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Effects of teachers' aides on students with special needs

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Abstract

This study investigates the effect of increasing educational support in regular classes on students with special education needs (SEN). We exploit previous randomized controlled trials that added teachers' aides to Danish grade 6 classes combined with rich register data informative about SEN and school assignment. There were three types of teachers' aides: a co-teacher with a teaching degree, a teaching assistant without a teaching degree, and a coach. We find that the treatments increased inclusion of SEN students in regular classes, by 7–8 percentage points, and that SEN students gained academically. Long-term follow-up data indicate that while the academic gains evaporated over time, the treated students were able to stay in regular classes throughout compulsory education and largely followed the same progression as their counterparts in the control group when they moved to upper secondary education. Finally, we show that the costs of the teachers' aides are outweighed by the saved costs of special education.

Keywords: Randomized controlled trial; segregated classes; special class; special school

JEL classification: *I*21; *J*14; *J*24

1. Introduction

As the number of students enrolled in special education and the amount of money spent on these services has grown in the United States (Schwartz et al., 2021) and in Denmark (Rangvid, 2019; Lindeberg et al., 2022), it becomes a pressing concern to examine the most effective ways of providing education to this group of children. Some research estimates positive causal effects of segregated special education (Hanushek et al., 2002; Hurwitz et al., 2020) or special education support in regular classes (Ballis and Heath, 2021; Schwartz et al., 2021) as opposed to treatment as usual in the

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regular school system. However, school authorities spend two to four times as much money per student in special education as per student in regular classes (Setren, 2021). Therefore, an alternative to special education may be to include students with special education needs (SEN) in regular classes while investing some of the saved resources in higher school quality in terms of more teacher support. Besides potentially benefiting SEN students, the added resources to regular classes may have positive spillovers on their peers without special needs.[1] Importantly, it will also comply with the commitment to inclusive education programs in the Salamanca Declaration[2] in which SEN students are supposed to have access to regular schools and, to the extent possible, to be educated alongside their peers without special needs. However, even though new research indicates that investing in the inclusion of SEN students in regular classes is an effective approach (Setren, 2021; Jones and Winters, 2024), little is known about what type of teacher support is most effective.

To study these questions, we exploit previous randomized controlled trials implemented in representative municipalities of Denmark. The trials provided three different types of teachers' aides to classes. One treatment used teaching assistants without teaching degrees who spend 14.5 lessons per week in the class during nine months in grade 6. The second treatment used co-teachers with teaching degrees who spend 10.5 lessons per week in the class. The costs of these two interventions were the same (because collective agreements required that educated teachers had more preparation time and higher salary). The third treatment used coaches with expertise in SEN (or similar qualifications) who were allocated 2.5 hours per week per class to work with the teachers in the class as well as outside the class. The third treatment cost about 14 percent of each of the other two treatments (= 21,700/156,000 DKK per class). We examine how these investments attracted students from special education services, and how they affected the SEN students during the intervention. We also consider the downstream effects on the SEN students up to seven years after the intervention ended, and the short- and long-term costs and benefits of the teachers' aides. Even though the trials were not designed and powered to test these questions directly, the results provide new information about the potential effects of investing in SEN students.

[1]Previous studies examining the peer effects of including SEN students without providing additional teacher support in the classroom indicate that learning of peers may be attenuated.
[2]The declaration of the World Conference on Special Needs Education: Access and Quality, Salamanca, Spain, 1994, is signed by 92 countries. It constitutes a statement and a framework for action towards creating inclusive education programs where special-needs education is an integral part of the regular education programs. See https://unesdoc.unesco.org/ark:/48223/pf0000098427.

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The average treatment effects on regular students of the first two treatment arms have been reported elsewhere (Andersen et al., 2020). However, because of the apparent endogenous sorting of SEN students into treated schools and across classes within treated schools, we could not single out the effects for SEN students as part of the original study. In the present study, we have gathered administrative data on the allocation of SEN students in the years prior to the intervention when treatment assignment could not possibly have affected student allocation. In this way, we can handle the issue of selection of SEN students into treatment. Furthermore, we have not previously reported on the effects of the coaching treatment, which was a parallel trial.

First, our results document that assignment of these three forms of teachers' aides to schools indeed increased the inclusion of SEN students in these classes. At schools assigned a co-teacher or teaching assistant, the inclusion of SEN students increased significantly by 7–8 percentage points compared with their inclusion at control schools, while the increased inclusion was less and not significantly higher at schools assigned a coach compared with control schools.

Secondly, we find that SEN students benefited from two of the treatments – the co-teacher and the teaching assistant – during the period of the intervention. They increased their reading test scores by 0.16 and 0.18 standard deviation (SD), respectively, which is on a par with the effects on regular students reported earlier (Andersen et al., 2020). We find no significant effects of the low-cost coaching intervention, and we do not find significant differences between SEN students in the treatment and control group in the years following the end of the intervention.

Limited precision makes us caution that insignificant effects are not proof that there are no effects. However, taken together, the results indicate that assigning teachers' aides to regular classes attracts more SEN students, and as long as they are exposed to additional resources, SEN students improve their reading skills just like regular students do. As reported elsewhere, the two interventions at the same time improved the test scores of the other students in the class (Andersen et al., 2020), so we find no negative effects of the inclusion. When the two interventions ended, the SEN students stayed in the regular classes with their new peers. Their educational performance and attainment thereafter resemble that of SEN students in the control group.

The costs of a teaching assistant or a co-teacher for one class in our intervention are similar to the additional costs of assigning 1.1 students to a special class rather than a regular class for one school year. In comparison, we estimate that the teachers' aide treatments caused an additional 1.5 students to become included in general education per class. So, the costs of the teachers' aide policy were outweighed by the saved costs by enhancing the inclusion of SEN students. However, the low-cost coaching intervention was not enough to make a difference.

