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## Abstract

We exploit variation stemming from school consolidations in Denmark from 2010-2011 to analyze the impact on student achievement as measured by test scores. For each student we observe enrollment and test scores one year prior to school consolidation and up to four years after. We find that school consolidation has adverse effects on achievement in the short run and that these effects are most pronounced for students exposed to school closings. Furthermore, students initially enrolled in small schools experience the most detrimental effects. The effects appear to weaken over time, suggesting that part of the effect is due to disruption.

JEL-Codes: I210, J240.

Keywords: school size, school resources, disruption effects, educational production function, test scores.

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## I. Introduction

In recent years, policy makers all over the world have imposed structural changes on schools and students to improve student achievement that encompass large and small changes in the students' current learning environment and that range from major school consolidations to minor adjustments in the quantity or quality of inputs. In economics of education, school size, in addition to student-teacher ratio, class size, and teacher qualifications, is considered one of many inputs in the educational production function. A growing literature exists on the causal impact of each of these policy instruments on student achievement in the medium and long term, but not much is known about the magnitude of the potential short-term disruption effect on the students' learning environment while implementing the changes.

In this paper, we estimate the short-term effect of school consolidation by exploiting a recent wave of school consolidations in Denmark. Our findings suggest that school consolidation adversely affects student achievement. In addition, at least part of the effect seems to be caused by a short-term disruption effect.

In North America and many European countries, including Denmark, policy makers are convinced that larger schools are less costly than smaller schools due to economies of scale (e.g. Leithwood and Jantzi 2009). Further, it is often argued that larger schools are better than smaller schools when it comes to teacher specialization, qualifications and course quality. As a result, school consolidations (school closings, expansions, and mergers) are spreading and primary school size trends upwards (Ares Abalde 2014). There is not much hard evidence, however, to support the supposedly beneficial effects of school consolidation, even though the impact of school consolidation and the closely related issue of the impact of school size have been studied intensively; see e.g. Kuziemko (2006), Berry and West (2010), Brummet (2014) and de Haan et al. (forthcoming).

This paper examines the impact of school consolidation on individual student achievement by employing a difference-in-differences (DID) strategy on detailed, student-level data. In contrast to many previous studies, we are able to follow the development in individual student test scores throughout a school consolidation. For each student, we follow yearly enrollment and test scores one year prior to consolidation and then up to four years after. The consolidations we consider were the result of local school reforms that took place in Denmark in 2010 and 2011. During these two years, 312 out of about 1,500 schools were closed, expanded, or merged, leaving approximately 15% of all students affected by the consolidations. This led to an average increase in school size at the individual level that ranged from 70 students for schools that expanded to 230 students for schools that closed. For the remaining schools unaffected by the reforms, the average increase in school size was only about five students. In contrast to the typical school restructuring taking place in the U.S., e.g. as studied by Engberg et al. (2012), the closing or restructuring of schools was not primarily targeting low-performing schools. Hence, we are able to investigate heterogeneous effects that reflect more than just the effects for low-performing, 'displaced' students and better-performing, 'receiving' students.[1] This does not influence the internal validity of the estimates but it does possibly improve the external validity.

We contribute to the existing literature by exploiting variation from school consolidations in a setting where the decision to consolidate schools is not dominated by performance measures. We are able to follow the development in individual student test scores across a period of school consolidation. In addition, our rich student-level panel data allows for detailed heterogeneity analyses, which can be informative about the effects of consolidations for different types of students.

[1] We use the term 'displaced students' to describe students who were exposed to a school closing while students in expanding schools are labelled 'receiving' students.

We find that individual student test scores declined with consolidation and that the negative overall effect of consolidation appears to be driven by school closings, with student test scores decreasing by 5.9% of a standard deviation (SD). Furthermore, comparing the two- and four-year achievement gains, the results indicate that the detrimental effect of consolidations seems to diminish over time. Finally, we interpret the short-term negative effects as evidence that a disruption cost exists but that the magnitude is not larger than could be compensated for by, for example, smaller class sizes or having teacher's aides.

The remainder of this paper is structured as follows. First, Section II discusses why school consolidation may affect student achievement. Section III then presents the relevant educational institutions and Section IV describes the data and the consolidations. Next, Section V presents the empirical analysis as well as robustness checks. Finally, Section VI investigates disruption as a potential mechanism and Section VII concludes the paper.

## II. Why Should School Consolidation Matter for Student Achievement?

School consolidation primarily affects schools by increasing school size and saving school costs, which is often the purpose of the merger. School consolidation, however, could also potentially impact the composition of the peer group, which may be another motivation for consolidation. In addition, it is likely that consolidation represents a structural change that exerts psychological costs on the students and teachers and therefore potentially distorts the learning environment. From the perspective of the student, a school transition has been hypothesized to lead to two main effects.[2] First, a school transition causes a *disruption effect*, which is a short-term effect – although it could potentially have long-lasting repercussions.

[2] See e.g. Hanushek et al. (2004) and Behaghel et al. (forthcoming).

Second, a school transition typically causes a change in *school quality*, e.g. school size and peer composition, which would possibly result in longer-term effects.

Most previous studies on the effect of consolidations focus on school size and are based on data from the U.S. or the U.K. They generally expect larger schools to produce positive effects due to the increased specialization of teachers, a more heterogeneous teacher and student composition, and, based on economies of scale, improved opportunities for the school to recruit and attract high-quality teachers, in addition to better time allocation between teaching and administrative work; see, for instance, Garrett et al. (2004), Leithwood and Jantzi (2009) and Ares Abalde (2014). On the other hand, researchers also recognize the possibility that smaller schools constitute a more intimate and safer environment, which may give teachers and students a more positive perception of schooling and thereby better support the learning environment. Thus, there are financial, sociological, and psychological arguments as to why the size of a school might affect student learning and achievement. To date, the empirical evidence on the signs of the effects is ambiguous. Leithwood and Jantzi's (2009) survey, for example, arrives at two main conclusions. First, the empirical evidence generally favors small schools, both in terms of student test scores and social factors. Second, the more recent research indicates that cost-effectiveness and efficiency are not a justification for larger schools.

Recent attempts to identify the impact of school size based on quasi-experimental variation yields ambiguous results; this is clear from the recent review by Humlum and Smith (2015b). Schwartz et al. (2013) and Barrow et al. (2015) are based on instrumental variable methods exploiting variation in distance between the student's home and the closest small high school as an instrument for school choice. They report favorable effects of attending a small high school on various outcomes. Because these studies draw upon distance to school as an instrument, they only identify the effect of interest under the restrictive assumption that the effect of school size is homogeneous. If effects are in fact heterogeneous, the instrument is invalid if there is a systematic relationship between the distance to a small school for those students opting for a small school and their expected return from attending a small school. Abdulkadiroğlu et al. (2013) also focus on the effects of attending a small high school but are able to use assignment lotteries to identify the causal effect. They find positive effects of small high school size on a range of outcomes, including course scores and college enrollment.

While the previous authors investigated high schools, Kuziemko (2006), Berry and West (2010), Liu et al. (2010), de Haan et al. (forthcoming), and Humlum and Smith (2015a) study primary schools. Kuziemko (2006) uses variation stemming from aggregate school-grade data on school mergers, student background and outcomes, and implements an instrumental variable method. She finds that small schools are more favorable for student outcomes than large schools. Berry and West (2010) exploit variation in the timing of school consolidation across the U.S. and find that students educated in states with smaller schools obtain higher returns from education. On the other hand, Liu et al. (2010) study mergers occurring in China in 2002 and use DID and propensity score matching methods to document the absence of effects on test scores. De Haan et al. (forthcoming) consider a reform of the Dutch school system that implied a decrease in the number of schools and find positive effects on student test scores upon completion of primary school. Their empirical strategy compares the cohort completing primary education before the reform with the cohort enrolled and completing primary education after implementation of the reform. They investigate four potential mechanisms and conclude that the positive effect of consolidation is mainly driven by school size. A recent longitudinal panel study from Denmark supports the non-negative impacts of increasing school size on long-term outcomes such as educational outcomes and earnings. To arrive at this conclusion, Humlum and Smith (2015a) exploit registry data on the total population and school catchment areas in order to apply multiple estimators and instruments. Combining the evidence from different identification strategies, studies by de Haan et al. (forthcoming) and Humlum and Smith (2015a) seem to suggest that larger schools (in countries where the average school size is small) do not harm students.

However, consolidation potentially affects students, teachers, and schools in other ways than through school size. Consolidation often leads to the relocation of students and changes in their learning environment. As such, the effects of consolidation can be expected to be similar to what happens as a result of voluntary school moves. Of course, voluntary school moves are different in nature, just as the magnitude and direction of the effects on student academic performance may differ substantially. School moves are generally viewed as being associated with disruption costs and changes in school quality, see e.g. Hanushek et al. (2004) and Behaghel et al. (forthcoming). Disruption costs may play a particularly important role when moves occur due to consolidation, which inherently affects multiple students simultaneously.

Only a few of the above-mentioned studies, however, focus on the impacts of consolidation other than change in school size. Three recent articles (Liu et al. 2010, Engberg et al. 2012, Brummet 2014) with an approach similar to the one in this paper analyze the short-term effects of consolidation on displaced and receiving students in particular. Liu et al. (2010) study the closure of small schools in remote areas of rural China. Engberg et al. (2012) examine school closings in an anonymous U.S. urban school district, while Brummet (2014) explores school closings in Michigan. They generally find that displaced students are hurt more by consolidation than receiving students. The policy for school closings analyzed in Engberg et al. (2012) directly targeted low-performing schools for closure. Brummet (2014) observes a dip in math scores in schools prior to their closure, but state that the lowest-performing schools were not necessarily the ones in the district that were closed. Our paper contributes to this literature by exploiting variation from school consolidation in a setting where restructuring of the municipalities and rationalization triggered consolidation as opposed to low performance.

## III. Institutional Environment

### *A. Danish Schools and Educational System*

In Denmark, the period of compulsory education is ten years, during which students are not tracked. Generally, students are divided into classes when they enroll and stay in the same class throughout primary school. The maximum official class size is 28 students but varies considerably across schools and cohorts.

The local authorities are responsible for compulsory education in Denmark and in 2010 comprised 98 municipalities, each of which is divided into one or more catchment areas with one school each. The catchment area a child belongs to is determined by the parents' residency and is where the child goes to school. Since 2006, however, parents have been allowed to freely choose which school to enroll their child in, thus allowing them to select a catchment area other than their own if an opening is available.[3] Parents can also choose to enroll their child in a private or independent school, some of which have a religious or ideological foundation, though others are simply an independent alternative to public schools. Private schools receive substantial financial support based on the number of students enrolled (Ministry of Education 2012). In contrast to other countries, low-income parents are eligible for a voucher if they choose to send their child to a private school, thus making them

[3] See the Danish Public School Act.

accessible to more than just privileged children. Parents who choose a private school over a public school, however, may share other observable and unobservable characteristics.

The average school size in Denmark is relatively small. In 2011 it was 374 students per public primary school (KORA 2012), making Danish schools much smaller than their counterparts in, for instance, the U.S. and the U.K., where 500-600 students is considered small (Berry and West 2010).

*B. National Tests*

The performance of Danish primary school children was not systematically evaluated until 2010, which is when the Danish Parliament introduced yearly systematic nationwide testing in compulsory education. Reading tests are administered every other year from second to eighth grade and math tests are given in third and sixth grade (Beuchert and Nandrup 2014). Since the tests are mandatory and students are tested in the same subject in different grades, they are ideal for analyzing achievement gains and learning progress during compulsory education. Each national test assesses three different cognitive areas per subject, known as a profile area. More specifically, reading tests measure: 1) language comprehension, 2) word recognition, and 3) text comprehension, while math tests measure: 1) numbers and algebra, 2) geometry, and 3) applied mathematics.

