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Educational Peer Effects with Student Mobility: Evidence from Moving Disabled Students to General Education Classrooms*

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Abstract

Prior work has argued that a student's peers affect the student's own academic outcomes, but has more rarely considered how student mobility shapes peer effects. We study this, focusing on the placement of disabled students into general education classrooms, a practice known as "mainstreaming". Using Danish administrative data and a difference-in-differences design, we show that the arrival of a mainstreamed student makes peers 11% more likely to switch schools in a given year. We isolate the direct impacts of the arrival of a mainstreamed student from the indirect impacts of school switching by studying a sizeable — although potentially selected — subgroup of students who never change schools. For this subgroup, we find little evidence that the arrival of a mainstreamed student causes negative effects on attendance, test scores, or social tensions in the class. In contrast, students who do change schools experience negative impacts, which may be due to direct effects from the arrival of a mainstreamed student or the indirect disruption caused by switching schools. Both reduced form and structural approaches suggest that the disruption of school switching is a plausible explanation. Research and policy on peer effects and the inclusion of disabled students in classrooms must account for the role of student mobility in shaping outcomes.

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1 Introduction

During their school years, children often interact closely with their peers, and a large body of literature has attempted to quantify the effects of these interactions. These effects, known as "peer effects", may be substantial factors affecting academic performance and wellbeing, especially for some students (Sacerdote 2011).

However, relatively little prior work quantifying peer effects on students' outcomes has addressed the role of student mobility—that is, students changing schools or classrooms—in shaping these effects and their measurement. In this paper, we show that accounting for how students change schools is crucial for a proper understanding of how a student's peers affect their academic achievement and socio-emotional wellbeing.

In particular, we focus on understanding the peer effects of the arrival of a disabled student in a classroom alongside their non-disabled peers, a practice known as mainstreaming. Debates about the impacts of peers on students' outcomes have been important for policymakers deciding whether to include disabled "special education" students in classrooms alongside non-disabled students.[1] In this paper, we highlight a previously understudied element shaping peer effects when disabled students join a classroom: peers' school choice in response to classroom composition. We argue that ignoring this can lead to misleading conclusions about the peer effects of mainstreaming.

Countries vary substantially in their approaches to mainstreaming, that is, in educating disabled students in classrooms alongside their non-disabled peers rather than in segregated "special education" classes. The share of disabled students educated in mainstream environments ranges from over 90 percent in countries like Norway to as low as 10 percent in countries like Denmark.[2] Proponents of mainstreaming argue that it can have positive effects for both disabled and non-disabled students: relative to providing support for disabled students in separate environments, mainstreaming is often less expensive per student and may make funds available for other students. Mainstreaming might also reduce the stigma

[1] In this paper, we adopt the term "disabled" to refer to students who have an impairment and need additional support services in order to participate in education. Although education legislation in Denmark uses a term that more closely translates to "special needs" (see Ringø and Falster (2025)), we use "disabled" to align with the language preferred by the Disabled People's Organizations Denmark (DPOD 2025) as well as the US-based National Center on Disability and Journalism and the Associated Press Stylebook (NCDJ 2021; Associated Press 2025). We use "non-disabled" to refer to other students.

[2] While many countries have similar levels of students with identified disabilities, around 7 percent, they differ in what share of disabled students in primary and lower secondary education are educated in mainstream classrooms alongside their non-disabled peers. In Denmark, around 10 percent of disabled students are taught in mainstream classes, whereas 51 and 91.2 percent of disabled students in Germany and Norway receive their education in mainstream classes (Special Needs and Inclusive Education 2018; European Agency for Special Needs and Inclusive Education 2023). In the United States, about 67 percent of students requiring additional support are educated in mainstream settings (NCES 2024).

of disability and increase opportunities for peer interactions. On the other hand, opponents of mainstreaming contend that disabled students may negatively impact their non-disabled peers when entering the classroom by requiring teacher attention or causing disruptions.

In this paper, we study the dynamic effects of mainstreaming disabled students on their peers, focusing on the important role of school switching. School switching responses to mainstreaming can have important implications for estimating the peer impacts of mainstreaming for at least three reasons. First, prior work shows that switching schools can have important impacts on a student's educational achievement (e.g., Schwartz, Stiefel, and Cordes (2017)), meaning that naive estimates of mainstreaming effects comparing students who do and do not get a mainstreamed classroom peer may include both direct effects of the arrival of a mainstreamed student and indirect effects caused by switching schools away from that student. Second, school switching may cause attrition from the sample when outcomes are not observed in the new school, e.g., in test-optional private schools. In this case, estimated effects will only reflect outcomes for observed stayers. Finally, given the high degree of interaction between peers in a classroom, school switching could create unintended peer effects by disrupting the education of other students in both the classrooms they leave and the ones they join, as argued by Gibbons and Telhaj (2011).

To estimate dynamic causal impacts of mainstreaming on learning and wellbeing, we use comprehensive Danish registry data and a difference-in-differences design. The Danish setting and administrative data offer a unique opportunity to study student mobility because they allow us to follow students and the classrooms they attend over time and across schools and observe whether students receive support for disabilities. We observe a student's school and classroom over time from 2011–2020, across mandatory grade levels 0–9, for both public and private schools. We also observe whether any individual student has received educational support for a disability. Since we have both classroom identifiers and data on disability, we can identify exposure to mainstreaming on a detailed level by observing when a disabled student moves from a segregated class into a mainstream class. We construct a difference-in-differences design comparing the trajectories of students in classes which receive a mainstreamed student ("exposed" students) and never-exposed students to examine the impacts of mainstreaming and how they depend on grade level and years since exposure.

Our findings indicate that the arrival of a mainstreamed disabled student leads their new peers to be substantially more likely to switch schools unexpectedly, that is, for reasons other than reaching the maximum grade in the school or simultaneously moving home addresses. After exposure to a mainstreamed student, non-disabled students' unexpected school switching increases by 11 percent per year. This means that, by the time they reach the end of mandatory education in grade 9, students exposed to the arrival of a mainstreamed student

are 3 percentage points more likely to have made this kind of unexpected school switch. We also find increases in classroom switching within a school, although this is much more rare in the Danish context in general.

Further, school switching caused by a mainstreamed student's arrival appears to be selective. Switching is more common for students whose parents have above-median incomes and students switch almost exclusively into private schools and schools where peers have above-median parental incomes. In contrast, we find limited evidence of substantial differences in switching by academic performance on grade 2 Danish tests. Thus, our findings suggest that reactions to a policy that aims to integrate students can potentially increase school-level socioeconomic segregation.

Having shown this substantial impact on student school mobility, we illustrate the meaningful bias this can generate when researchers attempt to assess the impacts of mainstreaming on peers' outcomes, such as school attendance, if switching students' outcomes are not measured at their new schools or are subject to switching costs. In our registry data, we observe school attendance, test scores from Danish and Math national tests, and self-reported questionnaires on students' wellbeing for all students in public schools — but not in private schools. We construct bounds on the unobserved true impacts of the arrival of the mainstreamed student in the absence of school switching. The resulting estimated impacts vary widely, from a null impact to a large impact across grade levels. The direction and size of the biases depend strongly on what assumptions one is willing to make about unobserved effects, meaning that naive estimates of effects may either over- or understate the true effects.

We attempt to further separate the direct impacts of the arrival of a mainstreamed student from the indirect impacts caused by school switching. First, we isolate the direct effects of a mainstreamed student from those caused by school switching by studying the subgroup of students who never change schools. Although this is a selected subgroup, we can identify the direct impacts of the arrival of the mainstreamed student for this substantial group of students.

We contrast these results with those for students who do change schools but never go to a private school, who may be affected by both switcher-specific direct and indirect switching impacts. While we cannot separate the effects of selection between the two subgroups (some students switch school because they are negatively impacted) from indirect impacts (some students are negatively impacted because they have the disruption of switching schools), this analysis highlights the divergence in effects for two subgroups with different switching choices. It also highlights what may be missed in a study focusing only on students who remain in the classroom.

Test score impacts for these two subgroups are quite different. Among those who never

change schools, we find weak evidence of negative impacts on attendance or test scores — suggesting that, for at least this group of students, there are no direct negative impacts. On the other hand, for students who do change schools, we document small negative impacts. These are largest in the secondary school years, when student mobility is the highest, suggesting that disruptions caused by greater mobility may play a role in determining effects.

Beyond attendance and test scores, do mainstreamed students cause disruptions or social tensions in a classroom when they arrive? The Danish registry data contain detailed information on self-reported social and academic wellbeing for public school students. Using this data, we find little evidence that the arrival of a mainstreamed student impacts students' social wellbeing. Meanwhile, we find a small decline in students' ability to concentrate and in their teachers' ability to calm the class after a disruption both for students who do and do not ever switch schools. These impacts may be either direct results of the arrival of the mainstreamed student or indirect impacts caused by the disruption of higher student mobility.

Given these diverging impacts for the groups who switch schools and never switch schools, we show suggestive evidence that the negative effects for students who switch schools are likely driven by the disruption of switching schools rather than by selection. We show that, among students exposed to a mainstreamed student, those who switch schools experience substantial increases in absences shortly after making a school switch, rather than before such a switch. We also find little evidence of improvements in test scores for these students after a school change.

Finally, we use a decomposition approach and illustrate that the effects of the arrival of a disabled student are not substantively attributable to changes in school quality or contemporaneous exposure to a disabled student. We also simulate a policy experiment that allows us to quantify the size of total effects on test score outcomes when varying the degree of inclusion.

We make several contributions to the literature on estimating peer effects.

We contribute directly to a literature which has attempted to quantify peer effects (see, e.g., Sacerdote (2011)). Although extensive work has attempted to understand how a student's peers impact their academic outcomes and suggests that these effects can be substantial, relatively little prior work has addressed the importance of student mobility in shaping these effects and their estimation. With data that allow us to observe students across schools in Denmark, we show that the possibility of students changing schools introduces potentially large biases in the estimation of peer effects, both through attrition to private schools where outcomes are not observed and through changes in class composition.

Specifically, we address the estimation of the effects of the placement of disabled students

on their peers. Numerous papers have suggested that the inclusion of disabled children in mainstream classrooms generates negative effects on their peers, particularly when resources are lacking (Rangvid 2019; Kristoffersen et al. 2015; Fletcher 2009; Fletcher 2010; Balestra, Eugster, and Liebert 2022; Huang, Lu, and Zhu 2021; Gottfried and Harven 2015). On the other hand, Ruijs (2017) and Friesen, Hickey, and Krauth (2010) do not find evidence of spillover effects from peers with learning disabilities. However, these papers have generally not addressed the role of school choice and student mobility in measuring peer effects, potentially causing biased estimates of the impacts of mainstreaming. An exception is Rangvid (2019), who finds no statistically significant impact of mainstreaming on contemporaneous attrition to private schools. However, Rangvid (2019) only focuses on contemporaneous switching, and therefore does not capture cumulative dynamic effects. Our findings indicate that an important dimension of response for peers of mainstreamed students is that of moving to other schools.[3] Without taking school switching into account, we find negative peer effects in line with those documented in prior work, but we suggest that these negative effects are explained almost entirely by student mobility rather than direct peer effects.

In this way, we bridge the estimation of peer effects with the literature on school choice in reaction to class or school composition. One large strand of this literature considers how immigrants shape the school choice of other local students. Farre, Ortega, and Tanaka (2018), Betts and Fairlie (2003), Cascio and Lewis (2012), Kristen (2008), Gerdes (2013), Schneeweis (2015), and Rangvid (2010) all investigate the role of "native flight" away from schools when exposed to a larger share of immigrants. The evidence suggests that the arrival of more immigrants leads to a larger degree of native flight. In the Danish context, both Gerdes (2013) and Rangvid (2010) find that increased immigrant concentration in schools may cause students to move to private schools. We add evidence on school switching patterns in reaction to a different kind of composition change, that is, the arrival of mainstreamed disabled students. We expand on this literature by focusing on how the choice of school in reaction to school composition affects test scores and socio-emotional wellbeing, as well as the estimation of peer effects on these outcomes.

Our research highlights the importance of accounting for student mobility over time after exposure to new peers when estimating peer effects. Not accounting for these dynamics can lead to biased estimates of the impacts of mainstreaming disabled students on their peers and to misleading policy conclusions. Although prior work has found evidence that placing disabled students in mainstream classrooms causes negative impacts for their peers, we find that there are no negative effects for a substantial group of students and that effects on other

[3]We also add new evidence on the impacts of inclusion on several socio-emotional wellbeing outcomes, in line with previous work studying feelings of social inclusion among disabled students (Stiefel et al. 2018).

students are not necessarily direct impacts of mainstreaming but may instead be caused by second-order reactions.

The rest of the paper is structured as follows. The next section introduces the Danish mandatory education system. We describe our data in Section 3 and discuss main features of the data, including the number of disabled students entering mainstream classrooms, in Section 4. We introduce our empirical strategy in Section 5 and present our main results on how mainstreaming affects school switching in Section 6. We turn to estimates of impacts on students' attendance, and academic and socio-emotional wellbeing outcomes in Section 7. In Section 8, we further unpack school switching as a mechanism for negative effects using both reduced-form and structural approaches. The final section concludes.

2 Background and institutional setting

This section contains a description of school choice and the important role of private schools within the Danish primary and lower secondary education system. It also contains an overview of the education system for disabled students.

2.1 The Danish school system

Danish students are required to attend grades 0–9, during which the typical student is aged 6–15. Practically all students attend public or private schools for their entire mandatory education.[4] Some schools offer only select grade levels, such as 0th to 6–8th grade. This includes around 30 percent of schools in our data. Students attending these schools must switch schools to complete their remaining mandatory education.

The public school system in Denmark is characterized by having *Free School Choice* (Børne– og Undervisningsministeriet 2024), where parents can freely choose their child's school. However, a child is only guaranteed a seat at a single school. This school is usually called the district school and is typically assigned based on geographical proximity. In contrast to district school assignment, school switching is not managed through a centralized mechanism, and implementation of switching processes can vary between schools and municipalities. Even if there is excess capacity, a family applying to enroll in a public school other than the district school is not guaranteed a spot. If a school is over-subscribed in the application process, the school is required to use objective standards when accepting students. However, being denied a place is relatively rare: a 2017 survey of parents of school-aged

[4]Less than 0.1 percent of children were homeschooled in 2015/2016 (Rambøl 2018).

children conducted by Epinion (2017) showed that only around 12 percent of parents who have attempted to switch their child's school were unable to get a seat at their desired school.

Private school attendance is relatively common in Denmark, and more so among families with high socioeconomic status. From 2009 to 2015, the share of students starting first grade in a private school rose by four percentage points from 13.5 to 17.4 percent, compared to an OECD average of around 10 percent (Pihl 2019). On average, the socioeconomic background of students attending private schools tends to be higher than those attending public schools (Epinion 2017). Private schools are funded by two sources: public funding (*koblingsprocenten*) and household payments, with costs averaging around 1250 DKK to 2500 DKK (185 USD to 370 USD) per month (Undervisningsministeriet 2023).[5] Enrollment and admission policies vary between private schools. Usually, private schools are over-subscribed and parents will be required to contact the school some time in advance to secure a seat. The Board of Education and Quality, on behalf of the Ministry of Children and Education, oversees the quality of education in independent and private schools.

School switching during mandatory education is common in Denmark. Around 25 percent of parents indicate that their child had changed schools at some point according to a representative survey sent to parents of primary school aged children by Epinion (2017). About half of these moves were necessitated by either geographical moving, reaching the highest grade in the local school, or the school closing. The remaining half of moves were freely chosen. Among those who changed freely, about 47 percent said the switch was due to the child not doing well in the previous school. The survey also showed that, overall, parents reported that their children's general wellbeing at school was the most important factor in choosing which school to attend.

2.2 Education for disabled students in Denmark

Since the early 2000s, many schools in Denmark have chosen to educate disabled students in segregated environments outside of mainstream classrooms. By 2009, as many as 5.4 percent of all Danish students were educated in these segregated settings away from their non-disabled peers (Egelund and Dyssegaard 2019). As of 2021, about 7.7 percent of Danish students received support for a disability at school (had a "special education needs" designation), and most of them (59.9 percent) were educated in mainstream *schools* serving both disabled and non-disabled students but in segregated *classrooms* (90.0 percent) (European

[5]For the average family with children aged 6–15 in 2015, this corresponds to a cost of sending all school-aged children to private school of 4–8 percent (1,250–2,500 DKK per child) of total post-tax family income. We calculated these numbers using the Danish registry data that we describe in Section 3. Appendix Figure A.3 shows the distribution of these cost shares across families.

Agency for Special Needs and Inclusive Education 2023).

Denmark's share of students identified as having "special education needs" taught in segregated classes is somewhat higher than those in comparable countries like Norway and Germany. While Norway and Germany have a similar share of students with this designation (7.8 percent and 6.4 percent, respectively, compared to Denmark's 7.7 percent), they educate many more of these students in mainstream classes. In Norway, 96.9 percent of "special education needs" students were educated in mainstream schools and 91.2 percent in mainstream classes in 2021 (see Appendix Table A.1). In Germany, 51.5 percent of these students were educated in mainstream classes. Because of this, it is possible that the disabled students who are being mainstreamed in Denmark are more similar to the disabled students already participating in mainstream classes in other countries. If so, our findings from Denmark may have more general relevance by also reflecting the effects of moving disabled students between mainstream classrooms in these other countries.

Our analysis focuses on the students switching from segregated classrooms to mainstream classrooms in Denmark. In 2012, the Danish parliament attempted to address the segregation of disabled students by passing the Inclusion Act, which mandated that no more than 4 percent of Danish students should be excluded from mainstream classrooms (van Kessel et al. 2019). Using our registry data, we document in Section 4 that the number of segregated classes declined from 2012 to 2014. The mainstreaming we observe and use in the analysis partly reflects this mainstreaming policy.

Mainstreaming disabled students has often been met with mixed opinions from parents and teachers across countries. Interviews with teachers in Greece found that teachers found mainstreaming challenging due to a lack of resources and training, difficulty managing student behavior, social tensions in the class, and difficulty adapting academic material to suit the needs of different students (Lyra, Koullapi, and Kalogeropoulou 2023). Evidence from Germany shows that the attitudes of parents of non-disabled students towards mainstreaming disabled students are mixed, depending on the type of disability of the mainstreamed student (Paseka and Schwab 2020).

Within Denmark, opinions about the impacts of mainstreaming students have been similarly mixed, in part because teachers feel they do not have the resources needed to teach disabled students. In 2015, a survey found that only 15 percent of Danish teachers reported that they agreed with the goal of mainstreaming disabled students (Egelund and Dyssegaard 2019). An expert panel studying implications of the 2012 Inclusion Act found that mainstreaming had benefits for disabled students, but they faced challenges caused by a lack of teacher and school preparation and resources (Ministeriet for Børn, Undervisning og Ligestilling 2016). This finding is confirmed by anecdotes from disabled children

and their families (Benninghoff 2019). These perceptions of the impacts of mainstreamed students could potentially drive some students to switch to other schools if they believe mainstreaming worsens the school environment substantively.

3 Data

Studying the effects of mainstreaming disabled students on their new mainstream classroom peers requires comprehensive data on both students' school and classroom enrollments over time, as well as measurements of relevant outcomes. Using Danish administrative data provided by Statistics Denmark, we construct a dataset which gives a rich picture of students' school and classroom enrollments and outcomes. Importantly, our dataset allows us to observe school-aged children across the entire education system in Denmark, both in public and private schools. This gives us the ability to follow students over time even if they change schools and study the dynamic impacts of the placement of disabled students on both student school mobility and learning and behavioral outcomes.

First, we obtain information about which students are enrolled in which schools and classrooms from the Danish education and student-class registers. These data are available from 2007 onwards. We use them to create identifiers for classes that persist over time so that we can track whether students remain in class together or switch to other classes or schools. Our class identifier persists from one year to the next if at least 75 percent of the students in the classroom remain together in a classroom the following year and 75 percent of students in the next-year class come from the same previous year's class. This means that if a class of students in one year splits 50/50 into two classrooms in the following year, we consider the prior class to have stopped existing and all these students to have entered new classes.

In our analysis, we primarily focus on unexpected school switching, which we define as switching taking place only when a student does not change address and their previous class does not stop existing. That is, school switches are only unexpected if students have the option to stay with at least 75 percent of their peers in the following year. In case a large share of students switch schools in response to receiving a mainstreamed student, this would mean that the class identifier stops, and we cannot make a meaningful determination of whether students "switched" classes since there is no option to stay with most of their peers. While we believe that this type of extreme responses is uncommon, our subsequent switching estimates should be viewed as lower bounds on the true switching effects.

We merge information on disability status onto this student-class-school panel. We collect information on students' disability status and the reason for referral to special education from

the Special Education register, which contains yearly information on all students who receive at least nine hours of support per week from 2012 onwards, and all students receiving special education in 2011. While the data do not contain a description of the specific type of support students receive, they do let us observe whether students do or ever have received support for a disability.

We also use demographic information on students and their socio-economic background, which we use to characterize the students. The demographic data come from the Danish demographic and income registers and includes information on gender and ethnicity, as well as family links. We measure parental socio-economic background using parents' individual total income deflated to 2015 DKK values and average these across the child's school age years, 6–16.

Our test score, school attendance, and socio-emotional wellbeing outcome data, which we can link to the student-class-school panel, come from Statistics Denmark and the Danish Ministry of Education (the division STIL, *Styrelsen for IT og Læring*). For test score data, we rely on national tests taken by the full population of students in public schools in Denmark throughout 2011–2019. We observe Danish (reading) tests taken in grades 2, 4, 6, and 8, and Mathematics tests taken in grades 3 and 6.[6] We collect wellbeing information from mandatory wellbeing surveys sent out to all public school grades 4–9 from 2011 by STIL. We also observe absences from school measured in days per school year, from 2011. Given the availability of the administrative data and the fact that we can only measure entry or exit from a classroom starting from the second year of our observed data, our analysis covers the period 2012 to 2020.

