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The Short-Term Effects of School Consolidation on Student Achievement: Evidence of Disruption?

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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 prior to school consolidation and up to four years after. We find that the achievement of students in closing schools is adversely affected in the short run. 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 classification: I21; J24.

Keywords: School size; school resources; school mergers; 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, which could possibly justify compensatory policies.

In North America, Europe, and Scandinavia in particular, many 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 investigated (e.g. Kuziemko 2006, Berry and West 2010, Brummet 2014 and de Haan et al. 2016). To our knowledge, there is no evidence on the effects across types of consolidations.

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. Like a few other 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. School consolidations were not generally targeted at low-performing schools.¹ 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.² This possibly improves the external validity of the estimates.

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; test scores of students exposed to school closure decrease 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

¹ Nonetheless, consolidating schools, particularly closing schools, tend to have lower performance and less favorable student characteristics than nonconsolidating schools. We cannot rule out that the decisions of some policy makers may have been affected by school performance and we therefore try to address this issue in our analysis.

² 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.

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.³ First, a school transition causes a *disruption effect*, which is a short-term effect – although it could potentially have long-lasting repercussions. 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

³ See e.g. Hanushek et al. (2004) and Behaghel et al. (2017).

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 yield 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 assumption that the effect of school size is homogeneous across distance. The instrument is invalid if there is a systematic relationship between the distance to a small school and the 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. (2016), 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. (2016) 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. (2016) 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 the 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. Grade retention is very uncommon after first grade. The maximum official class size is 28 but actual class size 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 school other than their neighborhood school if an opening is available (See the Danish Public School Act). 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 many other countries, low-income parents are eligible for a voucher if they choose to send their child to a private school, thus making them 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 spring 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. 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 quick and radical reform of the municipalities in Denmark was implemented after two years of preparation. From the perspective of school policy, this initiated a process of 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 put school consolidations on the political agenda and led to a wave of school consolidations in the amalgamated *and* the not amalgamated municipalities (Bækgaard, 2010). One of the arguments in favor of the reform was that larger municipalities could generate gains based on a reorganization of their school structure.⁵ For example, larger municipalities were hypothesized to have better opportunities for establishing

⁴ For more details, see Beuchert and Nandrup (2018).

⁵ See Strukturkommissionen (2004).

large schools, limit per students costs, and adapt to demographic changes. However, in the aftermath of the reform, Blom-Hansen et al. (2016) have found that cost savings in administration were offset by rising costs of labor market activities (including income transfers), while school expenditures were not affected by jurisdiction size at all.

School consolidations are controversial and, even though the topic was high on the political agenda, decisions were postponed in many municipalities because of the local elections coming up in 2009. 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 implement unpopular decisions.⁶

This process was sparked in 2010 when the Danish government as a consequence of the financial crisis, the EU stability pact and budget overruns in the municipalities initiated an enforced sanction regime (see KORA 2016). The government imposed a binding expenditure ceiling on public expenditures (subsequently formalized in the Budget Law). This expenditure ceiling affected school budgets, which took up on average almost one-quarter of local government budgets. Consequently, the total school expenditures were reduced by 6.3% from 2009 to 2014 in real terms (7% per inhabitant aged 6-16), and average school size, class size, student-teacher ratio and student-leader ratios went up.⁷ School consolidation has presumably contributed considerably to this development.⁸

⁶ See <http://www.folkeskolen.dk/509735/hver-femte-skole-er-vaek-til-sommer>

⁷ Municipalities have a large degree of fiscal autonomy. They decide the level and distribution of resources across individual schools and school structure (as long as class size does not exceed 28 students and the minimum number of lessons is satisfied).

⁸ The enforced sanction regime likely triggered school consolidations but it might also have had a direct effect on student outcomes. Students from municipalities where the budget ceiling was more binding might have been more likely to be affected by consolidation, but also more likely to be directly affected by other cutbacks in school expenditures or other expenditures. Local government functions such as labor market activities (including income

As a result of the municipal mergers and/or the enforced sanction regime, 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. More than half of school consolidations after the 2007 reform happened in 2010-2011, see KORA (2016).

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 ceiling 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 oppose 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 municipality borders.⁹ Since the consolidations appear to have been motivated primarily by

transfers), daycare, elderly care, and road service were other large budget posts influenced at the same time. To account for these simultaneous effects, we perform a robustness check where we include municipality-specific time-trends.

⁹ 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.

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 test scores 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, which is the subject most frequently tested.