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Our study most directly relates to two recent studies on SEN students in regular classes. A US study from Massachusetts indicated some positive effects of co-teaching on students with disabilities in English language arts (ELA) in primary grades, but the effects were negative in secondary grades, and there were no (or small positive) effects in mathematics (Jones and Winters, 2024). Setren (2021) uses admission lotteries to charter schools to examine the effects of a high-performing general education program (including high-intensity tutoring, data-driven instruction, and increased instructional time) on SEN students. She finds that the inclusion of SEN students in regular – but high-quality – schools increases the likelihood of them graduating from two-year colleges. Whereas both of these studies are in line with our results indicating the positive effects of investing in the quality of regular classes, they are not able to directly compare the effects of the three types of teachers' aides that we study.

Our study is also closely related to the literature on the consequences of the assignment of SEN students to segregated classes as opposed to regular classes. Hanushek et al. (2002) and Hurwitz et al. (2020) study the effects of special education programs on academic achievement by exploiting the fact that children move in and out of targeted programs. Both find that the average special education program significantly increases the academic achievement of SEN students entering the program. Also, Schwartz et al. (2021) study the intent-to-treat effects of special education classification and find positive effects on academic achievement for students who are at some point assigned to special education after school entry. In contrast to Hanushek et al. (2002) and Hurwitz et al. (2020), Schwartz et al. study four types of special education ranging from integrated to more segregated settings: (i) support services such as counseling, physical therapy, or speech/language therapy; (ii) supplemental instruction; (iii) integrated teaching support; and (iv) self-contained services where SEN students are educated in special classes. Similarly, Ballis and Heath (2021) study the effects of reducing access to special education in Texas in 2005, which constrained the special education caseload to 8.5 percent. They exploit district-specific reform exposure, and the results suggest that the marginal student benefits from special education services.

Finally, our study speaks to the literature on the effects of school resources on academic outcomes more generally. These studies exploit plausibly exogenous variation to make inference on the effects of school funding (Holmlund et al., 2010; Hægeland et al., 2012; Jackson et al., 2016; Hyman, 2017; Lafortune et al., 2018; Rothstein and Schanzenbach, 2022), staffing (Fredriksson et al., 2013; Hemelt et al., 2021), or teacher quality (Chetty et al., 2014; Jackson, 2018). They find favorable effects of school resources that are larger for students from low-income families. Our findings are in line with these results and further suggest that the availability of

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abundant resources attracts SEN students, who, in particular, gain from extra school resources.

In Section 2, we present the institutional context and policy content. Next, in Section 3, we present the experimental design of the trial and the data used. In Section 4, we present the results, in terms of inclusion, the short-term effects at the end of the intervention period, and the long-term effects after the intervention ended. Finally, we conclude in Section 5.

2. Inclusion of SEN students in Danish public schools

At age four, the vast majority of Danish children are enrolled in some form of subsidized public daycare. For the children in our study, education was compulsory from the calendar year in which they turned seven[3] until completing grade 9.

At the end of grade 9 (around age 16), students sit compulsory exit exams, which must be passed with satisfactory results in order to enroll in upper secondary education.[4] After grade 9, students may enroll directly in upper secondary education (if qualified), or they may attend grade 10 (53 percent of the cohort did that in 2020)[5] either at the same public schools that also offer grades 0 to 9 or at independent schools (i.e., private schools or boarding schools),[6] and then progress to upper secondary school afterwards (i.e., high school or vocational training). Compulsory education, as well as most post-compulsory education, is free of charge at public schools, whereas independent schools charge tuition fees. In grade 10, both public and independent schools have many degrees of freedom to focus on developing particular skills in students and to teach according to their views and values. However, they are obligated to provide minimum mandatory coursework, and to prepare students for moving to upper secondary schools.[7]

[3]Kindergarten class was not compulsory for students in our study. It became compulsory for the cohort starting school in 2009. Before 2009, average enrolment in the optional kindergarten class was 83 percent (2005 figures; UNI-C 2012).

[4]Since 2006, school principals have been responsible for ensuring that students sit the compulsory exit exams. Students with special needs are offered circumstances accommodating their needs (extra time, assistive technology, digital rather than handwritten exams, etc.), and only in rare cases are students formally exempted from sitting the exams. Government auditors have recently expressed concern that this regulation is not enforced (see https://www.ft.dk/statsrevisorerne).

[5]See Statistics Denmark, UDDAKT20: Educational activity in basic school by education, age, ancestry, national origin, sex, type of institution from basic school, region and status, https://www.statbank.dk/UDDAKT20.

[6]Access to private or independent schools is very limited for SEN students due to the associated costs.

[7]See Law of Public Schools §19, https://www.retsinformation.dk/eli/lta/2024/989 and Law of Independent and Private Schools §1, https://www.retsinformation.dk/eli/lta/2024/161 (accessed 7 October 2024).

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Public daycare, schools, and after-school programs are the responsibility of the municipalities, who are also responsible for providing additional support for SEN children. School authorities may offer support in terms of extra teacher resources or aides in regular classes (Andersen et al., 2020), or they may use segregated settings in terms of special classes or special schools (Egelund and Rangvid, 2014). Kindergarten class, public school, and after-school programs are typically located in the same geographical location, and in many cases special classes for children with special needs are integrated at these locations, whereas special schools are located at other physical locations.

