Long-Term Effects of School Size on Students' Outcomes

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Abstract

We estimate the effect of school size on students' long-term outcomes such as high school completion, being out of the labor market, and earnings at the age of 30. We use rich register data on the entire population of Danish children attending grade 9 in the period 1986–2004. This allows us to compare the results of different fixed effect and instrumental variables estimators. We use the natural population variation in the residential catchment areas and school openings and closures to instrument for actual school size. We find a robust positive but numerically fairly small relationship between school size and alternative measures of long-term success in the educational system and the labor market. The positive impact of school size seems mainly to be driven by boys and students from families with a low educational level.

JEL classification: I21; I28; J24

Keywords: School size; High school graduation; Student outcomes; Educational economics.

1 Introduction

School size is an important policy parameter. During recent decades a major consolidation process has taken place in many OECD countries that resulted in a rapidly declining number of schools and school districts and an increase in average school size, see Newman et al. (2006) for the US. The same develpoment is observed in Denmark where schools on average have been fairly small but during the latest decade the political focus has unambiguously been to increase school size, see Feilberg (2013). Policymakers often appear to prefer large schools due to scale economies associated with administrative costs and most often the arguments behind school consolidation have been cost savings and economies of scale. Only recently, an opposite movement has started in some countries towards smaller schools. In the US, the Small Schools Initiative has got support from rich private foundations like the Bill and Melinda Gates Foundation to invest in small school projects. School size may also have effects on the local community and this may in practice be one of the more important arguments in favor of small schools and against school consolidation. Closing a local school may have negative effects on social activities in the local community and in the long run it may also have implications for the social composition of residents in the community which may generate more negative peer effects, see Egelund and Laustsen (2006). However, in this study we restrict our analysis to the more direct long-term effects of school size on students' educational and labor market careers.

There is a growing literature on the impact of class size on student outcomes, see for instance Fredriksson et al. (2013) for a recent study from Sweden which in many respects has a school system similar to the Danish primary and secondary school system. In line with many other studies, they find that larger school classes have a causal negative effect on school outcomes in the short run as well as on long-run outcomes in the labor market. The empirical research on the impact of school size on children's cognitive and noncognitive skills is more sparse and the results are more mixed. Most of the studies find negative, but numerically small effects of larger schools when controlling for parental inputs and other background characteristics, see the recent survey in Leithwood and Jantzi (2009). But even if the effects of school size are numerically small, school size is a political instrument which is much more amenable to change by policymakers than parental background. Thus, even small effects of school size on students' academic achievement and behavior may have important consequences for policy decisions. Our definition of the effect of school size is the total policy effect of school size, i.e. including both the direct effect of school size and any potential indirect effects, such as an effect through class size.

While most existing studies have focused on the in-school performance of children or short-run effects of school size, we mainly consider the long-run consequences of school size, for example, educational attainment after compulsory school and the earnings capacity later in life at the age of 30. While estimates of the effect of school size on short-run outcomes (typically test scores or behavioral measures while attending school) of course are informative, it is crucial to know whether these potential short-run effects also translate into differences in the long run, see the general discussion of the effect of school inputs on student outcomes in Hanushek (2006). We consider outcomes that are generally considered to be affected by both cognitive and noncognitive skills. In a sense this allows us to obtain a more overall picture of the effects of school size than if we considered more narrow outcomes that measured particular forms of either cognitive or noncognitive skills.

The study is based on register data on the total population of students in grade 9 in Denmark during the period 1986–2004. We follow the students until 2010. For these students, who are typically 16 years old when they complete grade 9, we focus on the educational outcomes 6 years later, i.e. at the age of 22. For students in the cohorts from 1986 to about 1998, we also observe earnings at the age of 30.[1] The richness of the data allows for the comparison of different alternative estimators that are often used in the empirical literature on the causal relationship between school characteristics and student outcomes. As the point of departure, we estimate the effect of school size by simple OLS including a wide range of individual-level characteristics available in the register data such as birth order, family type, and parental earnings and education. In addition, alternative fixed effect strategies based on schools and siblings are employed. Finally, we implement two different instrumental variables strategies based on natural population variation in the catchment area and school openings and closings, respectively.