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 2012¹⁰, thus resulting in a final sample of 90,495 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 2 nd or 4 th grade in a public school during the 2009-2010 school year	114,875	16,994	(14.8%)	100.0%
Exclude students who exit to special needs education	114,248	16,740	(14.7%)	99.5%
Exclude students who exit to private or independent schools	110,389	16,277	(14.7%)	96.1%
Exclude students whose pre-test scores are not available	96,638	15,462	(16.0%)	84.1%
Exclude students whose post-test scores are not available	90,495	14,025	(15.5%)	78.8%
Students in 2 nd grade (age 8)	45,155	6,967	(15.4%)	39.3%
Students in 4 th grade (age 10)	45,340	7,058	(15.6%)	39.5%

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

	Type of school consolidation							
	(1) Non-consolidated schools		(2) Closings		(3) Expansions		(4) Mergers	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
<i>Pre-test:</i>								
2 nd grade	0.038	(0.975)	-0.062	(0.992) ***	-0.028	(0.979) ***	0.058	(0.969)
4 th grade	0.042	(0.962)	-0.038	(0.979) ***	-0.063	(0.984) ***	0.020	(0.954)
<i>Post-test:</i>								
4 th grade	0.055	(0.960)	-0.082	(0.983) ***	-0.014	(0.979) ***	0.076	(0.941)

¹⁰About 15% (8%) of the students in the sample had missing test scores in 2010 (2012) due to an unsystematic technical breakdowns in the online test system. 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 (3% of the schools). We found no systematic differences in missing pre-test scores based on consolidation status.

6 th 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.

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 standardize 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 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. The differences in pre-test scores across different groups of schools are likely to reflect differences in background characteristics, see Table A1. Another explanation for these differences could be anticipation effects. We explore this issue further in section V.B.

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.¹¹ Child characteristics include sex, immigration status, birth weight, and whether the child had been diagnosed with ADHD or any other mental or behavioral disorder¹² or had special educational needs.¹³ 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 have a mental or behavioral disorder. Parental characteristics include completed higher education, years of work experience, earnings, employment, labor market attachment, age of the parents at birth, and whether the mother is a single mother. 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 exposed to 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.

¹¹ All characteristics are measured in 2009, i.e. prior to potential exposure to consolidations.

¹² Students' psychiatric diagnoses are obtained from psychiatric hospital records. Based on these records, we define an indicator of whether the child has any mental or behavioral disorders. Any diagnosis covered by ICD-10 classification group F is included in this measure. Out of those with an F-diagnosis, 24% have ADHD (F90).

¹³ Information on students' special needs are obtained from the school register on special needs instruction. Students are referred to special needs instruction by the school principal and a pedagogical and psychological evaluation of the student's needs is carried out. The parents and the student can also request an evaluation.

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 merged with registry information from Statistics Denmark on national test scores and student enrollment.

In Denmark, the majority of public schools enroll students in grades 0 through 9, i.e. primary and lower secondary students 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 students 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 definition 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.

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. Consolidations took place during the two summer holidays in this period. 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.

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. 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 depending on the school context and environment prior to potential exposure to consolidation.

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					
- School closings	42	270		312	(133)
- School expansions	24	110		134	(230)
- School mergers	11	71		82	(69)
- School mergers	7	89		96	(128)
No. of schools remaining	18	160		178	
Schools unaffected by consolidation the following summer					
Newly opened schools	1,437	1,188		1,167	(5)
No. of schools remaining	3	3		6	
All public schools	1,440	1,191		1,173	
No. of schools remaining	1,458	1,351		1,351	

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

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

	Type of consolidation							
	(1)		(2)		(3)		(4)	
	Non-consolidated schools		Closings		Expansions		Mergers	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
<i>School characteristics</i>								
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	**
<i>One year before consolidation</i>								
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 2 nd grade	-0.012	(0.418)	-0.122	(0.539) **	-0.051	(0.497)	-0.077	(0.392)
Test score 4 th grade	-0.017	(0.381)	-0.077	(0.449)	-0.086	(0.399)	-0.082	(0.379)
Test score 6 th grade	-0.003	(0.396)	-0.052	(0.471)	-0.066	(0.348)	-0.017	(0.401)
<i>Two years before consolidation</i>								
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 2 nd grade	-0.022	(0.417)	-0.020	(0.528)	-0.086	(0.388)	-0.023	(0.412)
Test score 4 th grade	-0.015	(0.354)	-0.029	(0.453)	-0.171	(0.394) ***	-0.057	(0.419)
Test score 6 th grade	-0.011	(0.369)	0.024	(0.395)	-0.126	(0.378) **	-0.036	(0.379)
<i>Average student background</i>								
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	
Mental or behavioral disorder	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)
<i>Municipality</i>								
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	
<i>Population of town school placed in</i>								
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:

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

where y_{it} denotes the test score for student i in period t , D_t is an indicator variable that equals one if $t = 1$ (post consolidation) 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 $t = 0$. α_i is a student-specific effect and ε_{it} is the idiosyncratic error. The parameter β_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 β_2 is a DID type estimate. Formally, we require the exposure to school consolidation to be strictly exogenous conditional on α_i for consistent estimation of β_2 (common trends assumption). In order to account for the fact that students are clustered within schools, which implies potential correlation of the ε_{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. A related issue is that parents with certain types of characteristics are likely to sort their child into, or out of, schools based on school performance. 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 poor school performance 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. A related issue is that schools with a declining performance profile may also experience decreased enrollment and unfavorable changes in the student composition if parents react to changes in school performance over time. The assumption of common trends is investigated in section V.C.

