Switching Strategies? Peer Impacts of Moving Disabled Students to General Education Classrooms*

Laura Caron[1], Jesper Eriksen[2], and Jeppe Johansen[2]

[1]Columbia University
[2]University of Copenhagen

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Abstract

Prior work has shown that placing disabled students in classrooms with non-disabled peers ("mainstreaming") can have negative impacts on these peers, but has not fully considered the role of student mobility in understanding these impacts. Using Danish administrative data and a difference-in-difference design, we show that the arrival of a mainstreamed student in a classroom makes peers 11% more likely to switch schools in a given year. Because school switching is selective by socio-economic background and choice of public versus private schools, this may increase socio-economic segregation. 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. Research and policy on 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. In particular, 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, also known as mainstreaming.[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 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 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 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

[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 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).

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 in Gibbons and Telhaj (2011).

To estimate dynamic causal impacts of mainstreaming, 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. 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-difference 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 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 no evidence of differences in switching by academic performance in grade 2 Danish tests. Thus, our findings suggest that reactions to a policy that aims to integrate students can potentially increase school-level socio-economic segregation.

Having shown this substantive 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. To construct bounds on the unobserved true impacts of the arrival of the mainstreamed student in the absence of school switching, we impute best- and worst-case missing outcomes for students in private schools (for whom outcomes are not observed) and for students who switch schools (for whom outcomes may

be disrupted by school switching rather than the arrival of a mainstreamed student). 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 test scores, do mainstreamed students cause disruptions or social tensions in a classroom when they arrive? The Danish registry data contain detailed information on students' 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 ever and never do 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.

We make several contributions to the literature on disability in the classroom. 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 only focuses on contemporaneous switching, and therefore does not capture the 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] We show that, between 2012–2020, school switching is concentrated in secondary school, even for students exposed to a mainstreamed student earlier in their school lives. Our findings also indicate that these switching responses may cause substantive bias in estimates of effects on other outcomes, such as test scores, if students or their outcomes are not observed at the arrival schools.

We also contribute to 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.

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 students are not necessarily direct impacts of mainstreaming but may instead by 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. 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. While student disability rates are comparable between Denmark and other countries, a higher share of disabled students are educated in segregated classes

[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).

away from their non-disabled peers. This implies that the marginal disabled student switching from a segregated class to a mainstream class in Denmark is likely comparable to the disabled students in mainstream environments in other countries.

2.1 The Danish school system

Danish students are required to attend grades 0–9 and can attend an optional 10th grade. During these grade levels, the typical student is aged 6–16. While practically all students attend schools for their entire 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 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.

Families also have a choice to attend a private rather than a public school. 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.

Private schools are relatively popular in Denmark, and even 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 2023). On average, the socioeconomic background of students attending private schools tends to be higher than those attending public schools (Epinion 2017).

School switching during mandatory education is common in Denmark. A representative survey sent to parents of primary school aged children by Epinion (2017) investigated parents' main reasons

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

[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.2 shows the distribution of these cost shares across families.

for switching schools and showed that about 25 percent of the surveyed parents indicated that their child had changed schools at some point. About half of these switches were freely chosen. The remainder were necessitated by either geographical moving, reaching the highest grade in the local school, or the school closing. 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) (Table 1).

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 (Table 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 needs students between mainstream classrooms in these other countries.

Table 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).

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 controversial Inclusion Act, which mandated that no more than 4 percent of Danish students should be excluded from mainstream classrooms (Kessel et al. 2019). Using our registry data, we document in Section 4 that the number of segregated classes declined from 2012 to 2014.

Mainstreaming disabled students has often been met with mixed opinions from parents and teachers. 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 Reform 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. We construct a dataset which gives a rich picture of students' school and classroom enrollments and outcomes using Danish administrative data provided by Statistics Denmark. 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 are 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 obtain 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

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

more detail in Section 6 and Appendix C below.