In 2011, 6 percent of Danish schoolchildren were educated in segregated settings in terms of either special classes or special schools (henceforth "segregated education"). In 2015, the number had decreased slightly to 5 percent, and financial incentives had been installed to bring the number further down to enhance inclusive settings.[8] Segregated settings are targeted at children with particular needs (e.g., ADHD, ASD) and characterized by one to two professionals in a class of five to seven SEN students (Mortensen et al., 2020).

Teaching and learning content in segregated settings is vastly different from that in regular settings. According to Egelund and Rangvid (2014), who gathered information from a survey of special school principals, the principals focus on stimulating the students' personal and social development rather than teaching them the course content. Furthermore, Egelund and Rangvid (2014) report that 70 percent of students are taught below their age-appropriate grade level, and only one-third of children are taught the full course package.[9] Most often, the focus is on language arts, mathematics, and English (Mortensen et al., 2020). Hence, being taught in a segregated setting shifts focus from course content to socio-emotional education.

Each municipality has an authority responsible for pedagogical–psychological counseling, which assesses and refers children to segregated classes after referral from local schools or public daycare institutions in close collaboration with parents and, if children have mental health diagnoses, specialist physicians.[10] If the assessment concludes that the SEN student is

[8]See Lynggaard and Rangvid (2014), who also show that SEN students in special schools and special classes are quite similar along many observable dimensions, including background characteristics, test attendance, and test scores.

[9]See Law of Public Schools enclosure 1, https://www.retsinformation.dk/eli/lta/2024/989 (accessed on 7 October 2024).

[10]Note, however, that students may or may not be considered "disabled", and the existence of "disability" is not formally linked to the services that the student is entitled to. In this respect, the institutional setting may differ from other countries, for example, the United States, where many of the cited studies concern "students with disabilities", who have certain guaranteed rights to appropriate education. In the Danish setting, entitlement to individualized special needs services

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suited for a regular class, the pedagogical–psychological counseling assists the local school in how to include the student. Referral to segregated classes is driven by three main reasons (mentioned in order of importance): (i) the local public school cannot accommodate the special needs of the student at the relevant grade level; (ii) their classmates are negatively affected by noise or conflicts in rare cases, leading to school refusal; or (iii) the teacher cannot cope with the mental and physical stress from including the student in a regular class. The type of segregated class the student is referred to depends on the potential mental health diagnosis (Mortensen et al., 2020). A study by Junge (2022) using school-cohort fixed effects documents that teachers' referrals of students to special education are influenced by the academic performance of the students' classmates.

In 2012, the Danish government implemented a reform of special-needs education that had an explicit goal of reducing the share of students in segregated education to 4 percent by 2015. In practice, this was accomplished by redefining the characterization of SEN students and by introducing economic incentives for schools to accommodate SEN students in regular classes. Rangvid (2019) shows that the reform worked as intended and caused a movement of some students already enrolled in segregated classes into regular classes in particular during 2012 and 2013. Rangvid (2021) shows that this movement back into regular classes is important for results on the grade 9 exit exam and successful progression from lower to upper secondary school.

3. Experimental methods

3.1. Interventions

Along with the reform of special-needs education, the Danish government launched a randomized controlled trial that should test the effect of three types of teachers' aides. The experiment should inform municipalities about the effects of these types of investments in regular classes on regular and SEN students.

The intervention consisted of assigning a teacher aide to the grade 6 class in all of the classes at a given school. The intervention was in place for roughly 85 percent of the school year, from 1 October 2012 to 20 June 2013. The intervention and control groups were announced on 15 August 2012, leaving the schools 1.5 months to search for and employ the respective teachers' aides.

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in public schools relies on an expert assessment of whether the child's development requires at least nine hours of weekly support (see the Law of Public Schools). Alternatives to public schools and services for SEN students are almost non-existent.

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Three types of teachers' aides were part of the experiment:

(1) a co-teacher with a teaching degree (minimum 10.5 lessons per week per class);

(2) a teaching assistant without a teaching degree (minimum 14.5 lessons per week per class);

(3) a coach with a teaching degree with expertise in special education or similar qualifications (minimum 2.5 hours per week per class).

The first two interventions cost about 156,000 DKK per class. The third intervention cost about 21,700 DKK per class or 14 percent of the other two interventions. By comparing the three interventions, we are able to assess the relative costs and benefits of having more-educated teachers' aides versus more time in the classroom with less-educated aides.

As regards the first two interventions, those teachers' aides were present in most language arts and mathematics lessons as well as a number of other lessons (Andersen et al., 2020). Out of approximately 28 lessons per week, the co-teacher's presence corresponds to 38 percent, and the teaching assistant's presence corresponds to 52 percent. When asked about their primary duties, about 80 percent of the main teachers responded that the main duty of the aide was improving academic achievement or well-being (avoiding conflict, etc.), while 12 percent responded that they supported students with SEN, and only 7–8 percent responded practical support. While the co-teachers spent some time teaching students in various flexible ways (inside/outside the classroom or in smaller groups), the teaching assistants who were typically educated pedagogues spent more time handling conflicts and disruptive students. The overall picture, based on the main duties and how they spent their time, supports the fact that both types of teachers' aides may have played an important role in facilitating inclusion of more SEN students in regular classes with teachers' aides. As regards the coach intervention, they spent very little time in class and were to a greater extent used for professional development of the regular teacher of the class (e.g., instructions on class management or how to teach students with reading disabilities).

3.2. Experimental design and participants

The experiment was run as two parallel, stratified, cluster-randomized trials with two treatment arms (co-teacher and teaching assistant interventions) and one control arm in trial 1, and with one treatment arm (coach intervention) and one control arm in trial 2.