One limitation of our data on students' test scores, attendance, and wellbeing is that it is collected only for students in public schools and, as such, we do not observe them for students in private schools. This reflects a general limitation faced in using administrative education data in many settings and is a particular emphasis in our analysis. We discuss the implications of what can be learned about mainstreaming effects given selective switching towards private schools in much more detail in Section 7 and Appendix C below.

4 Patterns of mainstreaming, school switching, and outcomes

In this section, we provide an overview of our data and examine the placement of disabled students in either segregated or general education classrooms. We also show descriptive

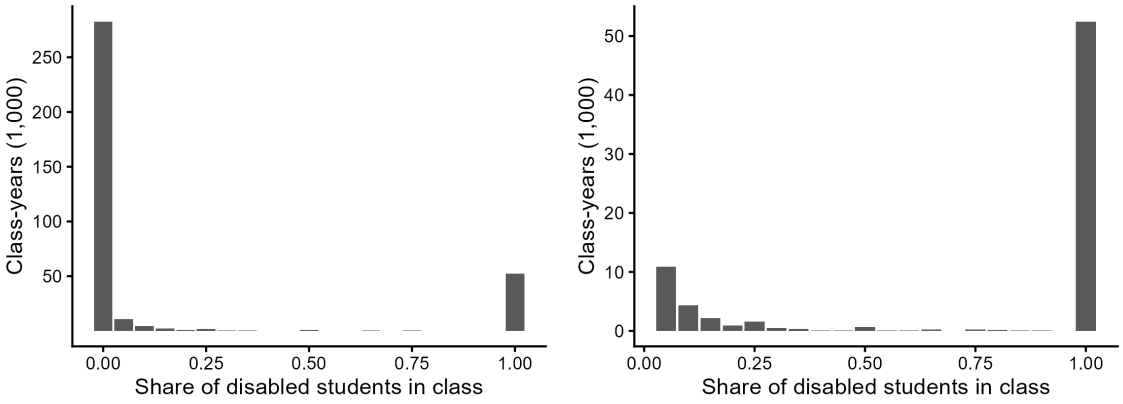
[6]Test scores are not available in 2020 due to the COVID-19 pandemic.

patterns highlighting that exposure to mainstreaming is associated with increased school switching and private school attendance.

4.1 Patterns of mainstreaming

As discussed above, education for disabled students in Denmark tends to be highly segregated. Figure 1 shows the number of classes we observe in our data by the share of disabled students receiving additional support services ("special education") attending the class. The distribution is bimodal: most classes have no disabled students, and a sizable number contain 100 percent or nearly 100 percent disabled students. Some classes contain a smaller share of disabled students; these will be of interest to our study as some of these classes contain students who were formerly taught in segregated classes and are now mainstreamed.

Figure 1: Distribution of classes by share of disabled students

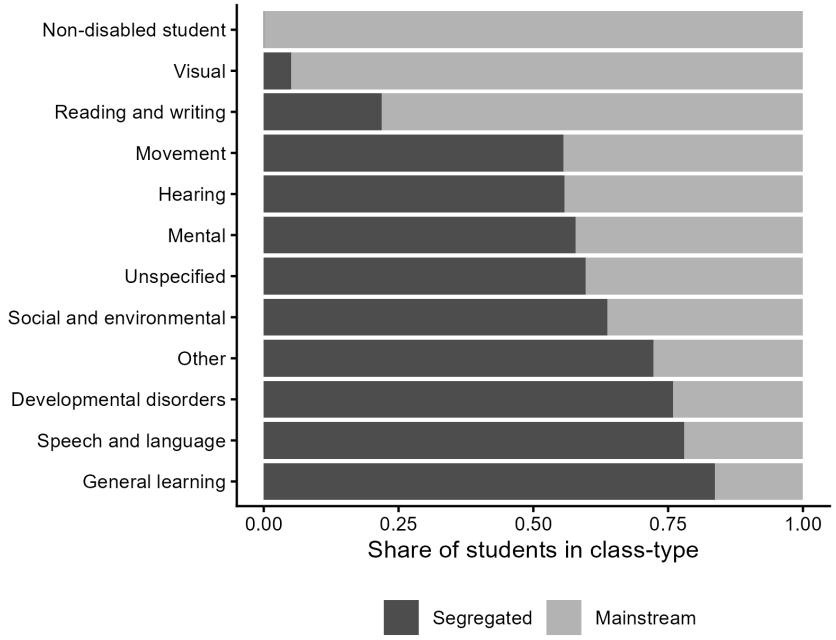


(a) Overall (b) Excluding zeros

Notes: The figure shows the distribution of classes by share of disabled students. Panel (a) includes all classrooms, while panel (b) limits the sample to classrooms with at least one disabled student.

Patterns of segregation vary somewhat by type of disability. Figure 2 shows the type of classes students attended (segregated classes where other disabled students receiving support services are a majority vs. mainstream classes) by their recorded reason for receiving support. Most disabled students in segregated classes receive support for general learning disabilities, which include developmental delays and intellectual disabilities. Less commonly, students in segregated classes also receive support for developmental disorders like autism and ADHD, speech and language difficulties, and reading and writing difficulties. Most students with visual disabilities receive their education in mainstream classrooms. We have only limited information about the support that disabled students receive, but our data suggest that disabled students in mainstream classrooms receive less support than their peers in

Figure 2: Education for disabled students by disability and modality



Notes: The figure shows the placement of students with various types of disabilities. Each bar shows the share of students with the given disability in mainstream classrooms and segregated classrooms. Students with general learning disabilities, which include intellectual disabilities, experience the most segregation out of mainstreamed classrooms.

segregated classrooms.[7]

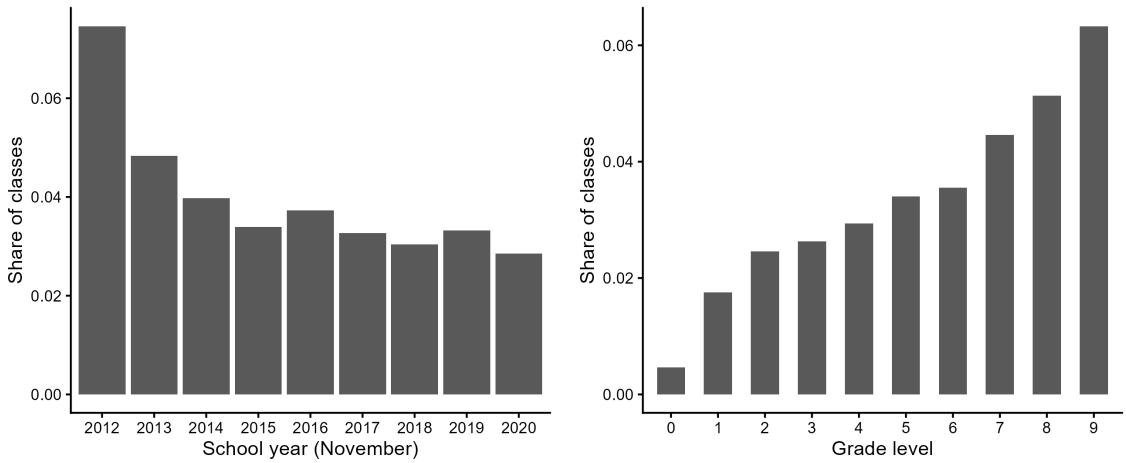
An important feature of our data is that we observe disabled students as they move into and out of segregated classes throughout the period we study.[8] We find that mainstream classes receive disabled students from segregated classes across all years and are more likely to receive disabled students in higher grade levels. When students who were formerly educated in a segregated class join a mainstream classroom, we call the peers in their new classroom "exposed" to a mainstreamed student.[9] Figure 3a shows the share of mainstream classes receiving a student from a segregated class each year from 2012–2020. In 2012, more than 6

[7]In our data, we only observe hours of support received for all disabled students in 2011. Appendix Figure A.1 shows the distribution of hours of support among those registered as receiving more than zero hours by the type of class they attend, segregated or mainstream, in 2011. Students in the segregated classes typically get more than 15 hours of support, while students in mainstream classes typically get fewer than 9 hours with a high density near zero hours.

[8]As described above, following a set of reforms, many students with disabilities who were previously educated in segregated classrooms were moved to mainstream classrooms. However, the reforms did not abolish segregated classes. Accordingly, Appendix Figure A.2 plots the number of segregated classes in each year and shows a decline in the number of segregated classes after 2011, from about 6600 in 2011 to a low of about 5600 in 2016.

[9]We focus on the first exposure after mainstreaming in the analysis as subsequent school switches by the mainstreamed student may be a result of particular endogenous switching due to a poor first class-match, which would differ from the mainstreaming effect of interest.

Figure 3: Classrooms receiving mainstreamed disabled students



(a) By year (b) By grade level

Notes: This figure shows the share of mainstream classrooms receiving students who were previously in segregated settings. Panel (a) illustrates this share by academic year, showing an initial decline and no discernable trend from 2014. Panel (b) breaks down the data by grade level.

percent of mainstream classes received a mainstreamed student. It is worth noting that this share on average decreases slightly over time, though it remains above 3 percent. This may be due to the declining number of disabled students put in segregated classes following the 2012 Inclusion Act.

Figure 3b shows the share of mainstream classes receiving disabled students by grade level, highlighting that students are mainstreamed across grade levels, but more likely at higher grade levels. This increase by grade levels may be due to the fact that disability rates increase with grade level as disabilities are identified over time. The figure also shows pronounced jumps in shares of classes receiving mainstreamed students at grades 7 and 9. This may be explained by disabled students attending schools offering only 0–6th or 0–8th grade who are required to switch schools to complete their mandatory schooling.

Students who are exposed to a mainstreamed student (i.e., share a classroom with a mainstreamed student) differ somewhat in their characteristics from those who are never exposed. Table 1 shows means of background, exposure, and outcome characteristics, as well as differences in means across the exposed and never-exposed students. Those exposed to mainstreamed students are slightly likelier than their peers who were never exposed to mainstreamed students to have received services for a disability themselves (4.7 percentage points), and their parents have about 0.5 years less education and slightly lower incomes on average.[10] In line with the 2012 Inclusion Act, we observe more mainstreamed students and

[10]We note that exposed students are older than never-exposed students on average and, thus, have older

more exposed students between 2012 and 2015. They also, on average, have slightly lower test scores (0.175 SD in Danish and 0.168 in Mathematics) and are 6.8 percentage points more likely to have switched schools. These differences in background characteristics between exposed and not-exposed students indicate that a simple comparison of their average school switching and other outcomes might reflect both treatment effects and selection effects. We account for this type of selection bias through our difference-in-differences design that controls for time-invariant differences across exposed and non-exposed students. We next illustrate the plausible validity of this design, focusing on exposed and non-exposed 4th grade students.

Table 1: Descriptive statistics, exposed vs. never-exposed

	Exposed			Never exposed				
Variable	Mean	SD	N	Mean	SD	N	Diff. mean	SE
Year	2015.26	2.41	1007882	2016.17	2.58	4018957	-0.91	0.003
Female	0.50	0.50	1007361	0.50	0.50	4011231	0.00	0.001
1st gen. immigrant	0.03	0.17	1007361	0.03	0.18	4011231	-0.01	0.000
2nd gen. immigrant	0.08	0.27	1007361	0.08	0.27	4011231	0.00	0.000
Ever special ed. student	0.10	0.30	1007829	0.05	0.23	4018441	0.05	0.000
Mom's age at birth	30.11	4.95	1004349	30.64	4.88	4000646	-0.53	0.005
Dad's age at birth	32.81	5.78	986203	33.26	5.71	3933328	-0.45	0.006
Mom's years of schooling	14.43	2.71	998020	14.89	2.73	3977477	-0.46	0.003
Dad's years of schooling	14.19	2.61	975920	14.62	2.67	3897750	-0.44	0.003
Mom's income (1,000 DKK)	358.08	158.97	1004041	381.50	190.21	3998288	-23.42	0.206
Dad's income (1,000 DKK)	457.76	292.26	986551	512.99	358.71	3930729	-55.22	0.390
First year observed	2012.62	1.34	1007882	2013.76	2.21	4018957	-1.13	0.002
Last year observed	2017.90	2.18	1007882	2018.58	2.12	4018957	-0.68	0.002
First grade observed	3.08	2.17	1007882	2.44	2.13	4018957	0.64	0.002
Last grade observed	8.34	1.39	1007882	7.25	2.21	4018957	1.09	0.002
Danish score (std.)	-0.11	1.01	350780	0.07	0.98	1099777	-0.17	0.002
Math score (std.)	-0.11	1.00	213220	0.06	0.98	633305	-0.17	0.002
Ever switched classes	0.04	0.19	1002047	0.03	0.17	3919493	0.01	0.000
Ever switched schools	0.24	0.43	1001872	0.17	0.38	3918276	0.07	0.000
Ever moved addresses and school	0.13	0.34	1007211	0.08	0.27	4010228	0.05	0.000
Ever moved addresses, same school	0.39	0.49	1006868	0.35	0.48	4002381	0.04	0.001

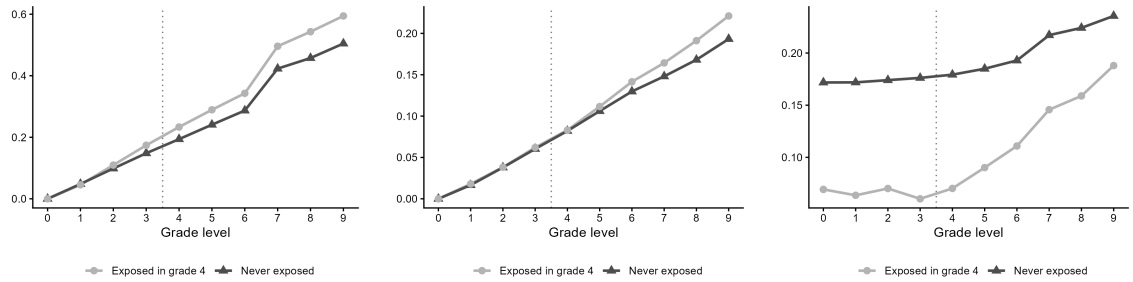
Notes: The table shows descriptive statistics comparing the characteristics of exposed and never-exposed students who are not themselves mainstreamed students.

4.2 Descriptive picture of school switching

Descriptively, we find that students exposed to mainstreamed students are more likely to change schools following the exposure than never-exposed students. Figure 4a shows the probability by grade level that a student has switched schools at least once among students

parents. Because of this, in later parts of the analysis, we standardize parents' income within age and gender to remove lifecycle effects in our measurement of socioeconomic status.

Figure 4: Never exposed versus exposed in 4th grade



(a) Probability of having changed school

(b) Probability of having switched school unexpectedly

(c) Probability of being in private school

Notes: The figures plot descriptives for students never exposed to a mainstreamed student's arrival and students who are exposed in grade 4. Panel (a) plots the share of students in each group who have changed schools at least once by the given grade level. Panel (b) shows the share who have made an unexpected school switch, as defined in the text. Panel (c) shows the probability that a student is enrolled in a private school.

exposed to a mainstreamed student in grade 4 compared with students who are never exposed to a mainstreamed student. We illustrate the switching patterns for students exposed in grade 4 as this allows us to visualize switching both prior to and after the arrival of a mainstreamed student. Before grade 4, the two groups follow similar trends, with the probability of having changed schools at least once increasing steadily over time. The figure illustrates a divergence in the propensity to have changed schools among the exposed students after exposure. Both groups experience a jump in the probability of switching schools at grade 7 (secondary school), likely because some students attend schools terminating at grade 6. However, the divergence becomes clear starting from grade 7, after which exposed students are much more likely to have changed schools than never-exposed students.

The divergence between exposed and never exposed students is even clearer when we focus on having made unexpected school switches (Figure 4b). We consider a switch unexpected if one's class persisted into the next year without interruption and one's address did not change. We again see that the two groups have nearly identical unexpected switching patterns before grade 4 but diverge afterward, with the exposed students having larger probabilities of having switched schools unexpectedly starting from grade five and later.

Along with being more likely to have switched schools, students exposed to a mainstreamed disabled student in grade 4 also experience an increase in private school attendance compared to students who are never exposed. Figure 4c shows that, although the two groups had different levels of private school attendance before grade 4 (with exposed students being less likely to attend private schools), they followed similar trends until grade 4. In grades 5 and later, those exposed in grade 4 experience a rapid and disproportionate increase in

private school attendance relative to the never-exposed.

5 Empirical approach

The above descriptive statistics and patterns of switching and private school attendance highlight that, although exposed students differ from other students on observable characteristics, they appear to have similar trends before exposure in school switching. Motivated by these descriptive statistics, we use a difference-in-differences event-study design to estimate the causal impacts of exposure to a mainstreamed student. That is, we estimate causal impacts by estimating the difference in trends over time between students who are exposed to a mainstreamed student relative to our comparison group. By focusing on the difference-in-differences, we eliminate the effects of any time-invariant characteristics (e.g., a student's underlying aptitude) that might differ between students exposed to a mainstreamed student and the comparison group.

Our difference-in-differences design uses variation in an individual's first exposure to a mainstreamed student across different classrooms and over time. This is a staggered treatment setup, and we estimate our parameters of interest using the framework proposed by Callaway and Sant'Anna (2021). In this framework, we estimate the average treatment effect on the treated (ATT) for each treatment cohort (g) and each time period (t). We take treatment cohorts to be given by the grade level in which a student is first exposed to a mainstreamed student, with the time dimension being the student's grade level. Our comparison group is never-exposed students, that is, students who are never in a classroom when a mainstreamed student arrives.[11] By always comparing students exposed to a mainstreamed student to students who have not been exposed but are in the same grade level, we control for any general trends associated with progression through the school system.

When estimating our models, we drop students who ever repeat a grade so that each student can only be observed in each t once. Additionally, we drop students who are themselves ever-mainstreamed or attend a segregated class, students attending non-mandatory 10th grade, and observations in classes with more than 39 students (above the legislated maximum classroom size of 32 students) as these typically reflect school structures where students do not belong to a particular classroom. Finally, for this analysis, we focus only on students who are exposed to a mainstreamed student before grade 7, given the substantial changes in school structure in grades 7 and beyond.

This design relies on an assumption of parallel trends in counterfactual outcomes; that

[11] It is possible that a student is in a class with more than one mainstreamed student. We aggregate over these types of intensive margin effects when estimating treatment effects.

is, in the absence of the arrival of a mainstreamed student, the outcomes of exposed and never-exposed students would have followed similar trends. We also must assume that there is no anticipation, meaning that the exposure does not cause any change in outcomes before it happens. We show evidence to support both assumptions by estimating trends before the exposure.

We use the repeated cross section estimators suggested by Callaway and Sant'Anna (2021) and cluster standard errors at the individual level.[12] We estimate $ATT(g,t)$ parameters for each treatment cohort (e.g., the ATT for those treated in grade 4 at each grade level) using the following estimator:

$$\widehat{ATT}(g,t) = \mathbb{E}_n[Y_{it}|G_i = g] - \mathbb{E}_n[Y_{it}|G_i = \infty] - \Big(\mathbb{E}_n[Y_{ig-1}|G_i = g] - \mathbb{E}_n[Y_{ig-1}|G_i = \infty]\Big)$$

where Y_{it} is the outcome of individual i in grade t, G_i is the first grade in which an individual is treated, $G_i = \infty$ indicates that an individual is never-treated, and $\mathbb{E}_n[Z_{it}] = \frac{1}{n}\sum_n Z_{it}$.

Given an estimate for the $ATT(g,t)$ for each g and t, we aggregate these into event-study ATT, which give the impact of exposure according to the number of years since exposure. We also aggregate $ATT(g,t)$ according to current grade level (i.e., average impacts for students when they are in grades 2–6 or 7–9). Finally, we report averages of these event-study coefficients using a simple average for the pre-period and post-period, which gives equal weight to each event-time, as well as the post-period average with weights as suggested by Callaway and Sant'Anna (2021), which gives weight to each $ATT(g,t)$ according to the share of observations in each g and t.

6 Student mobility in response to mainstreaming

When there is a change in the composition of a student's class, such as the arrival of a mainstreamed student, this may induce students to consider changing schools or classrooms. In this section, we show that being exposed to a mainstreamed student causes substantial student mobility between schools and, to a lesser extent, classrooms within a school. We unpack the timing of these effects and show that they occur across grade levels but are particularly large in the secondary school grades. We then highlight that these effects are most likely to drive students towards private schools and schools where their peers have a higher socioeconomic background.

[12] We use the Stata implementation of these estimators by Rios-Avila, Sant'Anna, and Callaway (2025) and Rios-Avila (2025).

6.1 Impacts on school and class switching

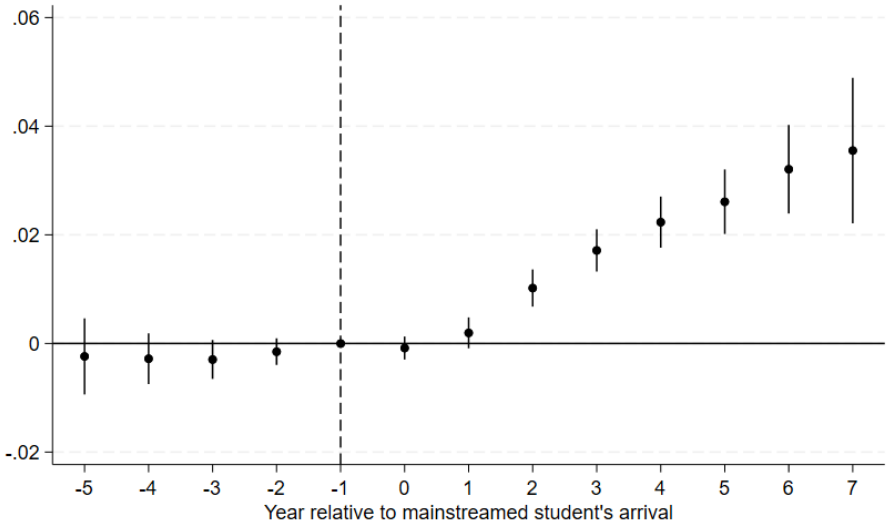
Throughout this analysis, we focus on what we term unexpected student mobility. As noted above, an unexpected school switch is one in which a student switches schools while at least 75 percent of their peers remained together in a classroom and the student did not simultaneously move addresses. In this way, we distinguish unexpected switches from those that regularly occur when students switch schools at higher grade levels because their school does not offer these grades or their family moves to a new location. In our results, school switching always refers to unexpected school changes.