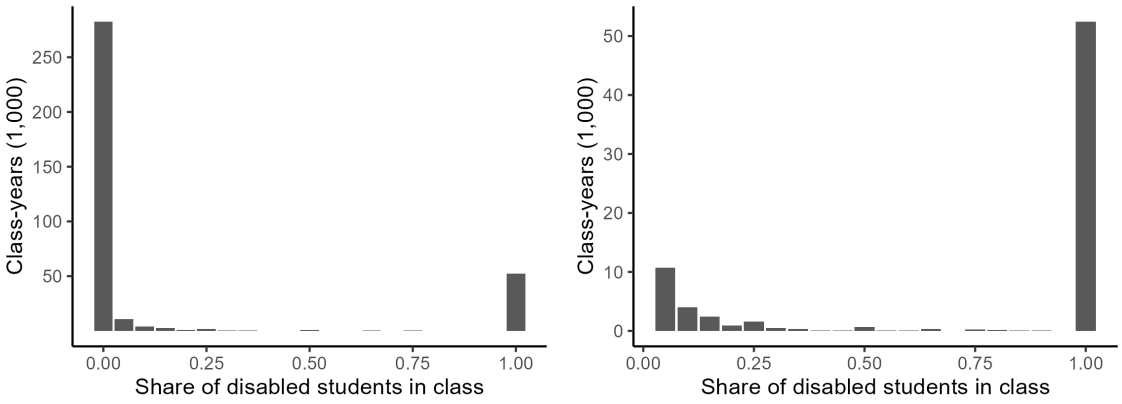
4 Patterns of mainstreaming, school switching, and outcomes

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 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

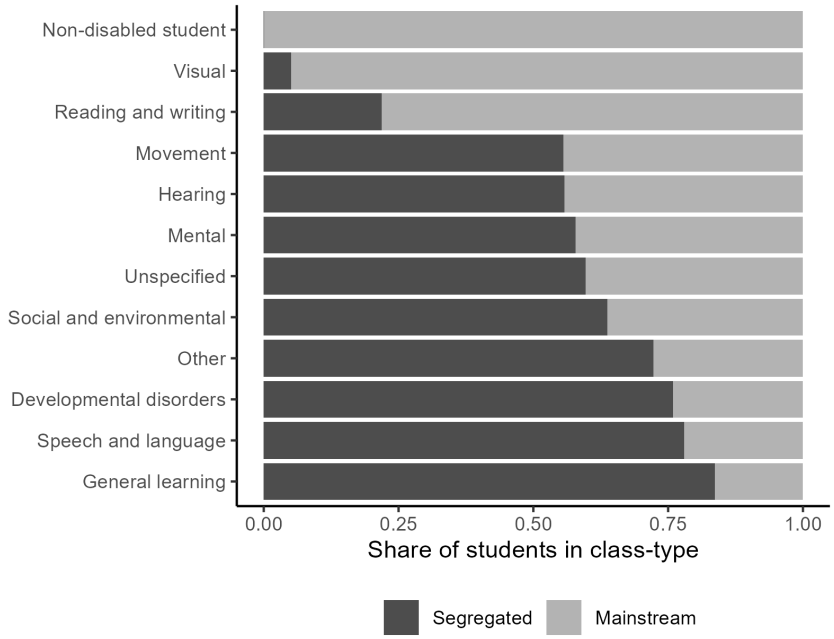


(a) Overall (b) Excluding zeros

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. Meanwhile, while most students with visual disabilities

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 including intellectual disabilities, experience the most segregation out of mainstreamed classrooms.

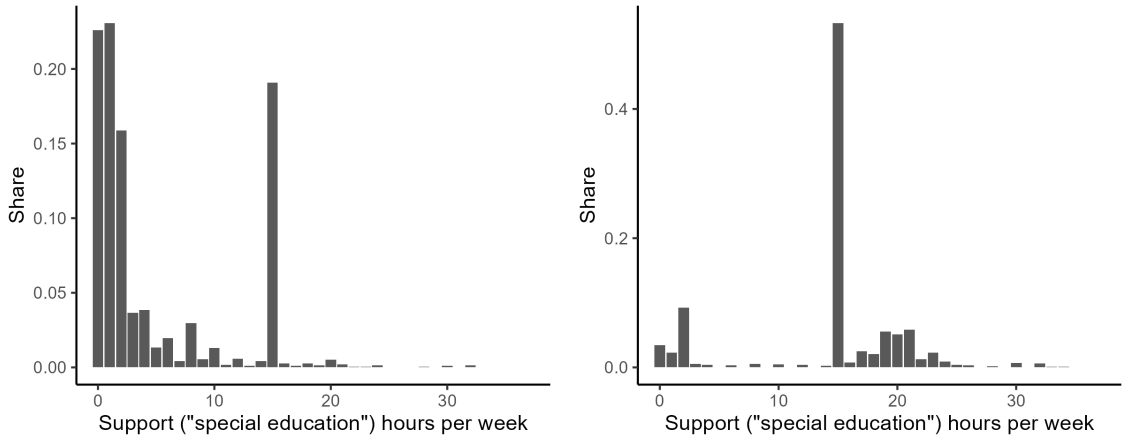
receive their education in mainstream classrooms, students with more common types of disabilities like general learning disabilities and developmental disorders are largely educated in segregated classes.