The national tests are adaptive and they objectively estimate the student's ability based on only two parameters: the difficulty level of the question and the student's estimated ability based on the previous questions. The final measure of ability is estimated using a Rasch calibrated logit scale. Thus, the test score is not a measure of how many correct answers the student gets but an estimate of the student's ability within the specific profile area.[4]

*C. School Consolidation*

On January 1, 2007 a large reform of the municipalities in Denmark was implemented. From the perspective of school policy, this was a large-scale school district consolidation. The number of municipalities was reduced from 271 to 98. The municipality is the local authority that delivers primary and lower secondary education in Denmark. This reform was probably one of the main reasons for the wave of school consolidations that followed. One of the arguments in favor of the reform was that larger municipalities could generate gains based on a reorganization of their school structure, see Strukturkommissionen (2004). For example, larger municipalities were hypothesized to have better opportunities for establishing large schools, limit per students costs, and adapt to demographic changes. School consolidations are, however, controversial and with local elections coming up in 2009, the topic was postponed in many municipalities.

In a number of municipalities, the politicians decided to have major investigations of the future school structure, demography, and school costs by external consultants combined with public hearings among the local citizens. Part of the purpose of these activities which obviously delayed the consolidation process considerably may have been to mature decision and to help the politicians to implement unpopular decisions.[5]

In 2010, the Danish government initiated an enforced sanction regime on central government and municipalities (Budget Law). The Budget Law introduced binding multi-annual

[4] For more details, see Beuchert and Nandrup (2014). For a more technical description of the model, underlying assumptions, and the estimation, see Wandall (2011).
[5] See http://www.folkeskolen.dk/509735/hver-femte-skole-er-vaek-til-sommer

expenditure ceilings on actual spending and includes a broad range of public expenditures (KORA 2015).[6] This initiated larger school restructurings.

As a result, 59 of the 98 municipalities consolidated a large number of public schools during 2010 and 2011. In total, 312 out of 1,479 schools were closed, expanded, or merged as a result of local school reforms.

We define three types of school consolidation:

- *i.* *School closings:* The school is completely closed and the students are moved to either an existing school or a new school.
- *ii.* *School expansions:* The school expands by taking in students from one or more school closings.
- *iii.* *School mergers:* The school merges administratively with one or more schools so they have a shared secretariat, principal, teachers, and some facilities; however, the students physically continue to attend their old school.

Consolidation took place during the 2010 or 2011 summer holiday. The municipal board typically announced local school reforms with the financial budget negotiations in October. The main arguments raised were the Budget Law and declining trends in the number of school-aged children (Eurydice 2013). The financial budget is passed no later than March 1 the following year. This means that the school boards and parents have less than one school year to opponent the consolidation plans. The restructuring mainly affected small schools and schools in old buildings as well as schools located close to each other at either side of the old

[6] From 2009 to 2014, the municipal expenditures for the public schools at large were reduced by approximately 6.3% (KORA 2015).

municipality borders.[7] Since the consolidations appear to have been motivated primarily by economic challenges, consolidation should only to a lesser extent be related to student characteristics. In the data section, we describe the consolidations and affected schools in more detail.

## IV. Data

### *A. Sample Selection*

Student enrollment is registered at the beginning of each school year for all students in Denmark, thus making it possible to individually track students, their school, and grades over time. The student registry covers all educational institutions in Denmark, including private schools, which means data is available on students who move to another school and the type of school. This information on schools is also merged with information from Statistics Denmark, which links students to their parents and extensive information on socioeconomic variables such as health status and employment status.

The main sample used in the estimations consists of all second and fourth grade students who attended a public school and completed the national reading test during the 2009-2010 school year and again during the 2011-2012 school year, where they attended fourth and sixth grade, respectively. Hence, we follow two cohorts of students for three subsequent school years and focus on students who have been tested in reading, a subject that is tested more frequently than other subjects.

[7] Bækgaard et al. (2015) use the reform-induced variation in distances between schools to estimate the effects of school size on costs and find that school closures in Denmark from 2007-2011 have reduced costs by 3.9 percent.

Table 1 summarizes the sampling process. First, all second and fourth grade students attending public schools in the 2009-2010 school year were selected. Next, the sample was restricted to students who continued in the public school system in order to have a sample with both pre- and post-test scores. In other words, all students who left a public school to enroll in a private school or in special needs education were excluded for both of the school years under study. Since these students are dropped from the main sample, this potentially introduces selection bias. We address this issue in Section V. Finally, the sample was restricted to students with no missing test scores in 2010 and 2012[8], thus resulting in a final sample of 90,496 students. In our sample, 15.5% of the students were affected by school consolidation.

TABLE 1. SAMPLE SELECTION

| Sample selection process | Total no. of students | No. of students affected by school consolidation (%) | | Percentage of total sample |
|---|---|---|---|---|
| All students enrolled in 2nd or 4th grade in a public school during the 2009-2010 school year | 114,875 | 16,994 | (14.8%) | 100.0% |
| All students minus students who exit to special needs education | 114,248 | 16,740 | (14.7%) | 99.5% |
| All students minus students who exit to private or independent schools | 110,389 | 16,277 | (14.7%) | 96.1% |
| Pre- and post-test scores available | 90,495 | 14,025 | (15.5%) | 78.8% |
| Students in 2nd grade (age 8) | 45,155 | 6,967 | (15.4%) | 39.3% |
| Students in 4th grade (age 10) | 45,340 | 7,058 | (15.6%) | 39.5% |

[8] About 15% (8%) of the students in the sample had missing test scores in 2010 (2012). The majority of missing test scores in 2010 were due to unsystematic technical breakdowns in the online test system (Wandall 2011). Due to missing pre-test scores, seven (three) of the schools that underwent consolidation in 2010 (2011) and 44 non-consolidated schools are not represented in the final sample, corresponding to approximately 3% of the schools. We did not find any systematic differences in missing pre-test scores based on consolidation status.

In order to observe pre-test scores in the data, we analyze school consolidations that took place no earlier than 2010. The tests in our study were taken by second and fourth grade students, typically aged 8 and 10 years, respectively, in spring 2010 (pre-test scores) and two years later in spring 2012 when the students were in fourth and sixth grade, typically aged 10 and 12 years, respectively (post-test scores). We standardize the test scores by year, grade, and profile area, and then we calculate an average test score across the three profile areas and stardardize again. Table 2 reports the means of test scores before and after potential exposure to school consolidation. Students are grouped based on their *predicted* – not necessarily their *actual* – exposure in order to allow for endogenous student mobility. On average, students exposed to school closings and expansions performed slightly below the mean on the second and fourth grade pre-test. Students exposed to school mergers did not perform significantly differently compared to students at non-consolidated schools.

TABLE 2. MEANS OF PRE- AND POST-TEST SCORES BY EXPOSURE TO SCHOOL CONSOLIDATION

| | (1) Non-consolidated schools | | Type of school consolidation: (2) Closings | | (3) Expansions | | (4) Mergers | |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| *Pre-test:* | | | | | | | | |
| 2nd grade | 0.038 | (0.975) | -0.062 | (0.992)*** | -0.028 | (0.979)*** | 0.058 | (0.969) |
| 4th grade | 0.042 | (0.962) | -0.038 | (0.979)*** | -0.063 | (0.984)*** | 0.020 | (0.954) |
| *Post-test:* | | | | | | | | |
| 4th grade | 0.055 | (0.960) | -0.082 | (0.983)*** | -0.014 | (0.979)*** | 0.076 | (0.941) |
| 6th grade | 0.071 | (0.953) | -0.092 | (0.996)*** | -0.045 | (0.961)*** | 0.015 | (0.948)*** |
| No. of students | 76,471 | | 3,900 | | 5,196 | | 4,928 | |

Note: *, **, *** indicate that the mean is statistically different from the mean of the non-consolidated schools at the 10, 5, and 1 percent levels. SD: standard deviation.

*B. Background Characteristics*

Since our estimation strategy effectively removes any time-constant individual variables, individual-level control variables are not central to our main analysis. However, the fact that we have rich information on the students and their parents allows us to describe the estimation sample and compare students from schools affected by consolidation with students from schools not affected by consolidation.

Table A1 in the Appendix provides descriptive statistics for the estimation sample. In order to assess the degree to which school consolidation is determined by or correlated with student characteristics, we split the sample based on whether or not the student attended a school that was affected by school consolidation and the type of consolidation.[9] Child characteristics include sex, immigration status, birth weight, and whether the child had been diagnosed with ADHD or any other mental or behavioral disorder[10] or had special educational needs.[11] With respect to child characteristics, the differences between consolidated and non-consolidated schools are not that pronounced. Students from closed schools, however, are more likely to be immigrants and have a slightly lower birth weight, while school expansion students are slightly more likely to have special educational needs. School merger students are less likely to be immigrants, less likely to have special educational needs, but more likely to be diagnosed with ADHD or similar diagnosis. Parental characteristics include completed higher education, years of work experience, earnings, employment, attachment to the labor market as measured in 2009, age of the parents at birth, and whether the mother is a single mother.

[9] All characteristics are measured in 2009, i.e. prior to potential exposure to consolidations.

[10] Identified based on the International Classification of Diseases (ICD-10) classification group F. In Denmark, a psychiatrist assesses psychiatric diagnoses.

[11] Identified based on the ICD-10 classification group H. In Denmark, a pedagogical team employed by the municipality assesses special education needs upon request from the parent or school headmaster.

Based on parental characteristics, students in consolidated and non-consolidated schools clearly differ. This is especially the case for students in closing schools. Table A1 reveals that students affected by school consolidation have fewer favorable characteristics compared to others and that some of the differences are non-negligible and statistically significant. Therefore, it is important that our approach allows for this.

*C. School Consolidation and School Size*

The official registry with information on all public and private schools in Denmark is maintained by the Ministry of Education and includes a unique identifier for each school and information on municipality, type of school, and the opening and closing date of the school. The ministry's registration system allows multiple ways for a municipality to report school consolidation. In some cases, we have to supplement the official registry with information on consolidation from other sources. We collect and match information from the ministry's registry, Statistics Denmark, the organization of public schools, municipalities, individual school websites, and notes from the school boards of the affected schools. Based on this comprehensive data collection, we identified three types of school consolidation, as defined in Section III. The school data is then merged with registry information from Statistics Denmark on national test scores and student enrollment.

In Denmark, the majority of public schools enroll students in grade 0 through 9, i.e. primary and lower secondary at the same school. A number of public schools (344 out of 1,479) are only primary schools (grades 0-6), called feeder schools, after which the student continue compulsory schooling at another public school within the catchment area. We define the school size as the number of students attending grades 1 through 6. We chose this parameter to be able to include the feeder schools in the analysis. Furthermore, our analysis explicitly focuses on students attending primary school (specifically grades 2-4, prior to the local school reforms) and assumes that the size of the closest environment (grades 1-6) is more relevant for school outcomes than the size of the entire school. Teamwork and classroom teaching across different grades is especially common in the youngest grades (i.e., pooling grades 1-3 and grades 4-6), whereas the oldest students are generally more commonly separated to engage in other activities and may even be placed in a separate unit at the school.[12]

Table 3 summarizes the number of schools, school size, and change in school size during the two-year period from the beginning of the 2009-2010 school year to the beginning of the 2011-2012 school year. During this period, there were two summer holidays in which consolidations took place. As a result, the number of public schools decreased by about ten percent. For schools not affected by consolidation, the average change in school size was positive, but the order of magnitude was only about five students. For schools affected by consolidation, the average change in school size (from a student perspective) ranged from 69 students for school expansions to 230 students for school closings.

