not reject the parallel trends assumption, the estimated effects are sensitive to pre-trend specifications. Despite these limitations, the two approaches yield consistent findings: small positive effects on exam performance

and no significant effects on overall educational enrollment. This suggests that most of the observed differences between public and private school students can be attributed to selection.

Our findings are therefore more closely aligned with the broader literature documenting limited average effects of expanded school choice and private schooling (see, e.g., the review in Epple, Romano and Urquiola 2017), rather than with studies reporting large positive achievement effects for certain U.S. charter schools and U.K. academies that adopt the "No Excuses" pedagogical approaches (e.g., Angrist et al. 2010, 2013; Abdulkadiroğlu et al. 2011; Bertoni et al. 2025).

The relatively small effects of Danish private schools may partly reflect the substantial heterogeneity among these schools, which differ widely in pedagogical aims and practices, as well as the generally high quality of the Danish public school system. That said, the estimated achievement gains - on the order of up to 0.1 standard deviations in mathematics test scores - are comparable in magnitude to those found for widely studied and much costlier educational interventions such as reductions in class size (which often yield larger effects in Scandinavian contexts; Opratny et al. 2025) and the introduction of assistant teacher programs (e.g., Andersen et al. 2020).

Moreover, while the estimated private school "effect" (from table 4 when all controls have been added) at face value is modest, the results also point to a non-negligible role of peer effects. This is important when discussing the "total" effect of private schools that includes results from sorting. Positive selection into private schools not only benefits the students who attend them but may also harm students remaining in public schools by increasing the likelihood of being exposed to more disruptive peers (Carrell et al. 2015; Kristoffersen et al. 2015; Horoi and Ost 2018). Although our evidence on peer effects is descriptive, it is also plausible that access to higher-achieving peer groups is an important factor in the decision of well-educated parents to opt for private schooling, as has been shown elsewhere (e.g., Burgess et al. 2015). The peer effects are,

however, not likely to matter for overall efficiency, as our results indicate small differences between lower and higher performing students.

In terms of policy implications, the modest achievement effects suggest that efficiency gains from further private school expansions are limited. Desegregation policies - such as requiring private schools to enroll larger shares of disadvantaged students - may reduce achievement gaps across socio-economic and ethnic groups by providing access to (slightly) better schools with better peer composition for disadvantaged groups. However, given the limited heterogeneity in private school effects, such policies are unlikely to generate substantial overall efficiency gains. Their primary justification therefore lies in promoting reduced socio-economic outcome differences, social mixing, equal opportunity, and a more balanced distribution of disadvantaged students across schools at relatively low (financial) costs. At the same time, these policies may face other costs in terms of public and political resistance since they could reduce the attractiveness of private schools as an exit option for dissatisfied middle-class families. This highlights a fundamental policy trade-off: tighter regulation of private schools may reduce segregation and narrow achievement gaps, but it may also weaken an institutional "safety valve" that helps contain middle-class dissatisfaction with public education.

Acknowledgements

We thank Lise Kirstine Nørgaard who provided excellent research assistance during main parts of the project. We also thank seminar participants at seminars at the Danish Centre for Social Science Research (VIVE), the Institute for Evaluation of Labour market and Education policy (IFAU), the Rockwool Foundation, and the Copenhagen Education Network for constructive comments.

REFERENCES

Abdulkadiroglu, A., J. D. Angrist, S. M. Dynarski, T. J. Kane, and P. A. Pathak (2011). "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots". *Quarterly Journal of Economics* 126: 699–748.

Abdulkadiroglu, A., J. D. Angrist, Y. Narita, and P. A. Pathak (2017). "Research Design Meets Market Design: Using Centralized Assignment for Impact Evaluation". *Econometrica* 85; 1373–1432.

Abdulkadiroglu, A., Pathak, P. A. and C. R. Walters (2018). "Free to Choose: Can School Choice Reduce Student Achievement?" *American Economic Journal: Applied Economics* 10: 175–206.

Act no. 409 (2013). Lov nr. 409 af 26/04/2013. Lov om forlængelse og fornyelse af kollektive overenskomster og aftaler for visse grupper af ansatte på det offentlige område (Decree on extension and renewal of collective agreements for specific groups of workers in the public sector). Ministry of Employment.

Allen, R. and A. Vignoles (2016). "Can school competition improve standards? The case of faith schools in England". *Empirical Economics* 50: 959–973

Andersen, S. C., Beuchert, L., Nielsen, H. S. and M. K. Thomsen (2020). "The Effect of Teacher's Aides in the Classroom: Evidence from a Randomized Trial". *Journal of the European Economic Association* 18(1): 469–505.

Andrews, I., Roth, J. and A. Pakes (2023). "Inference for Linear Conditional Moment Inequalities". *The Review of Economic Studies* 90(6): 2763–2791

Angrist, J. D. and G. Imbens (1995). „Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity". *Journal of the American Statistical Association* 90(430): 431-442.

Angrist, Joshua D., Dynarski, S. M., Kane, T. J. Pathak, P. A. and C. R. Walters (2010). "Inputs and Impacts in Charter Schools: KIPP Lynn". *American Economic Review: Papers & Proceedings* 100: 239–243

Angrist, J. D., Pathak, P. A. and C. R. Walters (2013). “Explaining Charter School Effectiveness.” *American Economic Journal: Applied Economics* 5 (4): 1–27.

Bertoni, M., G. Heller-Sahlgren and O. Silva (2025). ”Free to improve? The impact of free school attendance in England”. *Economics of Education Review* 109, 102717.

Bifulco, R., Ladd, H. F. and S. L. Ross (2009). “Public school choice and integration evidence from Durham, North Carolina”. *Social Science Research* 38(1): 71-85.

Billings, S. B., Deming, D. J. and J. Rockoff (2014). “School Segregation, Educational Attainment, and Crime: Evidence from the End of Busing in Charlotte-Mecklenburg”. *The Quarterly Journal of Economics* 1291: 435–476.

Blinder, A. (1973). “Wage discrimination: Reduced form and structural estimates”. *Journal of Human Resources* 8(4):436–455.

Boeskens, L. (2016). “Regulating Publicly Funded Private Schools: A Literature Review on Equity and Effectiveness”, OECD Education Working Papers, No. 147, OECD Publishing, Paris.

Burgess, Simon, Ellen Greaves, Anna Vignoles and Deborah Wilson (2011). “Parental choice of primary school in England: What Types of School do Different Types of Family Really Have Available to Them?” *Policy Studies* 325: 521-547.

Burgess, Simon, Ellen Greaves, Anna Vignoles and Deborah Wilson (2014). “What Parents Want: School Preferences and School Choice”. *The Economic Journal* 125587: 1261-1289.