B. Main Results

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

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.

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

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

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 the last row refers to a Chi2-test of equal coefficients across 2nd and 4th graders. DID: difference-in-differences; OLS: ordinary least squares.

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 only find a statistically significant effect of closings on student outcomes. 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.¹⁴ 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

¹⁴ The results are similar when estimated separately for boys and girls (results available on request).

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. Of course, the outcomes that we consider are 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 heterogeneous 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 have been the focus of this literature.

To gain a deeper understanding of the effects of consolidation, we investigate the heterogeneous effects of being exposed to school consolidation. Table 6 presents DID estimates for different subgroups of students and schools. In panel A in Table 6, the point estimates for students with non-academic parents suggest that students with a disadvantaged family background are more affected by school closings than other students (although the differences are not significantly different). The effect of school closings is larger than for the whole sample

and still statistically significant. We cannot reject that the coefficients are the same across the three types of consolidation.¹⁵

Interestingly, for students with ADHD or other mental or behavioral disorders, the sign of the estimated coefficient for closings is unchanged (though insignificant), while mergers seem to exert a positive effect on these students' test scores. Taken at face value, this suggests that merged schools cope better with the challenges of mentally disabled students.

In panel B in Table 6, we consider schools that 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.¹⁶ Note however, that the point estimates on mergers of rural tend to be negative. The weaker socio-economic background of students in rural areas possibly drives this. In addition, the subgroup estimates by pre-consolidation performance level of the students' school in panel C in Table 6 suggest that students from better performing schools seem to be more adversely affected by closings than students from low-performing schools.¹⁷ We also find evidence of heterogeneous effects on students' math scores in section V.E.

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

Sample	No. of students	Model I	Model II: Type of consolidation			P-value closings = expansions = mergers
		Consolidation	Closings	Expansions	Mergers	

¹⁵ The pattern is similar for other disadvantaged groups including students growing up with a single mother or in low-income families, and students with special educational needs (not reported).

¹⁶ 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).

¹⁷ We have classified schools as low-performing if the school is in the lowest quartile of the national reading test score distribution in 2010.

A. Student characteristics

Non-academic parents	48,610	-0.020 (0.017)	-0.070** (0.031)	0.005 (0.025)	-0.003 (0.026)	<i>0.127</i>
At least one academic parent	36,445	-0.022 (0.019)	-0.035 (0.036)	-0.012 (0.024)	-0.024 (0.036)	<i>0.843</i>
<i>P-value</i>		<i>0.914</i>	<i>0.212</i>	<i>0.486</i>	<i>0.476</i>	
Mental or behavioral disorders	2,430	-0.008 (0.050)	-0.113 (0.097)	-0.068 (0.077)	0.121* (0.069)	<i>0.060</i>
No mental or behavioral disorders	88,065	-0.026 (0.016)	-0.058* (0.030)	-0.005 (0.022)	-0.021 (0.026)	<i>0.332</i>
<i>P-value</i>		<i>0.721</i>	<i>0.571</i>	<i>0.418</i>	<i>0.034</i>	

B. Initial school size and area

Small (< 150 students)	12,069	-0.056 (0.036)	-0.111** (0.046)	0.003 (0.077)	0.033 (0.063)	<i>0.114</i>
All other (>150 students)	78,426	-0.017 (0.017)	-0.014 (0.038)	-0.007 (0.022)	-0.030 (0.028)	<i>0.796</i>
<i>P-value</i>		<i>0.330</i>	<i>0.100</i>	<i>0.902</i>	<i>0.354</i>	
Feeder school (grades 0-6)	10,844	-0.045 (0.040)	-0.086* (0.047)	-0.016 (0.063)	0.043 (0.102)	<i>0.401</i>
All other (grades 0-9)	79,651	-0.022 (0.017)	-0.047 (0.038)	-0.006 (0.023)	-0.025 (0.026)	<i>0.612</i>
<i>P-value</i>		<i>0.584</i>	<i>0.524</i>	<i>0.884</i>	<i>0.522</i>	
Rural municipal	35,561	-0.039* (0.021)	-0.070* (0.041)	-0.004 (0.030)	-0.052* (0.029)	<i>0.306</i>
Urban municipal	54,934	-0.006 (0.024)	-0.043 (0.042)	-0.009 (0.031)	0.023 (0.043)	<i>0.537</i>
<i>P-value</i>		<i>0.284</i>	<i>0.644</i>	<i>0.914</i>	<i>0.147</i>	

C. Initial school performance level

National test in lowest quartile	19,132	-0.018 (0.027)	-0.021 (0.048)	0.010 (0.032)	-0.047 (0.042)	<i>0.481</i>
National test above lowest quartile	71,363	-0.041** (0.018)	-0.106*** (0.038)	-0.024 (0.024)	-0.013 (0.031)	<i>0.114</i>
<i>P-value</i>		<i>0.478</i>	<i>0.164</i>	<i>0.392</i>	<i>0.521</i>	

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 and school characteristics are measured in 2009, i.e. before potential exposure to school consolidation. Below two subgroups, the p-value in *italics* 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. DID: difference-in-differences.