Using our difference-in-differences design, we find evidence of a large increase in school switching following the arrival of a mainstreamed student. This is in line with our previous descriptive findings. Figure 5 plots event study coefficient estimates of the impact of the arrival of a mainstreamed peer on the probability that a student has ever made an unexpected school switch. There is little evidence of non-parallel pre-trends between the exposed and never-exposed students, with all pre-period estimates being small and statistically insignificant. This confirms that, prior to the arrival of a mainstreamed student, exposed and never-exposed students followed similar trends. The post-exposure estimates, in contrast, show a marked increase in having switched schools, which begins one year after the arrival of a mainstreamed student and becomes larger over time. By 4 years after the arrival of a mainstreamed student, exposed peers are more than 2 percentage points more likely to have switched schools.

Summarizing these effects into averages over a student's years spent in school, we find that increases in school switching are quite large. Table 2 presents aggregations of the estimated average treatment effects by time relative to treatment. Column (1) shows estimated effects on the probability that a student has ever made an unexpected school switch up until the time of measurement. The pre-period average shown in the first row serves as a test for parallel pre-trends between exposed and never-exposed students before the arrival of the mainstreamed student. We find little evidence of a violation: the estimate is statistically insignificant and very small. The post-period average and C-S aggregation appearing in rows two and three both refer to an average over all post-exposure periods, averaged with different weights described in Section 5. These results both show evidence of a large increase in unexpected school switching following the arrival of a mainstreamed student in one's class. This amounts to an average of a 1.2–1.8 percentage point (pp) increase in the probability of having made such a switch, which is quite large relative to a mean of 9 percent.

The impacts on school switching do not occur evenly across all years a student spends in mandatory schooling, and the remaining rows of Column (1) in Table 2 illustrate their dynamics. The fourth row contains the event-study coefficient for period 0 to test for ef-

Figure 5: Event-study ATT impact on student unexpected school switching



Notes: The figure plots event-study estimates from the main difference-in-differences specification, estimating the impacts of the arrival of a mainstreamed student on the probability that a student has made an unexpected school switch, which include switches where the origin class does not end and the individual does not move address simultaneously. 95% confidence intervals are shown, with standard errors clustered at the individual level.

fects that occur in the same year as the mainstreamed student's arrival. As the arrival of a mainstreamed student during a school year is unlikely to have affected a student until the following school year, finding no effect in this period shows that the mainstreamed student's arrival was not concurrent with other important rearrangements in the classroom environment. [13] Meanwhile, in the Danish school system, school changes are more common after students enter grades 7–9. To study whether effects are larger given the higher overall mobility in these years, the table next presents effects averaged according to a student's current grade level, for those in grades 2–6 (before secondary school) and those in grades 7–9 (secondary school). School switching is much larger once students reach secondary school, with the impact on the probability of having made a school switch being 2.0pp in grades 7–9 compared to 0.4pp in grades 2–6.

While Column (1) illustrates effects on having ever switched school unexpectedly, students may also become more likely to switch school multiple times. We next show estimated effects on contemporaneous unexpected school switching between the prior and current academic year (Column (2) of Table 2). Although there is some evidence of a pre-trend for this outcome, with a significant negative pre-period average estimate, the results are consistent with those in Column (1) for the probability of having ever made an unexpected switch

[13] Our analysis sample removes the mainstreamed student and only considers their peers. This removes any mechanical switching effects.

Table 2: Impacts on unexpected school switching

	(1) Has switched school	(2) Switched school	(3) Has switched class	(4) Switched class
Pre-period avg.	-0.002	-0.005***	0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.000)
Post-period avg.	0.018***	0.005***	0.001**	-0.000
	(0.002)	(0.001)	(0.001)	(0.000)
C-S aggregation	0.012***	0.004***	0.001	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Event-study coefficient, period 0	-0.001	0.002*	0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Avg. effect in grade 2-6	0.004***	0.004***	0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Avg. effect in grade 7-9	0.020***	0.004***	0.001**	0.000
	(0.002)	(0.001)	(0.001)	(0.000)
Observations	4663802	3025347	4663802	3027465
Dep. var. mean	0.090	0.036	0.008	0.003

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on school and class switching outcomes. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2-6 and 7-9 presenting aggregated effects for students currently in the given grades. Each column presents impacts on a given outcome, with Column (1) showing impacts on having ever made an unexpected school switch, Column (2) on having unexpectedly switched school going into the current year, Columns (3) and (4) on having ever made an unexpected class switch and making an unexpected class switch going into the current year. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

up until the time of measurement. As when studying whether a student has ever made an unexpected school switch, we find an overall increase in the probability of making such a switch after exposure. This increase has a magnitude of 0.4 percentage points per year when using the C-S post-period average, or an increase of 11 percent over the mean of 3.6 percent of students who switch schools in each year. In a class of 30, this would translate to 0.12 additional students switching per year.

Having shown that the arrival of a mainstreamed student causes substantial mobility between schools, we next document suggestive evidence of increases in the probability of switching classrooms *within* a school following exposure. Many schools have multiple classes at a grade-level and it may be less costly for parents to have the school assign their child to another class than switch school entirely. In that case, within-school switching may be

important.

Table 2 shows that these within-school effects mirror those for school switching. Again, we focus on unexpected switches, defined as switches where 75 percent of one's classmates remained together in a year and one did not change addresses or schools. Overall, unexpected class switching within a school is rare, with only 0.8 percent of students having done this and only 0.3 percent of the sample making this kind of switch in a given year. As with school switching, there is some evidence that the arrival of a mainstreamed student causes an increase in having made an unexpected class switch, with the largest effects occurring in secondary school (Column (3)). Although this effect is small in absolute terms (only 0.1 percentage points in grades 7–9, or approximately 0.03 in 30 students), it is large relative to the very low rates of class switching we observe in our data. Given the rarity of class switches, we lack variation to detect effects on contemporaneous within-school class switching in a given year, as evidenced by the vanishingly small estimates in Column (4).

Are these students switching away from a mainstreamed student or switching into a class with a mainstreamed student? Distinguishing these two helps unpack whether increased switching is likely a negative reaction to the arrival of the mainstreamed student (with other students leaving the classroom) or correlated with increased mobility into these classrooms in general (with other students joining the classroom). We further break down effects on moving away from and towards a mainstreamed student in Appendix B. We show that a large share of school and class switching is driven by switching away from a mainstreamed student.

One possible concern about our analysis of student mobility is that students who are in classrooms with returning students have otherwise high rates of mobility in later school years. In Appendix B, we find no evidence that these students are more likely to move homes after the arrival of a mainstreamed student. This helps to confirm that they do not have higher rates of school switching for other reasons.

6.2 Who switches schools and where do they go?

Having shown that the arrival of a mainstreamed student induces higher rates of school switching, we show that these effects vary substantially by (1) student background characteristics, (2) characteristics of the mainstreamed student, and (3) the type of school the student joins. We find that the impacts on school switching are largest for high income students, who may have more information about or financial resources to attend private schools. Students also become more likely to switch into private schools and schools where their peers have high socioeconomic status, and effects are largest when mainstreamed students have

learning and developmental disabilities.

Studying heterogeneity according to students' incomes, we find that students whose parents have above-median incomes are more likely to switch schools unexpectedly.[14] In Appendix Table A.2, we show that students whose parents have incomes above the median experience a larger impact on their likelihood of switching schools than their peers whose parents have incomes below the median. At the same time, we also find that students with below-median 2nd grade Danish national test scores are slightly more likely to switch. However, these results should be read with more caution, as we only observe 2nd grade test scores for students who were in grade 2 in public schools between 2011-2019.

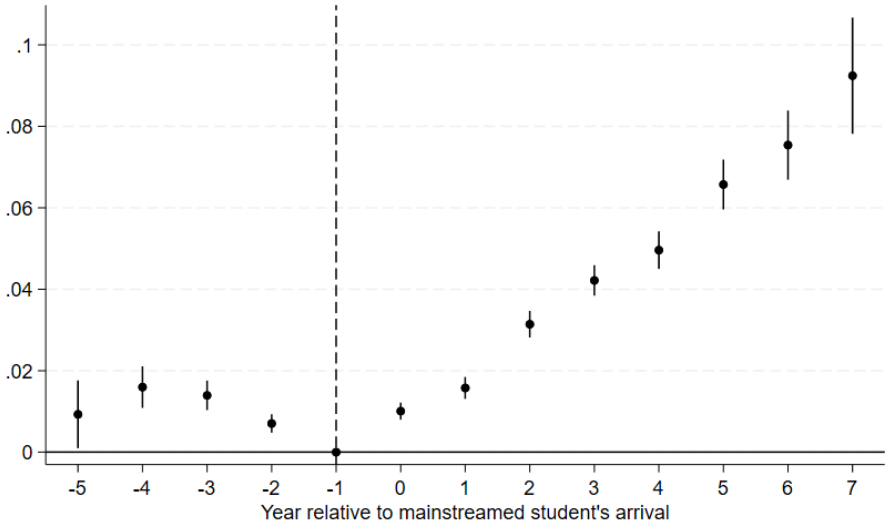
When students switch schools, where do they go? We find that students exposed to a mainstreamed student become more likely to switch to certain types of schools: private schools and schools where other students have high incomes. Figure 6 plots event study coefficients showing impacts on the probability that a student is in a private school. Although there is some evidence of a significant pre-trend as suggested by the significant pre-period estimates, these effects are relatively small and there is a substantial change in trend following the arrival of the mainstreamed student. We find large increases in the probability that a student is in a private school, over 6 percentage points 5+ years after exposure. This is quite large relative to the mean probability of being in a private school of 18 percent. Among a class of 30 students, a 6 percentage point increase corresponds to an additional 1.8 students moving to private school. The effects increase steadily over time since exposure.

Consistent with this increase in private school enrollment, we also find that exposed students are more likely to switch into schools where their peers have higher socioeconomic status. Appendix Table A.3 studies the impact on the probability that a student switches into a school where students' parental income is above or below the median in a given year. Here, we see that increases in school switching are concentrated in schools with above-median income, with an average post-period increase of 0.3pp in the probability of switching into this kind of school in each year. In contrast, there is little increase in the probability of switching into a school with below-median income.

Although there is a large difference in the types of schools students switch to by income, we are limited in our ability to study evidence of switching into schools based on the schools' academic performance. This is because test scores are not observed for students attending private schools. Thus, Appendix Table A.3 shows no increase in switching into schools with above or below median average Math or Danish scores, suggesting that most switching is concentrated in private schools.

[14]Parents' incomes are averaged over the years we observe a student from age 6–16, so that this characteristic is a time-invariant measure of socioeconomic status for a given student.

Figure 6: Event-study ATT impacts on private school enrollment



Notes: The figure plots event-study estimates from the main difference-in-differences specification, estimating the impacts of the arrival of a mainstreamed student on the probability of being in a private school. 95% confidence intervals are shown, with standard errors clustered at the individual level.

Given that the decisions of peers and their parents may vary according to how they react to the mainstreamed student, we study school switching behavior according to the type of disability the mainstreamed student has. Prior literature on the arrival of disabled students has focused on the disruption caused by peers with emotional and behavioral difficulties (e.g., Kristoffersen et al. (2015) and Horoi and Ost (2015)). Other work from schools in the United States, such as Fish (2019), suggests that disabilities like ADHD and autism (here, developmental disabilities) are treated as "high status" disabilities, while intellectual disabilities and emotional/behavioral problems are treated as "low status".

Table 3 studies the probability of having switched school according to the disability of the mainstreamed student. Column (1) contains overall estimates for comparison. As our dataset does not contain the type of disability for all students, the remaining columns represent a subsample of those exposed to mainstreamed students whose disabilities we observe. Further, we limit the analysis to the four most common types of disabilities to ensure sufficient power to draw conclusions. These include General Learning, Developmental, Social, and Other disabilities.

We find that the school switching effect is largest when the mainstreamed student has a general learning difficulty (including "psychically developmentally disabled, late developed, etc.", that is, mainly including intellectual disabilities) or a developmental disability (including autism, ADHD, etc.). Columns (2) and (3) of Table 3 show estimated impacts

Table 3: Heterogeneity in unexpected school switching by type of mainstreamed student disability

	(1) Overall	(2) Learning	(3) Developmental	(4) Other	(5) Social
Pre-period avg.	-0.002	0.009	0.015	0.002	-0.018
	(0.002)	(0.008)	(0.012)	(0.007)	(0.018)
Post-period avg.	0.018***	0.018**	0.017	0.008	-0.032**
	(0.002)	(0.007)	(0.014)	(0.008)	(0.014)
C-S aggregation	0.012***	0.012**	0.019*	0.008	-0.008
	(0.001)	(0.006)	(0.010)	(0.005)	(0.013)
Event-study coefficient, period 0	-0.001	-0.001	0.009	0.003	-0.001
	(0.001)	(0.005)	(0.008)	(0.004)	(0.009)
Avg. effect in grade 2-6	0.004***	0.002	0.007	0.006	-0.011
	(0.001)	(0.005)	(0.012)	(0.004)	(0.008)
Avg. effect in grade 7-9	0.020***	0.022***	0.032**	0.008	-0.002
	(0.002)	(0.008)	(0.013)	(0.007)	(0.022)
Observations	4663802	4344384	4334156	4349666	4329878
Dep. var. mean	0.090	0.089	0.089	0.089	0.089

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on school switching outcomes according to the type of the mainstreamed students' disability. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts on having ever made an unexpected school switch, depending on the type of disability of the mainstreamed student. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

on unexpected school switching for peers of students with learning and developmental disabilities. For peers exposed to mainstreamed students with these types of disabilities, the probability of having switched schools increases by 1.2 percentage points and 1.9 percentage points, respectively. In contrast, there is no evidence of increased switching when the mainstreamed student's disability is classified as social or environmental difficulties (including behavioral difficulties) or a disability classified as "other" (Columns (4) and (5)).

Our results contrast with prior research that would suggest that peers with social and behavioral problems should induce the most school switching, but are potentially explained by salience of disability type in the classroom. We find that the largest impacts occur with the arrival of mainstreamed students with intellectual and developmental disabilities. These students may be more likely to have well-defined labels like autism or ADHD that make

them more readily labelled as "disabled" or "inclusion" students and thus more apparent to their peers. This is in contrast to the arrival of mainstreamed students with less well-defined disabilities like social and environmental difficulties and other types of disabilities, in which case we find little evidence of an impact.

6.3 Benchmarking magnitudes of school switching

We have shown that the arrival of a mainstreamed student induces substantial mobility among peers in the classroom. How do the magnitudes of these effects compare with the impacts documented in prior literature?

Prior work, including Balestra, Eugster, and Liebert (2022), Kristoffersen et al. (2015), and Fletcher (2010), finds negative impacts of being in a classroom with a disabled student on peers' test scores. However, these effects are estimated using a measure of whether an individual is currently in a class with a disabled student (or the current number of disabled students in an individual's classroom). Because of this, if students switch out of these schools and classrooms, these estimates will contain an important bias.

We argue that the magnitudes of the negative effects documented by these papers can also plausibly be explained by the selection we document. For example, Balestra, Eugster, and Liebert (2022) find that adding 1 additional disabled student to a class of 30 students (a 3 percentage point increase in the share of disabled students) causes an approximately 0.02 SD decline in test scores. A decline of a similar magnitude (0.02 SD) is documented in Kristoffersen et al. (2015), who study the impacts of having a peer with a psychiatric diagnosis on reading scores. However, the magnitude of this decline in the average student's test scores could instead equivalently be explained by a single student with test scores 0.7 SD higher than the mean leaving a class of 30 students. Although our findings do not show only limited heterogeneity by students' 2nd grade Danish test scores, this magnitude of switching is plausible given our results showing that the arrival of a mainstreamed student caused about a 1 student out of 30 increase in having switched schools by the secondary school years.

Although impacts will certainly depend on features of the context, such as the ease of switching schools or classrooms, this back-of-the-envelope calculation highlights that the magnitudes of negative effects documented in prior work are similar to the magnitudes that can plausibly be explained by selective switching alone.

7 Estimating impacts on student absences, test scores, and wellbeing

In this section, we highlight the importance of accounting for selective school-switching when estimating the impacts of exposure to a mainstreamed student. We suggest that estimates of the impacts of mainstreaming on student outcomes, such as learning and socio-emotional wellbeing, may be significantly biased by school switching, including due to attrition to private schools. To help better untangle the role of school switching in determining impacts on students' outcomes, we attempt to assess effects for two subgroups of students: (1) those who never change schools and (2) those who do change schools but never attend a private school. We show that, for students who never change schools, there is little evidence of negative impacts on attendance or test scores. Negative effects for students who do change schools may be explained by selection, direct impacts of the mainstreamed student, or general disruptions caused by school changes.

7.1 Impacts of mainstreaming on school attendance of school switchers and never-switchers

In this section, we attempt to estimate impacts of the arrival of a mainstreamed student on their peers' school attendance. We show that naive attempts to estimate these effects can be biased by school switching. We then isolate a subgroup of students who never change schools and show that, in contrast to their peers who do change schools, this subgroup experiences no negative impact on their attendance.

We study the impacts on peers' number of unexcused absences from school. School attendance and absenteeism have been used in the literature as a measure of student wellbeing (e.g., Imberman, Kugler, and Sacerdote (2012), Oosterbeek and van Ewijk (2014), and Hill et al. (2023)) and have been causally linked to reductions in academic performance, test scores, educational attainment, and adulthood labor market outcomes (Cattan et al. 2023; Gottfried 2009; Liu, Lee, and Gershenson 2021; Yaow et al. 2026). In our data, studying absences from school is attractive because this information is available annually for all public school students in our sample.[15] However, this outcome is not observed for students in private schools. As such, any naive analysis of the impacts of mainstreaming will be biased by the switching patterns documented in the previous section.

[15]In this analysis, we remove student-year observations with more than 10 unexcused absences. This represents observations above the 95th percentile in our sample and ensures that results are not driven by a few cases with a very high number of absences. Effects on chronic absenteeism are discussed below.

Given that we show that the arrival of a mainstreamed student causes other students to be more likely to change schools and to attend private schools, we highlight that the potential bias caused by ignoring this effect may be large. One reason for this is that outcomes may not be observed for students in private schools, while the arrival of a mainstreamed student causes peers to become more likely to attend these schools. This is also a common problem in the broader education literature, where private school outcomes may not be observed. Another concern is that school switching may be disruptive to students' education irrespective of its cause, making it difficult to separate the direct effects of the arrival of a mainstreamed student from indirect effects caused by school switching.

In Appendix C, we bound estimated effects on students' absences from school given best- and worst-case estimates of outcomes for students in private schools and students who make unexpected school switches. The bounds are quite wide, with effects ranging from overall null impacts to impacts that are nearly 4 times larger than those estimated by a naive approach. Our bounding exercises show that the estimated effects of mainstreaming on student outcomes are highly sensitive to assumptions about true switching effects.

Instead, we attempt to assess impacts on students' outcomes by creating subgroups in which we limit patterns of school switching. A key difficulty with separating the direct impacts of the arrival of a mainstreamed student from the indirect impacts caused by school switching is that we cannot identify individuals who are induced to switch schools due to the mainstreamed student's arrival. That is, while we can observe which students switch schools and that the rates of switching are higher among exposed students, for any particular exposed student, we cannot tell whether they would have switched schools anyway or whether this switch was induced by the exposure. Instead, we attempt to identify impacts by separating students who never change schools from those who do. We study two subgroups: (1) students who never change schools and (2) students who change schools at least once but always remain in the public school system. By focusing only on students who always remain in public education, we also limit the bias caused by attrition to private schools, but note that impacts on these subgroups do not fully represent the impacts on all students. These subgroups are substantial: students who never change schools represent 43 percent of student-year observations in our data, while students who change schools but never attend a private school represent 27 percent. The remainder are students who ever attend a private school.

A concern for conducting this kind of analysis is the choice of comparison group. When constructing a subgroup of exposed students that is restricted based on school switching behavior, an appropriate comparison group can be never-exposed students who are similarly limited. Thus, we compare exposed students who never change schools to their never-exposed peers who also never change schools, and analogously for students who do change schools.

We discuss this issue in more detail in Appendix D.

The results indicate that students who have changed schools experience increases in absences, while students who never change schools do not. In both cases, there is some evidence of significant pre-trends; however, the event-study plots in Figure 7 show a clear difference in patterns between the two groups. The increase in absences for those who change schools is particularly large in secondary school years, which are the years with the highest student mobility. Under the assumption that negative impacts would otherwise not be concentrated in upper grades, this suggests that worsening outcomes for students who change schools are driven at least in part by broader disruptions in the school environment due to changing schools, which is concentrated in the secondary school years. Effects on chronic absenteeism (more than 20 unexcused absences in a year) are qualitatively similar and appear in the event study plots in Appendix Figure A.4.