Bækgaard, M. (2010). ”Skolelukninger i Kommunalreformens Skygge? En Analyse af Kommunalreformens Policy-konsekvenser”. *Politik* 13(3). https://doi.org/10.7146/politik.v13i3.27456

Böhlmark, A., Holmlund, H. and M. Lindahl (2016). “Parental Choice, Neighbourhood segregation or cream skimming? An analysis of school segregation after a generalized choice reform”. *Journal of Population Economics* 294: 1155-1190.

Callaway, B. and P. C. Sant'Anna (2021). "Difference-in-Differences with Multiple Time Periods". *Journal of Econometrics* 225: 200-230.

Card, David and Jesse Rothstein (2007). "Racial Segregation and the Black-White Test Score Gap". *Journal of Public Economics* 91(11–12): 2158-2184.

Carrell, S. E., Hoekstra, M. and E. Kuka (2018). The Long-Run Effects of Disruptive Peers". *The American Economic Review* 108(11): 3377-3415.

Chingos, M. and P. Peterson (2015). "Experimentally estimated impacts of school vouchers on college enrollment and degree attainment". *Journal of Public Economics* 122: 1-12

Clark, D. (2010). "Selective Schools and Academic Achievement". *B.E. Journal of Economic Analysis and Policy*, 10(1) (Advances).

Costa, P. X. and R. R. McCrae (1985). "NEO Personality Inventory (NEO-PI)", APA PsycTests. https://doi.org/10.1037/t07564-000

Cullen, J. B., B. A. Jacobs and S. D. Levitt (2006). "The impact of school choice on student outcomes: An Analysis of the Chicago Public Schools". *Journal of Public Economics* 89: 729-760.

Cullen, J., Jacob, B. and Levitt, S. (2006). "The Effect of School Choice on Participants: Evidence from Randomized Lotteries". *Econometrica* 74(5): 1191-1230.

Cohodes, S. R., E. M. Setren, and C. R. Walters (2021). "Can Successful Schools Replicate? Scaling Up Boston's Charter School Sector". *American Economic Journal: Economic Policy* 13(1): 138–167

Egalite, A. J., and P. J. Wolf. (2016). "A Review of the Empirical Research on Private School Choice". *Peabody Journal of Education* 91 (4): 441–54.

Eshaghnia, S. M., Heckman, J. J. and G. Razavi (2023). "Pricing Neighborhoods", IZA Discussion Paper No. 16234.

Epple, D., Romano, R. E. and M. Urquiola (2017). "School Vouchers: A Survey of the Economics Literature". *Journal of Economic Literature* 55 (2): 441–92.

Eyles, A. and S. Machin (2019). "The Introduction of Academy Schools to England's Education". *Journal of the European Economic Association* 17(4): 1107–1146.

Frankel, D. M. and O. Volij (2011). "Measuring school segregation". *Journal of Economic Theory* 1461: 1-38.

Gandil, M. and A. Bjerre-Nielsen (2024). "Defying Attendance Boundary Policies and the Limits to Combating School Segregation". *American Economic Journal: Economic Policy* 16(1): 190-227.

Gill, Brian, P. Mike Timpane, Karen E. Ross, Dominic J. Brewer and Kevin Booker (2007). "Rhetoric versus reality. What We Know and What We Need to Know About Vouchers and Charter Schools". *RAND Education.*

Gul, T. S., Hvidman, U. and H. H. Sivertsen (2021). "Quasi-Market Competition in Public Service Provision: User Sorting and Cream-Skimming", *Journal of Public Administration Research and Theory*, 740–755, doi:10.1093/jopart/muab002.

Hanushek, E. A., Kain, J. F., Markman, J. M. and S. G. Rivkin (2003). "Does Peer Ability Affect Achievement?" *Journal of Applied Econometrics* 18: 527-544.

Hanushek, E. A., Kain, J. F. and S. G. Rivkin (2009). "New Evidence about Brown v. Board of Education: The Complex Effects of School Racial Composition on Achievement". *Journal of Labor Economics* 27 3: 349-383.

Horoi, I. and B. Ost (2015). "Disruptive peers and the estimation of teacher value added". *Economics of Education Review* 49: 180192.

Houmark, M. A., Jørgensen, C. M. L., Kristensen, I. L. and M. Gensowski (2024). "Effects of extending paid parental leave on children's socio-emotional skills and well-being in adolescence". *Economics of Education Review* 170, doi.org/10.1016/j.euroecorev.2024.104880

Irmert, N., Bietenbeck, J., Mattison, L. and F. Weinhardt (2023). "Autonomous Schools, Achievement, and Segregation". Cesifo Working Paper No. 10831.

James, D. R. and K. E. Taeuber (1985). “Measures of Segregation”. *Sociological Methodology* 15: 1-32.

Jensen, V. M., B. Bjørnholt, M. F. Mikkelsen, C. P. Nielsen & E. Ladekjær (2020). ”Den længere og mere varierede skoledag – En analyse af reformens elementer”. Copenhagen: VIVE.

Kristoffersen, J. H. G., Krægpøth, M. V., Nielsen, H. S. and M. Simonsen (2015). “Disruptive school peers and student outcomes”. *Economics of Education Review* 45: 1-13.

Landersø, R. and J. J. Heckman (2016). “The Scandinavian Fantasy: The Sources of Intergenerational Mobility in Denmark and the U.S”. *Scandinavian Journal of Economics* 119 (1), 178–230.

Lavy, V., Silva, O. and F. Weinhardt (2012). “The Good, the Bad, and the Average: Evidence on Ability Peer Effects in Schools”. *Journal of Labor Economics* 30 2: 367-414.

Marcotte, D. E., and K. Dalane (2019). “Socioeconomic Segregation and School Choice in American Public Schools”. *Educational Researcher* 488: 493-503.

Massey, D. S., and N. A. Denton (1988). “The Dimensions of Residential Segregation”. *Social Forces* 67: 281-315.

Monarezz, Tomás, Brian Kisida and Matthew Chingos (2022). “The Effect of Charter Schools on School Segregation”. *The American Economic Journal: Economic Policy* 141: 301–340.

Neri, L. and E. Pasini (2023). “Heterogeneous effects of school autonomy in England”. *Economics of Education Review* 94, https://doi.org/10.1016/j.econedurev.2023.102366

Nielsen, C. P. & B. S. Rangvid (2016). “Inklusion i folkeskolen. Sammenfatning af resultaterne fra inklusionspanelet”. Copenhagen: VIVE.