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 segregated classrooms. In our data, we only observe hours of support received for all disabled students in 2011. Figure 3 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.

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.[7] 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

[7]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 not abolish segregated classes. Accordingly, Appendix Figure A.1 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.

Figure 3: 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.

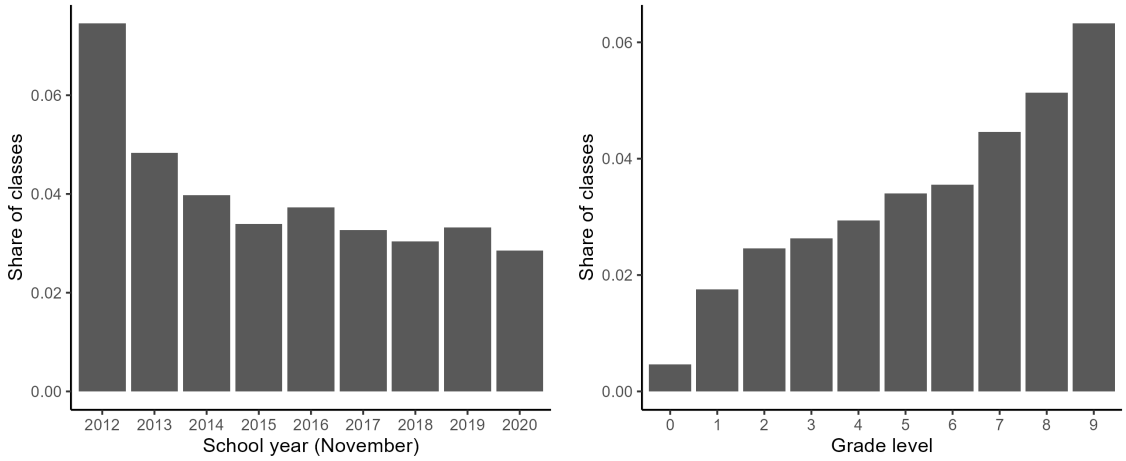
student.[8] Figure 4 shows the share of mainstream classes receiving a student from a segregated class each year from 2012–2020. In 2012, more than 6 percent of mainstream classes received a mainstreamed student. It is worth noting that this share on average decreases slightly over time, though it remains above 3 percent. This may be due to the declining number of disabled students put in segregated classes following the 2012 inclusion reform.

Finally, Figure 4 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 grade 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 2 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 on average. In line with the 2012 Inclusion Act, we observe more mainstreamed students and 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

[8]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 4: 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 no discernible trend over time. Panel (b) breaks down the data by grade level.

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 differences-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 2: Descriptive statistics, exposed vs. never-exposed