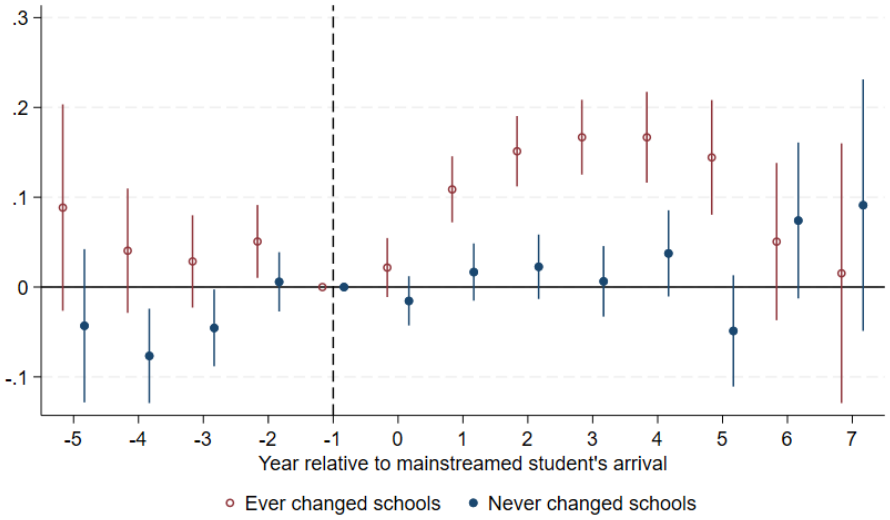
The results in Figure 7 must be interpreted carefully for at least two reasons. First, while we compare exposed students who change schools to never-exposed students who also change schools, it is still possible that exposed students change schools more often or at different times than their never-exposed peers. Second, it is important to remember that students who are induced to change schools by the arrival of a mainstreamed student are a selected group. For example, it is possible that students who are induced to change schools are also those who are exceptionally sensitive to the class environment. Because of this, any difference in impacts between those who have changed and who have never changed schools may be due either to the composition of the groups or due to the choice to change schools, and we cannot separate these effects.

This analysis highlights an important point about the impacts of mainstreaming on students' outcomes: for at least some students, there are no negative impacts from exposure to a mainstreamed student. The results are precisely estimated and we can rule out even small increases in absences. For example, the 95% confidence interval of the post-period impact on absences for those who never change schools rules out effects larger than 0.04 days per year, or a 5.8 percent increase.

7.2 Impacts on test scores

Having studied impacts on students' absences, for which data are available annually, we next turn to estimating impacts on test scores. In Denmark, all public school students take national exams in Danish and mathematics in grades 2, 4, 6, and 8 (for Danish) and grades 3 and 6 (for Math). We standardize scores to have mean zero and standard deviation 1 within grade and year. In order to create an annual series for the difference-in-differences analysis,

Figure 7: Event-study ATT impact on unexcused absences for students who never and ever change schools



Notes: The figure plots event-study estimates from the main difference-in-differences specification, estimating the impacts of the arrival of a mainstreamed student on a student's unexcused absences, split by whether the student never changes schools or does change schools but always remains in public schools. 95% confidence intervals are shown, with standard errors clustered at the individual level.

we carry forward students' test scores from year to year. This may delay the time at which we observe impacts as effects on learning in earlier grades may not appear until an updated test score is taken. However, an annual series is crucial for the difference-in-differences setup in order for all students to have observations in the pre-period (i.e., at time $g-1$).

The results again highlight that there is no evidence of negative impacts of mainstreaming for students who never change schools. Table 4 shows that, among those who never change schools, there is little evidence of a decline in test scores due to the arrival of a mainstreamed student. In Columns (1) and (2), we estimate precise null effects of less than 0.005 SD on both Math and Danish scores in grades 2–6. At the same time, there is evidence of a negative impact of 0.048 SD in Math scores in secondary school. This could be caused by negative spillovers of their peers' mobility in these grades (e.g., Gibbons and Telhaj (2011) and Hanushek, Kain, and Rivkin (2004)). However, we find no evidence of negative impacts in secondary school in Danish scores, a precisely estimated null that rules out, with 95% confidence, any effect larger than 0.03 SD.

The negative impacts on test scores are larger when looking at students who do change schools at least once. Among this group, there is evidence of a small decline in test scores in both Math and Danish and across all grades. In both cases, the declines in test scores do not improve towards the later grades, when student mobility is higher. The event study plots

Table 4: Impacts on test scores by subgroup

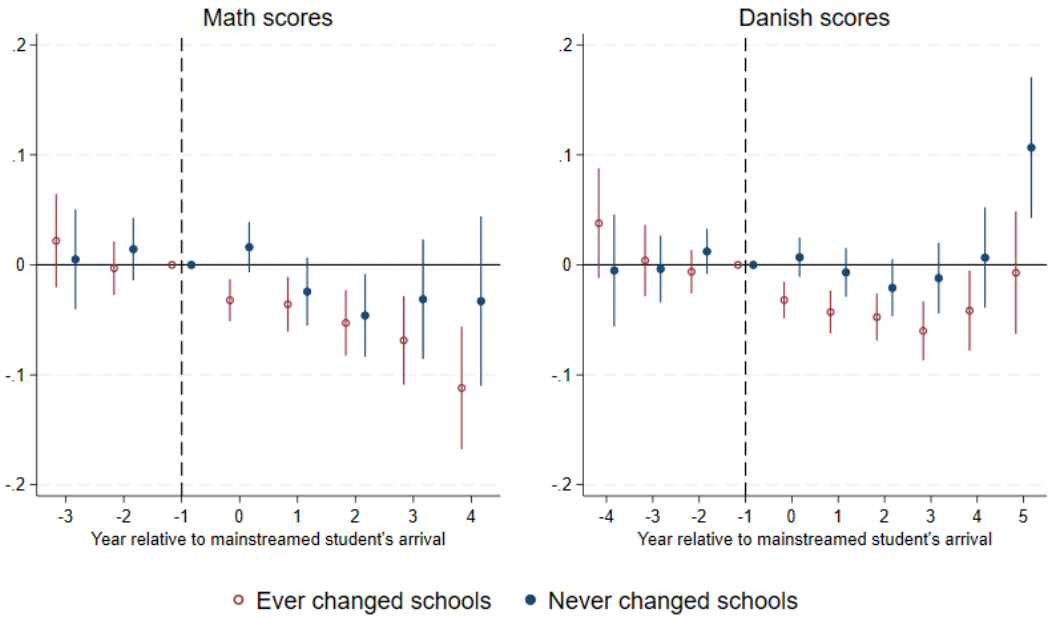
	(1) Math, never changed	(2) Danish, never changed	(3) Math, ever changed	(4) Danish, ever changed
Pre-period avg.	0.010	0.001	0.009	0.012
	(0.016)	(0.014)	(0.014)	(0.014)
Post-period avg.	-0.024	0.013	-0.060***	-0.039***
	(0.016)	(0.012)	(0.013)	(0.011)
C-S aggregation	-0.018	-0.000	-0.048***	-0.042***
	(0.013)	(0.010)	(0.011)	(0.009)
Event-study coefficient, period 0	0.016	0.007	-0.032***	-0.032***
	(0.012)	(0.009)	(0.010)	(0.008)
Avg. effect in grade 2-6	0.005	0.005	-0.038***	-0.042***
	(0.013)	(0.011)	(0.011)	(0.011)
Avg. effect in grade 7-9	-0.048**	-0.003	-0.060***	-0.044***
	(0.021)	(0.013)	(0.016)	(0.011)
Observations	480497	805732	521059	800121
Dep. var. mean	0.137	0.141	0.043	0.043

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on a student's test scores, split by whether the student never changes schools or does change schools but always remains in public schools. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts under different assumptions to address selection of students in the sample. Columns (1) and (2) present impacts on Math and Danish scores among those who never change schools. Columns (3) and (4) present impacts on Math and Danish scores among those who do change schools. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

in Figure 8 show that there is weak evidence that test scores recover substantially over time for this group; in fact, Math test scores appear to worsen over time. Again, these negative impacts are not necessarily direct, first-order impacts of the arrival of the mainstreamed student. They may also be due to test score declines that occur when students change schools for any reason. The size of this impact is on par with or somewhat larger than estimates from past work estimating the negative effects of mainstreamed students on test scores in Denmark without accounting for switching in detail (Rangvid 2019).

These results highlight a key point that is missing from prior literature. If our results showing no impacts for students who never change schools extend to that context, the estimated negative impacts may be driven in large part by selection out of the classroom. In

Figure 8: Event-study ATT impact on test scores for students who never and ever change schools



Notes: The figure plots event-study estimates from the main difference-in-differences specification, estimating the impacts of the arrival of a mainstreamed student on a student's test scores, split by whether the student never changes schools or does change schools but always remains in public schools. 95% confidence intervals are shown, with standard errors clustered at the individual level.

fact, Kristoffersen et al. (2015) find results similar to ours by showing that, when studying only those who do not move schools, the negative impacts of a student with a psychiatric diagnosis become insignificant and much smaller (about half the size of the main estimates, or about 0.01 SD).

Having shown these impacts, we also study patterns of heterogeneity in test score impacts according to students' performance before the arrival of the mainstreamed student. On one hand, students in the lower half of the test score distribution may benefit from the adaptations in the class environment that occur when a mainstreamed student arrives, but, on the other hand, they may also be more sensitive to disruptions in the class environment. In Appendix E, we show that, for students who do change schools, negative impacts are largely concentrated among students below the median in the pre-period test score distribution.

7.3 Impacts on social and academic wellbeing

What might drive these impacts on student test scores? Common narratives include that the arrival of new students in a classroom, and disabled students in particular, might cause social difficulties, disruptions to the classroom environment, or distractions for the teacher. The data available in Denmark are unique because they allow us to study this by including students' self-reported measures of wellbeing. In this section, we find no evidence that the arrival of a mainstreamed student causes social tensions in a classroom. However, there is some evidence of increased difficulty concentrating, which (as with all results in this section) may be either a direct or indirect impact of the arrival of the mainstreamed student.

We focus on two kinds of wellbeing outcomes: social wellbeing, which concerns relationships with peers at school, and academic wellbeing, which concerns a student's perception of their academic experience. Outcomes are measured on a 1–5 Likert scale, with higher values always indicating greater wellbeing. These outcomes are self-reported and available for students in grade 4 and above in public schools.

A first concern about introducing disabled students into the classroom might be an increase in bullying or other social tensions among classmates. We find little evidence that this is the case. Table 5 presents outcomes on measures of social tensions, for each subgroup of students. Column (1) shows that students who never change schools are slightly less likely to report being bullied following the arrival of a mainstreamed student. This would be consistent with, for example, increased monitoring of bullying by school staff to ensure the smooth integration of the mainstreamed student. Overall, these students report a 1.7 percent improvement in this measure. Columns (2) and (3) show that students who never change schools do not experience major overall changes in their reports of whether they

bullied another student and whether they are lonely. There is evidence of a slight increase in loneliness in secondary school. Under the assumption that direct impacts on social experiences caused by the arrival of a mainstreamed student in the class would not be larger during secondary school than other years, this offers suggestive evidence that the disruption caused by peers' switching may have a spillover impact for this group.

For students who do change schools, there is also weak evidence of changes in these social wellbeing outcomes. Despite potential disruptions to their school environment, these students do not report any changes in being bullied or bullying another student (Columns 4 and 5). As for students who never change schools, they also report a slight increase in loneliness, which is concentrated in secondary school years (Column 6).

Table 5: Impacts on social wellbeing by subgroup

	(1) Was not bullied, never changed	(2) Did not bully others, never changed	(3) Not lonely, never changed	(4) Was not bullied, ever changed	(5) Did not bully others, ever changed	(6) Not lonely, ever changed
Pre-period avg.	-0.038	-0.027	-0.003	-0.040	-0.060**	0.002
	(0.023)	(0.021)	(0.021)	(0.026)	(0.024)	(0.025)
Post-period avg.	0.063***	0.006	-0.018	0.002	0.005	-0.025*
	(0.014)	(0.013)	(0.013)	(0.015)	(0.014)	(0.015)
C-S aggregation	0.065***	0.006	-0.019	-0.001	0.003	-0.024*
	(0.013)	(0.012)	(0.013)	(0.015)	(0.014)	(0.014)
Event-study coefficient, period 0	0.083***	0.016	-0.006	0.006	0.014	0.004
	(0.015)	(0.013)	(0.014)	(0.017)	(0.015)	(0.016)
Avg. effect in grade 2-6	0.089***	0.024*	0.003	0.005	0.028*	-0.006
	(0.015)	(0.014)	(0.015)	(0.018)	(0.017)	(0.018)
Avg. effect in grade 7-9	0.044***	-0.000	-0.034**	-0.012	-0.009	-0.037**
	(0.015)	(0.013)	(0.014)	(0.016)	(0.014)	(0.015)
Observations	694913	698140	690272	594576	596858	591024
Dep. var. mean	3.714	4.254	3.737	3.652	4.233	3.748

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on social wellbeing outcomes, split by whether the student never changes schools or does change schools but always remains in public schools. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts under different assumptions to address selection of students in the sample. All outcomes are reported on a scale of 1–5, with higher values representing higher wellbeing. Column (1) presents estimates on a student's experience of not being bullied, among those who never change schools; Column (2) on the student not bullying others, among those who never change schools; Column (3) on a student not feeling lonely, among those who never change schools. Columns (4), (5), and (6) present impacts on the same outcomes among those who do change schools. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

A second concern about mainstreaming disabled students is that they might disrupt other students' learning by distracting them or redirecting the teacher's attention. To study this, in Table 6, we study students' self-reports about whether they are making good academic

progress, whether they are able to concentrate in class, and whether their teacher is able to calm the class following a disruption. Column (1) shows that, among those who never change schools, the arrival of a mainstreamed student has no impact on perceptions of academic progress, consistent with little impact on test scores. In the secondary school years, this group experiences a decline in being able to concentrate and in the teacher's ability to calm the class (Columns 2 and 3). These effects are small, with a 1.3 percent decline in the ability to concentrate in the secondary school years. The average effects are driven by effects in grade 7-9.

Those who do change schools experience small negative effects on all three of these measures. Consistent with impacts on their test scores, Column (4) of Table 6 shows that they are less likely to feel they are making good academic progress. Columns (5) and (6) show negative impacts on concentration and the teacher's ability to control the class, with magnitudes similar to those for students who never change schools.

These results highlight that, for students who do not change schools, the arrival of a mainstreamed student does not appear to cause social tensions among peers and, in grades 4-6, does not cause any increase in difficulty concentrating. On the other hand, students who do change schools experience worsened social and academic wellbeing. Although we cannot causally identify the impact of the decision to switch schools, the fact that negative impacts are largest in secondary school, when student mobility is also highest, suggests that they may be driven in part by this increased mobility.

Table 6: Impacts on self-perceived academic progress by subgroup

	(1) Academic progress, never changed	(2) Concentrate, never changed	(3) Teacher control, never changed	(4) Academic progress, ever changed	(5) Concentrate, ever changed	(6) Teacher control, ever changed
Pre-period avg.	-0.010	-0.014	-0.029	-0.002	-0.011	-0.008
	(0.013)	(0.022)	(0.019)	(0.015)	(0.027)	(0.023)
Post-period avg.	-0.003	-0.049***	-0.038***	-0.022**	-0.036**	-0.047***
	(0.009)	(0.014)	(0.012)	(0.009)	(0.016)	(0.013)
C-S aggregation	-0.000	-0.044***	-0.033***	-0.023***	-0.033**	-0.044***
	(0.008)	(0.013)	(0.011)	(0.009)	(0.015)	(0.013)
Event-study coefficient, period 0	0.003	-0.008	-0.013	-0.023**	-0.015	-0.013
	(0.008)	(0.015)	(0.012)	(0.010)	(0.017)	(0.014)
Avg. effect in grade 2-6	0.009	-0.004	-0.004	-0.023**	-0.033*	-0.016
	(0.009)	(0.015)	(0.013)	(0.011)	(0.019)	(0.016)
Avg. effect in grade 7-9	-0.001	-0.054***	-0.041***	-0.025***	-0.035**	-0.052***
	(0.009)	(0.014)	(0.013)	(0.009)	(0.016)	(0.014)
Observations	681878	691699	682119	583797	591965	584960
Dep. var. mean	4.833	4.023	3.889	4.808	3.965	3.826

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on academic wellbeing outcomes, split by whether the student never changes schools or does change schools but always remains in public schools. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts under different assumptions to address selection of students in the sample. All outcomes are reported on a scale of 1–5, with higher values representing higher wellbeing. Column (1) presents estimates on self-reported academic progress, among those who never change schools; Column (2) on the student's concentration in class, among those who never change schools; Column (3) on a student's reports that the teacher could calm the class after a disruption, among those who never change schools. Columns (4), (5), and (6) present impacts on the same outcomes among those who do change schools. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

8 Separating peer effects from school mobility effects

Having shown that being exposed to mainstreamed disabled students significantly impacts the mobility of other students in the school, we next present two analyses aiming to separate the direct impacts of mainstreaming from second-order impacts driven by student mobility. In both analyses, we argue that the patterns shown in previous sections are unlikely to be driven by direct peer effects of mainstreaming and may instead be explained by other factors, like the presence of adjustment costs around school changes. First, we present difference-in-difference results around the time of a school change to examine patterns in students' outcomes following a school change, and we find that students changing schools experience temporary worsening of outcomes in the short term following a change. Second, we take a structural approach to decompose changes in test score outcomes and find that changes

in test scores are neither explained by direct effects from the exposure to a mainstreamed student nor changes in school quality, but rather by unobservable effects, such as adjustment costs. As a final analysis, we illustrate how researchers can use a set of assumptions about relations between treatment, school moving, exposure effects, and test score outcomes to evaluate counterfactual mainstreaming policies and their heterogeneous impacts across student groups. In a set of simulations we show that even large increases in mainstreaming have only small effects of up to 0.02 SD on test scores on children from families with and without tertiary educated parents as both the change in school value-added from moving and contemporaneous exposure effects are limited.

8.1 The disruptive impacts of school switching

In this section, we study the trajectories of students around the time of a school change to quantify the potential disruption associated with school changes for the students making them. We do this using a similar difference-in-differences event study design as above, but centered on the timing of a student's first school change movement. Following the approach by Callaway and Sant'Anna (2021), we estimate

$$\widehat{ATT}(g,t) = \mathbb{E}_n[Y_{it}|G_i = g] - \mathbb{E}_n[Y_{it}|G_i = \infty] - \Big(\mathbb{E}_n[Y_{ig-1}|G_i = g] - \mathbb{E}_n[Y_{ig-1}|G_i = \infty]\Big)$$

where Y_{it} is the outcome of individual i in grade t and G_i is the first grade in which an individual moves schools. We estimate these for two groups of students: those who are never exposed to a mainstreamed student and those who have been previously exposed to a mainstreamed student. As in prior sections, we focus only on students who are always observed in public schools to avoid making assumptions about outcomes for those who attrit to private schools.[16] Standard errors are clustered at the individual level.

There are two substantial caveats hindering a fully causal interpretation of these parameters. The first is that we do not suppose most school changes to be random; in fact, we suppose that many school changes are undertaken due to a student struggling at a particular school and in the hopes of improvement in outcomes. Because of this, we do not expect to find evidence of parallel pre-school-change trends for students who change schools relative to those who do not. Instead, in order to compare counterfactual trajectories, we estimate and project a linear trend from the pre-school-change period into the post-school-change period. We consider this trend to be one possible counterfactual trajectory for these students had they not changed schools.

[16] Our bounding approach from Section 7 can also be applied to evaluate the importance of assumptions on outcomes under attrition.

The second caveat is that our estimation of long-term outcomes in this analysis is necessarily limited by the length of time for which we observe students at school. At maximum, a school change for any given student can only be observed from grade 1 to grade 9. In this analysis, our time frame is further limited by studying school changes made by students after exposure to a mainstreamed student. For example, suppose a student is exposed to a mainstreamed student in grade 2, and changes schools 2 years later, in grade 4. For this student, we can only observe post-school-change outcomes, at maximum, in grades 5-9. As such, we caution that estimates of long-term effects are necessarily noisy and estimated on a selected sample of students who are possibly exposed early in their school careers, change schools soon after, and observed in later grades.

Given these caveats on causal interpretation, Figure 9 shows evidence of two key facts: (1) school changes and switches appear to come with substantial increases in student absences, indicating possible adjustment costs, and (2) there is little evidence that school changes improve long-term outcomes for exposed students.

First, we find that students changing schools experience substantial, often temporary, worsening in outcomes immediately after the change.[17] We find that, in the year after a school change, both exposed and never-exposed students experience an increase in unexcused absences from school (Figure 9 and Appendix Table A.4). This increase is significantly above what would be expected from simply projecting pre-trends, and, for exposed students, amounts to an average increase of 0.23 days (21%) relative to the year before the school change.[18] With respect to test scores, Appendix Tables A.4 and A.5 show less clear evidence of this decline. This may be because, as noted above, our "carried forward" measure of test scores, while necessary for our staggered difference-in-differences setup, results in estimates that are less sensitive to immediate changes. We address impacts on test scores in more detail in the next section. Even so, without more granular test score data and given a body of evidence showing a causal link between absences and student achievement,[19] this immediate increase in student absences is a meaningful indicator of disrupted student wellbeing.