Nielsen, C. P., V. M. Jensen, M. G. Kjer, & K. M. Arendt (2020). Elevernes læring, trivsel og oplevelser af undervisningen i folkeskolen – En evaluering af udviklingen i reformårene 2014-2018. Copenhagen: VIVE.

OECD (2019). "Balancing School Choice and Equity. An international perspective based on PISA". PISA, OECD Publishing, Paris. https://doi.org/10.1787/2592c974-en.

Oaxaca, R. (1973). "Male-female wage differentials in urban labor markets". *International Economic Review* 14(3):693–709.

Opatrny, M. Havranek, T., Irsova, Z. and M. Scasny (forthcoming). "Publication Bias and Model Uncertainty in Measuring the Effect of Class Size on Achievement", *Journal of Labor Economics*.

Owens, Ann, Sean F. Reardon and Christopher Jencks (2016). "Income Segregation between Schools and School Districts". American Educational Research Journal 53 (4), https://doi.org/10.3102/0002831216652722

Rangvid, B. (2010). "School Choice, Universal Vouchers and Native Flight from Local Schools". *European Sociological Review* 26(3): 319–335.

Rambachan, A. and J. Roth (2023). A More Credible Approach to Parallel Trends. *Review of Economic Studies* 2555-2591.

Reber, S. J. (2010). "School Desegregation and Educational Attainment for Blacks". *The Journal of Human Resources* 45 4: 893-914.

Ritter, G. W., Jensen, N. C., Kisida, B. and D. H. Bowen (2014). "Urban School Choice and Integration: The Effect of Charter Schools in Little Rock." *Education and Urban Society* 48 (6): 535–55

Rogne, A. F., Borgen, S. T., and E. I. Nordrum (2021). "School segregation and native flight. Evidence from school catchment area borders". Working paper. SocArXiv. DOI: https://doi.org/10.31235/osf.io/xykdg

Roth, J., Sant'Anna, P. H. C., Bilinski, A. and J. Poe (2023). What's trending in difference-in-differences? A synthesis of recent econometrics literature. *Journal of Econometrics* 235(2): 2218-2244.

Sacerdote, B. (2011). "Peer Effects in Education: How Might They Work, How Big Are They and How Much Do We Know Thus Far?", Chapter 4 in Erik Hanushek & Stephen Machin & Ludger Woessmann (Eds.): *Handbook of the Economics of Education*, volume 3: 249-277, Elsevier Science.

Setren, E. (2019). "Targeted vs. General Education Investments: Evidence from Special Education and English Language Learners in Boston Charter Schools". *Journal of Human Resources* 59(S). https://doi.org/10.3368/jhr.56.4.0219-10040R2

Skov, P. R. and L. Hønge Flarup (2020). "De nationale tests samvariation med karakterer – Delrapport 2: Evalueringen af de nationale test". Copenhagen: VIVE.

Sun, L. and S. Abraham (2021). Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects. *Journal of Econometrics* 225: 175-199

Urquiola, M. (2016). "Competition among Schools: Traditional Public and Private Schools". In *Handbook of the Economics of Education*, edited by Eric A. Hanushek, Stephen Machin, and Ludger Woessmann, pp. 209–37. Amsterdam: Elsevier

Wolfe, P., Kisida, B., Gutmann, B., Puma, M., Eissa, N., Rizzo, L. (2013). "School vouchers and student outcomes: experimental evidence from Washington DC". *Journal of Policy Analysis & Management* 32 (2), 246–270.

APPENDIX A. Additional figures and tables.

Table A.1. Relationship between private school share and segregation by alternative student dimensions.

	Outcome: School segregation		
	Lowest income decile	Highest income decile	Non-Western background
	(1)	(2)	(3)
Private school share (uncontrolled)	0.218***	0.317***	0.147***
	(0.011)	(0.013)	(0.013)
Private school share	0.259***	0.030	0.186***
	(0.049)	(0.057)	(0.056)
Baseline mean	0.329	0.337	0.421
Elasticity	13.5%	1.5%	7.6%
Pairwise area, grade and time FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Local residential segregation	Yes	Yes	Yes
Observations	14700	14700	14700

Notes: Results from population weighted regressions of local segregation on private school share, measured by school municipality. The groups are described in section III. Fixed effects include grade-by-time, grade-by-area and time-by-area fixed effects. Controls include number of schools, number of students, and the size of the minority groups. Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2. Relationship between segregation private school shares with alternative segregation measures and areas of aggregation.

	Outcome: School segregation				
	Different segregation index			Different area†	
	Theil	D-index	Variance ratio	Postal district	Merged areas
Segregation by:	(1)	(2)	(3)	(4)	(5)
Parent's low educational attainment	0.044***	0.290***	0.080***	0.312***	0.430***
	(0.009)	(0.047)	(0.018)	(0.038)	(0.074)
Mean segregation level	0.036	0.361	0.052	0.389	0.389
Parent's high educational attainment	0.046***	0.137***	0.058***	0.138***	0.133
	(0.014)	(0.045)	(0.022)	(0.037)	(0.085)
Mean segregation level	0.044	0.279	0.060	0.261	0.261
Pairwise FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Local residential segregation	Yes	Yes	Yes	No	Yes
Observations	14700	14700	14700	77990	6900

Notes: Results from population weighted regressions of local segregation on private school shares. See table 2 for definitions of outcomes. The size of the effects cannot be compared because the indices have different scales, but the effects are of a similar size as for the Gini index when measured relative to the mean segregation level (reported in the row below the estimated coefficient). The elasticities vary from 14% to 27% for segregation of students whose parents have low educational attainment. The analysis based on different areas of aggregation is discussed in section A.1. Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. †Results for the different areas have N = 6440 in (4) and N=4600 in (5).

Table A.3. Relationship between the instrument in table 3 (lagged private school shares) and local characteristics.