Based on the wide range of estimators implemented, our results show a robust and positive, but numerically fairly small, relationship between school size and alternative measures of long-term success in the educational system and the labor market, such as the probability of high school completion and earnings at the age of 30. This result is also robust to controlling for grade 9 exit exam grades. The positive impact of school size seems mainly to be driven by boys and students from families with a low educational level.

In Section 2 we give an overview of the existing evidence on the effects of school size. In Section 3 the institutional context is described. Section 4 describes the data and offers some descriptive statistics of Danish schools while Section 5 describes our empirical strategy. Finally, Sections 6 and 7 present the results of our analyses and Section 8 concludes.

2 Earlier Empirical Research on School Size and Student Outcomes

The estimates of the impact of school size in the literature vary considerably and based on the existing research it is not easy to give an unambiguous policy advice about school size, see the survey in Leithwood and Jantzi (2009). Firstly, the results may of course vary across countries because of institutional differences. Secondly, the notions 'small schools' and 'large schools' vary from country to country. In some countries average school size is relatively large, for instance in the UK, where many of the studies are based on an average school size of around 1,000 students. Average school size also varies considerably in US studies, ranging from a few hundred students to very large average school sizes, see the survey in Newman et al. (2006). As examples, Iatarola et al. (2008) and Bloom et al. (2010) categorize schools with less than 500-550 students as 'small schools' in their studies of US high

[1] For the last couple of cohorts, we only observe earnings for the oldest members of the cohorts, though.

schools which is considerably higher than the average school size for all Danish public elementary schools analysed in this study, see Section 3 and Blom-Hansen (2004), Heinesen (2005), and Feilberg (2013).

Some studies aim at identifying an optimal school size, and this figure also seems to vary considerably. Bradley and Taylor (1998) find that the optimal school size (based on a study of English schools) is larger for older children, for children aged 11-16 it is about 1,200 students and for children aged 11-18 it is about 1,500 students. The dependent variable in their study is exam results of the students. However, their study does not control for potential endogeneity of school size and a later study from Wales indicates that the optimal school size for children in secondary school (age group 11-16) is about 600 students, see Foreman-Peck and Foreman-Peck (2006), who use school attendance as their dependent output variable.

Thirdly, 'optimal' depends on which objective function is being optimized. The above results concern student performance (grades or test scores, school attendance etc.). If the focus instead is on minimizing costs per student, the survey of US schools by Andrews et al. (2002) indicates an optimal school size of about 300-500 for elementary schools and 600-900 for high schools. Barnett et al. (2002) apply a Data Envelopment Analysis to UK secondary school data in order to identify 'optimal school size' in the case of two key objectives: school costs and student achievement. They find that schools with more than 1,000 students (the largest group in the study) performed relatively better than smaller schools with less than 1,000 students when taking both cost effectiveness and school grades into account.

Some studies focus on other social outcomes. Small schools may have less alienation effects, Strang (1987). Students, parents and teachers in small schools may feel more 'connected' to the school, more responsible for the overall functioning of the school and may be more involved in school activities compared to large schools, see McNeely et al. (2002). Thus, small schools often have more extracurricular activities than large schools despite the fact that large schools may be more successful in sports competitions. Students may have more attendance problems in large schools and the individual student may be subject to less adult supervision, Jones et al. (2008). Sometimes small schools have mixed-age grouping, peer tutoring etc. which may have positive effects on students' social behavior, feeling responsibility for their peers and even criminal behavior, see Leung and Ferris (2008). Walsh (2010) examines the relationship between parental involvement and school size and finds that an increase in school size decreases the extent of parental involvement. Leithwood and Jantzi (2009) conclude based on their survey of school size effects that dropout rates tend to increase and attendance rates, engagement by students, and extracurricular activities to decrease with school size. However, in some of the reported studies, the optimal school size (secondary or high schools in most cases) is calculated and found to be in the interval 600-1,000 or even larger. This means quite large schools from a Danish perspective, see Leithwood and Jantzi (2009, p. 473). The impact of school size also seems to vary among groups of students. Students from families with low socioeconomic status (SES), immigrant children, and students with low previous achievements seem to benefit most from smaller schools, compared to students from high-SES families or high-achieving children, see Leithwood and Jantzi (2009).