The timing of national testing and consolidation implies that it is possible that the schools exposed to consolidation anticipate and react on this information *prior* to actual exposure to consolidation. In order to investigate to which extent this may be the case, Figure A1 in the Appendix shows the average test score from 2010 to 2013 of schools that were potentially exposed to consolidation in 2011. These graphs show that if we consider consolidating schools in general then there is not much evidence of an anticipation effect. However, when we consider different types of consolidation, we find that there is considerable heterogeneity across types. For closing schools, we see a decline in test scores (at the school level) in the spring of the year of consolidation. On the other hand, expanding schools experience an increase in test scores. The implication is that our main results in Table 5 are likely to be upward biased for closing schools implying that our negative estimate of school closings will be conservative. On the other hand, the estimate for expanding schools will tend to be downward biased.

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 2010.¹⁸ Ideally, we would compare the trends in value-added performance for students in consolidated and non-consolidated schools.^{19,20} Instead, we consider the time trends in school demographics. Trends

¹⁸ Both Brummet (2014) and Engberg et al. (2012) face similar data limitations with only one or two pre-consolidation test score observations.

¹⁹ The only test outcome available for the local authorities is the GPA of students taking the ninth grade exit exams. Plotting the schools' average GPA from 2002 to 2010 shows that, although schools exposed to consolidation performed at a lower level, the average trends in performance levels are similar (figures available on request). However, since about 20% (60%) of the schools (closing schools) are feeder schools, not offering ninth grade, this measure is not available for the consolidation decision of many small and rural feeder schools.

²⁰ If schools are negatively selected based on past test scores and student test scores are mean reverting, this would imply that DID estimates would be biased upwards (towards finding positive effects of consolidations). However, if weaker students are on flatter learning trajectories than other students, estimates would be biased downwards.

in school demographics most likely reflect the school performance measures available to local authorities in the period when they were making decisions about consolidation. First, 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 rate out of schools was 11% at public schools which are later observed to be consolidated and 8% at other public schools. This is supported by Figure A2 showing similar trends in average school size comparing non-consolidated schools and closing, expanding, and merging schools.

Figure A3 illustrates the trends in two selected school demographics: the schools' share of students with low-educated parents and the schools' average parental income level. These are observable by the local authorities deciding how to consolidate schools – an increase in the share of low educated or low-income students may be a sign of decreasing school quality and more advantaged parents moving their children to other schools. This is, however, not what we observe in data: although non-consolidated schools on average have fewer low-educated families and higher average income levels, the trends from 2007 to 2010 look similar for closing, expanding, merging schools, and non-consolidating 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 of the common trend assumption 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 share of the students that transfer to the school they are predicted to move to. Almost 90% (85%) of the students exposed to school consolidation (closings) 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 20 students potentially violates the identifying assumption.²¹

D. Robustness Checks

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 A3 in the Appendix. Panel A 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 both insignificant (mergers is the reference group), supporting the above conclusions of no statistically significant differences between the three types of consolidating schools. Panel B 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 the mergers implemented during the summer 2010, which only counted seven schools. In order to account for potential differences in time trends across municipalities, panel C in Table A3 presents results from a specification that allows for municipality-specific time trends. The main results are robust to the inclusion of these time trends. This alleviates the concern that the results are driven by differential time trends in municipalities with consolidating schools versus municipalities with non-consolidating 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 and student characteristics that based on Table 4 were correlated with the exposure to later consolidation. Such interactions control for the trend in test scores

²¹ 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.

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 D in Table A3.

Panels E and F in Table A3 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 unobserved and hence the students are dropped from the sample. A simple attrition analysis in the form of a regression of the indicator for a missing test score on indicators for the three types of consolidation shows that students from closing schools are about 10 percentage points more likely to have a missing test score after consolidation. The mean of pre-test scores of movers is -0.33 (1.22 SD), which suggests that movers tend to be low performing. Panel E addresses this by imputing missing post-test scores and estimate lower and upper bounds on the parameters of interest, while panel F implements inverse probability weighting.

Panel E in Table A3 shows that if we impute missing post-test scores with pre-test scores, the estimated effects are of the same magnitude and the negative effect of school consolidation (2.4% of a SD) is now borderline significant. As an alternative strategy, we also consider the robustness of our estimates to scenarios where the students with missing test scores are at the bottom versus the top of the ability distribution. To get reasonable lower and upper bounds on our estimates, we impute missing post-test scores with the 10th and 90th percentiles of the pre-test score distribution, respectively. The results are shown in Panel E of Table A3 and leads to a lower bound on the effect of school closings of -0.178 and an upper bound of 0.065. Even if the best students in the closing schools are the students that exit public schooling, it is important to keep in mind that the students in the closing schools perform worse on average. Thus, while these bounds are of interest, especially the upper bound is an extreme scenario.