	Dependent: Enrollment in new private school in grade:							
	1	2	3	4	5	6	7	8
Lagged segregation	-0.007	0.007	0.001	-0.006	0.006	-0.004	0.003**	-0.001
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)
Number of schools	-0.000	-0.000	0.000	0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ln(population size)	0.009	-0.009	0.005	-0.006	-0.002	0.011	0.001	0.001
	(0.007)	(0.008)	(0.007)	(0.006)	(0.007)	(0.007)	(0.003)	(0.003)
Fractions:								
Low education	-0.043	-0.062	-0.033	-0.026	0.027	-0.031	-0.010	-0.006
	(0.035)	(0.040)	(0.031)	(0.038)	(0.045)	(0.034)	(0.014)	(0.017)
High education	-0.005	0.038*	-0.010	0.034**	-0.012	-0.004	0.009	-0.003
	(0.013)	(0.021)	(0.014)	(0.016)	(0.023)	(0.017)	(0.010)	(0.004)
Low income	0.018	0.017	0.001	0.047	0.010	0.011	0.015	0.012
	(0.026)	(0.018)	(0.019)	(0.032)	(0.033)	(0.028)	(0.015)	(0.012)
High income	-0.012	-0.029	0.041	-0.000	-0.013	-0.017	0.015	-0.016
	(0.032)	(0.018)	(0.035)	(0.017)	(0.020)	(0.018)	(0.011)	(0.010)
Mental diagnosis	-0.010	0.055	0.029	0.003	-0.011	0.004	0.015	-0.004
	(0.044)	(0.034)	(0.071)	(0.029)	(0.032)	(0.024)	(0.016)	(0.010)

Non-Western	0.006	-0.014	0.008	-0.021	-0.052	0.020	0.004	-0.005
	(0.020)	(0.023)	(0.021)	(0.020)	(0.034)	(0.017)	(0.018)	(0.009)
F-value joint significance (ex. FE)	0.989	1.896	0.954	1.355	1.421	0.804	1.252	0.417
Time and municipality fixed effects	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Results from population weighted regressions of the instrument (lagged local private school shares) on local mean characteristics in the same year and local segregation two years prior to the opening (students of low educated parents, measured by the Gini coefficient). Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4. Relationship between local private school shares and local segregation of students by parental education level.

	Segregation of student group:				
	Low education	High education	Low income	High income	Non-Western
	(1)	(2)	(3)	(4)	(5)
	Urban municipalities				
FE	0.331***	0.004	0.243**	-0.009	0.155*
	(0.117)	(0.072)	(0.107)	(0.093)	(0.087)
IV	0.955	-1.169**	0.111	-0.213	0.632
	(0.858)	(0.533)	(0.464)	(0.707)	(0.776)
First-stage:	0.437***	F-value:	13.26		
	(0.120)				
	Rural municipalities				
FE	0.330***	0.193***	0.266***	0.035	0.156**
	(0.061)	(0.065)	(0.057)	(0.075)	(0.068)
IV	0.812	0.815	0.684	0.083	1.892***
	(0.666)	(0.547)	(0.638)	(1.074)	(0.519)
First-stage:	0.173***	F-value:	29.23		
	(0.032)				

Notes: Results from instrumental variables estimations of population weighted regressions of local segregation on private school share, measured by school municipality (model 1 described above). Local segregation is measured by the Gini index for segregation of students with different parental characteristics: Low education is a dummy taking value one if neither parent has educational attainment beyond compulsory schooling. Parent's high educational attainment is a dummy taking value one if one parent has a master's degree. All models control for the following fixed effects: grade-by-time, grade-by-area and time-by-area fixed effects. Controls include number of schools, number of students, and the size of the group (students whose parents have low or high educational attainment). Rural municipalities are those where the largest city has less than 30.000 inhabitants. Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5. Mean non-cognitive scores prior to school transfer, by years prior to moving

	Periods prior to move	Danish test	Conscien-tuousness	Agree-ableness	Emotional stability	Subjective achievement	School satisfaction
Move to public school	1	0.122	0.074	0.051	0.040	0.040	-0.011
	2	0.102	0.060	0.061	0.062	0.066	0.030
Move to private school	1	0.312	0.110	0.064	-0.189	0.057	-0.424
	2	0.286	0.161	0.079	-0.010	0.157	-0.049

Notes: The five scores are standardized and formed from compulsory surveys in public schools, see section III. A higher score denotes better outcomes. The periods prior to the school transfer vary for the Danish test score, since it is only taken every second year.

Table A.6. OLS estimate of private versus public school differences in student outcomes, Different sets of controls.

	Effect of private school attendance:			
Student outcomes:	(1)	(2)	(3)	(4)
9th grade Danish exam GPA	0.133***	0.230***	0.135***	0.131***
	(0.014)	(0.015)	(0.015)	(0.011)
9th grade Math exam GPA	0.186***	0.266***	0.161***	0.201***
	(0.014)	(0.014)	(0.015)	(0.012)
Enrollment in high school	0.062***	0.086***	0.055***	0.065***
	(0.006)	(0.007)	(0.007)	(0.006)
Vocational training	-0.051***	-0.070***	-0.048***	-0.053***
	(0.006)	(0.006)	(0.006)	(0.006)
Observations	22893	22893	22893	22893
Background characteristics (individual)	YES	NO	NO	NO
Peers, pre-move	NO	YES	NO	NO
Peers, post-move	NO	NO	YES	NO
Past achievement	NO	NO	NO	YES

Notes: Results from regressions of student outcomes on a dummy for private school enrollment for movers to private schools relative to movers to public schools. All models control for calendar year, municipal dummies and the grade at which the school move was made (grade 4-8), pre-move national tests, personality traits and well-being, and condition on non-missing on all controls. Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.7. OLS estimate of private versus public school differences in student outcomes, Heterogeneity: Below average performing groups

Student group:	Danish (1)	Math (2)	High school (3)	Vocational (4)	Any education (5)
All	0.029***	0.089***	0.023***	-0.023***	0.005
	(0.010)	(0.011)	(0.006)	(0.006)	(0.004)
Boys	0.047***	0.095***	0.022***	-0.019***	0.007
	(0.015)	(0.017)	(0.008)	(0.007)	(0.005)
Low income	0.047***	0.097***	0.040***	-0.034***	0.011***
	(0.014)	(0.015)	(0.008)	(0.007)	(0.004)
Parents not together	0.058***	0.091***	0.034***	-0.031***	0.009**
	(0.013)	(0.015)	(0.007)	(0.007)	(0.004)
Non-Western origin	0.046***	0.101***	0.031***	-0.029***	0.007*
	(0.012)	(0.013)	(0.007)	(0.006)	(0.004)
Low education	0.045**	0.097***	0.030***	-0.031***	0.004
	(0.013)	(0.015)	(0.008)	(0.008)	(0.004)
Background characteristics	YES	YES	YES	YES	YES
Past achievement / NC skills levels	YES	YES	YES	YES	YES
Post transfer peer group mean	YES	YES	YES	YES	YES

Notes: Results from regressions of student outcomes on a dummy for private school enrollment for movers to private schools relative to movers to public schools. All models control for calendar year, municipal dummies and the grade at which the school move was made (grade 4-8), pre-move national tests, personality traits and well-being, and condition on non-missing on all controls. Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8. OLS estimate of private versus public school differences in student outcomes, Heterogeneity: Above average performing groups.