The Ministry of Education invited all 98 municipalities in Denmark to sign up their schools for the trial. A total of 68 municipalities applied for

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participation. Of these, 12 municipalities were selected for participation in trial 1, and six municipalities were selected for trial 2. Municipalities were selected in order to create variation in municipality size, school size, and geography while excluding municipalities engaged in school consolidations. This selection process was chosen to create balance (with few municipalities, random selection might have created more imbalance) and to handle the logistics of having coaches working at multiple schools. This means that the effects of the two trials might not be perfectly comparable. However, as they were run within the same country at the same time with the same outcome measures and without any self-selection into the trials, we assess that effect sizes are at least as comparable as meta-analyses comparing intervention effects across multiple trials. The number of enrolled schools were based on the available funding for the intervention, which was financed by the ministry.

Within participating municipalities and to further increase statistical power, schools that signed up were stratified (based on their predicted average achievement level for the relevant grade 6 cohort) and randomly allocated to either treatment or control group within the strata. Table A1 in the Online Appendix presents summary statistics by participating schools. The table illustrates that the selection of municipalities and random allocation of schools into treatment and control groups succeeded in creating balanced groups on a long list of observable school characteristics.

3.3. Sample selection

In this paper, we are interested in the effect of the intervention on SEN students, i.e., students classified with special educational needs by school authorities. In total, 183 schools participated in the trials (105 schools in trial 1 and 78 schools in trial 2).

If a school was assigned to treatment, all regular grade 6 classes received the treatment. This means that there was no within-school selection concerning which regular classes received the teachers' aides. Some schools also had special classes for students at grade 6, which would not receive the additional teaching resource/intervention.

Importantly, within participating schools, some students shifted from special classes to regular classes after the announcement of the randomization result (i.e., between 15 August and 1 October when the intervention began).[11] In addition, some students entered the participating schools from outside. Such transitions happened in both treatment and control groups, and are

[11] It was necessary to announce the randomization result prior to the start of the intervention in order to allow schools time to hire the additional teaching staff meeting the intervention requirements.

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partly due to normal processes when students are assessed ready for regular instruction and/or switch school (e.g., because their family moves to another school district). However, in the following analyses, we test whether the interventions increased the influx of SEN students to the treated classes. The intervention was not and could not be blinded to participating schools and families, so the investment in teachers' aides in the regular classes may have caused more SEN students to be included in the regular classes.[12]

At the time when the intervention period began, trial 1 comprised 5,168 students in regular classes of which 615 students (12 percent) had special needs and an additional 45 SEN students were moved from a segregated to a regular class, resulting in 660 SEN students in total. In trial 2, the regular classes included 2,996 students of which 328 (11 percent) had special needs and an additional 26 SEN students were moved from segregated to regular class, resulting in 354 SEN students. Thus, the initial sample consists of 5,213 students (hereof 660 SEN students) associated with trial 1, and 3,022 students (hereof 354 SEN students) associated with trial 2.

During the intervention period, 18 and 15 additional SEN students enrolled in trials 1 and 2, respectively. If we include students in segregated classes as well, we have a total of 829 SEN students in trial 1 and 455 SEN students in trial 2, and these constitute the final estimation sample.

In the analyses, we consider all SEN students in grade 6 in both regular and segregated classes in treatment and control group schools in order to estimate the total effect of adding additional teacher resources in regular classes for all students with special needs at the school. Our final estimation sample thus includes SEN students in regular classes, SEN students reallocated from segregated to regular classes, SEN students staying in segregated classes, and incoming SEN students. This is to ensure that we compare SEN students included due to the treatment with their best available counterfactual, i.e., SEN students at control schools who stayed in or arrived at the segregated classes. Leaving out the latter group would potentially underestimate the effect of the intervention because the sample of students with special needs at the treated schools would include students with worse disabilities than the sample at control schools. We find that treated schools included more SEN students from other schools compared with control schools; however, we cannot identify the corresponding additional students in the control group. Yet, analyses based only on SEN students who were enrolled at participating schools before the beginning of the intervention generate results very similar to the results presented.

[12]More details on participant flows are available in Table A2.

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3.4. Data and outcomes

To evaluate the effect of the interventions on the short- and long-term outcomes of the students, we combine several data sources. Data collected as part of the randomized controlled trial include survey data on teachers' aides used to characterize their background and tasks in the classroom. By using personal identifiers (similar to social security numbers), we are able to merge the trial data with administrative register data on both the assignment of students (in special or regular classes) in the years prior to the initiation of the trial, and on student data after the beginning of the intervention including data on the students' reading and mathematics skills and their continuation into post-compulsory education up to seven years after the end of the intervention. Furthermore, we augment the data with socio-demographic background characteristics of the children and their parents from administrative registers. Below, we explain how we define background variables, SEN status, and outcome variables.

Background variables. We use pre-treatment data on school, student, and parent characteristics to assess the balance of the experimental groups at baseline and to increase precision in the statistical estimates. Tables A1–A3 in the Online Appendix show balance across experimental conditions at baseline.

Special education needs. We define SEN students based on the administrative register on special education in public schools. The school administration reports to Statistics Denmark yearly (in April) if they have assessed that students have special education needs (e.g., due to behavioral or social disabilities), the primary cause (classified according to the ICD-10 H-classification), and the type of special education received and number of hours per week. We define a student as having special education needs if the student was registered with a cause in grades 4, 5, or 6. By including SEN assessments from the two school years prior to the intervention, we include students who might have been reclassified in the intervention year due to either the 2012 reform (which redefined and increased the requirements to be classified as a SEN student) or the treatment assignment. In total, 71 percent (72 percent) of the SEN students in our sample had already had an SEN assessment in grade 4 and 71 percent (88 percent) in grade 5 for trial 1 (2), respectively (see Table 1). On average, 30 percent (24 percent) had learning disabilities, 36 percent (31 percent) had reading disabilities (e.g., dyslexia), and 44 percent (48 percent) had other disabilities (e.g., ADHD or autism) or causes of special needs in trial 1 (2).