Panel F in Table A3 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.²²

In our main analysis inference is robust to clustering at the level of the pre-consolidation school. In principle, inference should also be made robust to clustering at the level of the post-consolidation school as these may vary. Panel G of Table A3 shows the results when we adjust standard errors for two-way clustering (at the level of the pre-consolidation school *and* the level of the post-consolidation school). The estimated standard errors change only slightly and the estimated coefficient on closings is still statistically significant.

Finally, Table A4 shows that the main results are robust to using a combination of DID and propensity score matching. Given that consolidating and non-consolidating schools to some extent vary in their observable characteristics, the validity of the standard difference-in-differences setup may be questioned. To take such initial differences into account, Table A4 shows the estimation results when we use a combination of propensity score matching and difference-in-differences to ensure that we are comparing students with the same baseline characteristics, see Abadie (2005). Matching is based on all student and parental characteristics reported in Table A1 including school characteristics (rural area, feeder school, and three school size indicators). The estimated coefficients are very similar to those obtained using basic difference-in-differences. The main difference is that the estimated coefficient on closings becomes numerically larger. Additional analyses show that the estimated coefficients are affected by the choice of matching variables, but the estimated coefficients remain in the same order of magnitude.

²² Probability weights are estimated using a logit model conditioning 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.

E. Results on Math

Using math test scores as the outcome measure, we replicate the main analysis. The results are shown in Table 7.²³ The above results on the effect of school consolidation on achievement gains are generally robust, and the point estimates tend to be larger in absolute size compared to those found in our main results in Table 5. Students exposed to school consolidation lose 0.075 SD in math achievement compared to students from non-consolidated schools and the effect is significantly larger for students from initially small schools (0.161 SD) and feeder schools (0.210 SD), compared to students from larger consolidated schools.

For math, we again find adverse effects from school closings. The point estimates tend to be larger in absolute size than the corresponding estimates for reading. And, for math scores, we also find evidence of adverse effects on students exposed to administrative mergers. These results are consistent with Brummet (2014) who also finds larger impacts on math scores. The findings are consistent across subgroups; see selected subgroups in Table 7.²⁴ For expanding schools, the results are still largely insignificant, although the estimates do suggest a positive effect of expansions for low-performing schools. In line with this, the estimated effect of closings is larger (in absolute terms) when relatively high-performing schools are closed. Brummet (2014) and Engberg et al. (2012) also show results that reflect the tendency of relatively low-performing students to gain by being placed with relatively high-performing students.

²³ Students are only tested in math every third year (in grades 3 and 6, respectively) thus the results on math are from an alternative sample, and measured three years after consolidation. The sample includes all third graders enrolled during the same period of school consolidations (i.e. third grade students with a pre math test from spring 2010 and a post math test in spring 2013, when they are in sixth grade).

²⁴ For girls, students without mental or behavioral disorders, and students with an academic parent or students from feeder schools, we are able to reject the tests of equal coefficients across types of consolidation at the 10% significance level.

TABLE 7. DID ESTIMATES OF THE EFFECT OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT IN MATH

Sample	No. of students	Model I	Model II: Type of consolidation			P-value
		Consolidation	Closings	Expansions	Mergers	closings = expansions = mergers
All	45,408	-0.075*** (0.026)	-0.139*** (0.039)	-0.019 (0.044)	-0.085** (0.040)	<i>0.113</i>
A. Student characteristics						
Non-academic parents	24,248	-0.075*** (0.028)	-0.126*** (0.043)	-0.019 (0.046)	-0.089** (0.045)	<i>0.213</i>
At least one academic parent	18,485	-0.075** (0.031)	-0.170*** (0.050)	-0.011 (0.050)	-0.080* (0.045)	<i>0.068</i>
<i>P-value</i>		<i>0.990</i>	<i>0.383</i>	<i>0.834</i>	<i>0.827</i>	
Mental or behavioral disorders	1,244	-0.122 (0.088)	0.025 (0.171)	-0.311* (0.183)	-0.075 (0.079)	<i>0.360</i>
No mental or behavioral disorders	44,164	-0.073*** (0.026)	-0.143*** (0.038)	-0.011 (0.043)	-0.083** (0.041)	<i>0.058</i>
<i>P-value</i>		<i>0.560</i>	<i>0.304</i>	<i>0.077</i>	<i>0.928</i>	
B. Initial school size and area						
Small (< 150 students)	6028	-0.161*** (0.050)	-0.208*** (0.061)	-0.096 (0.087)	-0.088 (0.092)	<i>0.363</i>
All other (>150 students)	39380	-0.045 (0.030)	-0.043 (0.052)	-0.016 (0.047)	-0.080* (0.045)	<i>0.589</i>
<i>P-value</i>		<i>0.047</i>	<i>0.037</i>	<i>0.418</i>	<i>0.940</i>	
Feeder school (grades 0-6)	5307	-0.210*** (0.055)	-0.295*** (0.061)	-0.032 (0.066)	-0.114 (0.134)	<i>0.005</i>
All other (grades 0-9)	40101	-0.049* (0.029)	-0.052 (0.050)	-0.018 (0.048)	-0.081* (0.042)	<i>0.586</i>
<i>P-value</i>		<i>0.009</i>	<i>0.002</i>	<i>0.860</i>	<i>0.817</i>	
Rural municipality	17829	-0.093*** (0.033)	-0.180*** (0.055)	-0.043 (0.048)	-0.075 (0.053)	<i>0.129</i>
Urban municipality	27579	-0.038 (0.042)	-0.065 (0.053)	0.033 (0.082)	-0.084 (0.061)	<i>0.483</i>
<i>P-value</i>		<i>0.296</i>	<i>0.131</i>	<i>0.418</i>	<i>0.914</i>	
C. Initial school performance level						
National test in lowest quartile	9054	-0.009 (0.055)	-0.083 (0.065)	0.201** (0.084)	-0.160* (0.085)	<i>0.004</i>
National test above lowest quartile	36354	-0.096*** (0.029)	-0.159*** (0.049)	-0.089* (0.048)	-0.059 (0.045)	<i>0.292</i>
<i>P-value</i>		<i>0.159</i>	<i>0.351</i>	<i>0.003</i>	<i>0.296</i>	