Student outcomes:	Danish (1)	Math (2)	High school (3)	Vocational (4)	Any education (5)
All	0.029***	0.089***	0.023***	-0.023***	0.005
	(0.010)	(0.011)	(0.006)	(0.006)	(0.004)
Girl	0.023	0.101***	-0.012	-0.008	-0.018
	(0.030)	(0.0384)	(0.020)	(0.020)	(0.015)
High income	0.024	0.128***	0.023*	-0.025**	-0.000
	(0.022)	(0.025)	(0.013)	(0.012)	(0.008)
Parents together	0.023	0.152**	0.008	-0.007	0.002
	(0.066)	(0.073)	(0.032)	(0.030)	(0.020)
Western origin	0.017	0.101	0.015	-0.084	-0.041
	(0.090)	(0.096)	(0.053)	(0.057)	(0.044)
High education	0.001	0.098***	0.019**	-0.010	0.011*
	(0.024)	(0.027)	(0.010)	(0.008)	(0.006)
Parental and peer background	YES	YES	YES	YES	YES
Past achievement / NC skills	YES	YES	YES	YES	YES
Post transfer peer group mean	YES	YES	YES	YES	YES

Notes: Results from regressions of student outcomes on a dummy for private school enrollment for movers to private schools relative to movers to public schools. All models control for calendar year, municipal dummies and the grade at which the school move was made (grade 4-8), pre-move national tests, personality traits and well-being, and condition on non-missing on all controls. Robust standard errors in parentheses, clustered at the municipal level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

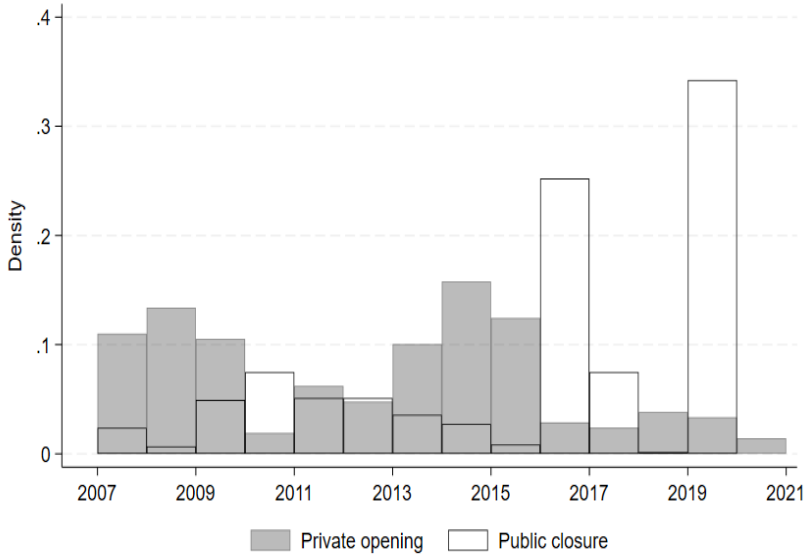
Table A.9. Lower bound for event-study estimate in the 4th period.

	Outcome			
90%-significance	Private school	9th grade GPA	High school enrollment	Vocational training
	(1)	(2)	(3)	(4)
M				
0.2	0.015	0.002	-0.016	-0.015
0.4	0.013	-0.002	-0.019	-0.017
0.6	0.011	-0.008	-0.023	-0.019
0.8	0.009	-0.013	-0.027	-0.022
1	0.006	-0.020	-0.032	-0.025
		Urban areas		
0.2	0.006	0.015	0.014	-0.018
0.4	0.003	0.01	0.012	-0.021
0.6	-0.001	0.004	0.009	-0.025
0.8	-0.005	-0.001	0.005	-0.029
1	-0.009	-0.009	0.000	-0.034
		Rural areas		
0.2	0.024	-0.056	-0.056	-0.028
0.4	0.023	-0.061	-0.06	-0.031
0.6	0.021	-0.067	-0.065	-0.035
0.8	0.018	-0.076	-0.072	-0.039
1	0.015	-0.086	-0.080	-0.043

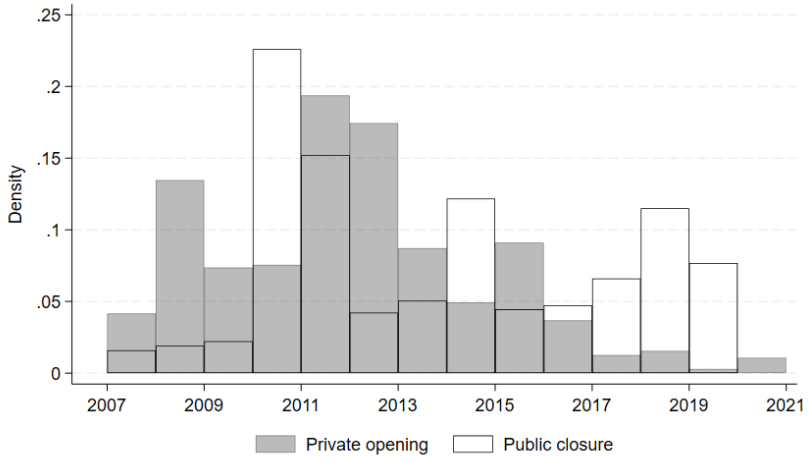
Notes: Obtained from the stata package honestdid (Rambanach and Roth 2023). M is the size of post-treatment effects as a proportion of estimated pre-treatment differences.

Figure A.1. Number of private school openings and public school closures over time, by urban and rural areas.