[12] Our results are robust to the exclusion of feeder schools from the analysis, see Table 7.

TABLE 3. SCHOOL CONSOLIDATION AND SCHOOL SIZE

| School year | 2009-2010 | 2010-2011 | 2011-2012 | Total no. of schools for 2009 to 2012 | Average (individual) change in school size |
|---|---|---|---|---|---|
| **All public schools** | | | | | |
| No. of schools at the beginning of the school year (average school size) | 1,479 (233) | 1,458 (234) | 1,351 (252) | 1,479 | (19) |
| **Schools affected by consolidation the following summer** | 42 | 270 | | 312 | (133) |
| - School closings | 24 | 110 | | 134 | (230) |
| - School expansions | 11 | 71 | | 82 | (69) |
| - School mergers | 7 | 89 | | 96 | (128) |
| No. of schools remaining | 18 | 160 | | 178 | |
| **Schools unaffected by consolidation the following summer** | 1,437 | 1,188 | | 1,167 | (5) |
| Newly opened schools | 3 | 3 | | 6 | |
| No. of schools remaining | 1,440 | 1,191 | | 1,173 | |
| **All public schools** | | | | | |
| No. of schools remaining | 1,458 | 1,351 | | 1,351 | |

Note: School size is defined as the number of students in grades 1-6.

Next, Table 4 summarizes average characteristics for schools unaffected by consolidation (column 1) and schools affected by consolidation by type of consolidation (columns 2 to 4). Asterisks indicate whether the average characteristics of the schools are significantly different from those of the schools unaffected by consolidation. From the school data, we are able to obtain information to classify schools based on size (number of students and feeder schools), location in a rural or urban municipality, and town size. Additionally, average student characteristics are measured one or two years prior to consolidation to get a picture of the enrollment patterns, school performance, and student background information available to the local authorities who would determine potential school restructuring. Overall, schools exposed to school closings appear to have been negatively selected based on performance, which is measured as test scores of previous cohorts, school grade point average (GPA), and parental background.[13] Schools that underwent expansion also performed relatively poorly prior to consolidation in terms of test scores, while schools that were merged appear to be significantly smaller than non-consolidated schools. In general, consolidated schools were more likely to be located in rural areas, while especially schools that closed were more likely to be small schools and located in small towns.

Our analysis of the effect of school consolidation considers the heterogeneous effects based on the student characteristics and the characteristics of the school described above that the child was enrolled in prior to potential exposure to consolidation. The influence of consolidation on student learning may well differ due to the school context and environment prior to potential exposure to consolidation.

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[13] Auxilliary regressions show that more than 40 percent of the variation in schools' average GPA is explained by students' background (susch as parental education and income), while the school's characteristics (such as number of students, town size, and rural location) explain less than 10 percent of the variation. This may points toward why consolidated schools underperform.

TABLE 4. AVERAGE SCHOOL CHARACTERISTICS OF SCHOOLS AFFECTED BY CONSOLIDATION IN 2010 OR 2011 AND BY TYPE OF CONSOLIDATION

| | | | Type of consolidation | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) Non-consolidated schools | | (2) Closings | | | (3) Expansions | | | (4) Mergers | | |
| | Mean | SD | Mean | SD | | Mean | SD | | Mean | SD | |
| *School characteristics* | | | | | | | | | | | |
| Small school (<150 students) | 0.265 | | 0.762 | | *** | 0.159 | | ** | 0.351 | | * |
| Feeder school (grades 0-6) | 0.206 | | 0.600 | | *** | 0.110 | | ** | 0.202 | | |
| School national test in lowest quartile | 0.237 | | 0.315 | | ** | 0.293 | | | 0.266 | | |
| School GPA (avg. 2002-2010) | -0.024 | (0.007) | -0.109 | (0.030) | ** | -0.058 | (0.018) | | -0.051 | (0.032) | |
| School GPA in lowest quartile | 0.107 | | 0.182 | | | 0.071 | | | 0.055 | | |
| Non-Western students, above 20% | 0.116 | | 0.108 | | | 0.098 | | | 0.106 | | |
| Special education students, above 15% | 0.138 | | 0.123 | | | 0.146 | | | 0.064 | | ** |
| *One year before consolidation* | | | | | | | | | | | |
| No. of students, grades 1-6 | 257.0 | (129.6) | 108.0 | (78.8) | *** | 248.5 | (100.5) | | 204.6 | (91.6) | *** |
| Transit to private school, grades 1-6 | 0.018 | | 0.147 | | *** | 0.032 | | | 0.021 | | |
| Test score 2nd grade | -0.012 | (0.418) | -0.122 | (0.539) | ** | -0.051 | (0.497) | | -0.077 | (0.392) | |
| Test score 4th grade | -0.017 | (0.381) | -0.077 | (0.449) | | -0.086 | (0.399) | | -0.082 | (0.379) | |
| Test score 6th grade | -0.003 | (0.396) | -0.052 | (0.471) | | -0.066 | (0.348) | | -0.017 | (0.401) | |
| *Two years before consolidation* | | | | | | | | | | | |
| No. of students, grades 1-6 | 246.7 | (122.4) | 115.2 | (80.3) | *** | 237.3 | (97.0) | | 203.7 | (90.1) | *** |
| Transit to private school, grades 1-6 | 0.012 | | 0.026 | | | 0.009 | | | 0.015 | | |
| Test score 2nd grade | -0.022 | (0.417) | -0.020 | (0.528) | | -0.086 | (0.388) | | -0.023 | (0.412) | |
| Test score 4th grade | -0.015 | (0.354) | -0.029 | (0.453) | | -0.171 | (0.394) | *** | -0.057 | (0.419) | |
| Test score 6th grade | -0.011 | (0.369) | 0.024 | (0.395) | | -0.126 | (0.378) | ** | -0.036 | (0.379) | |
| *Average student background* | | | | | | | | | | | |
| Non-Western immigrant or descendent | 0.077 | | 0.061 | | | 0.085 | | | 0.066 | | |
| Living with both parents | 0.734 | | 0.761 | | | 0.728 | | | 0.731 | | |
| ADHD or similar diagnoses | 0.030 | | 0.028 | | | 0.034 | | | 0.032 | | |
| Single mother | 0.195 | | 0.161 | | | 0.199 | | | 0.194 | | |
| At least one parent with an academic degree | 0.377 | | 0.292 | | * | 0.325 | | | 0.363 | | |
| Both parents, non-academic education | 0.552 | | 0.648 | | ** | 0.595 | | | 0.570 | | |
| Both parents, compulsory education only | 0.110 | | 0.122 | | | 0.131 | | | 0.106 | | |
| At least one parent employed | 0.911 | | 0.924 | | | 0.900 | | | 0.922 | | |
| Both parents unemployed | 0.089 | | 0.076 | | | 0.100 | | | 0.078 | | |
| Both parents outside labor market | 0.048 | | 0.040 | | | 0.056 | | | 0.040 | | |
| Mother's income (DKK10,000) | 22.89 | (5.060) | 20.83 | (3.984) | *** | 21.06 | (4.260) | ** | 23.08 | (4.080) | |
| Father's income (DKK10,000) | 31.27 | (8.509) | 27.52 | (6.423) | *** | 28.88 | (6.667) | ** | 30.09 | (7.103) | |
| *Municipality* | | | | | | | | | | | |
| Rural | 0.445 | | 0.723 | | *** | 0.646 | | *** | 0.596 | | *** |
| Urban | 0.426 | | 0.215 | | *** | 0.293 | | ** | 0.287 | | *** |
| Capital | 0.129 | | 0.062 | | ** | 0.061 | | * | 0.117 | | |
| *Population of town school placed in* | | | | | | | | | | | |
| Less than 300 | 0.038 | | 0.085 | | ** | 0.012 | | | 0.053 | | |
| 300 – 999 | 0.195 | | 0.377 | | *** | 0.122 | | | 0.213 | | |
| 1,000 – 2,999 | 0.186 | | 0.069 | | *** | 0.305 | | *** | 0.213 | | |
| 3,000 – 4,999 | 0.069 | | 0.031 | | * | 0.146 | | *** | 0.021 | | * |
| 5,000 – 9,999 | 0.075 | | 0.069 | | | 0.110 | | | 0.074 | | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 10,000 – 49,999 | 0.203 | 0.108 | *** | 0.195 | | 0.330 | ** |
| More than 50,000 | 0.234 | 0.262 | | 0.110 | *** | 0.096 | *** |
| Missing | 0.068 | 0.223 | *** | 0.024 | | 0.074 | |
| No. of schools | 1,173 | 134 | | 82 | | 96 | |

Note: *, **, *** indicate that the mean is statistically different from the mean of the non-consolidated schools at the 10, 5, and 1 percent levels, respectively. Average student and parental background are calculated based on all students enrolled in grades 1 to 6 at the respective school two years before consolidation for consolidated schools and in 2009 for non-consolidated schools. GPA: grade point average; SD: standard deviation.

## V. Empirical Analysis

### *A. Empirical Model and Assumptions*

We are interested in estimating the effect of school consolidation on student achievement. Consider the following simple model:

$$y_{it} = \beta_0 + \beta_1 D_t + \beta_2 C_i \times D_t + \alpha_i + \varepsilon_{it} \qquad t = 0,1,$$

where $y_{it}$ denotes the test score for student *i* in period *t*, $D_t$ is an indicator variable that equals one if $t = 1$ and zero otherwise, and $C_i$ is an indicator variable that equals one if student $i$ is exposed to a school consolidation. No students are exposed to a school consolidation in the first period, i.e. $C_i \times D_t = 0$ for all $i$ at time $t = 0$. $\alpha_i$ is a student-specific effect and $\varepsilon_{it}$ is the idiosyncratic error. The parameter $\beta_2$ captures the effect of consolidation on student achievement. No individual-level time-constant control variables are included since the inclusion of student fixed effects does not also allow for inclusion of time-invariant individual characteristics.

The resulting estimate of $\beta_2$ is a DID type estimate. Formally, we require the exposure to school consolidation to be strictly exogenous conditional on $\alpha_i$ for consistent estimation of $\beta_2$ (common trends assumption). In order to account for the fact that students are clustered within schools, which implies potential correlation of the $\varepsilon_{it}$'s, we report standard errors clustered at the level of the school that the student attended prior to potential consolidation.

The main problem in identifying the effect of school consolidation or that of any policy change that affects school inputs is the potential endogeneity of school resources and selection into, and out of, schools. For instance, if decisions about school closings are based on the previous performance of the school or an unfavorable peer composition at the school, comparing students who experience a school closing with students who do not, is likely to lead to downward biased estimates of the effect of experiencing a school closing on student performance. Another problem is that parents with certain types of characteristics are likely to sort their child into, or out of, consolidating schools if they believe consolidation to be important. For example, parents who already invest considerably in their child's education may also be more likely to move the child to another school in the event of consolidation if they believe that this can increase the quality of the school the child attends.

To circumvent these problems, we employ the above DID strategy based on individual fixed effects. Thus, we compare the outcome of the individual student prior to potential exposure to consolidation with the outcome of the same student after the potential exposure. In this way, we implicitly control for all time-invariant individual characteristics, such as parental background characteristics. Specifically, we assume that whether or not an individual is exposed to school consolidation is independent of the time-varying error terms conditional on the student fixed effect. However, this implies that we are assuming that consolidation cannot be based on, e.g. the development in school performance over time. Students at consolidated and non-consolidated schools must have a similar performance profile up until the point of consolidation. One concern that remains is that of the external validity of the resulting estimates. If the effects of consolidation are heterogeneous and specific types of individuals are more likely to be exposed to consolidation, caution should be exercised when extrapolating to the remaining part of the population.