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 student and school characteristics are measured in 2009, i.e. before potential exposure to school consolidation. Below two subgroups, the p-value in *italics* 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.

Since the estimates using math and reading scores generally point in the same direction, it may be reasonable to assume that the effects of consolidation on math and reading scores are equal. Table 8 shows the results when we pool math and reading scores. This increases power and the overall estimate of the effect of consolidation is now -4.2% of a SD and statistically significant at the 5% level. The estimated effect on closings is still negative and statistically significant and there is now also a negative and statistically significant effect of mergers. In addition, we can reject the hypothesis that the effects are equal across the different types of consolidation ($P=0.069$). In conclusion, the estimates using the pooled sample strengthen our main results.

TABLE 8: DID ESTIMATES OF THE EFFECT OF SCHOOL CONSOLIDATION ON STUDENT ACHIEVEMENT
POOLING READING AND MATH

Sample	No. of Students	Model I	Model II: Type of consolidation			P-value
		Consolidation	Closings	Expansions	Mergers	closings = expansions = mergers
Reading and math	135,903	-0.042*** (0.015)	-0.085*** (0.027)	-0.011 (0.021)	-0.039* (0.023)	0.069

Note: This table replicates the main model specification pooling the reading and math samples. 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. The pooled sample includes 2nd graders (with reading scores from 2010-2012), 3rd graders (with math scores from 2010-2013), and 4th graders (with reading scores from 2010-2012) enrolled in the school year 2009/2010, i.e. the school year before potential exposure to school consolidation.

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.

In this section, we compare students exposed to closings, expansions, and mergers across time, and 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, e.g. changes in school inputs or increased travel time.

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 After four years, the detrimental effect of consolidation is small and

²⁵ The second graders exposed to school consolidation will now be sixth graders and the fourth graders will be eighth graders. In 2014, the response rates are 94% and 89% for the second and fourth grade sample, respectively (in total, 7,741 students are lost from 2012 to 2014). A simple analysis of attrition suggests that this attrition is not systematic across consolidation types. Also, if we restrict the sample for the two-year achievement gains to resemble the sample for the four-year achievement gains, we find similar results.

insignificant; see Table A5 in the Appendix. Although, with the exception of two subgroups (rural schools and students with low-educated parents), we cannot reject equality of coefficients across years making it hard to draw any definitive conclusions.

However, we do observe significant differential effects of closings, expansions, and mergers after four years. These are mainly driven by the effect of expansions, which is significantly larger, and positive, after four years compared to the estimated effect after two years. The complete set of results are presented in Table A5, including test of equality across years. For an illustration, Figure 1 presents the DID estimates on the two and four-year achievement gains in reading by type of consolidation for two groups of students. 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 6). 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**

Note: Each dot illustrates the gain in reading scores from spring 2010 to spring 2012 or 2014, respectively, and for each consolidation type (closings, expansions, and mergers). Point estimates are from DID estimation using the preferred model specification. For more details, see Tables 5 and 6 (spring 2012) and Table A4 (spring 2014). Panel A includes all students and panel B the subgroup of students initially enrolled in a small school. DID: difference-in-differences.

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.²⁶ 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 A5 in the Appendix. This 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. This finding is consistent across most subgroups; see Table A5 in the Appendix.

At the same time, however, students exposed to school mergers now experience a negative and statistically significant achievement gain; see Appendix Table A5. The estimated effect of mergers was statistically insignificant for the two-year achievement gains; see Table 5. This supports the hypothesis of negative effects of consolidation, even when consolidation is only of an administrative nature. Table A5 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.