a) Urban areas

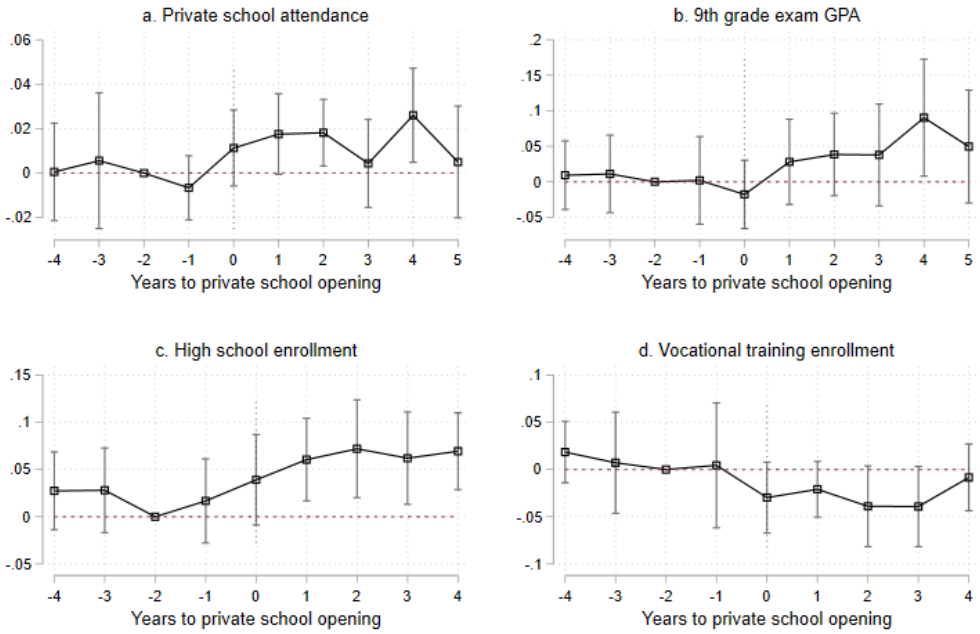


b) Rural areas



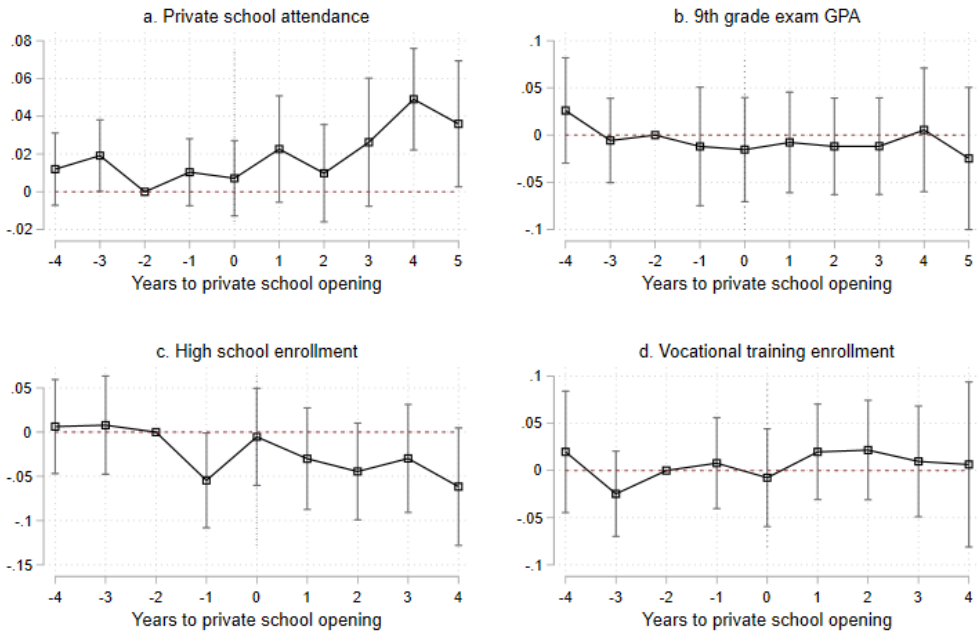
Notes: Number of private school openings and public school closures by calendar year. An opening year is defined as the first year, a closure the last year, when a school at a specific address has a positive number of students. Urban areas are municipalities where the largest city has at least 100.000 inhabitants.

Figure A.2a. Event study estimates of the effect of a private school opening on private school attendance and standardized student achievement in *urban* districts.



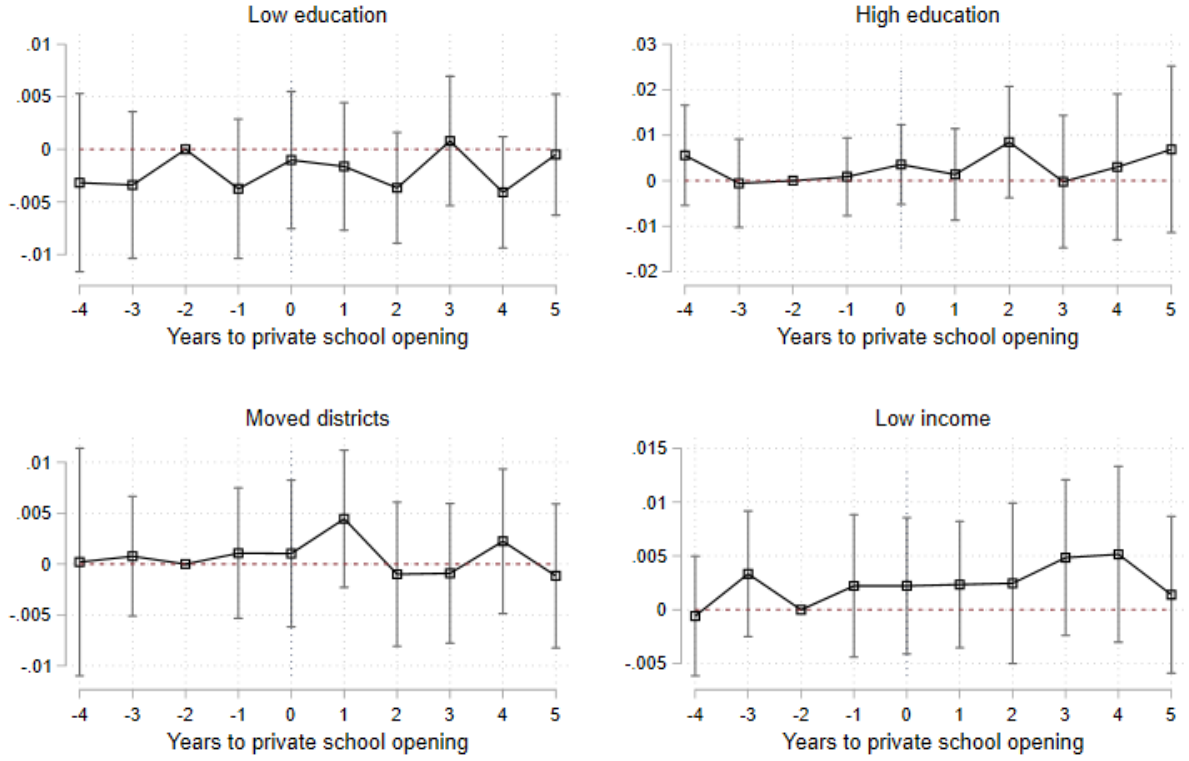
Notes: Estimates of the effect of private school openings in a postal district from event study model (2) before and after the first observed opening. The control group are students from similar cohorts in never treated districts. The "9th-grade exam GPA" is the standardized grade point average of the final national examination scores in Danish and Math. The figure shows the difference in cohort mean outcomes for students before and after the first observed private school opening in the district, compared with students in districts without a private school opening. Vertical bars are 95%-confidence intervals based on robust standard errors, clustered at the cohort times municipal level.