*B. Main Results*

The DID estimates of school consolidation and ordinary least squares (OLS) estimates for comparison are presented in Table 5.

TABLE 5. DID ESTIMATES OF THE EFFECT OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT

| | | | Model I | Model II: Type of consolidation | | | P-value |
|---|---|---|---|---|---|---|---|
| Estimation Method | Sample | No. of Students | Consolidation | Closings | Expansions | Mergers | closings = expansions = mergers |
| OLS | All | 90,495 | -0.046*** | -0.074** | -0.047* | -0.022 | *0.434* |
| | | | (0.017) | (0.031) | (0.027) | (0.027) | |
| DID | All | 90,495 | -0.025 | -0.059** | -0.007 | -0.017 | *0.322* |
| | | | (0.015) | (0.029) | (0.021) | (0.026) | |
| DID | 2nd graders | 45,155 | -0.011 | -0.037 | -0.002 | 0.001 | *0.708* |
| | | | (0.022) | (0.038) | (0.032) | (0.037) | |
| DID | 4th graders | 45,340 | -0.039** | -0.082** | -0.012 | -0.034 | *0.273* |
| | | | (0.018) | (0.039) | (0.023) | (0.027) | |
| | | | *0.270* | *0.352* | *0.796* | *0.380* | |

Note: Standard errors in parentheses are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured by student test scores in reading in spring 2010 and 2012. The p-value in the last column refers to an F-test of equal coefficients across type of consolidation. The p-value in *italic* refers to a Chi2-test of equal coefficients across 2nd and 4th graders. DID: difference-in-differences; OLS: ordinary least squares.

OLS estimates are based on a simple regression of student achievement (post-consolidation) on an indicator for being exposed to consolidation and a range of background variables. We report estimates from two different specifications for school consolidation. In the first specification, the three different types of consolidation are pooled (model I), whereas in the second, we estimate the effects of the three types of consolidation separately (model II). The type of school consolidation (i.e. closings, expansions, mergers, or no consolidation) that a student experience is predicted based on the school that the student attended the year before the consolidation took place.

The DID estimates show that the overall effect of school consolidation is negative, albeit insignificant. The estimated effect is -0.025, which means that being exposed to school consolidation decreases student test scores by about 2.5% of a SD, which is arguably not a large effect. Interestingly, we also see that the different types of school consolidation have different effects on student outcomes. At least the negative overall effect of consolidation appears to be driven by school closings, which have a statistically significant effect of -0.059 on student test scores. For school expansions, the estimated effect is close to zero and statistically insignificant. For school mergers, the estimated effect is negative but small and statistically insignificant. This was to be expected since the category of school mergers constitutes administrative mergers. Students are not physically relocated and are, as such, not exposed to increases in school size or given new class- or schoolmates. In comparison, a student who is exposed to a school closing would be forced to relocate to another neighborhood school, which is likely to increase school size, change peer group composition, and increase travel distance. One could argue that administrative mergers may generate many of the benefits of large schools while still maintaining some of the benefits of small schools. We observe, however, that school mergers only have a small, negative and insignificant effect. The outcomes that we consider are, of course, measured in the very short run, and it is entirely possible that some of the effects of, for example, an administrative merger, would not show up in the test scores for at least the first couple of years. An F-test of equality of the effects of the three types of consolidation cannot reject that the effects are equal at conventional significance levels. However, it may mask heterogenous effects across students and schools, which we will return to. Finally, we split the sample by grade and find stronger results for older students in grade 4. The coefficient estimates are not statistically different, but the different point estimates suggest that learning among fourth graders (around 10 years) is more sensitive to disruption than among second graders (around 8 years). As a point of caution, our conclusions may not carry over to other grade levels; in particular not to high schools, which has been the main focus in this literature.

To gain a deeper understanding of the effects of consolidation, we investigate the heterogeneous effects of being exposed to school consolidation. Tables 6 and 7 present DID estimates for different subgroups of students and schools, respectively. In Table 6, there appear to be no differences in how boys and girls react to school consolidation. Interestingly, for students with ADHD or similar diagnoses, the sign estimate for closures is unchanged (though insignificant), while mergers seem to exert a positive effect on test scores. Taken at face value, this suggests that merged schools cope better with the challenges of mentally disabled students. The group of non-Western immigrant students is fairly small and, maybe as a result of this fact, none of the estimates for this group are statistically significant. The point estimates for students with non-academic parents suggest that students with a disadvantaged family background are more affected by school consolidation than other students (although the differences are not significantly different).[14] The effect of school closings is larger than for the whole sample and still statistically significant. With a few exceptions, we cannot reject that coefficients are the same across subgroups and across types of consolidation.

[14] The pattern is similar for other disadvantaged groups including students growing up with a single mother and students with special educational needs (not reported).

In Table 7, we look at schools which were initially particularly small (below 150 students). In this case, the point estimate of the detrimental effect of consolidation is doubled to reach a negative impact of 5.6% of a SD and 11% of a SD if exposed to a school closing. The estimated effect of expansions is close to zero and the effect of mergers is positive, although insignificant.The same pattern across each type of school consolidation is observed looking at students initially enrolled in a feeder school and schools in rural areas.[15] Note however, that the point estimates on mergers of rural and low-performing schools tend to be negative. This is possible driven by the weaker socio-economic background of students in rural areas.[16] Considering other characteristics than school size, we cannot reject equality of coefficients across subgroups and types of consolidation.

[15] The pattern of results is similar when we classify schools according to the proportion of immigrants and the proportion of students with special education needs (not reported).

[16] We have classified schools as low-performing if the school is placed in the lowest quartile of the test score distribution of i) the school's average national test score in reading in 2010 or ii) the school's grade point average (average of cohorts completing ninth grade from 2002 to 2010).

TABLE 6. DID ESTIMATES OF HETEROGENEOUS EFFECTS OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT BY STUDENT CHARACTERISTICS

| Sample | No. of students | Model I: Consolidation | Model II: Type of consolidation: Closings | Expansions | Mergers | P-value closings = expansions = mergers |
|---|---|---|---|---|---|---|
| Boys | 45,332 | -0.025 | -0.067* | -0.011 | -0.007 | *0.335* |
| | | (0.018) | (0.035) | (0.024) | (0.032) | |
| Girls | 45,163 | -0.025 | -0.052* | -0.003 | -0.027 | *0.424* |
| | | (0.016) | (0.030) | (0.024) | (0.024) | |
| *P-value* | | *0.975* | *0.594* | *0.688* | *0.395* | |
| ADHD or similar diagnoses | 2,430 | -0.008 | -0.113 | -0.068 | 0.121* | *0.060* |
| | | (0.050) | (0.097) | (0.077) | (0.069) | |
| No ADHD or similar diagnoses | 88,065 | -0.026 | -0.058* | -0.005 | -0.021 | *0.332* |
| | | (0.016) | (0.030) | (0.022) | (0.026) | |
| *P-value* | | *0.721* | *0.571* | *0.418* | *0.034* | |
| Non-Western immigrants or descendants | 7,579 | -0.020 | -0.022 | -0.002 | -0.042 | *0.852* |
| | | (0.033) | (0.054) | (0.046) | (0.058) | |
| Western origin | 82,916 | -0.025 | -0.064** | -0.007 | -0.015 | *0.306* |
| | | (0.016) | (0.032) | (0.022) | (0.026) | |
| *P-value* | | *0.872* | *0.489* | *0.925* | *0.637* | |
| Non-academic parents | 48,610 | -0.020 | -0.070** | 0.005 | -0.003 | *0.127* |
| | | (0.017) | (0.031) | (0.025) | (0.026) | |
| At least one academic parent | 36,445 | -0.022 | -0.035 | -0.012 | -0.024 | *0.843* |
| | | (0.019) | (0.036) | (0.024) | (0.036) | |
| *P-value* | | *0.914* | *0.212* | *0.486* | *0.476* | |
| Parental income below median | 27,851 | -0.030 | -0.057* | -0.006 | -0.031 | *0.491* |
| | | (0.019) | (0.033) | (0.029) | (0.03) | |
| Parental income abow median | 62,644 | -0.022 | -0.060* | -0.007 | -0.011 | *0.369* |
| | | (0.017) | (0.033) | (0.022) | (0.028) | |
| *P-value* | | *0.666* | *0.936* | *0.982* | *0.489* | |

Note: Standard errors are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured by student test scores in reading in spring 2010 and 2012. All student characteristics are measured in 2009, i.e. before potential exposure to school consolidation. Parental income is categorized based on the parent with the highest income. Below two subgroups, the p-value in *italic* refers to a Chi2-test of equal coefficients across the two subgroups. The test is based on simultaneous model estimation (using the stata suest command) and the teststatistic is calculated as: (b_subgroup - b_refgroup) / [(se_subgroup^2 + se_refgroup^2)^(1/2)]. The p-value in the last column refers to an F-test of equal coefficients across type of consolidation. The coefficients on the post-indicator and constant are omitted from the table, due to space limitations. The coefficients on the post-indicator and constant are omitted from the table, due to space limitations. DID: difference-in-differences.

TABLE 7. DID ESTIMATES OF HETEROGENEOUS EFFECTS OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT BY PRE-CONSOLIDATION SCHOOL CHARACTERISTICS

| | | Model I | Model II: Type of consolidation | | | P-value |
|---|---|---|---|---|---|---|
| Sample | No. of students | Consolidation | Closings | Expansions | Mergers | closings = expansions = mergers |
| **A. Initial school size** | | | | | | |
| Small (< 150 students) | 12,069 | -0.056 | -0.111** | 0.003 | 0.033 | *0.114* |
| | | (0.036) | (0.046) | (0.077) | (0.063) | |
| All other (>150 students) | 78,426 | -0.017 | -0.014 | -0.007 | -0.030 | *0.796* |
| | | (0.017) | (0.038) | (0.022) | (0.028) | |
| *P-value* | | *0.330* | *0.100* | *0.902* | *0.354* | |
| Feeder school (grades 0-6) | 10,844 | -0.045 | -0.086* | -0.016 | 0.043 | *0.401* |
| | | (0.040) | (0.047) | (0.063) | (0.102) | |
| All other (grades 0-9) | 79,651 | -0.022 | -0.047 | -0.006 | -0.025 | *0.612* |
| | | (0.017) | (0.038) | (0.023) | (0.026) | |
| *P-value* | | *0.584* | *0.524* | *0.884* | *0.522* | |
| **B. Initial school area** | | | | | | |
| Rural municipal | 35,561 | -0.039* | -0.070* | -0.004 | -0.052* | *0.306* |
| | | (0.021) | (0.041) | (0.030) | (0.029) | |
| Urban municipal | 54,934 | -0.006 | -0.043 | -0.009 | 0.023 | *0.537* |
| | | (0.024) | (0.042) | (0.031) | (0.043) | |
| *P-value* | | *0.284* | *0.644* | *0.914* | *0.147* | |
| **C. Initial school performance level** | | | | | | |
| National test in lowest quartile | 19,132 | -0.018 | -0.021 | 0.010 | -0.047 | *0.481* |
| | | (0.027) | (0.048) | (0.032) | (0.042) | |
| National test above lowest quartile | 71,363 | -0.041** | -0.106*** | -0.024 | -0.013 | *0.114* |
| | | (0.018) | (0.038) | (0.024) | (0.031) | |
| *P-value* | | *0.478* | *0.164* | *0.392* | *0.521* | |
| School GPA in lowest quartile | 6,584 | -0.025 | 0.104 | -0.004 | -0.147 | *0.440* |
| | | (0.076) | (0.131) | (0.092) | (0.151) | |
| School GPA above lowest quartile | 69,710 | -0.015 | -0.079* | -0.014 | -0.002 | *0.376* |
| | | (0.021) | (0.046) | (0.025) | (0.040) | |
| *P-value* | | *0.947* | *0.236* | *0.845* | *0.462* | |

Note: Standard errors in parentheses are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured by student test scores in reading in spring 2010 and 2012. All school characteristics are measured in 2009, i.e. before potential exposure to school consolidation. Below two subgroups, the p-value in *italic* refers to a Chi2-test of equal coefficients across the two subgroups. The p-value in the last column refers to an F-test of equal coefficients across type of consolidation. The coefficients on the post-indicator and constant are omitted from the table, due to space limitations. DID: difference-in-differences; GPA: grade point average.