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.

²⁶ Chi²-tests of equal coefficients support that the four-year achievement gain is significantly different from the two-year achievement gain for students exposed to a school expansion.

The estimated negative effect of (any type of) consolidation on student test scores in reading is about 2.5% of a SD, albeit statistically insignificant. Pooling results from reading and math, we find an overall effect of consolidation that is negative and statistically significant at the 5% level. Also, for the pooled sample, we can reject the hypothesis that the effects are the same across type of consolidation at the 10% level of significance. When we limit the sample to reading, we lack statistical power to generally conclude that the effects differ across type of consolidation, but the fact that the size of the effects vary considerably by type of consolidation indicates that more mechanisms are at play than just the school size effect. Specifically, the adverse effects of consolidation are driven by displaced students rather 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.

Splitting the sample based on the characteristics of the school attended prior to potential exposure to consolidation, we find losses in achievement for displaced students that ranges between 2-11% of a SD. Students from small schools (less than 150 students) suffer the most detrimental effects of school closings. This 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 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. On the other hand, the negative effect of school mergers tends to be larger after four years, which may reflect that an administrative merger is a more gradual process.

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 implementation process, the negative effects could be compensated while achieving the positive long-term effects of school consolidation.

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APPENDIX – FIGURES

FIGURE A1: SCHOOLS' AVERAGE TEST SCORES BY YEAR

Note: Each marker represents the schools' average national test scores pooling reading and math scores from grades 2-4. We use schools exposed to consolidations in summer 2011. The vertical line represents time of potential exposure to consolidation.

FIGURE A2. TRENDS IN AVERAGE SCHOOL SIZE BY CONSOLIDATION TYPE

FIGURE A3. TRENDS IN SCHOOL DEMOGRAPHICS BY CONSOLIDATION TYPE

Note: Sample includes all students enrolled in grade levels 1 to 6 in school years 2007/08 to 2011/12.

APPENDIX – TABLES

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

	Type of consolidation							
	(1) Non-consolidated schools		(2) Closings		(3) Expansions		(4) Mergers	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
<i>Child characteristics</i>								
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	***
Mental or behavioral disorders	0.026		0.026		0.029		0.031	**
<i>Mother characteristics</i>								
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	
<i>Father characteristics</i>								
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

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

Note: The type of transition is defined based on the institution of the student in the beginning of the school year.

TABLE A3. DID ESTIMATES: ROBUSTNESS AND SPECIFICATION CHECKS

	No. of Students	Model I	Model II: Type of consolidation		
		Consolidation	Closings	Expansions	Mergers
Main model	90,495	-0.025 (0.015)	-0.059** (0.029)	-0.007 (0.021)	-0.017 (0.026)
A. Only exposed students					
Students exposed to consolidation	14,024		-0.042 (0.038)	0.010 (0.032)	
B. Time of school consolidation					
Summer 2010	78,257	-0.043 (0.034)	-0.032 (0.049)	-0.003 (0.060)	-0.168** (0.069)
Summer 2011	88,709	-0.022 (0.017)	-0.067* (0.035)	-0.008 (0.023)	-0.007 (0.027)
C. Municipality-specific time trends					
Add post-indicator interacted with municipality FE	90,495	-0.029 (0.019)	-0.057* (0.033)	-0.024 (0.022)	-0.002 (0.031)
D. Interactions with post indicator					
Add interactions w/ initial school char.	90,495	-0.022 (0.016)	-0.054* (0.030)	-0.002 (0.022)	-0.020 (0.028)
Add interactions w/ initial student char.	90,495	-0.023 (0.015)	-0.056* (0.029)	-0.006 (0.021)	-0.015 (0.026)
Add interactions w/ initial school and student characteristics	90,495	-0.022 (0.016)	-0.053* (0.030)	-0.002 (0.022)	-0.020 (0.028)
E. Impute missing post-test score					
Assume unaffected by school consolidations (imputed by student's pre-test score)	96,638	-0.024* (0.014)	-0.052** (0.025)	-0.007 (0.020)	-0.016 (0.024)
Lower bound	96,638	-0.067*** (0.017)	-0.178*** (0.038)	-0.007 (0.021)	-0.033 (0.027)
Upper bound	96,638	0.026 (0.019)	0.065* (0.036)	0.024 (0.030)	-0.006 (0.028)
F. Inverse probability weighting					
Sample weighted by the inverse probability of having a post-test score	90,495	-0.026* (0.015)	-0.060** (0.029)	-0.007 (0.021)	-0.018 (0.026)
G. Two-way clustering					
Add two-way clustering	90,495	-0.025 (0.016)	-0.059* (0.033)	-0.007 (0.021)	-0.017 (0.026)

Note: In panels A-F, standard errors are clustered at the level of the school that the student attended prior to potential exposure to consolidation. In panel G, standard errors are clustered at the school that the student attended prior to and after consolidation. *, **, *** indicate significance at the 10, 5, and 1 percent levels, respectively. Achievement is measured as the gain in students' reading scores from spring 2010 to 2012. In panel D, school characteristics (rural, feeder school, five school size indicators, school GPA, school GPA in lowest quartile) and student characteristics (ADHD, SEN, non-Western origin, family income, parental education) were measured in 2009 (see Table 4). In panel E, lower and upper bounds are obtained by replacing missing post-test scores with the 10th and 90th percentile values of the pre-test score distribution. In panel F, probability weights are estimated using a logit model including school characteristics from Table 4, student characteristics from Table A1, and the

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; FE: fixed effect.