Figure A.2b. Event study estimates of the effect of a private school opening on private school attendance and standardized student achievement in *rural* districts.



Notes: Estimates of the effect of private school openings in a postal district from event study model (2) before and after the first observed opening. The control group are students from similar cohorts in never treated districts. The "9th-grade exam GPA" is the standardized grade point average of the final national examination scores in Danish and Math. The figure shows the difference in cohort mean outcomes for students before and after the first observed private school opening in the district, compared with students in districts without a private school opening. Vertical bars are 95%-confidence intervals based on robust standard errors, clustered at the cohort times municipal level.

Figure A.3. Event study of private school opening on student composition: Fraction of students who (a) has a father with low education, (b) has a father with a high education, (c.) moved school district and (d) has a father with low income.



Notes: Estimates of the effect of private school openings in a postal district from event study model (2) before and after the first observed opening. The control group are students from similar cohorts in never treated districts. The figure shows the difference in cohort mean outcomes for students before and after the first observed private school opening in the district, compared with students in districts without a private school opening. Vertical bars are 95%-confidence intervals based on robust standard errors, clustered at the cohort times municipal level.

APPENDIX B. An Oaxaca-Blinder decomposition of private school enrollment

We use an Oaxaca-Blinder decomposition to examine the determinants of the rise in private school enrollment over time. This is based on annual linear probability models of private school enrollment, explained by student parental and peer background characteristics, grade level, calendar year and municipality dummies. Many public schools closed during the period of analysis, and in rural areas, they were often replaced by private schools. The model therefore also includes a dummy variable for whether a public-school closure has occurred in previous periods in the municipality. Using the annual estimates of private school enrollment from the model:

$$\boldsymbol{P_{it} = \alpha_t + \beta_t X_{it} + \epsilon_{it}}$$

We can decompose the rise in private school enrollment into changes in OLS coefficients and student composition:

$$\boldsymbol{P_{it} - P_{is} = (\alpha_t - \alpha_s) + (\beta_t - \beta_s) X_{is} + (X_{it} - X_{is}) \beta_s}$$

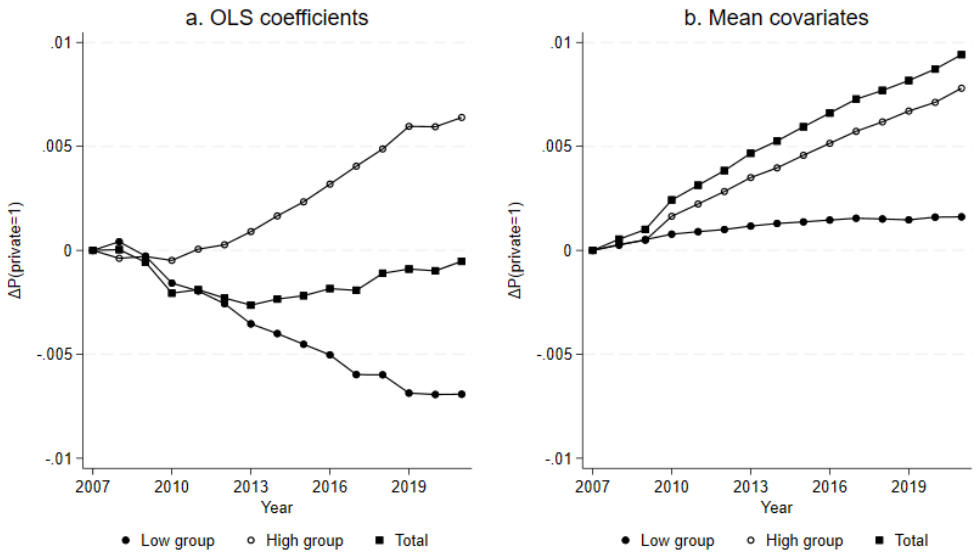
Where the last part is the "explained part", the compositional changes, and the first two parts contain the "unexplained part". The latter can be driven by changes in preferences for private school, in private school selection of students or other unobserved determinants of private schooling that correlate with socio-economic background. The OLS coefficients and mean covariates are shown in Appendix Table B.1 below. Figure B.1 shows each part corresponding to socio-economic variables. The low (high) socio-economic group is the sum of coefficients for parents being in the low (high) income or education groups.

Panel a) of Figure B.1 shows that while the OLS coefficients associated with the high socio-economic group have increased, they have decreased for the low socio-economic group (these changes are driven by both larger negative coefficients for low income and education groups, and larger positive coefficients for high income and education groups, cf. Table B.1). Each of these parts explain up to 0.7 percentage point of the increase in private school enrollment (0.007 on the y-axis). This means that, holding student

parental background fixed, the socio-economic gap in private school enrollment has increased by 1.4 percentage points. This is 23% of the total change from 2007 to 2021.

Panel b) shows the changes in private school enrollment due to changes in socio-economic composition (the sum of income and educational background). It shows that the increased fraction of students with parents in the high education group (+193%, see table B.1) and high income group (+30%) and the decreased fraction with low education (-31%) and increased fraction with low income (+30%) explain an increase in private school enrollment of around 1 percentage point, or 17% of the total increase over the period. This means that changes related to socio-economic groups explain roughly 40% of the total increase in private school enrollment.

Figure B.1. Oaxaca-Blinder decomposition: Predicted changes in private school enrollment due to OLS coefficients (a.) and covariates describing student composition (b.).



Notes: Results from the Oaxaca-Blinder decomposition (model 1). Panel a) presents the changes in the predicted probability of private school enrollment due to changes in annual OLS estimates with composition held constant at the 2007 level. Panel b) presents the changes in the predicted probability of private school enrollment due to changes in the annual composition of students with the OLS estimates held constant at the 2007 level. The model controls for grade level, age and gender, and whether parents are co-habiting, urban postal districts, past public school closures in the municipality, and municipality dummies. The low (high) group consists of students whose parents either have low (high) educational attainment or have low (high) income, see the data section for definitions.