	Exposed			Never exposed				
Variable	Mean	SD	N	Mean	SD	N	Diff. mean	SE
Year	2015.263	2.410	1007882	2016.17	2.58	4018957	-0.908	0.003
Female	0.501	0.500	1006788	0.50	0.50	4009383	0.004	0.001
1st gen. immigrant	0.028	0.165	1006788	0.03	0.18	4009383	-0.006	0.000
2nd gen. immigrant	0.081	0.272	1006788	0.08	0.27	4009383	0.003	0.000
Ever special ed. student	0.101	0.302	1007829	0.05	0.23	4018441	0.047	0.000
Mom's age at birth	30.106	4.945	1004315	30.64	4.87	4000443	-0.533	0.005
Dad's age at birth	32.812	5.781	986085	33.26	5.70	3932804	-0.449	0.006
Mom's years of schooling	14.445	2.686	983002	14.91	2.70	3911601	-0.466	0.003
Dad's years of schooling	14.211	2.596	961915	14.65	2.65	3839742	-0.438	0.003
Mom's income (1,000 DKK)	343.581	27.966	1004161	336.79	34.49	3999219	6.790	0.037
Dad's income (1,000 DKK)	433.139	35.697	983638	424.34	43.87	3924497	8.803	0.048
First year observed	2012.625	1.343	1007882	2013.76	2.21	4018957	-1.131	0.002
Last year observed	2017.903	2.185	1007882	2018.58	2.12	4018957	-0.680	0.002
First grade observed	3.079	2.172	1007882	2.44	2.13	4018957	0.641	0.002
Last grade observed	8.341	1.388	1007882	7.25	2.21	4018957	1.086	0.002
Danish score (std.)	-0.109	1.010	350780	0.07	0.98	1099777	-0.175	0.002
Math score (std.)	-0.106	1.004	213220	0.06	0.98	633305	-0.168	0.002
Ever switched classes	0.039	0.195	1002047	0.03	0.17	3919493	0.008	0.000
Ever switched schools	0.240	0.427	1001872	0.17	0.38	3918276	0.068	0.000
Ever moved addresses and school	0.135	0.342	1007211	0.08	0.27	4010228	0.055	0.000
Ever moved addresses, same school	0.391	0.488	1006868	0.35	0.48	4002381	0.038	0.001

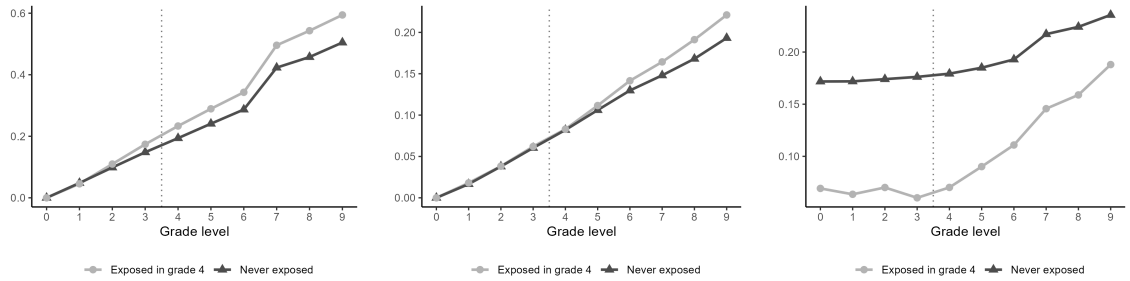
Notes: The table shows descriptive statistics comparing the characteristics of exposed and never-exposed students who are not themselves mainstreamed students.

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 5a shows the probability by grade level that a student has switched schools at least once among students 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 both 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 5b). 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

Figure 5: 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.

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 5c 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-difference 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-difference 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.[9] 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 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.[10] 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.

[9] 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.

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

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.

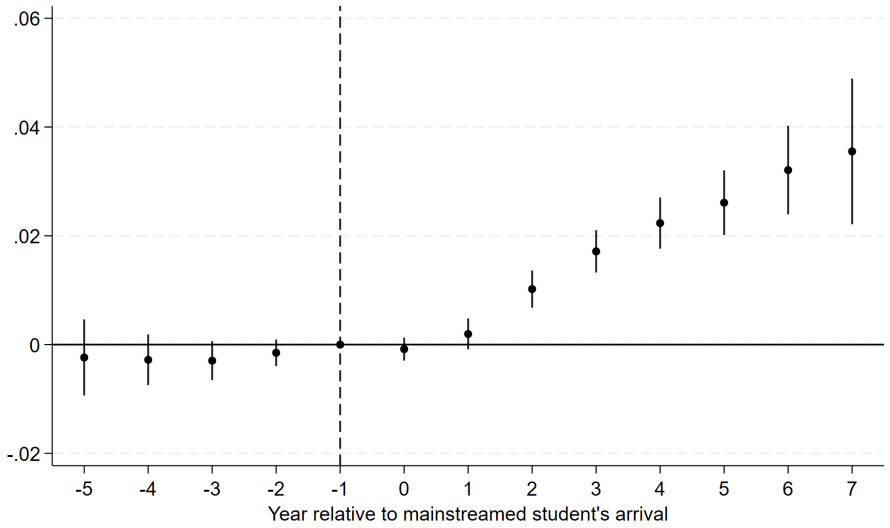
6.1 Impacts on school and class switching

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-difference 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 6 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 3 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 Section 5. These results both show evidence of a large increase in unexpected school switching following the arrival of a

Figure 6: Event-study ATT impact on student unexpected school switching



Notes: The figure plots event-study estimates from the main difference-in-difference 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.