Segregated education. To classify whether a student attends a special class or a regular class, we again use the administrative register on special

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Table 1. Students with SEN: sample means (final estimation sample)

	Trial 1 (Mean)	Trial 2 (Mean)
Boys	0.62	0.62
Psychiatric diagnosis	0.17	0.17
Timing and cause of SEN status		
SEN assessment from grade 4	0.71	0.72
SEN assessment from grade 5	0.71	0.88
SEN assessment from grade 6	0.21	0.23
SEN cause: learning disabilities	0.30	0.24
SEN cause: reading disabilities	0.36	0.31
SEN cause: other	0.44	0.48
Segregated education		
Special class in grade 4	0.18	0.18
Special class in grade 5	0.17	0.20
Special class in grade 6	0.21	0.23
Special school in grades 4–6	0.01	0.02
Inflow compared to initial sample		
Additional students	0.20	0.22
Observations	829	455

Notes: This table summarizes sample characteristics as regards gender, psychiatric diagnosis, and timing and cause of SEN assessment and segregated education. SEN causes are not mutually exclusive and include all causes registered in grades 4–6. Segregated education entails special class or special school, and the left-out category is regular class possibly with some special support services.

education in public schools. This register indicates what type of class the student is enrolled in: a regular class (possibly with special-needs assistance), a special class, or a special school. Table 1 shows that the dominant fraction of SEN students are included in regular classes whereas around 20 percent attend special classes and only 1–2 percent attend a special school. For our empirical analysis, we create an indicator variable for attending segregated education, which is equal to one if a student attends a special class or a special school, and zero otherwise. We register the status of all students in our sample in grades 4 and 5 (prior to the intervention), in grade 6 (towards the end of the intervention), and in grades 7–10 (after the intervention). Segregated education as of grades 4 and 5 is used to study inclusion due to the intervention; the segregated education status as of grades 7–10 is used to study whether inclusion effects persist.

Reading skills. We measure reading skills using national, standardized, computerized, adaptive tests. The tests are adaptive in the sense that after a

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run-in period of five items, the system selects the next item from an item bank based on the estimated skill level of the students (calculated based on responses to previous items) and the estimated difficulty level of the items. The students are tested simultaneously within three reading skill domains: language comprehension, decoding, and text comprehension. Following Beuchert and Nandrup (2018) and Andersen et al. (2020), the final test score within each domain is standardized (mean of 0 and SD of 1) across the full population of public school students taking the test. To compute a composite total reading test score, we calculate the average across the three standardized domain scores and re-standardize the total score. The national reading tests were mandatory for public school students in Denmark in grades 2, 4, 6, and 8 at the time of the intervention. In our final estimation sample, about 80 percent of SEN students took the reading test in grade 6, and the mean test scores in the control groups were −0.85 (SD 0.96) for trial 1 and −0.87 (SD 1.02) for trial 2.

Mathematics skills. Mathematics skills are measured using the same type of national test. They cover three domains: numbers and algebra, geometry, and statistics and probability. We standardize and summarize the mathematics test scores in the same way as the reading test scores. Mathematics tests were mandatory for grades 3 and 6 at the time of the intervention. In our final estimation sample, 76 percent took the mathematics test in grade 6, and the mean test scores in the control groups were −0.64 (SD 1.12) for trial 1 and −0.65 (SD 1.01) for trial 2.

Additional exploratory long-run outcomes. Some additional academic outcomes are available for grades 8 and 9.[13] In grade 8, public school students sit national tests in science and reading, which were of the same type as described above for grade 6. In grade 9, students sit the exit exams in a range of courses including language arts and written mathematics. We standardize test scores and exam scores with mean 0 and SD of 1 using the entire cohort of public school students. In addition to academic outcomes, we define outcomes that indicate whether the students follow normal progression (as opposed to being retained or enrolling in classes cutting across grades): enrolment in grade 7 in school year 2013/2014, grade 8 in school year 2014/2015, etc. Finally, we define the students' highest completed education at year end 2020, where students have turned 20 years old if they started school according to the legislated school starting age and have not repeated a grade before the

[13] See Online Appendix B for more details on the exploratory outcomes.

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intervention. At this point in time, they are expected to have completed at least compulsory education and – if on track – high school or vocational training as well (even if they have spent a year in the optional grade 10).

3.5. Statistical models

To evaluate the effect of the interventions on SEN students, we use the following statistical models depending on the trial evaluated:

$$\text{Trial 1,} \qquad Y_{1,isr} = \alpha + \beta_1 TC_s + \beta_2 TA_s + \partial_1 X_{0,isr} + \gamma_r + \varepsilon_{isr}; \qquad (1)$$

$$\text{Trial 2,} \qquad Y_{1,isr} = \alpha + \beta_3 COACH_s + \partial_1 X_{0,isr} + \gamma_r + \varepsilon_{isr}. \qquad (2)$$

Here, Y_1 is the outcome variable for individual i, in school s, in strata r. The indicator variable TC is equal to one if the school, s, is randomly assigned to the co-teacher intervention, while TA is equal to one if the school, s, is randomly assigned to the intervention with a teaching assistant. X_0 includes student characteristics measured prior to the intervention. In our main model specification, X includes student i's pre-test scores in reading (from grade 4) and mathematics (from grade 3) to control for student ability. γ_r is a randomization strata fixed effect, and ε is the error term. We evaluate trials 1 and 2 separately. Thus, for trial 1, we estimate equation (1) by OLS, and for trial 2, we estimate equation (2) by OLS, where the only change is the inclusion of an indicator variable $COACH$ equal to one if the school, s, is randomly assigned to the coach intervention.