Table B.1. Annual OLS regressions of private school enrollment on student characteristics and their means

	Constant	Age	Girl	Low income	High income	Parents together	Non-Western	Low education	High education	Public school closure
	OLS coefficient									
2007	0.248	-0.024	0.003	0.016	0.037	0.019	-0.033	-0.040	0.051	.
2008	0.304	-0.033	0.003	0.020	0.035	0.021	-0.029	-0.042	0.049	0.019
2009	0.302	-0.033	0.004	0.017	0.037	0.023	-0.020	-0.045	0.048	0.010
2010	0.304	-0.034	0.005	0.007	0.035	0.025	-0.012	-0.045	0.049	0.007
2011	0.266	-0.026	0.007	0.005	0.041	0.030	-0.009	-0.047	0.048	0.008
2012	0.288	-0.031	0.006	0.000	0.046	0.031	-0.005	-0.045	0.046	-0.019
2013	0.320	-0.035	0.006	-0.008	0.049	0.031	0.006	-0.044	0.048	-0.012
2014	0.280	-0.029	0.008	-0.011	0.053	0.033	0.001	-0.046	0.050	-0.007
2015	0.334	-0.035	0.007	-0.012	0.054	0.034	-0.001	-0.049	0.054	-0.013
2016	0.351	-0.038	0.007	-0.013	0.059	0.034	-0.015	-0.055	0.056	0.018
2017	0.343	-0.035	0.009	-0.019	0.065	0.032	-0.013	-0.056	0.058	-0.005
2018	0.382	-0.042	0.009	-0.019	0.069	0.032	-0.018	-0.058	0.061	0.001
2019	0.377	-0.040	0.008	-0.026	0.074	0.031	-0.018	-0.056	0.065	0.001
2020	0.382	-0.041	0.008	-0.025	0.070	0.030	-0.020	-0.059	0.068	0.013
2021	0.390	-0.043	0.010	-0.024	0.071	0.030	-0.021	-0.060	0.070	0.020

Table B.1, continued.

	Private	Age	Girl	Low income	High income	Parents together	Non-Western	Low education	High education	Public school closure
						Mean covariates				
2007	0.133	10.561	0.492	0.134	0.107	0.725	0.074	0.077	0.140	.
2008	0.137	10.584	0.492	0.142	0.105	0.723	0.075	0.074	0.146	0.255
2009	0.140	10.585	0.492	0.149	0.099	0.721	0.075	0.070	0.155	0.225
2010	0.144	10.574	0.491	0.158	0.120	0.720	0.075	0.066	0.163	0.195
2011	0.152	10.559	0.490	0.158	0.124	0.721	0.075	0.064	0.172	0.166
2012	0.156	10.553	0.490	0.158	0.127	0.720	0.075	0.061	0.181	0.071
2013	0.161	10.542	0.490	0.163	0.133	0.719	0.074	0.059	0.190	0.098
2014	0.165	10.535	0.489	0.167	0.133	0.719	0.075	0.057	0.200	0.242
2015	0.173	10.524	0.488	0.170	0.135	0.721	0.078	0.057	0.210	0.162
2016	0.178	10.519	0.488	0.175	0.137	0.724	0.081	0.056	0.220	0.110
2017	0.182	10.551	0.488	0.179	0.138	0.727	0.084	0.056	0.230	0.071
2018	0.184	10.598	0.488	0.175	0.136	0.728	0.087	0.055	0.241	0.108
2019	0.188	10.629	0.489	0.173	0.137	0.730	0.085	0.055	0.250	0.053
2020	0.191	10.649	0.489	0.179	0.135	0.732	0.088	0.054	0.260	0.156
2021	0.194	10.650	0.489	0.176	0.139	0.736	0.090	0.053	0.271	0.127

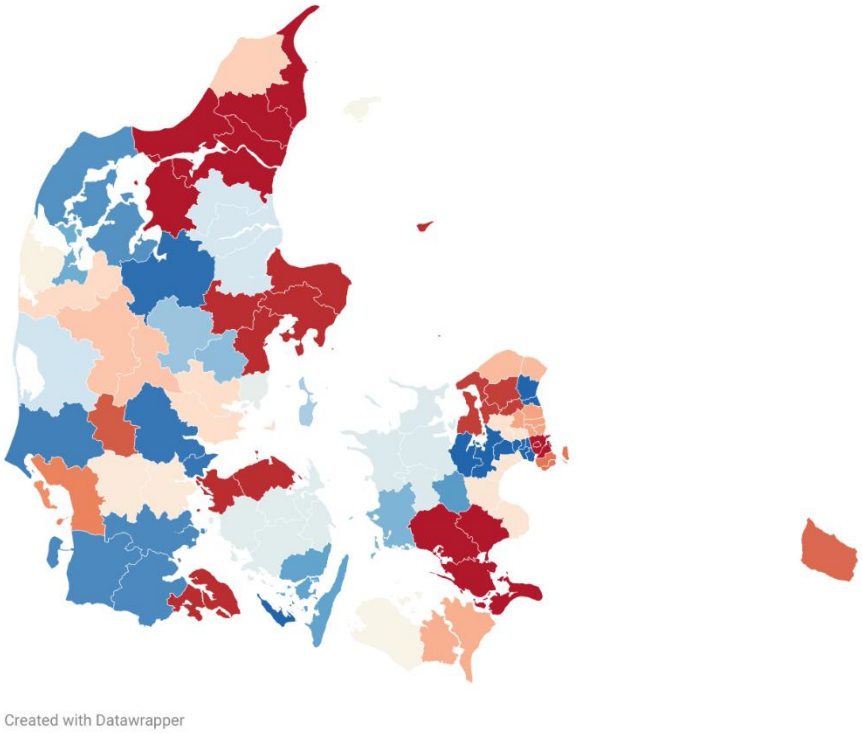
Notes: All regressions control for grade level and residential municipality. The first column contains the constant term from the regression in the first part of the table ("OLS coefficients") and the mean annual private school enrollment in the second part ("mean covariates").

APPENDIX C. Alternative area of aggregation

The main results are based on segregation of students across schools within municipalities, irrespective of where the student lives. However, many private schools enroll students from several municipalities. As an extreme example of how this can affect the estimates, consider a case where a private school opens in area A with higher educational attainment, and only enrolls students from families with higher educational backgrounds in the neighboring municipality B that has no private schools. Even though this traffic may leave public schools in municipality B more segregated, the private school student share in municipality B is unaffected (it is still zero), when recorded by their school location. By contrast, since the students from families with higher educational backgrounds from municipality B now go to school in area A where other students are also from homes with higher educational attainment, segregation across schools may not change in municipality A even though private school student shares have increased in municipality A. The total effect across the two municipalities is a higher private school student share and higher segregation between municipalities, but this is not captured with the current set-up.

We examine sensitivity towards such scenarios by considering the sensitivity towards the size of the area of aggregation over which segregation and private school shares are measured: We use larger areas with several municipalities, when the cross-border traffic constitutes more than 2% of the students in one of the municipalities. The set of merged municipalities is shown in Appendix Figure C.1. We also report results with smaller geographical areas (see Table A.2).

Figure C.1. Merged municipalities with a high fraction of cross-border students.



Notes: Geographically adjacent municipalities with the same color are merged into a common school area. Created with Data-wrapper ©.