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 3 illustrate their dynamics. The fourth row contains the event-study coefficient for period 0 to test for effects 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. Reassuringly, in the same year that the mainstreamed student arrives, we find no impact.[11] 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

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

Table 3: 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-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 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

on contemporaneous unexpected school switching between the prior and current academic year (Column (2) of Table 3). Although there is some evidence of a pre-trend for this outcome, with a significant negative post-period average estimate, the results are consistent with those in Column (1) for the probability of having ever made an unexpected switch 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 3 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 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.[12] In Table 4, 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 becomes below the median (Columns (1) and (2)). Across all post-periods, students with high parental incomes who are

[12]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.

exposed to a mainstreamed student have a 0.6–0.8 percentage point increase in the probability of switching schools in a given year, compared to statistically insignificant 0.3 percentage points for students with low parental incomes.

In contrast to the finding that students with higher socio-economic status are more likely to switch schools unexpectedly, we find that students with below-median 2nd grade Danish national test scores are slightly more likely to switch. Columns (3) and (4) of Table 4 show estimated effects by whether the student's test score was above and below the median. Students with below-median test scores are 0.4 percentage points more likely to switch school unexpectedly, corresponding to an increase of 11 percent over their mean. 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.

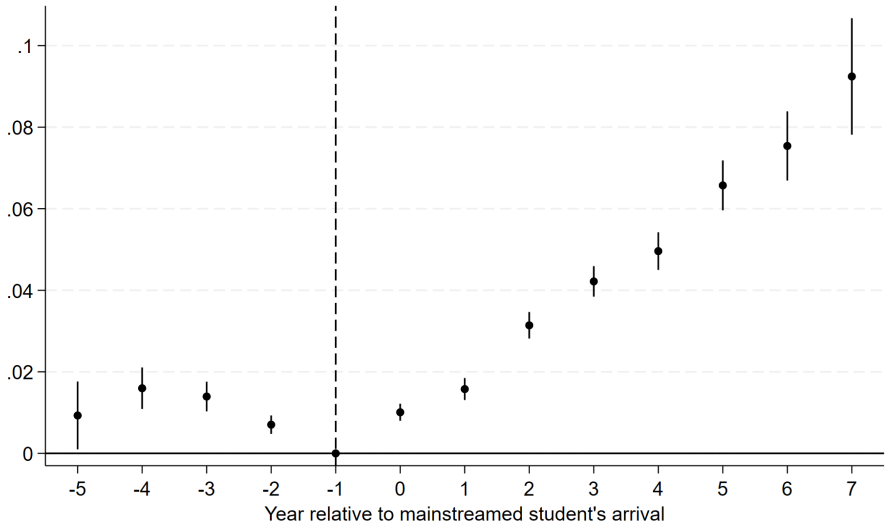
Table 4: Heterogeneity in unexpected school switching by student characteristics

	(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	1539058	1427767	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-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. 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

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 7 plots event study coefficients showing impacts on the probability that a student is in a private school. Although there is some evidence

Figure 7: Event-study ATT impacts on private school enrollment



(a) Private school enrollment

Notes: The figure plots event-study estimates from the main difference-in-difference 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.

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. 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. Columns (1) and (2) of Table 5 study 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, the null effects in Columns (3) and (4) in Table 5, which show no increase in switching into schools either with a Math score above or below the median, suggest that most switching is

Table 5: 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

concentrated in private schools. Effects are qualitatively similar when splitting schools by Danish scores (Columns (5) and (6)).

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 6 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

Table 6: 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-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 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

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 6 show estimated impacts 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 results with the impacts documented in prior literature?