4. Results

In this section, we examine how these interventions attracted students from segregated education, and how they affected the SEN students during the intervention. We also examine the downstream effects on the SEN students up to seven years after the intervention ended, even though effect estimates are less precise. Finally, we consider the financial costs and benefits of the treatments.

4.1. Effects on inclusion of SEN students

Table 2 reports the effect of the announcement of treatment assignment on enrolment of students that were in segregated education prior to the intervention.[14] Model 1 in Table 2 shows that treatment schools had more

[14]This showed up as a slight unbalance and was reported as such by Andersen et al. (2020).

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Table 2. Effects of the interventions on attending special education, prior to intervention (initial sample)

	All students (1)	SEN students (2)
Trial 1		
Co-teacher (CO) with degree	0.010**	0.071**
	(0.005)	(0.029)
Teaching assistant (TA) without degree	0.008**	0.081***
	(0.004)	(0.029)
Observations	5,213	660
Adjusted R^2	0.0455	0.214
Test: CO = TA (p-value)	0.639	0.811
Control group mean	0.004	0.029
Trial 2		
Coach	0.006	0.077*
	(0.005)	(0.043)
Observations	3,022	354
Adjusted R^2	0.063	0.287
Control group mean	0.005	0.042

Notes: The dependent variable is equal to one if the student attended segregated special education (special class or special school) in grades 4–6 prior to the start of the intervention. OLS model with controls for students' reading and mathematics pre-tests, indicator for missing pre-test, and randomization strata fixed effects. Robust standard errors in parentheses are clustered at intervention school. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

students enrolled in segregated education in grades 4–6 prior to the intervention compared with the control schools. An additional 1 percentage point in the co-teacher intervention, 0.8 percentage points in the teaching assistant intervention, and 0.6 percentage points (not statistically significant) in the coach intervention enrolled. The same pattern is seen when limiting the sample to SEN students, but the point estimates are scaled up to 7–8 percentage points instead of 0.6–1 percentage points (see Table 2, model 2). This corresponds to 1.5 students per class (or 3.7 students per school). Estimates reported in Table 2 are similar when estimated using probit or logit models (Table A4).[15]

4.2. Effects on SEN students during the interventions

In this section, we examine the effect on SEN students during the interventions. In order to correct for the sample selection, the final analysis sample of SEN

[15]To account for the randomization design, we control for randomization strata fixed effects in our preferred OLS model, which is not possible in probit/logit models. Estimates for trial 2 are slightly smaller when leaving out randomization strata fixed effects, but then estimates are similar for OLS, probit, or logit models (see Table A4, Columns 3–8).

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students includes all SEN students from the trial data and also all students in segregated classes at the school. The sample is generally disadvantaged (see Table A3): students scored on average between 0.7 and 1.0 SD below the population means in mathematics and reading in grades 3 and 4, three-quarters of the parents have no college degree, and 15 percent are diagnosed with a psychiatric diagnosis such as ADHD. Table A3 tests the sample balance across treatment arms after extension of the sample to include all SEN students. The sample is well balanced especially for trial 1. In total, 2/50 and 4/50 characteristics are significantly different across treatment status at the 5 percent and 10 percent levels for trial 1, and 3/25 and 4/25 for trial 2.

Table 3 reports the effects on the students' academic achievements measured at the end of the intervention period (grade 6). The table shows large and significant effects on reading of the co-teacher (0.158 SD) and the teaching assistant (0.184 SD) interventions, but not of the coach intervention. Furthermore, the effect of the co-teacher intervention on mathematics is of the same magnitude but only borderline significant, while the other two treatments show no statistically significant effects on mathematics.[16] In Tables A5 and A6, Columns 1–4, we gradually add control variables and show that the effects are robust after controlling for the reading pre-test.[17] In Columns 5 and 6, we add interaction effects with "additional students" (i.e., SEN students included after the intervention). The results suggest that effects are driven by SEN students already in the class before the intervention. However, the point estimates on the interaction effects suggest that "additional students" also gain from the co-teacher and the teaching assistant intervention but the sample is too small to make firm conclusions about this.

The effects of the treatments on test scores are, of course, conditional on test participation, which to some extent is reduced by treatment. Tables A5 and A6 show that there are no differences, or only minor ones, in test participation across control and treatments in trial 1, whereas the coach intervention in trial 2 induced significantly fewer SEN students to participate in the test compared with the control group. This might indicate that the coach assisted the main

[16]If we implement a Bonferroni correction for multiple hypotheses testing, which accounts for the three tests on our main outcomes – segregation in grades 4–6 (reported in Table 2, Column 2), reading, and mathematics scores (reported in Table 3) – most of the conclusions survive. The effect of segregation prior to the intervention is still significant at the 5 percent level for the co-teacher and teaching assistant intervention but not for the coach intervention, whereas the effect on reading scores is significant at the 10 percent and 5 percent levels for the co-teacher and teaching assistant intervention, respectively, while it is no longer significant for mathematics (p-value, 0.18).

[17]Controlling for pre-tests is important due to a slight imbalance; pre-tests were 9 percent and 5 percent of a standard deviation lower for individuals exposed to the co-teacher intervention (see Table A3).