*C. Common Trends*

As discussed above, the validity of the DID estimator is based on the assumption that the underlying trends in the outcome variable, here fourth and sixth grade test scores, are the same for both consolidated and non-consolidated schools. The common trend assumption is not directly testable in this application due to lack of data on test scores prior to 2010. Ideally, we would compare the trends in value-added performance for students in consolidated and non-consolidated schools. Instead, we investigate possible time trends in school GPA as measured by the results from the ninth grade exit exams; see Figure A1 in the Appendix. The school GPA most likely reflects the primary performance measure available to local authorities in the period when they were making decisions about consolidation.[17] Figure A1 shows that, although schools exposed to consolidation performed at a lower level, the trends in performance levels do not differ. Additionally, we do not observe a change in the trend in the outflow of students from schools until the year before the consolidation. During the school years 2007/8 and 2008/9 the transition out of schools was 11% at public schools which are later observed to be consolidated and 8% at other public schools.

The common trend assumption has some implications for the assumed behavior of the involved agents. First of all, the assumption implies that the policy makers do not take into account the potential heterogeneous effects of consolidation on test scores when they decide on a new school structure. A second implication is that children and their parents do not respond to the new school structure based on the potential heterogeneous effects of consolidation. Table A2 presents the amount of the students that transfer to the school they

[17] We investigate the school GPA of all ninth grade exit exams but also separately examine the ninth grade written exam that most closely reflects the material covered in the fourth and sixth grade national reading test. About 20% of the schools are feeder schools and thus excluded from this analysis.

are predicted to move to.[18] Almost 90% of the students exposed to school consolidation move to the school they are supposed to; compared to 95% of the students who are not exposed to school consolidation. Thus, we expect that approximately one in twenty students potentially violate the identifying assumption.[19]

*D. Robustness Checks*

We replicate the analysis with math test scores as the outcome measure. The above results on the effect of school consolidation on achievement gains are generally robust to using achievement in math as the outcome measure (see Tables A3 and A4 in the Appendix). The test results in math are from an alternative sample of third grade students followed during the same period of school consolidations.[20] For math, we again find adverse effects from school closings. The point estimates tend to be larger in absolute size. And, for math scores, we also find evidence of adverse effects on students exposed to other kinds of school consolidation.[21] For expanding schools, the results are still largely insignificant, with a few exceptions, which on the other hand suggest some positive effects of expansions for low-performing schools.

Overall, the conclusion is the same: on average students exposed to school consolidation achieve significantly less measured by the change in national test results before and after the consolidation compared to students not exposed to school consolidation in the same period.

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[18] As part of the school consolidations, some municipalities also restructured their special needs education, typically centralizing it by moving special needs students out of the mainstream schools and into one facility. The availability of registry data on the individual level allows us to identify these students, of whom there are very few. They are excluded from the predicted school size measures.

[19] Unfortunately, we did not observe test scores for students who transferred to private schools, which prevents an interpretation of the effects as intention-to-treat effects.

[20] More specifically, all students enrolled in third grade and tested in math in the school year prior to exposure to school consolidation (spring 2010) and re-tested three years later in sixth grade (spring 2013).

[21] Both of these results are consistent with Brummet (2014) who also finds larger impacts on math scores.

The description of the sample selection and the consolidating schools in Section IV gives rise to a set of specification and robustness checks, all presented in Table A5 in the Appendix. Panel A in Table A5 excludes students not exposed to school consolidation in order to compare only students who face uncertainty regarding future school restructuring at the time of the pretest (spring 2010). The coefficients on the closings and expansions are all insignificant (mergers is the reference group), supporting the above conclusions of only small negative effects from school consolidation. Panel B in Table A5 separately considers school consolidations in the summers of 2010 and 2011. These analyses mirror the above conclusions. The negative point estimate on mergers seems to be driven by those implemented during the summer 2010, which only counted seven schools.

Table 4 showed that, particularly the closing schools, seem to differ in terms of size and student composition. As a robustness check, we therefore include interactions between the post-indicator and initial school characteristics which based on Table 4 were correlated with the exposure to later consolidation. Such interactions control for the trend in test scores among students, e.g. at rural schools, regardless of whether they are exposed to a school consolidation or not. The estimates are robust to including interactions one-by-one and jointly. For space considerations, only the latter is shown, see Panel C in Table A5.

Panels D and E in Table A5 address concerns of sample selection bias. As described in Section IV, if students leave for private schools during the consolidation process, the students' post-test scores are lost and hence the students are dropped from the sample. The mean of pre-test scores of movers is -0.33 (1.22 SD), which suggests that movers tend to be low performing, and that our main results underestimate the effect of consolidation on student achievement. Panel D addresses this by imputing missing post-test scores and estimate lower and upper bounds on the parameters of interest, while panel E implements inverse probability weighting on the non-missing sample.

Panel D in Table A5 shows that if we assume that students who move to private schools are unaffected by the consolidation (i.e. impute their post-test score by their pre-test score) the estimated effects are of the same magnitude and the negative effect of school consolidation (2.4 % of a SD) is now borderline significant. Assuming the worst and best case about the missing post-test scores of students who move to private schools after the announcement of the public school consolidation, we obtain rough lower and upper bounds on the estimates. In the worst case, we assume that students are affected by twice the estimated effect of consolidation and in the best case we assume they are positively affected. This bounding exercise supports that the estimates of the parameters of interest are in a narrow interval around the estimated effects.[22]

Panel E in Table A5 shows that the estimated effects of consolidation and school closings are slightly larger after weighting each non-missing post test score observation with the inverse probability of having a post test score.[23]

To further understand who leaves for private school during the consolidation process, we separately estimate the main model including an indicator for missing post-test score and interaction terms with the types of consolidation.[24] The results show consistent negative interaction effects with school closings, irrespectively of whether we assume they are in the

[22] We have also imputed missing post test scores with students' pre test scores +/- 0.25 SD and estimates range from being negative and statistically significant (if movers are systematically low performing) to small and statistically insignificant (if movers are systematically high performing). The results are available on request.

[23] Probability weights are estimated using a logit model condition on school characteristics from Table 4, student characteristics from Table A1, and three consolidation type indicators. Standard errors are clustered at the level of the school that the student attended prior to potential exposure to consolidation.

[24] The results are available upon request.

high or the low end of the ability distribution, which again points towards the interpretation as a disruption effect. This exercise also supports that the effect of school closings may be slightly underestimated (i.e. the negative disruption cost may be even larger, when we take into account those students who leave for private school).

## VI. Discussion of Mechanisms: Evidence of Disruption?

To investigate whether part of the estimated effects can be interpreted as effects of disruption, we first look more carefully at the type of consolidation the students are exposed to. We see that the negative effect is driven by school closings. We expect that a student exposed to a school closure may experience a higher degree of disruption of the learning environment compared to a student who experiences a school expansion or merger. For school expansions, students continue at the same school but they are exposed to new classmates. For mergers, the disruption from physical relocation of students is even smaller, with only the administration changing. However, the consolidation process may distort the psychological learning environment, for example through uncertainty about the process and future consolidations. Comparing students exposed to closings, expansions, and mergers, we find that our results described earlier suggest disruption effects as opposed to effects from changing school environment after consolidation.

Furthermore, in this section we argue that if the negative short-term effect disappears in the years after consolidation, it supports the indicative evidence of a disruption effect. However, if the negative short-term effect of closings and expansions persists, or even increases, it may be suggestive of mechanisms other than just the disruption.

In Denmark, students are tested in reading every second year. Hence, it is possible to follow their achievement to determine if the seemingly disruptive effect on achievement persists or vanishes over time. More specifically, with test results from spring 2014, we compare the four-year achievement gains in reading of students exposed and unexposed to school consolidations.[25] Table 8 presents the DID estimates and shows that the detrimental effect of school consolidation seems to diminish over time. However, we cannot reject equality of coefficients across years making it hard to draw any definitive conclusions.

TABLE 8. DID ESTIMATES OF THE EFFECT OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT; TWO AND FOUR-YEAR ACHIEVEMENT GAINS

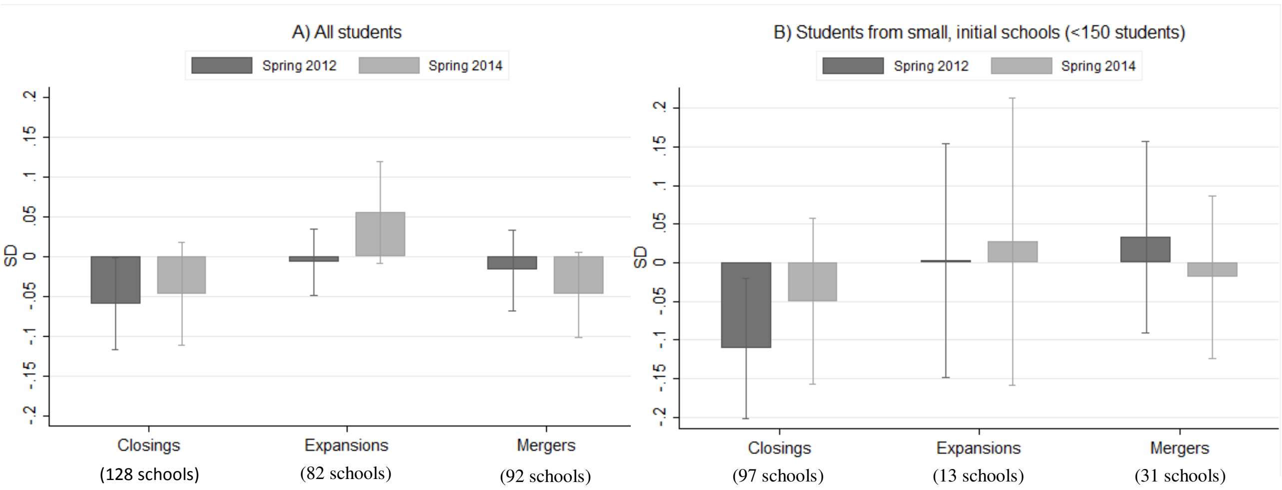
| | Spring 2010 (pre-test) | | | Spring 2012 | Spring 2014 |
|---|---|---|---|---|---|
| Sample | No. of students | Percent exposed to consolidation in 2010 or 2011 | Subject | Coef./s.e. | Coef./s.e. |
| All | 90,495 | 15.5% | Reading | -0.025 | -0.009 |
| | | | | (0.015) | (0.019) |
| | | | | | *[0.259]* |
| 2nd graders | 45,155 | 15.4% | Reading | -0.011 | 0.007 |
| | | | | (0.022) | (0.026) |
| | | | | | *[0.315]* |
| 4th graders | 45,340 | 15.6% | Reading | -0.039** | -0.026 |
| | | | | (0.018) | (0.021) |
| | | | | | *[0.476]* |

Note: Standard errors are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicates significance at the 10, 5, and 1 percent level, respectively. Achievement is measured by student test score gain in reading from spring 2010 to 2012 and 2010 to 2014, respectively, using the same sample of students. The p-value in bracket refers to a Chi2-test of equal coefficients across years. Coef./s.e.: coefficient/standard error; DID: difference-in-differences.