Prior work, including Balestra, Eugster, and Liebert (2022), Kristoffersen et al. (2015), and Fletcher (2010), find 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 significant heterogeneity according to students' 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 switching school 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 be plausibly be explained by selective switching alone.

7 Estimating impacts on student absences, test scores, and well-being

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. Instead, we 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 Ewijk (2014), and Hill et al. (2023)) and are attractive because they are available annually for all public school students in our sample.[13] 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.

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

[13] 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.

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.

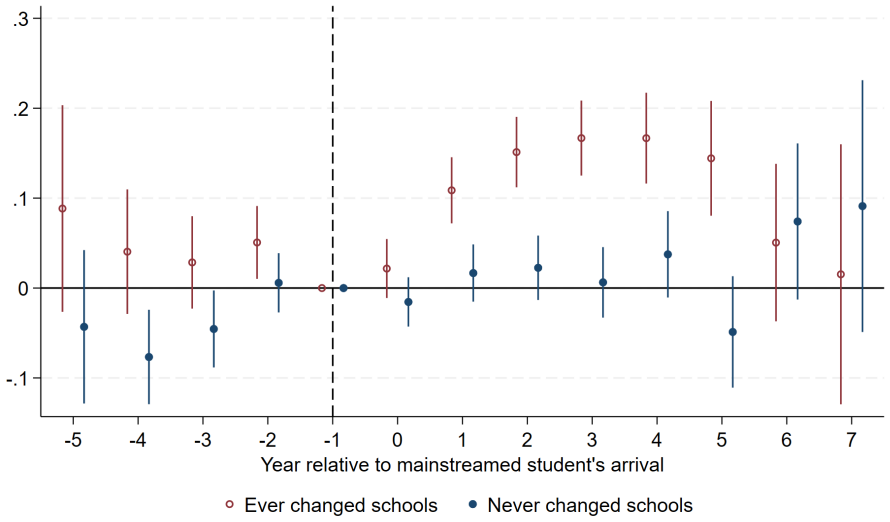
A concern for conducting this kind of analysis is the choice of comparison group. When constructing a subgroup of exposed students that is restricted based on school switching behavior, an appropriate comparison group must be never-exposed students who are similarly limited. Thus, we compare exposed 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 are some evidence of significant pre-trends; however, the event-study plots in Figure 8 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.3.

The results in Figure 8 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'

Figure 8: 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-difference 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.

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. In order to create an annual series for the difference-in-difference analysis, 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-difference 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 7 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.

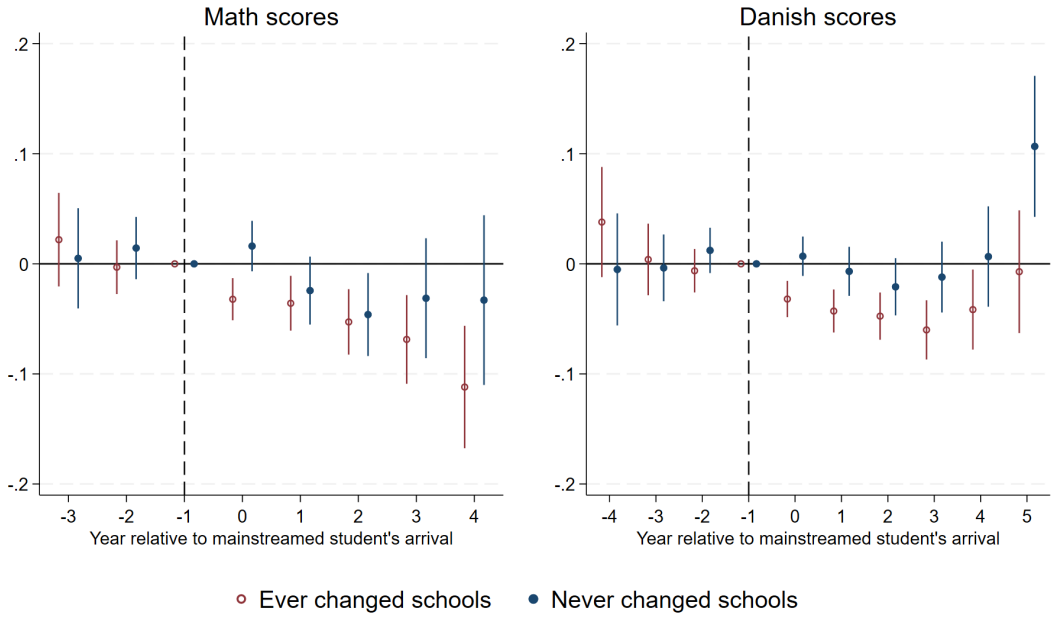
Table 7: Impacts on test scores by subgroup

	(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-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 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

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 in Figure 9 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

Figure 9: 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-difference 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.