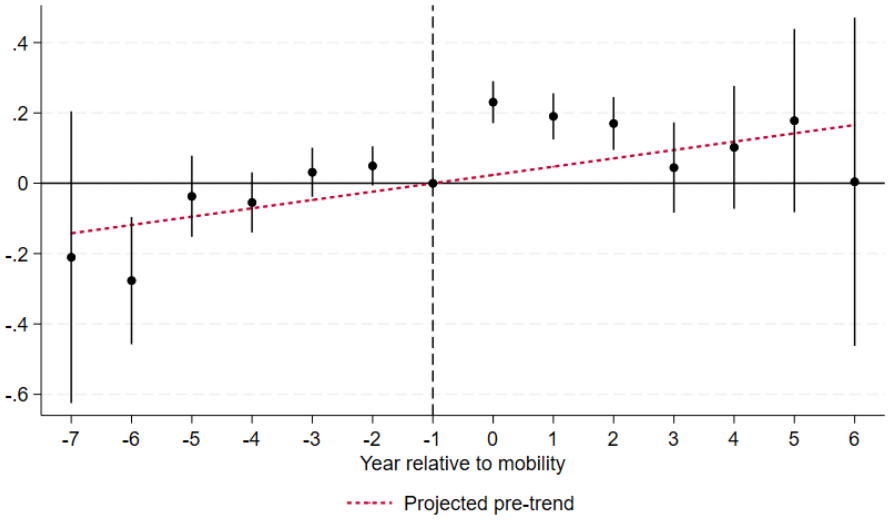
The second striking pattern is that we find limited evidence that school changes improve outcomes for exposed students. In the long term, exposed students experience no significant reduction in unexcused absences from school following a school change. Instead, absences remain elevated relative to their pre-school-change levels. Appendix Table A.4 also shows

[17] In interpreting these results, we highlight that, given the magnitudes of our estimates above, even among exposed students, many or most school changes are not induced by exposure to the mainstreamed student, but rather would have happened regardless.

[18] Appendix Table A.5 shows that this immediate increase in absences also occurs following unexpected school switches, but is more in line with pre-trends indicating increased absences before that kind of switch.

[19] See Cattan et al. (2023), Gottfried (2009), Liu, Lee, and Gershenson (2021), and Yaow et al. (2026)

Figure 9: Event-study ATT impact on unexcused absences among exposed students, according to time of school change



Notes: The figure plots event-study estimates from a difference-in-differences event study around the timing of a student's school change, estimating the patterns in unexcused absences before and after a student changes schools. 95% confidence intervals are shown, with standard errors clustered at the individual level.

no evidence of improved test scores following an exposed student's school change. When studying only unexpected school switches instead as in Appendix Table A.5, exposed students experience some increase in Danish scores, though we again caution that these long term results may be driven by changes in sample composition and are subject to similar concerns about selection of the appropriate comparison group discussed in Appendix D.

To more carefully account for the role of student mobility in shaping test score outcomes for exposed students, in the next section, we take a more structural approach to decomposing the relative impacts of various channels on test score outcomes.

8.2 Decomposing difference-in-differences into mechanisms

We have documented that both student mobility and exposure are important for understanding mainstreaming. But the estimates presented up until now leave open a policy-relevant question: how much did mainstreaming impact test scores on aggregate, and how important are different mechanism channels for these impacts? The estimated reduced form exposure effects alone cannot answer this, because the aggregate response depends on three things: 1) the share of students exposed, 2) the size of the effect on exposed students, and 3) the degree to which students move to schools of higher or lower quality.

In this section, we derive a decomposition for understanding how different channels con-

tribute to the observed change between a control and a treated group. We decompose the effect of being exposed to a mainstreamed disabled student into three channels: effects of changes in *school quality*, direct effects of the *exposure*, and the third channel being *unobserved factors*. We focus this decomposition on a 2×2 difference-in-differences comparison, across the control and treated groups, before and after time of treatment. The decomposition is based on the observation that aggregate change in an outcome or a given channel can be decomposed into a term with an explicit covariance that captures differential changes across treated and control groups. This exercise gives insights into potential effects of mainstreaming policies and reactions that arise through channel of aggregate change, and the observed covariance with being treated, i.e., sharing a classroom with a mainstreamed student.

Concretely, we model the student test score outcome Y as a linear function of school quality, exposure to mainstreaming, and other residual factors captured in U, suppressing i and t subscripts for notational simplicity. Let Q be a measure of school quality defined as the effect of the school on outcomes, and let D be an indicator for being or having been in a class that receives a mainstreamed disabled student. We refer to $D = 1$ as initially treated and $D = 0$ as initially untreated to distinguish between current exposure and treatment status. We define $\tau(X)A$ to be the effect of current exposure $A \in \{0, 1\}$ on the outcome, where $\tau(X)$ represents a contemporaneous exposure effect which may vary with individual-level characteristics X. We can then write the change in expected outcome across two periods as a linear function of changes in observed and unobserved variables:

$$\mathbb{E}[\Delta Y] = \underbrace{\mathbb{E}[\Delta Q]}_{\text{School quality}} + \underbrace{\mathbb{E}[\Delta \tau(X)A]}_{\text{Exposure effect}} + \underbrace{\mathbb{E}[\Delta U]}_{\text{Unobserved}} \tag{1}$$

For each channel, we can decompose the changes into a change for the untreated and a remaining gap to the change in the channel average. We derive the decomposition in Appendix F.2. Using school quality as an example, we can write:

$$\underbrace{\mathbb{E}[\Delta Q]}_{\text{Channel total}} = \underbrace{\mathbb{E}[\Delta Q | D = 0]}_{\text{Change for untreated}} + \underbrace{\frac{Cov(D, \Delta Q)}{1 - p}}_{\text{Remaining gap}} \tag{2}$$

The average change for the untreated shows the change in the channel for those not treated. A negative covariance between exposure to mainstreaming (D) and the change in school quality (ΔQ) implies that exposed students experience negative school quality shifts relative to their not-exposed peers. The decomposition lets us compare the descriptive importance of separate channels, the school quality and the direct effects of exposure to a mainstreamed student, relative to other variables in shaping differentials in total changes in outcomes across

the exposed and not-exposed students.

We implement the decomposition to illustrate the importance of school changes (via school value added) and contemporaneous exposure effects for test score outcomes by using our data on public school students.[20] We estimate the school quality using a school value added model, while we estimate the individual-level exposure effect with a linear heterogeneous treatment effect model where treatment effects depend on covariates. Appendix F.3 contains a detailed description of the estimation procedures. As in the previous analysis, we are limited by non-observable test-scores in private schools and therefore focus on students in public schools both when estimating the school quality and heterogeneous exposure effects. Using the estimated parameters, we decompose the change in expected outcome into the two channels by calculating the empirical equivalents of the estimands in Equation 2 for a sample of students with standardized 4th and 6th grade Danish national test scores. We describe the decomposition estimators and sample in Appendix F.4. This allows us to do simple pre-post comparisons analogous to a two-period difference-in-differences design.

Table 7: Channel-specific decomposition of aggregate change in test scores

	Decomposition of $\mathbb{E}[\Delta Z]$		
Channel	$\mathbb{E}[\Delta Z \mid D=0]$	$\frac{Cov(\Delta Z, D)}{1-p}$	Total
Estimate			
School quality	0.00113	-0.00026	0.00088
Contemporaneous exposure	-0.00014	-0.00029	-0.00043
Unexplained change	0.01537	-0.00033	0.01504
Estimate relative to total test score change			
School quality	0.073	-0.017	0.057
Contemporaneous exposure	-0.009	-0.019	-0.028
Unexplained change	0.992	-0.021	0.971
Test score change	0.01636	-0.00088	0.01548

Notes: Estimates measured in standard deviation of the test score distribution. Share ever exposed $p = 0.065$. Treatment D is defined as being in a class when a returner enters classroom. Contemporaneous exposure effect can be non-zero for those not yet treated if they enter a classroom where a returner has previously entered.

The decomposition estimates, shown in Table 7, suggest that, overall, the changes in school quality and direct exposure to mainstreamed students explain little of changes in test scores. In column (1), the table shows, for students who were not in a classroom with a

[20] Again, private school students can be included in the analysis through a bounding approach as in Section 7.

disabled student arriving by grade 6, how much of the trend in test scores can be explained by each channel. We find that 99% of the change in Danish test scores between grades 4 and 6 cannot be attributed to changes in school quality or contemporaneous exposure to a disabled student for these students. Column (2) shows that the link between these channels and being exposed to a disabled student is very small. In this sense, we find that exposed students do not move into schools of substantially different quality. We also do not find that the exposure channel suggests any substantive negative effects of being exposed to mainstreamed disabled students. As in our reduced form results, these findings suggest little evidence of a substantial reduction in test scores driven by contemporaneous exposure to a disabled student. Since it captures only short-term changes in test scores between grades 4 and 6, this analysis also highlights little evidence of any immediate declines in test scores for exposed students.

8.3 Mainstreaming as a policy tool

A policy-relevant question is what the expected consequences for test scores would be if policymakers increased (or decreased) the total share of learning disabled students who are mainstreamed. Spending on special needs classes is often a large share of the school budgets, making mainstreaming a relevant lever for policymakers. In this section, we conduct a simulation exercise based on a model of test score outcomes, school quality, and exposure effects, that highlights the impact of increasing levels of mainstreaming on test scores. As we have shown differential impacts on school switching by socio-economic background, we construct our estimations and simulation to allow for heterogeneous policy impacts across socio-economic background.

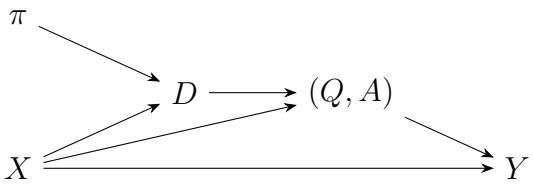
We start by formulating a simple model that allows us to link the effects of various levels of mainstreaming of disabled students, which we refer to as the *policy level* (π) to student test score outcomes via student treatment, school quality, and contemporaneous exposure effects.[21] As in the previous section, we assume that initial exposure to a mainstreamed student and exposure in later years may differ depending on whether or not students decide to change schools. We estimate and simulate our model using within-municipality across-time variation. Danish municipalities have substantive autonomy to set levels of mainstreaming by requiring schools to open or close special education needs classes and also determine school funding across the municipality. Thus, our results can be read as the expected local effects of political changes to the allocation of returning special education needs students.

We model the distribution and coupling between the covariates and outcome so that we

[21] Although we find above that the unobserved channel is the largest in explaining changes in test scores, we naturally cannot model this.

can construct estimates of conditional mean functions. Figure 10 illustrates the *directed acyclic graph* (DAG) of this model, where Q is the value-added and A is exposure to a learning-disabled student. A student's covariates X and municipal level of mainstreaming (π) together shape whether the student is exposed to a mainstreamed student (D). In this way, students with different background characteristics may have different rates of exposure even when facing the same municipal level of mainstreaming. Together with background covariates, exposure to a mainstreamed student affects a student's mobility, as captured by the student's school quality (Q) and whether they are currently exposed to a mainstreamed student (A). Finally, we assume that the outcome Y depends on the background characteristics, the quality of the school the student attends, and any effects of contemporaneous exposure. Thus, in our simulations we do not account for the unobserved factors that may drive outcomes, which we discussed in section 8. We add municipal fixed effects in our estimations to account for the fact that movement across schools, and therefore changes in school quality and contemporaneous exposure, is geographically constrained.

Figure 10: Graphical representation of the model



Notes: The figure summarizes the factorization used in the structural policy simulation. The policy level π and student covariates X determine treatment status D, while treatment and covariates determine school quality Q and exposure A. Student outcomes Y are then modeled as a function of school quality, exposure, and covariates.

Using the associated factorized densities,[22] we can define a set of objects that can be used to reason about the effects of increasing or decreasing the degree of mainstreaming. Additionally, we split students into two groups by socio-economic status (high and low), denoted $\tilde{X}$, to capture how these policy effects differ across student background. The four objects are: 1) the treatment probability $\mathbb{P}(D = 1|\tilde{X}, \pi)$; 2) the expected school quality $\mathbb{E}[Q|\tilde{X}, \pi]$; 3) the expected exposure effect $\mathbb{E}[\hat{\tau}(X) \cdot A|\tilde{X}, \pi]$; and 4) the expected outcome $\mathbb{E}[Y|\tilde{X}, \pi]$.

We can use empirical estimates of these models to simulate the effects of different policy levels. The main source of variation exploited to pin down parameters is within-municipality variation of mainstreaming levels across time. Appendix Section F.5.1 describes the simulation sample, consisting of all individuals for whom we can construct school quality variables

[22]The densities associated with the DAG can be factorized as $f(Y, Q, A, D, X, \pi) = f(Y|Q, A, X)f(Q, A|D, X, \pi)f(D|X, \pi)f(X)f(\pi)$.

and the municipal variation in share of exposed students that we use to estimate the regression models. Appendix F.5.2 contains a discussion of how we estimate the models using the school quality and heterogeneous contemporaneous exposure effects we construct from the previous decomposition analysis (see Appendix F.3).

Implementing our simulations, we show that even at high levels of mainstreaming, effects on test scores are limited, but that the effects vary by socio-economic background. We consider a simulation policy experiment varying the mainstreaming exposure level (π) between 0 and 20%. Figure 11 shows the predicted average share of students who ever meet a mainstreamed student, the contemporaneous exposure effect, school quality, and predicted average test scores. We construct the averages separately for high and low SES students, where low SES students have parental income below the average income. School quality and exposure effects are measured in terms of the outcome, standardized Danish test scores. The figure shows that the contemporaneous exposure channel has nearly no effect on students' outcomes, even at high levels of mainstreaming. In contrast, the predictions suggest that school switching is a possible channel for small negative effects to arise at high levels of mainstreaming. Overall, low SES students attend worse schools than high SES students in general, and both high and low SES students sort to worse schools when the policy exposure level increases. At the highest levels of exposure to mainstreamed students, the decline in school quality amounts to around a 0.02 decline in standard deviations of test scores for low SES students and around 0.03 for high SES students. The total predicted effect on test scores is a decline of around 0.02 standard deviations for the low SES students and 0.03 for the high SES students. This is driven by the school quality shifts, as the exposure channel still has a negligible effect on student outcomes.

9 Conclusion

In this paper, we highlight the importance of considering students' school choices in evaluating the peer effects of a change in classroom composition. Using a difference-in-differences design and studying the arrival of a disabled student in a mainstream classroom, we show that peers exposed to a mainstreamed disabled student are much more likely to switch schools than never-exposed students in the same grade. We focus on the increase in school switching driven by unexpected switching, that is, students who switch schools when their peers do not and without changing home addresses. Impacts on school switching are especially large once a student reaches secondary school. Exposed students are also more likely to change to private schools and to schools where their peers have higher socioeconomic backgrounds. The highest mobility occurs when the mainstreamed student has an intellectual or developmental

disability.

This kind of selective switching complicates the estimation of impacts on students' academic and wellbeing outcomes, with prior work often finding negative effects on mainstreamed students' peers' test scores. We compare naive estimates of impacts on student attendance with those after imputing values for students in private schools, for whom outcomes are not observed. Regardless of assumption for private school students, there is some evidence of an increase in unexcused absences from school. However, noting that school switching itself can be disruptive, we also bound outcomes for students who have switched schools and find that the estimated effects range from small increases occurring only in secondary school to large impacts for all grades.

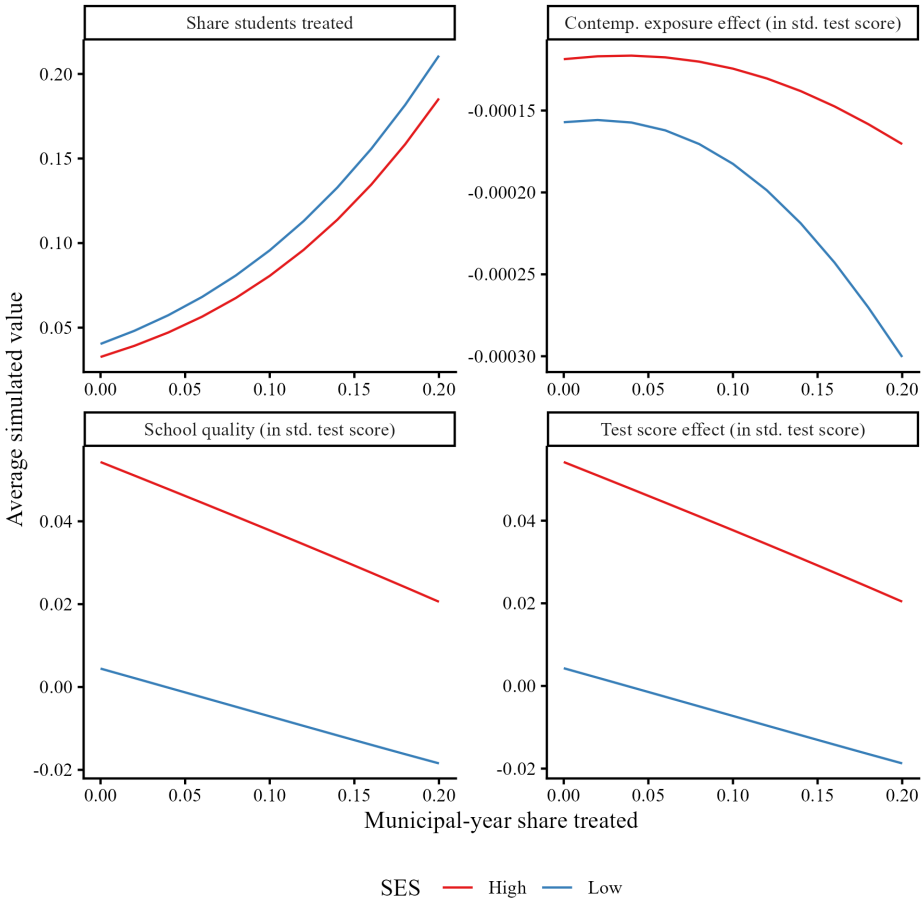
We study impacts on students' outcomes for two subgroups of interest: students who never change schools and students who do change schools, but always remain in public education. Although this analysis cannot separate the direct impacts of the arrival of a mainstreamed student from indirect impacts caused by school switching, it provides three important insights. First, it shows that there is a substantial group of students — those who never change schools — who are not negatively impacted in their attendance or test scores by the arrival of a mainstreamed student. Second, negative impacts for those who do change schools are concentrated in the secondary school years, when mobility is higher, providing suggestive evidence that the disruptions caused by school changes may play an important role in generating these negative effects. Finally, it highlights that the disruptions that come with the arrival of a mainstreamed student do not generate large changes in the social environment of a classroom, but they do cause slight negative impacts on students' concentration in secondary school.

Having shown these differing impacts for the two subgroups, we then use both reduced form and structural approaches to argue that these are unlikely to be due to direct effects of the arrival of a disabled student on these subgroups, but rather are plausibly associated with the disruptiveness of school switching.

This paper highlights the importance of considering mainstreaming policies' unintended general equilibrium effects. When a mainstreaming policy is implemented, especially in a context with school choice, peers' reactions to this policy may cause unintended effects. Further, to the extent that school switching is selective and concentrated in certain types of schools, these reactions may generate increased inequality or segregation. Future research should investigate methods to further unpack the relationship between school switching, classroom disruptions, and students' outcomes. It should also consider what drives this switching behavior, such as potential misperceptions about the impacts of including disabled students in mainstream classrooms. Policy should ensure that mainstreamed students and

their peers are properly supported during classroom adjustments.

Figure 11: Policy simulation and impact



Notes: The figure plots average simulated model variables and outcomes under counterfactual municipal-year mainstreaming exposure level π, separately for high- and low-SES students. The simulation sample consists of students who are in public schools and for whom we can estimate school quality. The panels show the simulated treatment rate, contemporaneous exposure effect, school quality, and predicted test score as π varies from 0 to 20 percent. We construct simulated current exposure and school quality based on individual characteristics and the policy variables using estimated models. We then construct the predicted outcome as the sum of individual exposure effect and school quality.

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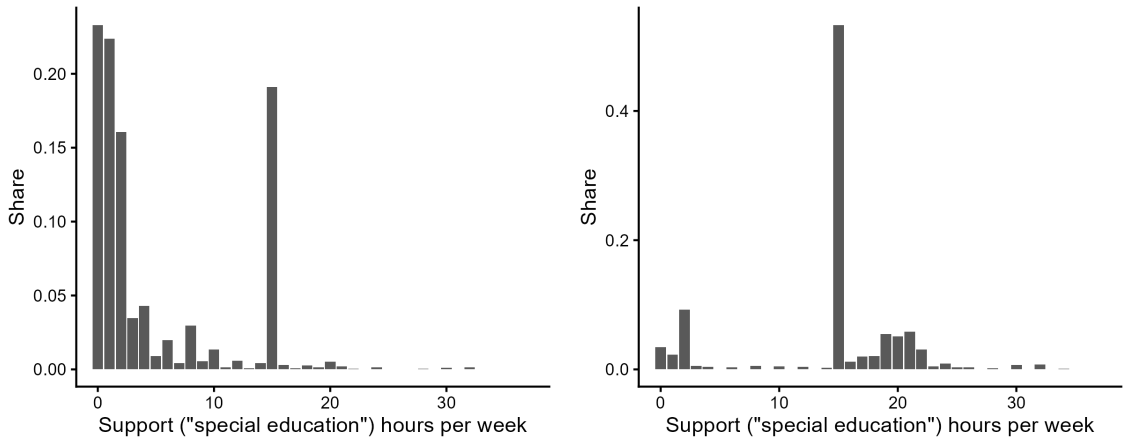
A Supplementary tables and figures

Table A.1: Statistics on education for disabled students, 2021

	Denmark	Norway	Germany
% of students with "special education needs"	7.7%	7.8%	6.4%
% of all students in mainstream classes	93.6%	99.3%	96.9%
% of "special education needs" students in mainstream schools	59.9%	96.9%	51.5%
% of "special education needs" students in mainstream classes	10.0%	91.2%	51.5%

Notes: The table shows statistics on "special education needs" students (using the language associated with the original data source) in Denmark, Norway, and Germany. Source: European Agency for Special Needs and Inclusive Education (2023).