TABLE A4: DID WITH PROPENSITY SCORE MATCHING

Sample	(1) Consolidation	(2) Closings	(3) Expansions	(4) Mergers
All students	-0.026*** (0.006)	-0.065*** (0.012)	-0.015 (0.009)	-0.009 (0.010)
<i>No. Students</i>	90,495	79,247	80,161	81,033

Note: Estimates obtained using Stata command `absdid` with default settings (Houngbedji 2016). `Absdid` implements the semi-parametric difference-in-difference estimator of Abadie (2005). Standard errors in parentheses are robust and take into account that the propensity scores are estimated, as described in Abadie (2005). Each column is a separate estimation. Propensity scores are conditional on all student and parental characteristics from Table A1 and school characteristics (rural area, feeder school, and three school size indicators).

TABLE A5. DID ESTIMATES OF HETEROGENEOUS EFFECTS OF SCHOOL CONSOLIDATION; FOUR-YEAR ACHIEVEMENT GAIN

Sample	No. of students	Model I	Model II: Type of consolidation			P-value closings = expansions = mergers
		Consolidation	Closings	Expansions	Mergers	
All	82,793	-0.009 (0.019) [0.259]	-0.047 (0.033) [0.527]	0.055* (0.033) [0.019]	-0.048* (0.027) [0.193]	0.025
A. Student background						
Non-academic parents	44,603	0.007 (0.020) [0.081]	-0.043 (0.035) [0.225]	0.075** (0.033) [0.010]	-0.024 (0.028) [0.443]	0.021
At least one academic parent	33,405	-0.019 (0.024) [0.824]	-0.039 (0.038) [0.961]	0.040 (0.038) [0.107]	-0.067* (0.036) [0.117]	0.096
<i>P-value</i>		0.134	0.884	0.137	0.145	
Mental or behavioral disorders	2,092	-0.003 (0.052) [0.899]	-0.227** (0.090) [0.222]	0.048 (0.080) [0.083]	0.098 (0.075) [0.671]	0.011
No mental or behavioral disorders	80,701	-0.009 (0.020) [0.250]	-0.042 (0.033) [0.455]	0.056* (0.033) [0.025]	-0.052* (0.028) [0.198]	0.025
<i>P-value</i>		0.889	0.052	0.907	0.036	
B. Initial school size and area						
Small (< 150 students)	10,861	-0.033 (0.040) [0.371]	-0.050 (0.054) [0.066]	0.027 (0.095) [0.716]	-0.019 (0.054) [0.236]	0.753
All other (>150 students)	71,932	0.002 (0.022) [0.276]	-0.024 (0.040) [0.774]	0.056 (0.034) [0.024]	-0.052* (0.031) [0.439]	0.051
<i>P-value</i>		0.446	0.700	0.772	0.583	
Feeder school (grades 0-6)	9,767	-0.026 (0.044) [0.505]	-0.054 (0.051) [0.320]	0.096 (0.098) [0.045]	-0.048 (0.081) [0.143]	0.364
All other (grades 0-9)	73,026	-0.004 (0.022) [0.280]	-0.034 (0.044) [0.641]	0.051 (0.034) [0.045]	-0.048 (0.029) [0.367]	0.065
<i>P-value</i>		0.640	0.770	0.660	0.991	
Rural municipality	32,785	0.018 (0.028) [0.009]	0.001 (0.050) [0.012]	0.084* (0.048) [0.036]	-0.043 (0.035) [0.743]	0.083
Urban municipality	50,008	-0.028 (0.026) [0.270]	-0.097*** (0.035) [0.169]	0.033 (0.041) [0.122]	-0.041 (0.043) [0.034]	0.045
<i>P-value</i>		0.230	0.104	0.425	0.958	
C. Initial school performance level						
National test in lowest quartile	17,261	-0.027 (0.031) [0.761]	0.008 (0.053) [0.479]	0.024 (0.031) [0.612]	-0.129** (0.053) [0.116]	0.025
National test above lowest quartile	65,532	-0.020 (0.023) [0.229]	-0.110*** (0.038) [0.991]	0.051 (0.042) [0.029]	-0.031 (0.032) [0.518]	0.015
<i>P-value</i>		0.870	0.072	0.631	0.109	

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 2014. All student and 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 *italics* 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.