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Table 3. Effects of interventions on academic outcomes, during intervention (final estimation sample)

	Reading, grade 6 (1)	Mathematics, grade 6 (2)
Trial 1		
Co-teacher with degree	0.158**	0.196*
	(0.074)	(0.105)
Teaching assistant without degree	0.184**	0.054
	(0.074)	(0.106)
Observations	699	679
Adjusted R^2	0.404	0.288
Test: CO = TA (p-value)	0.768	0.215
Control group mean	−0.850	−0.635
Trial 2		
Coach	−0.102	0.024
	(0.099)	(0.120)
Observations	359	363
Adjusted R^2	0.382	0.257
Control group mean	−0.867	−0.650

Notes: Due to test non-participation, the samples are reduced compared with the final estimation sample consisting of 829 (trial 1) and 455 (trial 2) observations. OLS model with controls for reading and mathematics pre-tests, indicator for missing pre-test, and randomization strata fixed effect. Robust standard errors in parentheses are clustered at intervention school. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

teacher in seeking exemption for taking the tests, or that they reclassified students to make the intervention look better, or that although more students were included in the regular class, the low-intensity coach intervention could not support test participation of the additional students.[18] The insignificant results on test scores for the coach intervention should be interpreted in the light of higher non-participation. If exempted students have systematically lower academic progression than average, then this means that the effects might be overstated, and vice versa if they have higher academic progression. If the coach indeed caused higher attrition, then this should be more frequent among SEN students in regular classes (as opposed to those in special classes, who were not exposed to the coach), which means they are not necessarily at the bottom of the latent test scores distribution. To investigate the importance of attrition empirically and to test robustness, we impute missing outcomes and compute Lee bounds. In Column 1 of Table A7, we assume that the

[18]In Columns 5 and 6 of Tables A5 and A6, we see that attrition is generally higher for "additional students", but this does not vary by treatment status. Moreover, the effects on test scores are no different for "additional students".

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missing observations would have obtained at least the same value as the control group's mean test score, and in Column 2, we assume they would have scored 1.5 SD below the population mean. For trial 1, the effects are of the same magnitude and significance in both reading and mathematics (as expected), while for trial 2, the effects are smaller but remain insignificant after imputing the missing outcomes. In Table A8, we provide Lee bounds, which trim the bottom (top) of the control group test scores to make the groups more comparable. The effects of the co-teacher and the teaching assistant are smaller, but largely verify our estimates in Table 3. For the coach intervention, the lower bound is large and significantly negative, whereas the upper bound for mathematics is large and significantly positive, which emphasizes that the results are uninformative about the sizes and signs of the effects.

4.3. Effects on SEN students after the end of the interventions

Using register data, we follow how the students progress throughout the educational system in the years following the intervention. We note that these analyses are to be considered exploratory as we test multiple outcomes and the estimates might have weaker statistical power. That said, longer-term outcomes can provide at least some indicative evidence as to whether investing in a high-cost intervention in one school year will change the educational progress of some students to consider it worth the money spent.

For the last three years of compulsory school (i.e., grades 7–9) the results suggest that once included, students are not referred back to segregated education when the extra teacher resources are removed, and SEN students who were already in the regular class before the intervention are also able to stay in the regular class afterwards to the same extent as in control schools (see Table A9). The students' academic achievement found during the intervention, however, has disappeared by grades 8 and 9 (see Table A10).

Evaluating the effects on educational attainment after the mandatory grade 9 is complicated because there are many routes through the educational system in Denmark via optional grade 10 (in public or independent schools) to completion of upper secondary school at either academic high schools (preparing for college) or vocational schools (preparing for skilled jobs). We therefore examine year-by-year whether students are on track, and then we summarize these analyses in a test of their highest completed level of education seven years after the end of the interventions. Tables A11 and A12 show that the treated SEN students are slightly delayed from moving to upper secondary school. However, Table A13 shows that students exposed to the co-teacher treatment have caught up completely in terms of highest attained education at the end of year 2020, which is in line with the favorable effects on academic achievement in the earlier grades. On the contrary, the students exposed to the teaching assistant or the coach intervention lag behind, and

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they are 4–8 percentage points less likely to have completed high school in 2020 compared with the control group.

Overall, the results in this section indicate that even though the co-teacher intervention attracted more SEN students, and even though the positive effects on academic outcomes faded, these students still seem to complete upper secondary education to the same extent as SEN students in the control group. In the teaching assistant group, SEN students completed high school to a lower extent (about 8 percentage points) than the control group. The effect of the coach on highest completed education was statistically insignificant but of negative sign, as for the teaching assistant.

4.4. Costs of the intervention

In Table A14, we report cost measures to be used for an analysis, comparing the intervention costs with saved costs on other special education services. The costs of a teaching assistant or a co-teacher for one class in our intervention is similar to the additional costs of assigning 1.1 student to a special class rather than a regular class for one school year. In comparison, we estimate that the teachers' aide treatments caused an additional 1.5 students to become included in regular education per class (see Section 4.1). So, the teachers' aide policy has an immediate favorable effect on the costs of educating SEN students.

The immediate effect compares the costs of a teachers' aide for 75 percent of a school year with the saved costs of including an SEN student in a regular class for one school year. When the included SEN student stays in the regular class until the end of compulsory education (i.e., four years) – as our results show they do – the cost–benefit ratio is obviously even more favorable, even though we discount the future savings by a discount rate of 3.5 percent (see Figure 1).