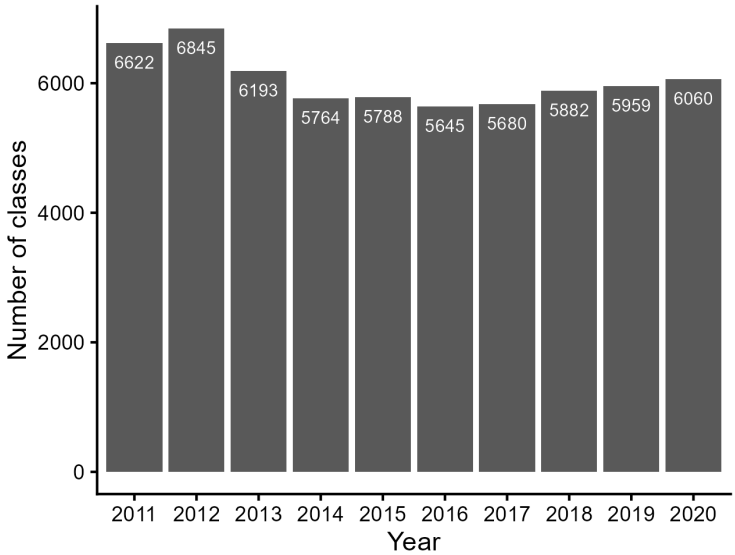
Figure A.1: Weekly hours of support disabled students receive by type of class attended



(a) Mainstream classes (b) Segregated classes

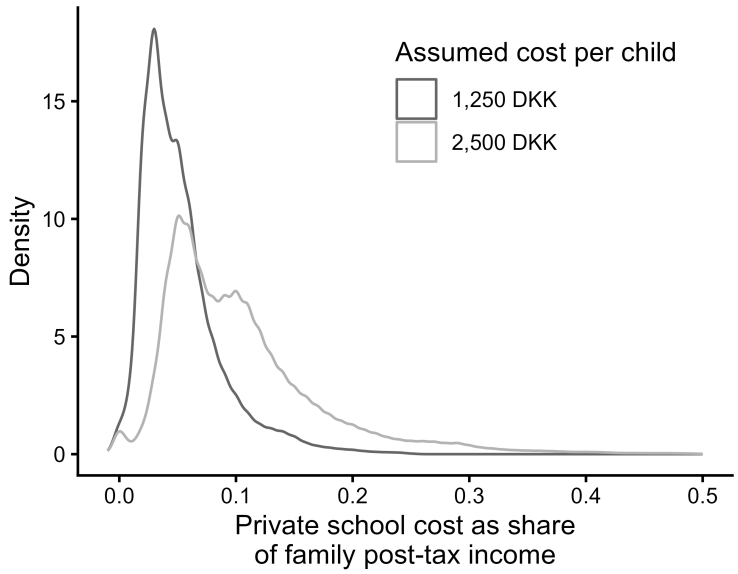
Notes: This figure shows the distribution of hours of support received by disabled students by type of classroom attended: segregated or mainstream classroom. We round hours to whole numbers. The figure shows the distribution for 2011, where all hours are recorded.

Figure A.2: Number of segregated classes with majority disabled students by year



Notes: The figure plots the number of segregated classrooms, where disabled students are a majority, observed in each year. The general trend is stable from a small decrease from the early years 2011 – 2013.

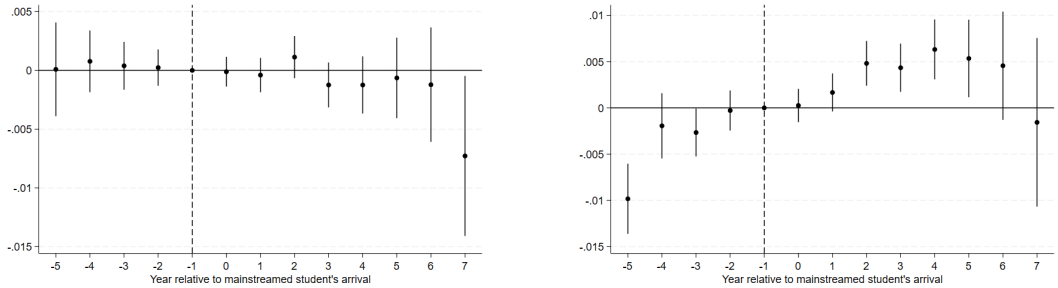
Figure A.3: The distribution of costs of sending all children aged 6-15 to private schools as share of family post-tax income, 2015.



Notes: The figure plots the number of families by the cost of sending all children aged 6-15 to private school as a share of the family's total disposable income. The data comes from the Danish administrative registers. The sample consists of all families observed in the Danish registers in 2015 with at least one child aged 6-15.

Figure A.4: Event-study ATT impact on chronic unexcused absences for students who never and ever change schools

(a) Students who never change schools (b) Students who ever change schools



Notes: The figure plots event-study estimates from the main difference-in-differences specification, estimating the impacts of the arrival of a mainstreamed student on whether a student is chronically absent from school, split by whether the student never changes schools or does change schools but always remains in public schools. 95% confidence intervals are shown, with standard errors clustered at the individual level.

Table A.2: Heterogeneity in unexpected school switching by student characteristics

	(1) Switched school, parental income above median	(2) Switched school, parental income below median	(3) Switched school, grade 2 Danish above median	(4) Switched school, grade 2 Danish below median
Pre-period avg.	-0.003	-0.006***	-0.003*	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Post-period avg.	0.008***	0.003	0.001	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
C-S aggregation	0.006***	0.002	0.001	0.004**
	(0.001)	(0.001)	(0.001)	(0.002)
Event-study coefficient, period 0	0.002	0.002	-0.000	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Avg. effect in grade 2-6	0.005***	0.003**	0.001	-0.000
	(0.001)	(0.002)	(0.002)	(0.002)
Avg. effect in grade 7-9	0.007***	0.002	0.001	0.008***
	(0.002)	(0.002)	(0.002)	(0.003)
Observations	1539264	1427885	1179699	890324
Dep. var. mean	0.033	0.038	0.031	0.036

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on school switching outcomes according to the characteristics of the student switching. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Columns (1) and (2) show impacts on having ever made an unexpected school switch among students who parents' income is above and below the median, respectively. Columns (3) and (4) show impacts by whether the student's grade 2 Danish test score was above or below the median. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

Table A.3: Heterogeneity in unexpected school switching by school characteristics

	(1) School avg. inc. above median	(2) School avg. inc. below median	(3) School avg. math above median	(4) School avg. math below median	(5) School avg. Danish above median	(6) School avg. Danish below median
Pre-period avg.	0.001	-0.005***	-0.001	-0.004***	-0.002**	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Post-period avg.	0.003***	0.001	0.000	0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
C-S aggregation	0.003***	0.000	-0.000	-0.001	-0.001*	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Event-study coefficient, period 0	-0.000	0.002**	0.000	0.000	-0.001*	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Avg. effect in grade 2-6	0.003***	0.001	-0.001	0.001	-0.001	0.001*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Avg. effect in grade 7-9	0.004***	-0.000	-0.000	-0.002***	-0.001	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	3025346	3025346	2555055	2555055	2686786	2686786
Dep. var. mean	0.020	0.016	0.011	0.011	0.011	0.011

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on unexpected school switching outcomes according to the characteristics of the school to which students switch. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-difference design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts on a given outcome, with Columns (1) and (2) showing impacts on making an unexpected school switch into a school where students' average parental income is above and below the median, respectively; Columns (3) and (4) into a school where students' average Math scores are above and below the median, respectively; and Columns (5) and (6) into a school where students' average Danish scores are above and below the median, respectively. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

Table A.4: Patterns in attendance and test scores following a school change

	Exposed			Never exposed		
	(1)	(2)	(3)	(4)	(5)	(6)
	Unexcused absences	Danish score	Math score	Unexcused absences	Danish score	Math score
Pre-period avg.	-0.083*	0.013	-0.008	-0.026**	0.030	0.058***
	(0.048)	(0.031)	(0.029)	(0.011)	(0.040)	(0.011)
First year after switch						
Estimate	0.231***	-0.011	-0.006	0.074***	0.059	0.147***
	(0.030)	(0.016)	(0.020)	(0.009)	(0.055)	(0.028)
Projected estimate	0.024*	-0.004	0.003	0.006**	-0.008	-0.019***
	(0.014)	(0.010)	(0.012)	(0.003)	(0.011)	(0.004)
Difference	0.207***	-0.007	-0.010	0.068***	0.068	0.167***
	(0.036)	(0.021)	(0.025)	(0.010)	(0.049)	(0.027)
Post-period average						
Average estimate	0.131**	-0.001	-0.026	0.089***	-0.043	0.271***
	(0.058)	(0.033)	(0.032)	(0.011)	(0.183)	(0.077)
Projected estimate	0.095*	-0.015	0.010	0.029**	-0.034	-0.068***
	(0.055)	(0.036)	(0.035)	(0.012)	(0.046)	(0.012)
Difference	0.037	0.014	-0.035	0.060***	-0.009	0.339***
	(0.081)	(0.048)	(0.046)	(0.017)	(0.172)	(0.075)
Observations	186834	121218	90612	2731985	1294250	804489
Dep. var. mean	1.076	0.027	0.016	0.749	0.111	0.113

Notes: The table shows estimated trends in attendance and test scores around the timing of a student changing schools, split by whether a student was previously exposed to a mainstreamed student or never exposed. The rows present aggregated $ATT(g,t)$ estimates using the difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively, and first period effects are event-study estimates for the first year after the change. Estimates are presented and compared with a linear projection of pre-trends before the school change. Each column presents impacts on a given outcome for the given subgroup, with Columns (1), (2), and (3) presenting unexcused absences and test scores for previously-exposed students and Columns (4), (5), and (6) presenting those outcomes for never-exposed students. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

Table A.5: Patterns in attendance and test scores following an unexpected school switch

	Exposed			Never exposed		
	(1)	(2)	(3)	(4)	(5)	(6)
	Unexcused absences	Danish score	Math score	Unexcused absences	Danish score	Math score
Pre-period avg.	-0.149*	0.086	0.059	-0.227***	0.058***	0.040**
	(0.077)	(0.065)	(0.053)	(0.040)	(0.016)	(0.016)
First year after switch						
Estimate	0.160**	0.046	-0.020	0.087***	-0.030	-0.078***
	(0.070)	(0.031)	(0.037)	(0.017)	(0.063)	(0.024)
Projected estimate	0.050*	-0.029	-0.024	0.057***	-0.017***	-0.013**
	(0.026)	(0.022)	(0.021)	(0.010)	(0.005)	(0.005)
Difference	0.110	0.074*	0.004	0.030	-0.013	-0.064***
	(0.082)	(0.041)	(0.047)	(0.021)	(0.064)	(0.025)
Post-period average						
Average estimate	0.069	0.119**	0.007	0.137***	0.035	-0.010
	(0.109)	(0.051)	(0.050)	(0.019)	(0.031)	(0.016)
Projected estimate	0.198*	-0.100	-0.071	0.255***	-0.058***	-0.040**
	(0.103)	(0.076)	(0.064)	(0.045)	(0.016)	(0.016)
Difference	-0.129	0.219**	0.078	-0.118**	0.094***	0.030
	(0.156)	(0.093)	(0.083)	(0.049)	(0.035)	(0.023)
Observations	187822	121916	91081	2893666	1415040	870290
Dep. var. mean	1.077	0.025	0.014	0.767	0.103	0.102

Notes: The table shows estimated trends in attendance and test scores around the timing of an unexpected school switch, split by whether a student was previously exposed to a mainstreamed student or never exposed. The rows present aggregated $ATT(g,t)$ estimates using the difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively, and first period effects are event-study estimates for the first year after the change. Estimates are presented and compared with a linear projection of pre-trends before the school change. Each column presents impacts on a given outcome for the given subgroup, with Columns (1), (2), and (3) presenting unexcused absences and test scores for previously-exposed students and Columns (4), (5), and (6) presenting those outcomes for never-exposed students. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

B Additional analysis of switching towards and away from mainstreamed students

In this appendix, we address additional results relating to students' school switching. First, we further break down students' school and class switching according to whether they switch into a school or classroom with a mainstreamed student or away from a mainstreamed student. We show that much of school and classroom switching is explained by switches away from mainstreamed students. Second, we show that students who are exposed to a mainstreamed student are not more likely to move homes, indicating that they do not have otherwise higher rates of mobility.

Given evidence that more students switch schools after exposure to a mainstreamed student, we highlight that much of this switching represents students leaving the classroom to move away from the mainstreamed student. In other words, there are two distinct but important types of unexpected school switching that may be induced by the arrival of a mainstreamed student: switching schools away from the mainstreamed student and switching schools towards a mainstreamed student. We define students as switching schools away from a mainstreamed student if they switch schools and leave a classroom with a mainstreamed student. Switching school towards a mainstreamed student occurs when a student switches schools and ends up in a new class where there is (also) a mainstreamed student.[23]

First, we find evidence that a large share of school switching is driven by switching away from a mainstreamed student, as shown in Column (1) of Table B.1. On average following exposure to a mainstreamed student, the probability of switching schools away from a mainstreamed student increases by 2.6 percentage points, with effects up to 3.2 percentage points in the secondary school grades. At the same time, we find much smaller impacts on the probability of switching towards a mainstreamed student, with positive effects mainly concentrated in period 0, that is, concurrent with the mainstreamed student's arrival (Column 2). This result shows that a number of new students enter the school along with the mainstreamed student (approximately 1 in 30 students). However, the small magnitudes of the increase in overall school switching in period 0 documented in Table 2 suggest that this influx of new students is not larger than the new students arriving in classrooms with no mainstreamed student.

Looking at the dynamics of these effects over time relative to treatment helps to show how switching away from a mainstreamed student diverges from switching towards these students.

[23]These are not exhaustive, as students can also switch schools with no mainstreamed student in either the class they leave or join. They are also not mutually exclusive, as a student could leave a classroom with a mainstreamed student and enter another classroom with a mainstreamed student.

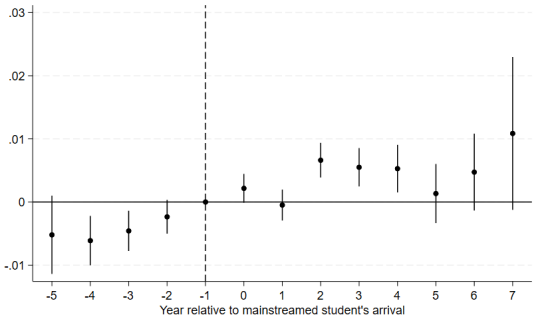
Figure B.1 plots the event study coefficients for each outcome. The overall increase in school switching is shown in panel (a), where the increase in school switching rates over time is clear and concentrated in years 2–4 following exposure to the mainstreamed student. Meanwhile, the probability of switching schools away from a mainstreamed student increases in the year following the arrival of the mainstreamed student and remains elevated over time (panel b). In contrast, in panel (c), the probability of switching schools towards a mainstreamed student increases concurrently with the mainstreamed student's arrival and remains elevated but smaller in the following periods. One interpretation for this finding is that mainstreamed students may arrive simultaneously with other students. This could happen if the receiving school is a receiving school for feeder schools and the other arriving students switched school at the end of grade levels at their original school. In fact, the average rate of school switching in our data, irrespective of exposure, is about 3.6 percent, nearly matching the magnitude of this estimate. This suggests that mainstreamed students are accompanied by other students joining the classroom at approximately the expected rate.

One may be concerned that these effects are induced by the comparison: never-exposed students might be less likely to switch away from a mainstreamed student because they are less likely to be in a class with a mainstreamed student in the first place. However, we argue that the effect is not purely mechanical. First, we note that a student is only considered exposed to a mainstreamed student if they are in the class with the mainstreamed student at the time of the student's arrival and potential disruption to the class environment. Therefore, it is possible (although relatively rare) for never-exposed students to join a classroom that already has a mainstreamed student (and thus to also move away from a mainstreamed student). Second, we further investigate these results by studying switching towards and away from any kind of disabled student, including students who will later receive support for a disability but are not yet. Even when including students who are never-exposed, the average probability of switching schools away from such a student is 1.1 percent in each year, substantially far from zero. In Columns (3) and (4) of Table B.1, we show estimated effects of a similar magnitude and with the same dynamics when using these variables. Still, we caveat this analysis by noting that an analysis of the probability of switching away or towards a mainstreamed student must necessarily incorporate both the likelihood of being in a class with a mainstreamed student and the likelihood of switching schools.

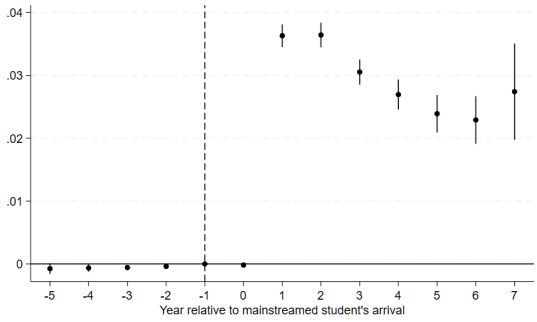
Along with school switching, we study switching between classes within a school. Again, as with school switching, we find larger effects on switching away from a mainstreamed student (Columns (1) and (3) in Table B.2) and positive but somewhat smaller effects on switching into a class with a mainstreamed student (Columns (2) and (4)).

Another possible concern about our analysis of student school mobility is that students

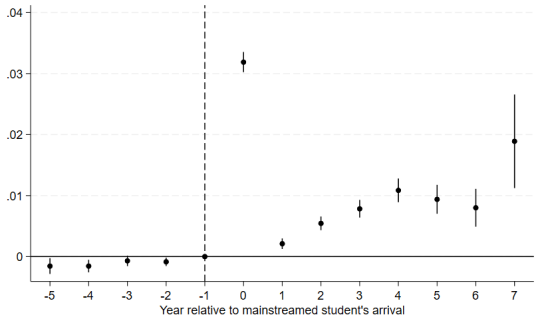
Figure B.1: Event-study ATT impacts on student mobility between classes and schools



(a) Switched school unexpectedly



(b) Switched school, away from mainstreamed student



(c) Switched school, towards mainstreamed student

Notes: The figure plots event-study estimates from the main difference-in-differences specification, estimating the impacts of the arrival of a mainstreamed student on unexpected school switching outcomes. Panel (a) shows impacts on a contemporaneous indicator for having unexpectedly switched schools coming into the current school year. Panel (b) shows impacts on unexpectedly switching away from a class with a mainstreamed student and panel (c) shows impacts on unexpectedly switching into a class with a mainstreamed student. 95% confidence intervals are shown, with standard errors clustered at the individual level.

Table B.1: Impacts on unexpected school switching

	(1) Has switched school	(2) Switched school	(3) Switched school away from mainstreamed	(4) Switched school towards mainstreamed	(5) Switched school away from SE student	(6) Switched school towards SE student
Pre-period avg.	-0.002	-0.005***	-0.001**	-0.001***	0.001	0.002*
	(0.002)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Post-period avg.	0.018***	0.005***	0.026***	0.012***	0.022***	0.009***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
C-S aggregation	0.012***	0.004***	0.026***	0.012***	0.020***	0.009***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Event-study coefficient, period 0	-0.001	0.002*	-0.000	0.032***	0.001	0.023***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Avg. effect in grade 2-6	0.004***	0.004***	0.017***	0.016***	0.012***	0.011***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Avg. effect in grade 7-9	0.020***	0.004***	0.032***	0.010***	0.028***	0.008***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	4663802	3025347	2698888	3025347	3024570	3025347
Dep. var. mean	0.090	0.036	0.002	0.004	0.011	0.009

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on unexpected school switching outcomes. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2-6 and 7-9 presenting aggregated effects for students currently in the given grades. Each column presents impacts on a given outcome, with Columns (1) and (2) showing impacts on unexpected switching into or away from a school where they are in a class with a mainstreamed student, and Columns (3) and (4) on unexpected switching into or away from a school where they are in a class with a student who ever participates in special education. Standard errors clustered at the individual level are shown in parentheses. * p <0.1, ** p <0.05, *** p <0.01

who are in classrooms with returning students have otherwise high rates of mobility in later school years. To address this, we also study several other measures of student mobility. As noted above, the unexpected school switches we focus on exclude school changes associated with an address change. We expect that these changes will be largely driven by changes that are orthogonal to the student's school experience, such as a change in family situation or parent's job.[24] We find little evidence of increases in the probability of changing schools concurrently with moving addresses after the mainstreamed students enter the individual's class (Table B.3, Column (1)). Instead, we find small negative effects concentrated in secondary school. Finally, we find no clear evidence of effects on the probability of having moved addresses without changing schools (Column (2)). This indicates that exposure to mainstreamed students is not associated with a higher overall propensity to move addresses in later grades.

[24]However, we also note previous work in the Danish context has shown evidence of manipulation of addresses in the high school admissions process after attending mandatory primary education (Bjerre-Nielsen et al. 2023).