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. In fact, 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 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 students' 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.

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 8 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).

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 9, 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

Table 8: 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-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 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

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 9 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 9: 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-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 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 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.

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 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.

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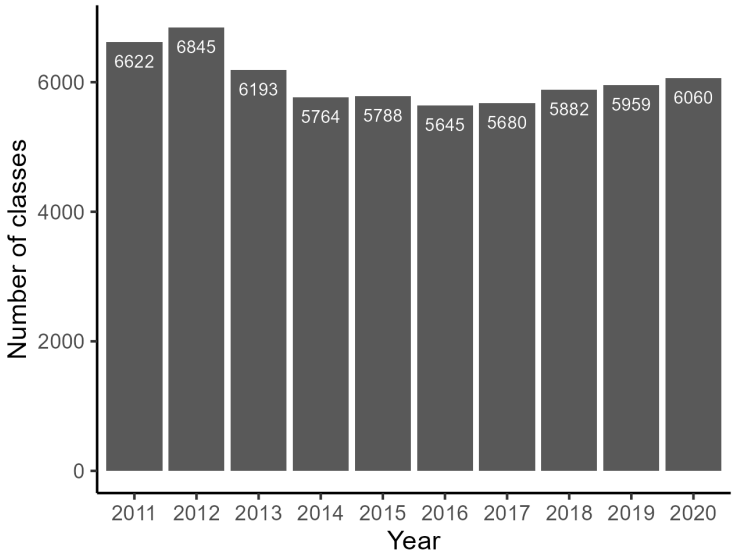
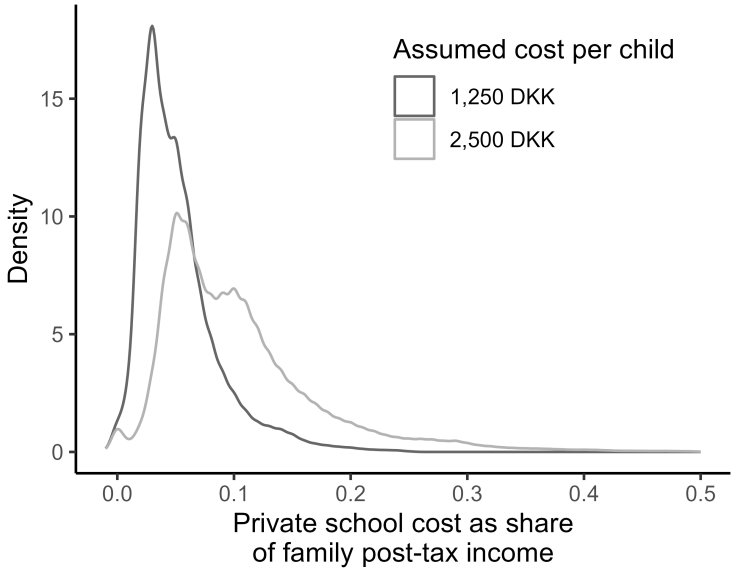
A Supplementary figures

Figure A.1: 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.

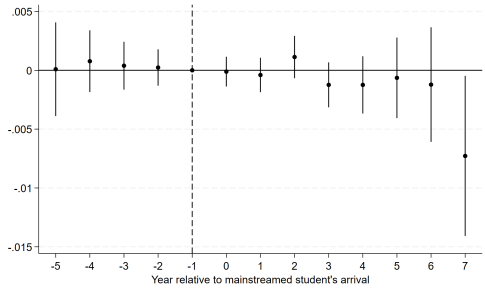
Figure A.2: The distribution of costs of sending all children aged 6-15 to private schools as share of family post-tax income, 2015.