[25] Our second graders exposed to school consolidation will now be sixth graders and the fourth graders will be eighth graders. The 2014 reading test response rates are 94.3% and 88.7% for the second and fourth grade sample, respectively.

Figure 1 presents the DID estimates on the two and four-year achievement gains in reading by type of consolidation. Panel A covers all students and panel B the subgroup of students initially enrolled in a small school, which was the group of students who experienced the most detrimental effect of school consolidation (see Table 7).[26] In 2009, 442 schools were classified as small schools, of which 97 closed, 13 expanded, and 31 merged during 2010 and 2011.

FIGURE 1. DID ESTIMATES OF THE EFFECT OF SCHOOL CONSOLIDATION; TWO AND FOUR-YEAR ACHIEVEMENT GAINS BY TYPE OF CONSOLIDATION




Looking at the difference in the four-year achievement gain in reading from 2010 to 2014, the students exposed to school closings are now less negatively affected. Furthermore, there are signs of a positive achievement gain among students exposed to school expansions.[27] The four-year achievement gain after a school expansion is 5.5% of a SD and significantly larger than the effect measured after two years (p-value 0.019); see Table A6 in the Appendix. This

[26] All DID estimates on achievement in reading from spring 2010 to 2014 are presented in Tables A6 and A7 in the Appendix and include subgroup estimates.

[27] $Chi^2$-tests of equal coefficients support that the four-year achievement gain from expansions is significant. This finding is consistent across most subgroups; see Tables A6 and A7 in the Appendix.

gain may indicate that the intended beneficial effects from school consolidation are beginning to appear. It also makes sense that students exposed to expansions, who are not physically displaced, harvest the positive economics of scale effects from consolidation first. At the same time, however, students exposed to school mergers now experience a negative achievement gain, albeit the coefficients are statistically insignificant for the main sample; see Table A6 in the Appendix. This may support small short-term disruption costs from consolidation, even when only administrative. Table A7 in the Appendix shows that the negative achievement gain is most pronounced when merging low-performing schools which may also point toward disruption costs from uncertainty about future consolidations.

In an effort to trace out the gain in achievement measured yearly after consolidation, we now extend the panel with additional cohorts. In the following, we consider only consolidations during the summer 2011 in order to follow the achievement of multiple cohorts exposed to the same set of consolidations.[28] Figure 2 plots the estimated effect of consolidation measured in 2012, 2013, 2014, and 2015, i.e. up to four years after the 2011-consolidations.

FIGURE 2. DID ESTIMATES BY TIME AFTER CONSOLIDATION; CONSOLIDATION IN 2011


[28] Specifically, we add the cohorts of second and fourth grade students tested in reading in the spring 2011 (pre-test) and tested again in reading in 2013 and 2015.

Figure 2 reveals a weak positive trend in achievement when exposed to school closings; among all students (left panel) and among students from rural schools (right panel). Similar results are found among students from small, initial schools and feeder schools: see Figure A2 in the Appendix. Thus, it seems that the negative effect of experiencing a school closing weakens over time, suggesting that at least part of the effect is due to disruption.

## VII. Conclusion

We investigate the impact of school consolidation (closings, expansions, and mergers) on individual student achievement by employing a DID strategy on detailed student-level data for Denmark. We find negative effects of school consolidation on student achievement in the short run. The variation in the point estimates across consolidation types and time suggests that at least part of the effect is due to disruption.

The estimated effect of consolidation on student test scores in reading is about 2.5% of a SD. The effects differ by the type of consolidation, which indicate that more mechanisms are at play than just the school size effect. Specifically, the adverse effects of consolidation are greater for displaced students than receiving students. One potential mechanism is that displaced students are exposed to a larger disruption of their physical learning environment compared to students exposed to school expansions.

Taking into consideration the characteristics of the school that the student attended before potential exposure to consolidation, the loss in achievement ranges from 2-12% of a SD. Students from small schools (less than 150 students or feeder schools) are affected the most, which is also supportive of the effect being partly interpreted as a disruption effect since students from small schools face the largest changes in daily learning environment. When the observed period is extended to cover up to four years after the students have been exposed to consolidation, the negative effect of school closings weakens. Interestingly, even for closed small or rural schools the loss in achievement is significantly smaller after four years compared to two years. Furthermore, we observe some evidence of the intended beneficial effect of school expansions after four years.

Although the structural changes imposed by policy makers have a negative short-term disruption effect on student learning, counteracting the generally small negative effect of consolidation should be possible. By targeting school resources during the actual implementation process, the negative effects could be compensated while achieving the positive long-term effects of school consolidation. De Haan et al. (forthcoming) and Humlum and Smith (2015a), for example, find evidence of positive impacts on student achievement and long-term outcomes from reducing the public school supply and increasing school size.

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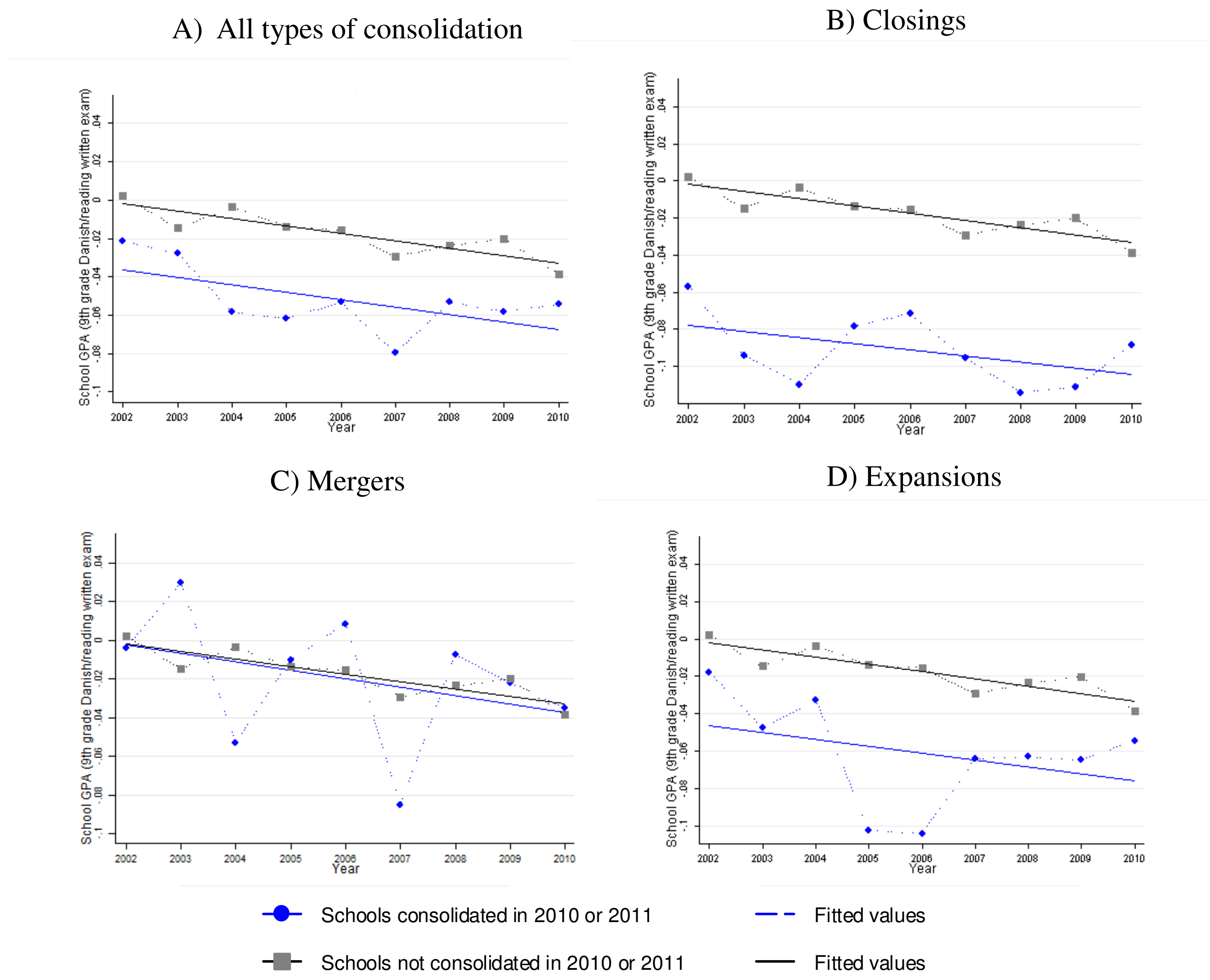
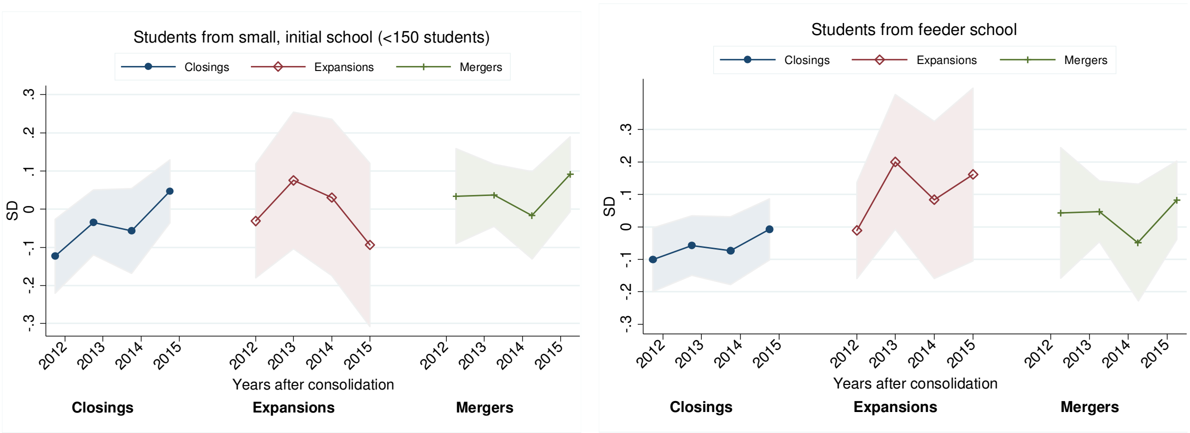
## APPENDIX - FIGURES

FIGURE A1. AVERAGE SCHOOL GPA AMONG SCHOOLS EXPOSED AND NOT EXPOSED TO CONSOLIDATION IN 2010 OR 2011; NINTH GRADE WRITTEN EXAM




Note: In total 344 out of 1,479 schools are feeder schools (grades 0-6), and are not included in the figure. GPA: grade point average

FIGURE A2. DID ESTIMATES OF THE EFFECT OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT BY YEARS AFTER CONSOLIDATION IN 2011; SMALL SCHOOLS AND FEEDER SCHOOLS