Table B.2: Impacts on class switching

	(1) Has switched class	(2) Switched class	(3) Switched class away from mainstreamed	(4) Switched class towards mainstreamed	(5) Switched class away from SE student	(6) Switched class towards SE student
Pre-period avg.	0.001	0.000	-0.000	-0.000	0.000*	0.001**
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Post-period avg.	0.001**	-0.000	0.002***	0.001***	0.001***	0.001***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
C-S aggregation	0.001	0.000	0.002***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Event-study coefficient, period 0	0.000	0.000	0.000	0.003***	0.000*	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Avg. effect in grade 2-6	0.000	-0.000	0.001***	0.002***	0.001**	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Avg. effect in grade 7-9	0.001**	0.000	0.002***	0.001***	0.002***	0.001***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	4663802	3027465	2700550	3027465	3026421	3027465
Dep. var. mean	0.008	0.003	0.000	0.000	0.001	0.001

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on unexpected class switching outcomes. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts on a given outcome, with Columns (1) and (2) showing impacts on unexpectedly switching into or away from a class with a mainstreamed student, and Columns (3) and (4) on unexpectedly switching into or away from a class with a student who ever participates in special education. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

Table B.3: Impacts on other kinds of student mobility

	(1)	(2)
	Moved, changed school	Moved, same school
Pre-period avg.	-0.001	-0.001
	(0.001)	(0.002)
Post-period avg.	-0.002***	0.003*
	(0.001)	(0.002)
C-S aggregation	-0.001**	0.002
	(0.001)	(0.001)
Event-study coefficient, period 0	0.001	0.002
	(0.001)	(0.002)
Avg. effect in grade 2-6	-0.001	0.002
	(0.001)	(0.002)
Avg. effect in grade 7-9	-0.002**	0.002
	(0.001)	(0.001)
Observations	4122187	4105697
Dep. var. mean	0.01	0.07

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on other student mobility outcomes. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts on a given outcome, with Column (1) showing impacts on having ever changed both schools and addresses and Column (2) on having changed addresses but not schools. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

C Bounding effects on attendance given school switching

In this appendix, we construct best- and worst-case bounds on the impacts of the arrival of a mainstreamed student on peers' attendance, taking into account the problem of missing data for private school students. We also construct bounds given that we do not observe counterfactual outcomes for students who switched schools, had they not switched schools. We find that the bounds are wide and that a naive approach to estimating the direct effect of the arrival of a mainstreamed student on peers' attendance may have large biases given the switching we observe.

In Column (1) of Table C.1, we estimate the impacts on a student's number of unexcused absences naively, without accounting for school switching patterns. Here, the results suggest a substantial increase in the number of unexcused absences, with the arrival of a mainstreamed student causing, on average, an increase of 0.064 days, or 8 percent relative to a mean of 0.79 days per year. This increase is especially large once students reach secondary school.

The potential size of the bias in estimated impacts due to school switching is large. In Columns (2) and (3), we address the problem of missing data for private school students by bounding their possible outcomes in the spirit of Horowitz and Manski (2000). In the best case, all private school students have zero yearly absences (Column (2)). In the worst case, all private school students have a very high number of unexcused absences per year. We assign this worst-case value to be 4, approximately the 90th percentile observed in our sample (Column (3)). In both cases, Columns (2) and (3) show that a mainstreamed student's arrival leads to an increase in unexcused absences. However, the magnitudes of these estimates differ substantially: assuming the worst-case for private school students leads to an impact that is nearly 6 times larger than assuming the best-case. Further, when assuming the best-case, the impact occurs entirely in the grades 7–9, while the worst-case shows increases in grades 2–6 as well. This highlights the difficulty of identifying an impact and its magnitude given the unobserved data for private school students.

Further, the results thus far show that the increase in absences is concentrated in the upper grades, but this is also the period in which students are more likely to have switched schools. Research has shown that school switching can come with adjustment costs for students and substantially impact test scores (e.g., Schwartz, Stiefel, and Cordes (2017) and Hanushek, Kain, and Rivkin (2004)), although some papers find null effects of switching schools (Hill et al. 2023). To the extent that school switching causes an increase number of unexcused absences due to this kind of adjustment cost, the impacts of the arrival of a

mainstreamed student will not be identified separately from the school switching cost.

To understand the bias potentially introduced by the disruption associated with switching schools, Columns (4) and (5) impute the best-case and worst-case outcomes as described above for students who have switched schools. Again, in both cases, there is evidence of an increase in absences caused by the arrival of a mainstreamed student particularly in secondary school, but the magnitudes of this effect are difficult to quantify given school switching. In the best case, the estimated effect is very small and insignificant, only 0.013 days per year when using the Callaway and Sant'Anna aggregation and less than 0.001 per year when using a simple average of all post-periods. The Callaway and Sant'Anna aggregated effect is less than 1/4 the size of the naive estimate of 0.057 days per year. Meanwhile, in the worst case (Column (5)), effects are nearly 15 times larger than in the best case.

Finally, in Column (6), we attempt to assess impacts for students whose switching may be limited by focusing on students in isolated schools to get a sense of the role of selection. We construct a measure of isolated schools in which switching between schools may be more difficult, making selection less strong. For each school, we measure the percentage of students moving in and out of the school in unexpected changes in a year. Averaging the inflow and outflow, we call a school isolated if the average unexpected flow is less than 2.2 percent of students. This corresponds to the 10th percentile of schools. In our setting, school switching may be more difficult, for example, because a school is located farther from large city centers. In this subsample, Column (6) of Table C.1 shows that effects are more imprecisely estimated but generally lie between the estimated bounds in Columns (4) and (5). In grades 2–6, effects are null, closer to the best-case bound. However, in grades 7–9, increases in absences are quite large.

Table C.1: Bounding impacts on unexcused absences from school

	(1) Illegitmate absences	(2) Unexcused absenses, private school 0	(3) Unexcused absences, private school 90th pctile.	(4) Unexcused absenses, switched school 0	(5) Unexcused absences, switched school 90th pctile.	(6) Unexcused absences, isolated school
Pre-period avg.	0.014	-0.006	0.046***	0.009	0.008	-0.018
	(0.014)	(0.013)	(0.015)	(0.013)	(0.015)	(0.045)
Post-period avg.	0.064***	0.039***	0.230***	0.000	0.242***	0.186***
	(0.012)	(0.011)	(0.012)	(0.010)	(0.012)	(0.067)
C-S aggregation	0.057***	0.043***	0.185***	0.013	0.193***	0.070*
	(0.010)	(0.009)	(0.010)	(0.008)	(0.010)	(0.037)
Event-study coefficient, period 0	-0.010	-0.011	0.031***	-0.017*	0.034***	-0.069**
	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)	(0.028)
Avg. effect in grade 2-6	0.017	0.008	0.088***	-0.004	0.096***	-0.021
	(0.011)	(0.010)	(0.011)	(0.010)	(0.011)	(0.030)
Avg. effect in grade 7-9	0.089***	0.068***	0.273***	0.021**	0.278***	0.231***
	(0.012)	(0.011)	(0.012)	(0.010)	(0.012)	(0.074)
Observations	3592375	4420346	4420346	4456577	4456577	121705
Dep. var. mean	0.791	0.640	1.408	0.564	1.569	0.292

Notes: The table shows bounds estimated impacts of the arrival of a mainstreamed student on a student's unexcused absences. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts under different assumptions to address selection of students in the sample. Column (1) presents naive estimates of impacts on unexcused absences from school, while Column (2) and Column (3) impute unobserved outcomes for individuals in private schools by assuming no absences or a high number of absences, respectively. Columns (4) and (5) make the same imputations for students who have made an unexpected school switch. Column (6) studies students in isolated schools, as defined in the text, where the impact of switching may be limited. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

D Choice of control group when splitting by student mobility

In our analysis, we attempt to better understand the impacts of the arrival of a mainstreamed student by considering impacts for subgroups according to their school switching behavior. In this appendix, we discuss considerations in defining suitable subgroups and comparisons for them.

Table D.1 presents results for several different approaches to attempting to separate students who switch from those who do not. In Column (1), results are presented for students who make an unexpected switch following the arrival of a mainstreamed student, compared to all never-exposed comparison group students. The results show that these students experience an increase in absences relative to comparison group students, across grades. Column (2) shows the results for those who do not make such a switch, relative to the same comparison group, and shows evidence that those who do not switch experience increased absences only in secondary school.

However, a potential fault with making this comparison is that we split students according only to unexpected school switching. However, for example, students entering into secondary school may be more likely to change schools in reaction to the arrival of a mainstreamed student, even if this switch is not unexpected because many of their peers change schools at this time as well. To address this, Columns (3) and (4) perform the same analysis splitting the students exposed to a mainstreamed student according to whether they made any school change following the arrival of a mainstreamed student. We find that students who make a school change experience no increase in absences, while those who do not make such a change do experience an increase in absences.

Another concern is that the analysis compares those who do not make a school change after the arrival of a mainstreamed student with a comparison group of never-treated students who may make such changes for other reasons. For example, imagine a comparison group (never-exposed) student who is unhappy in a particular school and so moves to another school, where their outcomes improve. This cannot be a suitable comparison for a student exposed to a mainstreamed student who is similarly unhappy, for reasons unrelated to mainstreaming, but does not change schools (e.g., due to location or monetary constraints). In other words, the parallel trends assumption requires that, in the absence of mainstreaming, the outcomes of never-exposed and exposed students would have followed the same trends. But if we condition on exposed students not changing schools for any reason, while not placing such a restriction on never-exposed students, there is likely a violation of the parallel trends assumption.

Columns (5) and (6) address this by also restricting the comparison group of students. This is our preferred specification. Column (5) studies students who make at least one school change over the period in which we observe them, but who are never in a private school (removing the kind of attrition bias described in Appendix C). Column (6) studies students who never make such a change.

Table D.1: Impacts on unexcused absences from school by subgroup

	(1) Has unexp. switched post-exposure	(2) Has not unexp. switched post-exposure	(3) Has changed post-exposure	(4) Has not changed post-exposure	(5) Ever changed	(6) Never changed
Pre-period avg.	-0.010	-0.035	-0.016	0.004	0.052**	-0.040**
	(0.019)	(0.065)	(0.018)	(0.028)	(0.024)	(0.020)
Post-period avg.	0.060***	0.236***	0.006	0.165***	0.103***	0.023
	(0.014)	(0.045)	(0.015)	(0.022)	(0.021)	(0.018)
C-S aggregation	0.063***	0.189***	0.013	0.142***	0.116***	0.012
	(0.011)	(0.036)	(0.012)	(0.017)	(0.016)	(0.014)
Event-study coefficient, period 0	0.006	-0.037	-0.017	-0.005	0.022	-0.015
	(0.012)	(0.037)	(0.013)	(0.018)	(0.017)	(0.014)
Avg. effect in grade 2-6	0.031**	0.012	0.002	0.041**	0.049**	0.003
	(0.013)	(0.043)	(0.014)	(0.021)	(0.022)	(0.015)
Avg. effect in grade 7-9	0.089***	0.349***	0.016	0.234***	0.151***	0.024
	(0.013)	(0.043)	(0.015)	(0.021)	(0.019)	(0.019)
Observations	3513342	3327785	3480650	3396260	1209468	1938835
Dep. var. mean	0.789	0.786	0.787	0.788	0.947	0.688

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on unexpected absences for several subgroups as described in the text. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts under different assumptions to address selection of students in the sample. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

E Heterogeneity of test score impacts

An interesting question is whether we observe patterns of heterogeneity in test score impacts according to students' performance before the arrival of the mainstreamed student. On one hand, the arrival of a mainstreamed student might improve some elements of the class environment, such as the methods used by the teacher, that benefit students in the lower half of the test score distribution. On the other hand, students in the lower half of the test score distribution may also be especially sensitive to disruptions.

To study this, we split the sample according to a student's Danish score in grade 2, including only students exposed after grade 2 in the treated group. Table E.1 shows that, among those who do not change schools, any negative impacts on Math scores are concentrated among those with below-median grade 2 test scores. Again, this impact is not necessarily a first-order result of the presence of a mainstreamed student in the classroom. The fact that it occurs only in secondary school grades would be consistent with it being due, at least in part, to disruptions in the class environment caused by higher mobility of peers. Prior work has shown that peer mobility can be more disruptive for low-income students (Hanushek, Kain, and Rivkin 2004). These results also show a small negative impact on Danish test scores

at all grade levels for those with grade 2 Danish scores above the median, suggesting that higher performing students who do not change schools may experience some small negative effects.

Table E.1: Impacts on test scores by grade 2 Danish scores among students who never change schools

	(1) Math, 2nd grade Danish below median	(2) Math, 2nd grade Danish above median	(3) Danish, 2nd grade Danish below median	(4) Danish, 2nd grade Danish above median
Pre-period avg.	-0.008	0.004	-0.013	-0.008
	(0.023)	(0.017)	(0.017)	(0.013)
Post-period avg.	-0.042*	-0.007	0.011	-0.023*
	(0.024)	(0.018)	(0.015)	(0.012)
C-S aggregation	-0.033*	-0.000	-0.001	-0.024**
	(0.019)	(0.015)	(0.012)	(0.010)
Event-study coefficient, period 0	0.000	0.024*	-0.008	-0.008
	(0.017)	(0.013)	(0.011)	(0.008)
Avg. effect in grade 2-6	-0.004	0.026*	-0.006	-0.019**
	(0.019)	(0.015)	(0.011)	(0.009)
Avg. effect in grade 7-9	-0.064**	-0.036	0.014	-0.032**
	(0.032)	(0.024)	(0.019)	(0.014)
Observations	186469	264923	296997	415923
Dep. var. mean	-0.359	0.493	-0.647	0.693

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on test score outcomes among those who never change schools. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts under different assumptions to address selection of students in the sample. Columns (1) and (2) present estimates on Math scores for students with 2nd grade Danish scores below and above the median, respectively. Columns (3) and (4) present the analogous estimates for Danish scores. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

Meanwhile, for students who do change schools, effects are more negative for those with grade 2 scores below the median. The results in Table E.2 show that, especially for Danish test scores, effects are somewhat larger for the students in the lower half of the grade 2 distribution. Again, this result may be caused either by these students being more sensitive to the arrival of the mainstreamed student or more sensitive to other disruptions in the school environment.

Table E.2: Impacts on test scores by grade 2 Danish scores among students who ever change schools

	(1) Math, 2nd grade Danish below median	(2) Math, 2nd grade Danish above median	(3) Danish, 2nd grade Danish below median	(4) Danish, 2nd grade Danish above median
Pre-period avg.	0.013	-0.004	0.009	0.002
	(0.020)	(0.017)	(0.015)	(0.012)
Post-period avg.	-0.056***	-0.050***	-0.071***	-0.022**
	(0.018)	(0.015)	(0.012)	(0.009)
C-S aggregation	-0.041***	-0.037***	-0.064***	-0.024***
	(0.015)	(0.013)	(0.010)	(0.008)
Event-study coefficient, period 0	-0.021	-0.010	-0.035***	-0.009
	(0.014)	(0.011)	(0.009)	(0.008)
Avg. effect in grade 2-6	-0.029*	-0.018	-0.045***	-0.016**
	(0.015)	(0.013)	(0.011)	(0.008)
Avg. effect in grade 7-9	-0.059***	-0.060***	-0.085***	-0.028**
	(0.022)	(0.019)	(0.013)	(0.011)
Observations	214779	267091	299741	364031
Dep. var. mean	-0.414	0.431	-0.633	0.613

Notes: The table shows estimated impacts of the arrival of a mainstreamed student on test score outcomes among those who do change schools but always remain in a public school. The rows present aggregated $ATT(g,t)$ estimates using the main difference-in-differences design, where pre-period and post-period average refer to a simple average of event study estimates for the periods before and after treatment, respectively; C-S aggregation refers to an aggregation of post-period effects according to the weights used by Callaway and Sant'Anna (2021); event-study coefficient, period 0 testing for switching that happens in the same year as the mainstreamed student's arrival; and avg. effect in grade 2–6 and 7–9 presenting aggregated effects for students currently in the given grades. Each column presents impacts under different assumptions to address selection of students in the sample. Columns (1) and (2) present estimates on Math scores for students with 2nd grade Danish scores below and above the median, respectively. Columns (3) and (4) present the analogous estimates for Danish scores. Standard errors clustered at the individual level are shown in parentheses.
* p <0.1, ** p <0.05, *** p <0.01

F Structural analysis

This section describes the data, models, and estimation approaches we use for the structural decomposition and simulation analyses in Sections 8 and 8.3.

F.1 Structural sample

Our structural analysis focuses on subsets of students for whom we can estimate exposure and school quality moving effects. We construct the structural sample from the full panel of students and impose the following restrictions:

- Years between 2012–2019, during which we observe outcomes.

Table F.3: Descriptive statistics for full student panel and structural sample.

	Full panel		Structural sample		
Variables	N	Mean	N	Mean	Normalized diff.
Year	9699857	2013.453	4468224	2014.999	0.486
Grade-level	9683838	4.791	4468224	4.478	-0.106
Female	9678005	0.491	4455449	0.496	0.011
Immigrant (1gen)	9678005	0.033	4455449	0.033	0.000
Immigrant (2gen)	9678005	0.076	4455449	0.078	0.006
Mother ave. std. income	9632562	0.098	4442523	0.131	0.035
Father ave. std. income	9408177	0.151	4356432	0.182	0.029
Special ed. class	6893541	0.026	4468224	0.000	-0.232
Ever in Special education	6893916	0.049	4468224	0.017	-0.184
Exposed - currently	6198080	0.044	4468224	0.032	-0.062
Exposed - previously	5574216	0.079	3984194	0.067	-0.049
Exposed - ever	6821577	0.215	4468224	0.210	-0.011
School - Max grade in school	9515639	8.995	4465330	8.965	-0.031
School - Min grade in school	9515639	0.504	4465330	0.183	-0.194
School - Private	9200720	0.182	4433962	0.181	-0.002

- Grade level 0 through 9.
- School identifier present.
- Student does not attend special education majority class.

Table F.3 shows the number of observations, means and normalized differences in means between the full panel and the structural sample. The structural sample observations are no more likely to be male or immigrant and have parents with similar incomes as the full panel. The structural sample, however, does not contains special education students, and is more likely to contain 0th grade. As expected, because we are missing information on special education status prior to 2011, the structural sample appears about equally likely to be exposed to returning students.

F.2 Derivation of outcome decomposition

We are interested in descriptively decomposing the change in the average outcomes for students who do and do not experience being in a class with a returning student into two main channels, school quality changes and changes in contemporaneous exposure to returning students, as well as other unobserved residual determinants. To construct the decomposition, we start by writing the outcome as a linear function of an individual fixed effect α_i,

school quality Q, heterogeneous exposure effects from contemporaneous exposure (A) captured by $\tau(X)A$, where X is a set of observable individual characteristics, other unobserved determinants U, and a mean zero error term ϵ:

$$Y_{it} = \alpha_i + Q_{it} + \tau(X_i)A_{it} + U_{it} + \epsilon_{it}. \tag{3}$$

Taking expecations and time-wise differences, we get

$$\mathbb{E}[\Delta Y] = \mathbb{E}[\Delta\alpha] + \mathbb{E}[\Delta Q] + \mathbb{E}[\tau(X)\Delta A] + [\Delta U] + \mathbb{E}[\Delta\epsilon], \tag{4}$$

where by assumption both the expected error term ϵ, the difference in the error term, and the student-specific effect are zero. Only the unobserved heterogeneity term, U, school quality changes and exposure effect changes can be correlated with the time-varying treatment variable, D.

We can use the law of total expectation to rewrite the change in expected outcome using the probability of treatment, p, expected changes under no treatment and the covariance between treatment and change in variables:

$$\mathbb{E}[\Delta Y] = \mathbb{E}[\Delta Q|D=0] + \frac{Cov(D,\Delta Q)}{1-p} \tag{5}$$

$$+ \mathbb{E}[\tau(X)A|D=0] + \frac{Cov(D,\tau(X)\Delta A)}{1-p} \tag{6}$$

$$+ \mathbb{E}[\Delta U|D=0] + \frac{Cov(D,\Delta U)}{1-p}. \tag{7}$$

We now illustrate how we arrive at this expression via illustration for a single channel (Q). Derivations for the other channels are analogous. The expected change in school quality, $E[\Delta Q]$, can be rewritten as:

$$\mathbb{E}[\Delta Q] = \mathbb{E}[\Delta Q|D=0]\cdot\mathbb{P}(D=0) + \mathbb{E}[\Delta Q|D=1]\cdot\mathbb{P}(D=1) \tag{8}$$

$$= \mathbb{E}[\Delta Q|D=0]\cdot(1-p) + \mathbb{E}[\Delta Q|D=1]\cdot p. \tag{9}$$

Note that the covariance between treatment and school quality can be written as: $Cov(D,\Delta Q) = \mathbb{E}[D\cdot\Delta Q] - \mathbb{E}[D]\mathbb{E}[\Delta Q]$. Using that D is binary, we can rewrite the first term as $\mathbb{E}[D\cdot\Delta Q] = p\mathbb{E}[\Delta Q|D=1]$, and we can rewrite the covariance as:

$$Cov(D,\Delta Q) = p\mathbb{E}[\Delta Q|D=1] - p\mathbb{E}[\Delta Q] \tag{10}$$

$$= p(1-p)\left(\mathbb{E}[\Delta Q|D=1] - \mathbb{E}[\Delta Q|D=0]\right) \tag{11}$$

Rearranging the terms and using the law of total probability:

$$\mathbb{E}[\Delta Q] = (1-p)\mathbb{E}[\Delta Q|D=0] + p\mathbb{E}[\Delta Q|D=1] \tag{12}$$
$$= \mathbb{E}[\Delta Q|D=0] - p(\mathbb{E}[\Delta Q|D=0] - \mathbb{E}[\Delta Q|D=1]) \tag{13}$$

Finally, we can multiply through the parenthesis with -1 and substitute in the covariance expression of the covariance to arrive at the decomposition expression:

$$\mathbb{E}[\Delta Q] = \mathbb{E}[\Delta Q|D=0] + p(\mathbb{E}[\Delta Q|D=1] - \mathbb{E}[\Delta Q|D=0]) \tag{14}$$
$$= \mathbb{E}[\Delta Q|D=0] + \frac{Cov(D, \Delta Q)}{1-p}. \tag{15}$$

F.3 Estimating school quality and exposure effects

School quality, We want to estimate school quality, the effect of the school over and above the across-sample effects of student background on standardized Danish test scores, using estimated coefficients from regressing test scores onto school fixed effects together with year fixed effects and binary control variables for student background. For student i in school s in year t, we estimate school fixed effects using the linear projection

$$y_{ist} = \beta_1^{FE} exposure_{ist} + X'_{ist}\beta_2^{FE} + \mu_s + \mu_t + \varepsilon_{ist},$$

where μ_s is the set of school fixed effects, μ_t are year fixed effects, ε_{ist} is the projection error for the observation, $exposure_{ist}$ is an indicator for whether the individual is currently exposed to a returning student in their classroom. We include the control for exposure to account for differential exposure probabilities across schools, which we subsequently want to model separately from the school fixed effects. The vector of other control variables, X_{ist}, includes individual and parental background controls for gender, being a returner, being a first generation immigrant, binned mother's income and binned father's income. The school estimations capture the true school effects if the controls adequately capture characteristics that drive student sorting across schools and may create correlations between unobserved student characteristics and conditional school average test scores.