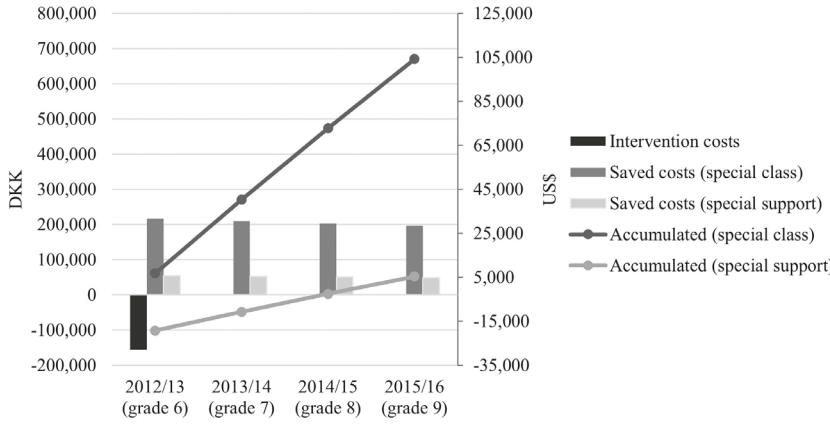
We consider only the direct costs of the nine-month interventions. School principals and policymakers building on our results might want to use co-teachers for a full year and even for multiple grade levels (i.e., grades 6–9). However, our study shows that it is feasible to include co-teachers only in grade 6, and it is one of the benefits of co-teachers – compared with, for example, reductions in class size – that they can be applied flexibly at some grade levels (not all), in some periods of a year (not the full year), in some classes (e.g., with special needs), in some subjects (e.g., reading) and not others. Therefore, in the cost analysis we only consider the direct costs of the intervention and the immediate savings in terms of moving students from special education classes to regular classes.

Thus, Figure 1 illustrates the costs of the intervention compared with benefits in terms of saved costs from including additional SEN students. The figure shows the accumulated saved costs from including one student in a regular class who would otherwise have been in a special class (dark

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Figure 1. Cost–benefit illustration



Notes: "Accumulated (special class)" is the accumulated saved cost assuming the student no longer has to attend a special class after inclusion in the regular class. "Accumulated (special support)" assumes that the student no longer has to receive special educational support in the regular class. The calculations only include the direct costs and benefits (saved costs on other special education services); benefits in terms of saved costs on other public services or in terms of higher academic skills are not included. Future costs and benefits (saved costs) are discounted to the year in which the intervention began (2012) with a discount rate of 3.5 percent.

gray) as well as the accumulated saved costs from including one student who would otherwise have received special education support in a regular class (light gray). While there is break-even during the first year (grade 6) if the intervention makes it possible to include one student from a special class with no further special education support, it takes another two years to recover the saved costs if the intervention "only" makes it possible to remove special education support in a regular class for one student. The illustration does not take any other costs or benefits into account that might relate to intervention effects or external effects on peers without special needs.[19]

5. Conclusion

Existing research tends to show that inclusion of SEN students in regular classes has negative impacts on both the SEN students themselves and their peers without special needs. However, these studies typically compare regular

[19]If we take into account that 3.4 percent of the SEN students in the co-teacher condition are delayed in grade 9 and therefore spend an extra year in school (see Table A11 in the Online Appendix), the accumulated saved costs on special classes are reduced by 0.2 percent and accumulated saved costs on special support are reduced by 3 percent.

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classes without any additional teacher support to segregated education, which is much more expensive due to a higher teacher–student ratio.

Our results document that assignment of a co-teacher, a teaching assistant, or a coach increased the inclusion of SEN students in regular classes. The teachers' aides interventions increased the proportion of SEN students by 7–8 percentage points corresponding to 1.5 students per class (or 3.7 students per school). As long as the co-teachers and the teaching assistants were present in the classroom, the SEN students (those included after the announcement of the intervention and those already included in the classes) gained academically in terms of better reading skills and – in the case of co-teachers – better mathematics skills. The point estimates are in the same ballpark as the effects of the treatments on regular students (Andersen et al., 2020). The much cheaper coach intervention did not improve the students' academic skills.

After the end of the interventions, the academic edge of the treated students faded. However, they stayed included in the regular classes, and despite a little delay in the progression through the educational system, by the age of 20, the SEN students in the co-teacher treatment group had completed upper secondary education to the same extent as the SEN students in the control group (where fewer SEN students were included in the first place). In the other two interventions, about 4–8 percentage points fewer students had completed upper secondary education by the age of 20. This corresponds to the 7–8 percentage points more students who were included in these classes in the first place as an effect of the intervention. Many factors, including compensatory investments in control group students, may explain the fading out of the interventions; for further discussion, see Andersen et al. (2022).

In sum, the results of these trials show that investing in regular classes using teachers' aides is a cost-effective way of including SEN students in the daily lives of their peers without special needs. Especially, the co-teacher intervention had immediate positive effects on their academic skills, and the students benefiting from this intervention stayed in the regular school system throughout upper secondary education. By comparing the co-teacher intervention to the much cheaper coach intervention, we can conclude that while the coach intervention was enough to attract students from segregated education into regular classes, it was not strong enough to have a positive impact on their academic skills.

There are limitations to the study presented here. In particular, we can only speculate about what the effects would have been if the co-teacher intervention had been maintained throughout grades 7–9 or if the co-teacher intervention had been combined with the coach intervention. The school system saved the additional costs of special-needs education during these years, and if these resources had been invested in the SEN students, they might have maintained the benefits of the intervention.

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Another limitation relates to the selection of students from segregated classes into the treated classes. We cannot distinguish statistically between effects on SEN students who were already included before the trial and those who moved to being included as a result of the treatment, because the latter group is too small. If the positive results of the present study inspire school leaders to test the effect of similar interventions, it would be very relevant to test more systematically what groups of SEN students may benefit the most from being included in regular classes together with support from a co-teacher.

With these caveats in mind, the results presented here point to a direction for future research and policy development aimed at fulfilling the promises of the Salamanca Declaration where SEN students are supposed to have access to regular schools and to the extent possible be educated alongside their peers without special needs.

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Supporting information

Additional supporting information can be found online in the supporting information section at the end of the article.

Online appendix
Replication files

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