## APPENDIX – TABLES

TABLE A1. MEANS OF (SELECTED) BACKGROUND VARIABLES BY EXPOSURE TO SCHOOL CONSOLIDATION

| | (1) Non-consolidated schools | | Type of consolidation | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | (2) Closings | | | (3) Expansions | | | (4) Mergers | | |
| | Mean | SD | Mean | SD | | Mean | SD | | Mean | SD | |
| *Child characteristics* | | | | | | | | | | | |
| Boy | 0.501 | | 0.502 | | | 0.504 | | | 0.494 | | |
| Non-Western immigrant or descendant | 0.084 | | 0.107 | | *** | 0.085 | | | 0.067 | | *** |
| Birth weight | 3440 | (777) | 3416 | (797) | * | 3433 | (776) | | 3435 | (792) | |
| Special education needs | 0.056 | | 0.051 | | | 0.062 | | * | 0.043 | | *** |
| Diagnosed with ADHD or similar diagnosis | 0.026 | | 0.026 | | | 0.029 | | | 0.031 | | ** |
| *Mother characteristics* | | | | | | | | | | | |
| Age at childbirth | 29.7 | (5.637) | 29.1 | (5.709) | *** | 29.3 | (5.434) | *** | 29.5 | (5.571) | |
| Single mother | 0.197 | | 0.183 | | ** | 0.196 | | | 0.199 | | |
| Academic education | 0.335 | | 0.246 | | *** | 0.288 | | *** | 0.313 | | *** |
| Non-academic education | 0.642 | | 0.733 | | *** | 0.690 | | *** | 0.665 | | *** |
| Education missing | 0.023 | | 0.021 | | | 0.023 | | | 0.022 | | |
| Log earnings (DKK) | 10.512 | (4.549) | 10.150 | (4.752) | *** | 10.236 | (4.728) | *** | 10.729 | (4.316) | *** |
| Work experience (years) | 12.214 | (7.018) | 11.304 | (6.948) | *** | 11.988 | (6.925) | ** | 12.531 | (6.908) | *** |
| Employed | 0.806 | | 0.775 | | *** | 0.788 | | *** | 0.821 | | *** |
| Unemployed, insured | 0.077 | | 0.093 | | *** | 0.081 | | | 0.078 | | |
| Unemployed, uninsured | 0.105 | | 0.121 | | *** | 0.122 | | *** | 0.091 | | *** |
| Data missing | 0.011 | | 0.011 | | | 0.009 | | | 0.010 | | |
| *Father characteristics* | | | | | | | | | | | |
| Age at childbirth | 31.1 | (8.635) | 30.8 | (8.489) | ** | 30.9 | (8.586) | ** | 31.2 | (8.640) | |
| Academic education | 0.240 | | 0.143 | | *** | 0.188 | | *** | 0.204 | | *** |
| Non-academic education | 0.701 | | 0.797 | | *** | 0.750 | | *** | 0.738 | | *** |
| Education missing | 0.060 | | 0.061 | | | 0.061 | | | 0.058 | | |
| Log earnings (DKK) | 10.455 | (4.946) | 10.074 | (5.106) | *** | 10.349 | (4.933) | | 10.383 | (4.956) | |
| Work experience (years) | 15.811 | (8.405) | 15.644 | (8.361) | | 16.005 | (8.327) | | 16.100 | (8.293) | ** |
| Employed | 0.825 | | 0.810 | | ** | 0.818 | | | 0.836 | | ** |
| Unemployed, insured | 0.058 | | 0.072 | | *** | 0.064 | | * | 0.058 | | |
| Unemployed, uninsured | 0.072 | | 0.075 | | | 0.072 | | | 0.062 | | *** |
| Data missing | 0.045 | | 0.044 | | | 0.045 | | | 0.045 | | |
| No. of students | 76,471 | | 3,900 | | | 5,196 | | | 4,928 | | |

Note: *, **, *** indicate significant difference from the mean of the students at the non-consolidated schools at the 10, 5, and 1 percent levels, respectively. All characteristics are measured in 2009, i.e. before potential exposure to school consolidation. SD: standard deviation

TABLE A2. TRANSITION RATES BY SCHOOL YEAR AND DESTINATION

| School year, primo | School year 2010-2011 | | | School year 2011-2012 | | |
|---|---|---|---|---|---|---|
| No. of students (column percent) | All schools | Non-consolidated | Consolidated | All schools | Non-consolidated | Consolidated |
| Transit to predicted school | 321,836 | 315,443 | 6,393 | 318,735 | 278,555 | 40,180 |
| | (94.5%) | (94.6%) | (86.4%) | (93.5%) | (94.2%) | (88.5%) |
| Transit not as predicted | | | | | | |
| - Transit to public school | 14,348 | 13,781 | 567 | 16,495 | 13,063 | 3,432 |
| | (4.2%) | (4.1%) | (7.7%) | (4.8%) | (4.4%) | (7.6%) |
| - Transit to private school | 4,521 | 4,083 | 438 | 5,782 | 4,005 | 1,777 |
| | (1.3%) | (1.2%) | (5.9%) | (1.7%) | (1.4%) | (3.9%) |
| Total | 340,705 | 333,307 | 7,398 | 341,012 | 295,623 | 45,389 |
| | (100%) | (100%) | (100%) | (100%) | (100%) | (100%) |

Note: Comparing the proportion of non-consolidated and consolidated schools, all transition rates are significantly different at the 1 percent level. Comparing the transition rates of ‘Non-consolidated’ and ‘Consolidated’ with ’All’, respectively, only the transition rate of students moving to other public schools is not significantly different.

TABLE A3. DID ESTIMATES OF HETEROGENEOUS EFFECTS OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT IN MATH BY STUDENT CHARACTERISTICS

| | | Model I | Model II: Type of consolidation | | | P-value |
|---|---|---|---|---|---|---|
| Sample | No. of students | Consolidation | Closings | Expansions | Mergers | closings = expansions = mergers |
| All | 45,408 | -0.075*** | -0.139*** | -0.019 | -0.085** | *0.113* |
| | | (0.026) | (0.039) | (0.044) | (0.040) | |
| Boys | 22,770 | -0.076*** | -0.126*** | -0.033 | -0.082* | *0.368* |
| | | (0.029) | (0.047) | (0.049) | (0.043) | |
| Girls | 22,638 | -0.074** | -0.151*** | -0.006 | -0.088* | *0.068* |
| | | (0.029) | (0.044) | (0.047) | (0.048) | |
| *P-value* | | *0.941* | *0.603* | *0.472* | *0.888* | |
| ADHD or similar diagnoses | 1,244 | -0.122 | 0.025 | -0.311* | -0.075 | *0.360* |
| | | (0.088) | (0.171) | (0.183) | (0.079) | |
| No ADHD or similar diagnoses | 44,164 | -0.073*** | -0.143*** | -0.011 | -0.083** | *0.058* |
| | | (0.026) | (0.038) | (0.043) | (0.041) | |
| *P-value* | | *0.560* | *0.304* | *0.077* | *0.928* | |
| Non-Western immigrants | 3,783 | 0.084 | 0.016 | 0.211** | -0.013 | *0.163* |
| or descendants | | (0.057) | (0.061) | (0.104) | (0.067) | |
| Western origin | 41,625 | -0.091*** | -0.156*** | -0.043 | -0.090** | *0.135* |
| | | (0.026) | (0.041) | (0.042) | (0.042) | |
| *P-value* | | *0.002* | *0.007* | *0.008* | *0.263* | |
| Non-academic parents | 24,248 | -0.075*** | -0.126*** | -0.019 | -0.089** | *0.213* |
| | | (0.028) | (0.043) | (0.046) | (0.045) | |
| At least one academic parent | 18,485 | -0.075** | -0.170*** | -0.011 | -0.080* | *0.068* |
| | | (0.031) | (0.050) | (0.050) | (0.045) | |
| *P-value* | | *0.990* | *0.383* | *0.834* | *0.827* | |
| Parental income below median | 13,985 | -0.055* | -0.125** | -0.009 | -0.045 | *0.211* |
| | | (0.030) | (0.049) | (0.051) | (0.045) | |
| Parental income abow median | 31,423 | -0.082*** | -0.140*** | -0.020 | -0.102** | *0.152* |
| | | (0.028) | (0.044) | (0.048) | (0.043) | |
| *P-value* | | *0.315* | *0.763* | *0.809* | *0.124* | |

Note: Standard errors are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured by student test scores in reading in spring 2010 and 2012. All student characteristics are measured in 2009, i.e. before potential exposure to school consolidation. Parental income is categorized based on the parent with the highest income. Below two subgroups, the p-value in *italic* refers to a Chi2-test of equal coefficients across the two subgroups. The p-value in the last column refers to an F-test of equal coefficients across type of consolidation. The coefficients on the post-indicator and constant are omitted from the table, due to space limitations. DID: difference-in-differences.

TABLE A4. DID ESTIMATES OF HETEROGENEOUS EFFECTS OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT IN MATH BY PRE-CONSOLIDATION SCHOOL CHARACTERISTICS

| Sample | No. of students | Model I | Model II: Type of consolidation | | | P-value closings = expansions = mergers |
|---|---|---|---|---|---|---|
| | | Consolidation | Closings | Expansions | Mergers | |
| **A. Initial school size** | | | | | | |
| Small (< 150 students) | 6028 | -0.161*** | -0.208*** | -0.096 | -0.088 | *0.363* |
| | | (0.050) | (0.061) | (0.087) | (0.092) | |
| All other (>150 students) | 39380 | -0.045 | -0.043 | -0.016 | -0.080* | *0.589* |
| | | (0.030) | (0.052) | (0.047) | (0.045) | |
| *P-value* | | *0.047* | *0.037* | *0.418* | *0.940* | |
| Feeder school (grades 0-6) | 5307 | -0.210*** | -0.295*** | -0.032 | -0.114 | *0.005* |
| | | (0.055) | (0.061) | (0.066) | (0.134) | |
| All other (grades 0-9) | 40101 | -0.049* | -0.052 | -0.018 | -0.081* | *0.586* |
| | | (0.029) | (0.050) | (0.048) | (0.042) | |
| *P-value* | | *0.009* | *0.002* | *0.860* | *0.817* | |
| **B. Initial school area** | | | | | | |
| Rural municipality | 17829 | -0.093*** | -0.180*** | -0.043 | -0.075 | *0.129* |
| | | (0.033) | (0.055) | (0.048) | (0.053) | |
| Urban municipality | 27579 | -0.038 | -0.065 | 0.033 | -0.084 | *0.483* |
| | | (0.042) | (0.053) | (0.082) | (0.061) | |
| *P-value* | | *0.296* | *0.131* | *0.418* | *0.914* | |
| **C. Initial school performance level** | | | | | | |
| National test in lowest quartile | 9054 | -0.009 | -0.083 | 0.201** | -0.160* | *0.004* |
| | | (0.055) | (0.065) | (0.084) | (0.085) | |
| National test above lowest quartile | 36354 | -0.096*** | -0.159*** | -0.089* | -0.059 | *0.292* |
| | | (0.029) | (0.049) | (0.048) | (0.045) | |
| *P-value* | | *0.159* | *0.351* | *0.003* | *0.296* | |
| School GPA in lowest quartile | 3460 | 0.177 | 0.074 | 0.410** | -0.036 | *0.234* |
| | | (0.153) | (0.217) | (0.163) | (0.255) | |
| School GPA above lowest quartile | 36917 | -0.063 | -0.061 | -0.033 | -0.122** | *0.522* |
| | | (0.039) | (0.111) | (0.051) | (0.061) | |
| *P-value* | | *0.090* | *0.054* | *0.009* | *0.936* | |

Note: Standard errors in parentheses are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured by student test scores in math in spring 2010 and 2013. All school characteristics are measured in 2009, i.e. before potential exposure to school consolidation. Below two subgroups, the p-value in *italic* refers to a Chi2-test of equal coefficients across the two subgroups. The p-value in the last column refers to an F-test of equal coefficients across type of consolidation. The coefficients on the post-indicator and constant are omitted from the table, due to space limitations. DID: difference-in-differences; GPA: grade point average.