Schools in the sample. The estimation procedure uses test scores for students in public schools. Table F.4 shows summary statistics including the normalized difference in means for all schools appearing in the structural sample and for the schools that enter the school quality estimation sample. The estimation sample schools consists almost entirely of public schools, and start at lower grade levels. They also have around 5 more students per cohort,

Table F.4: Descriptive statistics for schools in full and decomposition sample.

	All schools		Estimation schools		
Variables	N	Mean	N	Mean	Normalized diff.
Private school	2280	0.379	1321	0.000	-1.104
Minimum grade in school	2280	1.398	1321	0.064	-0.579
Minimum grade in school	2280	8.886	1321	8.569	-0.214
Average students per cohort	2280	41.238	1321	45.988	0.172
Average students in school	2280	324.817	1321	437.465	0.449
Average classes per cohort	2280	2.397	1321	2.760	0.244
First year observed	2280	2012.291	1321	2012.113	-0.190
Last year observed	2280	2018.525	1321	2018.501	-0.016
Share students receiving special education	2280	0.035	1321	0.031	-0.066

0.4 more classes per cohort, and around 110 more students at the school in any given year. This partly reflects that some private schools, called "Efterskoler", only offer 9th and 10th grade. The full sample and the structural estimation sample of schools have about the same share of students receiving special education.

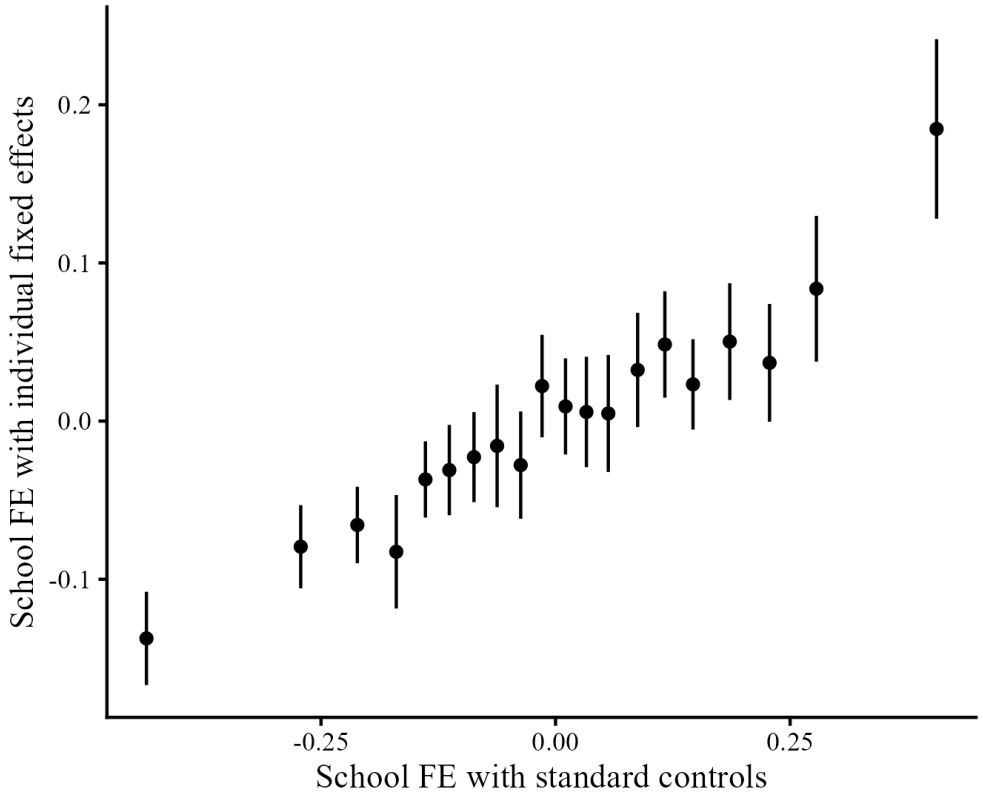
Alternative estimation approaches. Our estimation approach uses variation in test scores across students for the full sample of students for whom we observe national test scores. Another alternative is to estimate the effect of schools while controlling for student fixed effects, and thus identify school effects from students moving across schools. However, using this estimation approach results in qualitatively similar school fixed effects. Figure F.1 shows the binned mean of school fixed effects obtained from estimating equation F.3 with individual fixed effects on the second axis by the school quality estimates without including individual fixed effects. The Figure shows a linear and positively sloped relation between the full-sample and movers design coefficients. This indicates that using our specification captures the same variation in school quality as the individual-fixed effects movers design, while remaining easier to interpret.

Exposure effects. We want to obtain an estimate of the effect of current exposure that can vary with the individual background, $\tau(X^{exp})$. We estimate the conditional contemporaneous exposure effects using an extension of the school fixed effects model,

$$y_{ist} = \beta_1^{exp} exposure_{ist} + exposure_{ist} X'_{ist} \beta_2^{exp} + X'_{ist} \beta_3^{exp} + \mu_s + \mu_t + \mu_i + \epsilon_{ist}.$$

In addition to the variables in the school quality model, the heterogeneous exposure effect model includes interactions between exposure and the background characteristics and indi-

Figure F.1: School quality estimates using control-approach and within-individual-approach.



Note: The figure shows the relation between school quality estimates obtained from estimating equation F.3 without individual fixed effects (first axis) and with individual fixed effects (second axis). Dots indicate means and bars show 95 percent confidence intervals.

vidual fixed effects. The exposure coefficients are identified from within-individual changes in exposure. Under the assumption that exposure is uncorrelated with the potential outcomes of the students conditional on the control variables, then the exposure coefficients capture the causal effect of contemporaneous exposure.

Predicted exposure effects. We created individual-level predicted exposure effects, $\hat{\tau}(X)$, as the difference in predicted individual-year-level exposure effect for each observation in the structural sample if the individual is and is not exposed. We can assign predicted exposure effects to all observations for which we observe X_{ist}.

F.4 Decomposition sample and estimation

Sample. To construct the decomposition, we focus on a sample of students who were not yet treated by grade-level 4 who do or do not meet a returner in their class by 5th or 6th grade. We measure the outcomes of these students by grade level 4 and 6 and calculate the within-individual differences in outcome, school quality, and contemporaneous exposure effects between these two grade levels for each individual. We construct the contemporaneous exposure effect by multiplying the estimated individual exposure effect $\hat{\tau}(X_{ist})$ with an indicator for contemporaneous exposure, A. Table F.5 shows summary statistics for the decomposition sample students by grade 6 by whether the students were or were not exposed to a returning student in 5th or 6th grade. The decomposition sample consists entirely of students in public schools and there are only small normalized differences in background characteristics across the treated and non-treated students.

Estimation. We construct the decomposition estimates appearing in Table 7 using empirical estimators for the estimands. Let the number of students be $n = n_0 + n_1$ with n_0 the number of individuals not (yet) treated by grade 6 and n_1 the number of individuals treated in grade 4 or 6. Let μ_X correspond to the true mean of X, $\mu_{X|D=d}$ to the conditional mean of X given that $D = d$, and $\sigma_{a,b}$ the covariance between a and b. The set of all individuals is I, the set of individuals in the $D = 1$ group is I_1 and the set of $D = 0$ individuals is I_0. We indicate differences between grade 6 and 4 by Δ.

Table F.5: Descriptive statistics for decomposition sample when observed in 6th grade.

	Not treated		Treated		
Variables	N	Mean	N	Mean	Normalized diff.
Year	193955	2016.053	13572	2015.865	-0.133
Grade-level	193955	6.000	13572	6.000	NaN
Female	193954	0.497	13571	0.505	0.016
Immigrant (1gen)	193954	0.020	13571	0.025	0.031
Immigrant (2gen)	193954	0.076	13571	0.086	0.038
Mother ave. std. income	193644	0.152	13546	0.072	-0.093
Father ave. std. income	190697	0.180	13318	0.066	-0.122
Ever in Special education	193955	0.010	13572	0.012	0.011
Exposed - currently	193955	0.000	13572	0.501	1.417
Exposed - previously	193954	0.000	13572	0.499	1.412
Exposed - ever	193955	0.110	13572	1.000	4.015
School - Max grade in school	193932	8.790	13572	8.889	0.102
School - Min grade in school	193932	0.006	13572	0.000	-0.062
School - Private	193955	0.000	13572	0.000	NaN

$$\hat{p} = \frac{1}{n}\sum_{i\in I} D_i$$

$$\hat{\mu}_{\Delta Q|D=0} = \frac{1}{n_0}\sum_{i\in I_0} \Delta Q_i$$

$$\hat{\mu}_{\Delta Q|D=1} = \frac{1}{n_1}\sum_{i\in I_1} \Delta Q_i$$

$$\hat{\sigma}_{D,\Delta Q} = \hat{p}(1-\hat{p})\left(\hat{\mu}_{\Delta Q|D=1} - \hat{\mu}_{\Delta Q|D=0}\right)$$

$$\hat{\mu}_{\Delta\tau(X)A|D=0} = \frac{1}{n_0}\sum_{i\in I_0} \Delta\tau(X_i)A_i$$

$$\hat{\mu}_{\Delta\tau(X)A|D=1} = \frac{1}{n_1}\sum_{i\in I_1} \Delta\tau(X_i)A_i$$

$$\hat{\sigma}_{D,\Delta\tau(X)A} = \hat{p}(1-\hat{p})\left(\hat{\mu}_{\Delta\tau(X)A|D=1} - 0\right)$$

$$\hat{\mu}_{\Delta Y|D=0} = \frac{1}{n_0}\sum_{i\in I_0} \Delta Y_i$$

$$\hat{\mu}_{\Delta Y|D=1} = \frac{1}{n_1}\sum_{i\in I_1} \Delta Y_i$$

$$\hat{\mu}_{\Delta Y} = \frac{1}{n}\sum_{i\in I} \Delta Y_i$$

$$\hat{\mu}_{\Delta U} = \hat{\mu}_{\Delta Y|D=0} - \hat{\mu}_{\Delta Q|D=0} - \hat{\mu}_{\Delta\tau(X)A|D=0}$$

$$\hat{\sigma}_{D,U} = \left(\hat{\mu}_{\Delta Y} - \hat{\mu}_{\Delta Q|D=0} - \frac{\hat{\sigma}_{D,\Delta Q}}{1-\hat{p}} - \hat{\mu}_{\Delta\tau(X)A|D=0} - \frac{\hat{\sigma}_{D,\Delta\tau(X)A}}{1-\hat{p}} - \hat{\mu}_{\Delta U|D=0}\right)(1-\hat{p})$$

F.5 Policy simulation

This section describes how we construct the simulation sample and estimates in Section 8.3. Our policy simulations start from the observation that decisions about integrating special education students are allocated to the local government at the municipal level, and therefore municipalities have the opportunity to vary the degree of exposure to returning students. The higher the municipal share of treated, the more likely any individual is exposed to students who have returned to regular classes from special education majority classes. In our analysis we use a structural model based on three main equations to estimate the effects of increasing the policy variable, the share of exposed students in the municipality. The analysis can be considered short-run as increasing the share of exposed students at any point in time will deplete the number of students still in special education majority classes, and therefore reduce the long-run share of students who can return.

F.5.1 Simulation sample and municipal variation in exposure to returning students

Subsample. For the simulation analysis, we focus on a subsample of individuals from the structural sample for whom we can estimate school quality and individual-level effects of exposure using the approach in section F.3. Table F.6 shows observations, means, and normalized differences in means across the structural sample and the simulation subsample. The notable difference between the two samples is that the simulation sample does not include private schools as we cannot estimate school quality fixed effects for these institutions.

Municipal-year share of exposed students. In the simulation analysis, we vary the municipal-level share of individuals who are or have previously been exposed to a returning student. We obtain empirical values of this share from our data and include the empirical shares in our estimated models that we use to simulate the effects of the exposure. We construct the municipal-by-year shares of exposed students by aggregating within our simulation sample. Figure F.2 shows the municipality-year level of variation in the share of treated in the left panel. The right panel shows the left-over variation after controlling for municipality fixed effects, and illustrates that there is substantial variation in the share of treated within municipalities. The typical range of variation is between 2 to 20 percent.

The exposure to special education needs students varies across student backgrounds. Figure F.3 shows binned-mean plots of average student characteristics by within-municipality share of treated students. The figure illustrates two things: First, there is a negative correlation between parental income and the likelihood of exposure. This is true also within

Table F.6: Descriptive statistics for structural sample and simulation sample.

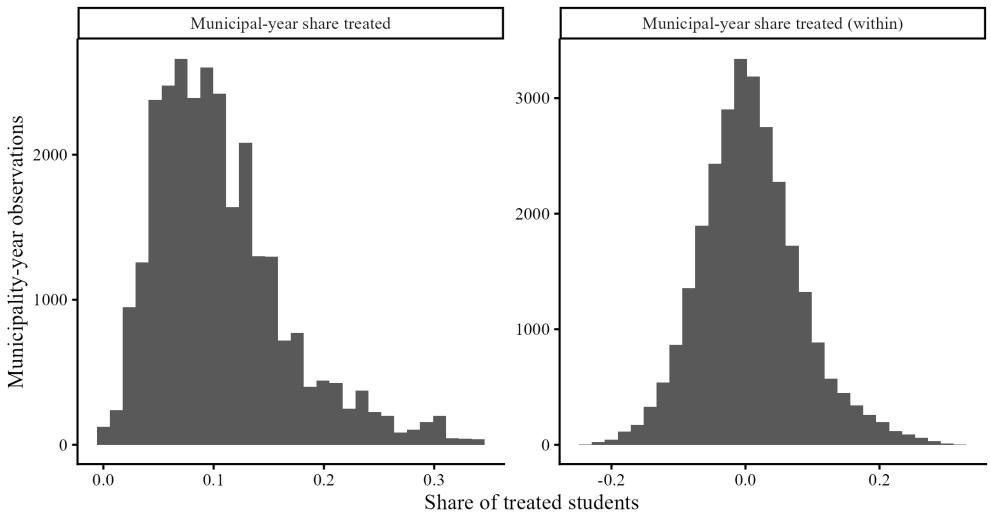
	Structural sample		Simulation sample		
Variables	N	Mean	N	Mean	Normalized diff.
Year	4468224	2014.999	3503910	2014.972	-0.013
Grade-level	4468224	4.478	3503910	4.355	-0.043
Female	4455449	0.496	3503910	0.493	-0.006
Immigrant (1gen)	4455449	0.033	3503910	0.020	-0.076
Immigrant (2gen)	4455449	0.078	3503910	0.078	-0.001
Mother ave. std. income	4442523	0.131	3503910	0.130	-0.001
Father ave. std. income	4356432	0.182	3503910	0.150	-0.032
Special ed. class	4468224	0.000	3503910	0.000	NaN
Ever in Special education	4468224	0.017	3503910	0.018	0.010
Exposed - currently	4468224	0.032	3503910	0.032	-0.004
Exposed - previously	3984194	0.067	3141084	0.070	0.014
Exposed - ever	4468224	0.210	3503910	0.222	0.028
School - Max grade in school	4465330	8.965	3503513	8.868	-0.105
School - Min grade in school	4465330	0.183	3503513	0.041	-0.157
School - Private	4468224	0.187	3503910	0.000	-0.678

municipalities. Secondly, there is a positive correlation between exposure and the share of immigrants affected, while there is no correlation with exposure and student gender. This suggest that there is some degree of sorting by parents towards areas that have fewer exposed students. As a result, we would expect lower-SES students to be more affected by increased policy levels than higher-SES students.

F.5.2 Estimating simulation regression models

Our analysis requires us to construct predicted values for a given policy level, π, for each individual in the sample. We construct these predictions using three models estimated on our simulation sample. We separately estimate models for treatment, D, current exposure, A, and school quality, Q and use simulated values from the models to predict the effects on standardized test scores of increasing the treatment share. Across all models, we include a set of binary individual levels controls that include parental background measured by income bins. Because we identify the effects of municipal level variation in the policy variable, one might worry that estimated relations purely capture across-municipality sorting. We control for this type of sorting by including indicators for previous-year-home-municipality for each observation.

Figure F.2: Municipal-year variation in share students previously or currently exposed to a returning special education needs student



Notes: The figure shows the distribution of the municipal-year mainstreaming exposure. The left panel shows the raw municipal-year share of treated students. The right panel shows the corresponding variation after removing municipality-level means.

Treatment model. We first model the likelihood of having previously or presently been in a class with a returner, D, in a logistic regression with flexible interactions between the municipality share and student characteristics and fixed effects for the individual previous year home municipality:

$$D_{istm} = f(\pi_{mt}, X_{ist}, X_{ist} \cdot \pi_{mt}, m^h_{t-1})$$

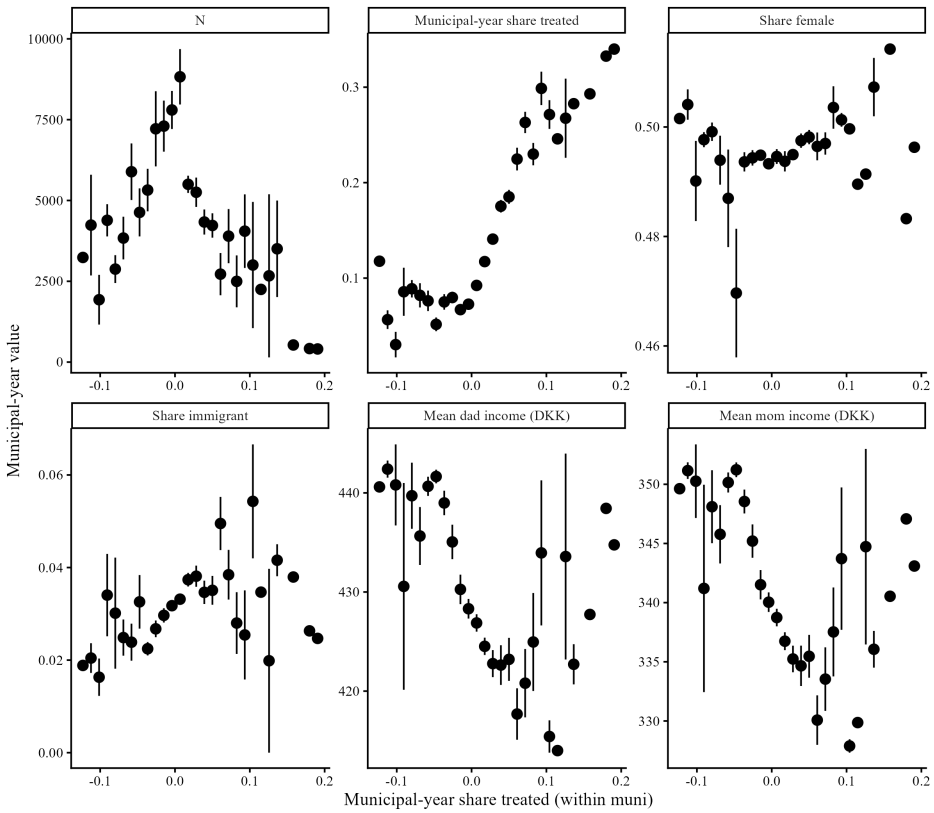
We include interactions between individual characteristics to account for differences in policy variable variation on the likelihood of treatment.

Exposure model. We secondly model the likelihood of exposure as a linear function of individual characteristics, treatment status, and the policy variable,

$$A_{istm} = \delta_1\pi_{mt}+\delta_2 D_{istm}+X'_{ist}\delta_3+\delta_4\pi_{mt}D_{istm}+\pi_{mt}X_{ist}\delta_5+D_{istm}X_{ist}\delta_6+\pi_{mt}D_{istm}X_{ist}\delta_7+\mu_{m^h_{t-1}}+\varepsilon^A_{istm},$$

where $\mu_{m^h_{t-1}}$ are time $t-1$ home municipality fixed effects.

Figure F.3: Student background by within-municipality mainstreaming exposure



Notes: The binned scatter plots show municipal-year averages of student background characteristics against within-municipality variation in the share of students treated by a mainstreaming event. The panels are used to assess whether the within-municipality variation in π is related to observable student composition.

School quality model. We model school quality also as a linear function with full interactions between treatment status, the policy variable, and individual background,

$$Q_{istm} = \gamma_1 \pi_{mt} + \gamma_2 D_{istm} + X'_{ist}\gamma_3 + \gamma_4 \pi_{mt} D_{istm} + \pi_{mt} X_{ist}\gamma_5 + D_{istm} X_{ist}\gamma_6 + \pi_{mt} D_{istm} X_{ist}\gamma_7 + \mu^{Q}_{m^{h}_{t-1}} + \varepsilon^{Q}_{istm}.$$

Like the exposure model, we control for home municipality fixed effects.

Simulation. We use the estimated models to simulate test scores for high and low SES students by varying π over the values $0, 0.02, ..., 0.2$ and use this policy share to predict $\hat{D}$. We define low SES individuals as those within the estimation sample whose parents earn less than the average parental income. With the policy municipal share of treated and the predicted treatment value we then predict the observations level expected quality of schools, $\hat{Q}$, and the expected exposure and exposure effect. We construct the exposure effect by multiplying $\hat{\tau}(X_i)$ with the predicted exposure level. Finally, we construct the predicted outcome for each individual as $\hat{y}_i = \hat{Q} + \hat{\tau}(X_i)\hat{A}_i$. Figure 11 shows average predicted values across the high and low-SES students in the simulation sample.