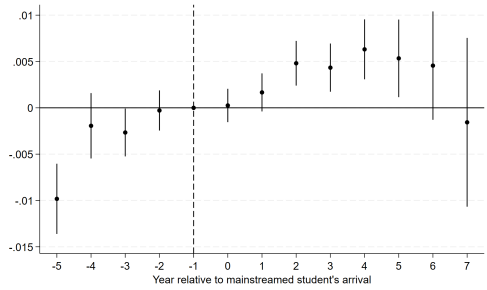
Notes: The figure plots the number of families by the the cost of sending all children aged 6-15 to private school as a share of the family's total disposable income. The data comes from the Danish administrative registers. The sample consists of all families observed in the Danish registers in 2015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

Figure A.3: Event-study ATT impact on chronic unexcused absences for students who never and ever change schools

(a) Students who ever change schools



(b) Students who never change schools



Notes: The figure plots event-study estimates from the main difference-in-difference 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.

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.[14]

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

[14]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.

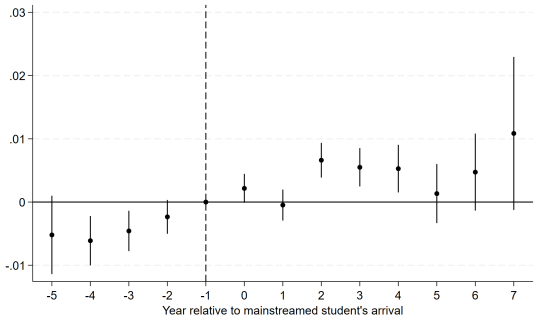
grades. At the same time, we find much smaller impacts on the probability of switching towards a mainstreamed student, with positive effects mainly concentrated in period 0, that is, concurrent with the mainstreamed student's arrival (Column 2). This result shows that a number of new students enter the 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 3 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. 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.

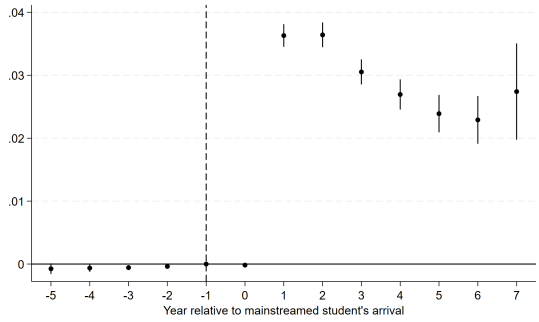
One may be concerned that these effects are induced by the comparison: never-exposed students might be less likely to switch away from a mainstreamed student because they are less likely to be in a class with a mainstreamed student in the first place. However, we argue that the effect is not purely mechanical. 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

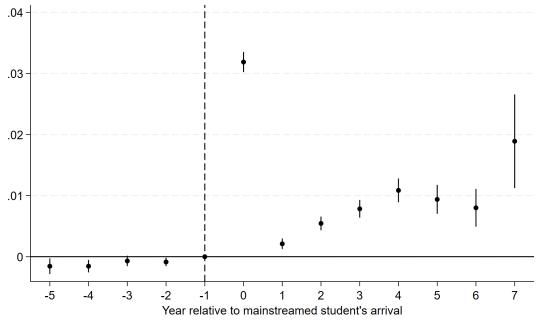
Figure B.1: Event-study ATT impacts on student mobility between classes and schools



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

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 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.[15] 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.

[15]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) Switched class away from mainstreamed	(2) Switched class towards mainstreamed	(3) Switched class away from SE student	(4) Switched class towards SE student
Pre-period avg.	-0.000	-0.000	0.000*	0.001**
	(0.000)	(0.000)	(0.000)	(0.000)
Post-period avg.	0.002***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
C-S aggregation	0.002***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Event-study coefficient, period 0	0.000	0.003***	0.000*	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Avg. effect in grade 2-6	0.001***	0.002***	0.001**	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Avg. effect in grade 7-9	0.002***	0.001***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2700550	3027465	3026421	3027465
Dep. var. mean	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-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 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) Moved, changed school	(2) 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-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 Column (1) showing impacts on having ever changed both schools and addresses and Column (2) on having changed addressed 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-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 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-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 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.

Meanwhile, for students who do change schools, effects are more negative for those with grade 2

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-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 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

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-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 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