TABLE A5. DID ESTIMATES OF THE EFFECT OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT;

ROBUSTNESS AND SPECIFICATION CHECKS

| | No. of Students | Model I | Model II: Type of consolidation | | |
|---|---|---|---|---|---|
| | | Consolidation | Closings | Expansions | Mergers |
| Main model | 90,495 | -0.025 | -0.059** | -0.007 | -0.017 |
| | | (0.015) | (0.029) | (0.021) | (0.026) |
| **A. Only students exposed to consolidations** | | | | | |
| Students exposed to consolidation | 14,024 | | -0.042 | 0.010 | |
| | | | (0.038) | (0.032) | |
| **B. Time of school consolidation** | | | | | |
| Summer 2010 | 78,257 | -0.043 | -0.032 | -0.003 | -0.168** |
| | | (0.034) | (0.049) | (0.060) | (0.069) |
| Summer 2011 | 88,709 | -0.022 | -0.067* | -0.008 | -0.007 |
| | | (0.017) | (0.035) | (0.023) | (0.027) |
| **C. Interactions with Post indicator** | | | | | |
| Add interactions w/ initial school char. (rural, feeder school, 5 school size indicators, school GPA, school GPA in lowest quartile) | 90,495 | -0.022 | -0.054* | -0.002 | -0.020 |
| | | (0.016) | (0.030) | (0.022) | (0.028) |
| Add interactions w/ initial student char. (ADHD, SEN, non-Western origin, family income, parental education) | 90,495 | -0.023 | -0.056* | -0.006 | -0.015 |
| | | (0.015) | (0.029) | (0.021) | (0.026) |
| Add interactions w/ initial school and student char. (all of the above) | 90,495 | -0.022 | -0.053* | -0.002 | -0.020 |
| | | (0.016) | (0.030) | (0.022) | (0.028) |
| **D. Impute missing post-test score** | | | | | |
| Assume unaffected by school consolidations (imputed by student's pre-test score) | 96,638 | -0.024* | -0.052** | -0.007 | -0.016 |
| | | (0.014) | (0.025) | (0.020) | (0.024) |
| Lower bound | 96,638 | -0.025* | -0.057** | -0.007 | -0.017 |
| | | (0.014) | (0.025) | (0.020) | (0.024) |
| Upper bound | 96,638 | -0.022 | -0.047* | -0.006 | -0.016 |
| | | (0.014) | (0.025) | (0.029) | (0.024) |
| **E. Inverse probability weighting** | | | | | |
| Sample weighted by the inverse probability of having a post-test score | 90,495 | -0.026* | -0.060** | -0.007 | -0.018 |
| | | (0.015) | (0.029) | (0.021) | (0.026) |

Note: Standard errors are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured by student test scores in reading in spring 2010 and 2012. In panel C, all student characteristics were measured in 2009, i.e. before potential exposure to school consolidation (see Table 4). In panel D, lower and upper bounds are calculated by assuming that the student's pre-test scores are negatively or positively affected by two times the estimated effect of consolidation, respectively (i.e. impute pre-test score +/- 2*estimated effect of consolidation). In panel E, probability weights are estimated using a logit model condition on school characteristics from Table 4, student characteristics from Table A1, and three consolidation type indicators. The coefficients on the post-indicator and constant are omitted from the table, due to space limitations. DID: difference-in-differences; GPA: grade point average; SEN: Special education needs.

TABLE A6. DID ESTIMATES OF HETEROGENEOUS EFFECTS OF SCHOOL CONSOLIDATION; FOUR-YEAR ACHIEVEMENT GAIN BY STUDENT CHARACTERISTICS

| Sample | No. of students | Model I: Consolidation | Model II: Type of consolidation: Closings | Expansions | Mergers | P-value closings = expansions= mergers |
|---|---|---|---|---|---|---|
| All | 82,793 | -0.009 | -0.047 | 0.055* | -0.048* | *0.025* |
| | | (0.019) | (0.033) | (0.033) | (0.027) | |
| | | [0.259] | [0.527] | [0.019] | [0.193] | |
| Boys | 41,624 | -0.010 | -0.047 | 0.045 | -0.042 | *0.105* |
| | | (0.022) | (0.041) | (0.035) | (0.032) | |
| | | [0.374] | [0.415] | [0.062] | [0.211] | |
| Girls | 41,169 | -0.007 | -0.046 | 0.066* | -0.053* | *0.012* |
| | | (0.020) | (0.031) | (0.034) | (0.029) | |
| | | [0.256] | [0.778] | [0.014] | [0.296] | |
| *P-value* | | *0.849* | *0.980* | *0.402* | *0.687* | |
| ADHD or similar diagnoses | 2,092 | -0.003 | -0.227** | 0.048 | 0.098 | *0.011* |
| | | (0.052) | (0.090) | (0.080) | (0.075) | |
| | | [0.899] | [0.222] | [0.083] | [0.671] | |
| No ADHD or similar diagnoses | 80,701 | -0.009 | -0.042 | 0.056* | -0.052* | *0.025* |
| | | (0.020) | (0.033) | (0.033) | (0.028) | |
| | | [0.250] | [0.455] | [0.025] | [0.198] | |
| *P-value* | | *0.889* | *0.052* | *0.907* | *0.036* | |
| Non-Western immigrants or descendants | 6,919 | -0.016 | -0.014 | 0.055 | -0.115 | *0.160* |
| | | (0.041) | (0.061) | (0.049) | (0.081) | |
| | | *[0.865]* | *[0.826]* | *[0.228]* | *[0.226]* | |
| Western origin | 75,874 | -0.008 | -0.053 | 0.055 | -0.041 | *0.037* |
| | | (0.020) | (0.035) | (0.034) | (0.027) | |
| | | *[0.256]* | *[0.588]* | *[0.027]* | *[0.279]* | |
| *P-value* | | *0.863* | *0.559* | *0.986* | *0.354* | |
| Non-academic parents | 44,603 | 0.007 | -0.043 | 0.075** | -0.024 | *0.021* |
| | | (0.020) | (0.035) | (0.033) | (0.028) | |
| | | *[0.081]* | *[0.225]* | *[0.010]* | *[0.443]* | |
| At least one academic parent | 33,405 | -0.019 | -0.039 | 0.040 | -0.067* | *0.096* |
| | | (0.024) | (0.038) | (0.038) | (0.036) | |
| | | *[0.824]* | *[0.961]* | *[0.107]* | *[0.117]* | |
| *P-value* | | *0.134* | *0.884* | *0.137* | *0.145* | |
| Parental income below median | 25,088 | -0.004 | -0.020 | 0.055 | -0.059* | *0.056* |
| | | (0.023) | (0.039) | (0.035) | (0.035) | |
| | | *[0.130]* | *[0.140]* | *[0.040]* | *[0.358]* | |
| Parental income abow median | 57,705 | -0.011 | -0.063* | 0.056 | -0.043 | *0.033* |
| | | (0.021) | (0.035) | (0.036) | (0.029) | |
| | | *[0.485]* | *[0.985]* | *[0.034]* | *[0.202]* | |
| *P-value* | | *0.724* | *0.183* | *0.956* | *0.630* | |

Note: Standard errors are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured by student test scores in reading in spring 2010 and 2012. All student characteristics are measured in 2009, i.e. before potential exposure to school consolidation. Parental income is categorized based on the parent with the highest income. The p-value in bracket refers to a Chi2-test of equal coefficients across years. Below two subgroups, the p-value in *italic* refers to a Chi2-test of equal coefficients across the two subgroups. The p-value in the last column refers to an F-test of equal coefficients across type of consolidation. The coefficients on the post-indicator and constant are omitted from the table, due to space limitations. DID: difference-in-differences.

TABLE A7. DID ESTIMATES OF HETEROGENEOUS EFFECTS OF SCHOOL CONSOLIDATION; FOUR-YEAR ACHIEVEMENT GAIN BY PRE-CONSOLIDATION SCHOOL CHARACTERISTICS

| Sample | No. of students | Model I<br>Consolidation | Model II: Type of consolidation<br>Closings | Expansions | Mergers | P-value closings = expansions= mergers |
|---|---|---|---|---|---|---|
| **A. Initial school size** | | | | | | |
| Small (< 150 students) | 10,861 | -0.033 | -0.050 | 0.027 | -0.019 | *0.753* |
| | | (0.040) | (0.054) | (0.095) | (0.054) | |
| | | *[0.371]* | *[0.066]* | *[0.716]* | *[0.236]* | |
| All other (>150 students) | 71,932 | 0.002 | -0.024 | 0.056 | -0.052* | *0.051* |
| | | (0.022) | (0.040) | (0.034) | (0.031) | |
| | | *[0.276]* | *[0.774]* | *[0.024]* | *[0.439]* | |
| *P-value* | | *0.446* | *0.700* | *0.772* | *0.583* | |
| | | | | | | |
| Feeder school (grades 0-6) | 9,767 | -0.026 | -0.054 | 0.096 | -0.048 | *0.364* |
| | | (0.044) | (0.051) | (0.098) | (0.081) | |
| | | *[0.505]* | *[0.320]* | *[0.045]* | *[0.143]* | |
| All other (grades 0-9) | 73,026 | -0.004 | -0.034 | 0.051 | -0.048 | *0.065* |
| | | (0.022) | (0.044) | (0.034) | (0.029) | |
| | | *[0.280]* | *[0.641]* | *[0.045]* | *[0.367]* | |
| *P-value* | | *0.640* | *0.770* | *0.660* | *0.991* | |
| **B. Initial school area** | | | | | | |
| Rural municipality | 32,785 | 0.018 | 0.001 | 0.084* | -0.043 | *0.083* |
| | | (0.028) | (0.050) | (0.048) | (0.035) | |
| | | *[0.009]* | *[0.012]* | *[0.036]* | *[0.743]* | |
| Urban municipality | 50,008 | -0.028 | -0.097*** | 0.033 | -0.041 | *0.045* |
| | | (0.026) | (0.035) | (0.041) | (0.043) | |
| | | *[0.270]* | *[0.169]* | *[0.122]* | *[0.034]* | |
| *P-value* | | *0.230* | *0.104* | *0.425* | *0.958* | |
| **C. Initial school performance level** | | | | | | |
| National test in lowest quartile | 17,261 | -0.027 | 0.008 | 0.024 | -0.129** | *0.025* |
| | | (0.031) | (0.053) | (0.031) | (0.053) | |
| | | *[0.761]* | *[0.479]* | *[0.612]* | *[0.116]* | |
| National test above lowest quartile | 65,532 | -0.020 | -0.110*** | 0.051 | -0.031 | *0.015* |
| | | (0.023) | (0.038) | (0.042) | (0.032) | |
| | | *[0.229]* | *[0.991]* | *[0.029]* | *[0.518]* | |
| *P-value* | | *0.870* | *0.072* | *0.631* | *0.109* | |
| | | | | | | |
| School GPA in lowest quartile | 6,468 | -0.051 | -0.119 | 0.082 | -0.291* | *0.038* |
| | | (0.080) | (0.118) | (0.068) | (0.155) | |
| | | *[0.696]* | *[0.105]* | *[0.249]* | *[0.367]* | |
| School GPA above lowest quartile | 67,358 | 0.015 | -0.079* | 0.039 | -0.010 | *0.098* |
| | | (0.027) | (0.041) | (0.038) | (0.038) | |
| | | *[0.151]* | *[0.331]* | *[0.084]* | *[0.676]* | |
| *P-value* | | *0.510* | *0.753* | *0.420* | *0.095* | |

Note: Standard errors in parentheses are clustered at the level of the school that the student attended prior to potential exposure to consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured by student test scores in reading in spring 2010 and 2014. All school characteristics are measured in 2009, i.e. before potential exposure to school consolidation. The p-value in bracket refers to a Chi2-test of equal coefficients across years. Below two subgroups, the p-value in *italic* refers to a Chi2-test of equal coefficients across the two subgroups. The p-value in the last column refers to an F-test of equal coefficients across type of consolidation. The coefficients on the post-indicator and constant are omitted from the table, due to space limitations. DID: difference-in-differences; GPA: grade point average.