Many of the earlier studies of the impact of school size on student outcomes are troubled by the fact that school size is arguably not exogenous. The previous studies have often used cross-section data where variation across schools at a given point in time is used to identify the effects of school size. But since school size is likely to be related to the characteristics of both the children attending the school and their parents, this may cause biased estimates of the relationship between school size and school outcomes. For example, in rural areas schools generally tend to be smaller than in urban areas since these areas are much less densely populated. Additionally, it is not random which families settle in rural areas. Thus, part of the reason for the variation in empirical results concerning impact of school size may be variation in the quality of the statistical method applied, see the survey and discussion of estimation methods in Newman et al. (2006) and Kuziemko (2006).

Few existing studies have tried to address the endogeneity issues that are likely to arise in this context. Kuziemko (2006) is one of the exceptions. She uses first-differences combined with instrumental variables to estimate the effect of school size on student achievement. Her data are school-level data (elementary schools), i.e. she does not have data on individual students and she is not able to control for individual background characteristics. The instrumental variable used is based on school openings, closings, and mergers. Kuziemko argues that the timing of school mergers and closures are random shocks to the neighboring schools and the change in school size in years of school closures in the neighborhood (school district) is used as an instrument. The findings suggest that increasing school size decreases Math scores and attendance rates. Bloom et al. (2010) analyze the New York City school reforms after 2002 where large underperforming public high schools were closed and more than 200 new smaller high schools were opened. They use a quasi-experimental design where students are allocated to small or large schools in a lottery-like process and find significantly higher high school graduation rates for the treatment group in small schools compared to the control group. Schwartz et al. (2013) also study the small school reform in New York and use students' place of residence (distance to school) to instrument for endogenous school selection. They find that attending a new small school has a favorable effect on the probability of earning a diploma in four years. In addition, they find that old, small schools do not have the same positive effects on academic outcomes. Similarly, Barrow et al. (2013) consider a high school reform in Chicago and use distance from a student's residence to a small school as an instrument for enrollment at a small school. They find no effects on student test scores, but they do find evidence that smaller schools have a favorable effect on outcomes that are also affected by noncognitive skills such as persistence and the probability of graduation. In the context of estimating the effects of school size, the most ambitious identification strategies thus rely on instruments related to school restructuring and distance. These instruments are not without shortcomings. School restructuring may have other effects on student achievement and behavior than just through school size in which case it would no longer be a valid instrument.

Thus, the previous research results are mixed with respect to the impact of school size on student outcomes. Most of the studies indicate that school size negatively affects test scores and school grades (cognitive skills) as well as different measures of behavioral (noncognitive) skills, but the results are dependent on which age groups are analyzed, the outcome measures, and they are sensitive to econometric specification and identification.

3 Institutions, Schools, and Catchment Areas in Denmark

Compulsory school in Denmark typically starts at the age of 6-7 years. Grades 1-9 are compulsory grades, corresponding to children aged roughly 7-16. There is a kindergarten class (grade 0) for children aged 5-6 and although this grade is voluntary, almost all children attend grade 0.[2] The typical school contains grades 0-9, and some schools also have grade 10 which is optional. However, some schools only include some of the grades, e.g. some smaller schools only have grades 0-7. There is no distinction between primary school and lower secondary school in Denmark, but we will use the notion 'lower secondary school' when referring to the highest grades in compulsory school, i.e. grades 7-9 or 7-10. After completing grade 9 or 10, the student may either enroll in high school (upper secondary level), a vocational education and training program (upper secondary level), find a job in the labor market or be 'inactive', i.e. not participating in the labor market and not enrolled in an education. The typical upper secondary education takes 3 years, but there is some heterogeneity across programs.

In Denmark, public compulsory schools are run by the municipalities. During the period considered in this study, 1986–2004, there were 275 municipalities. Some municipalities were very small with less than 5,000 inhabitants, while the largest municipality was the municipality of Copenhagen City with about half a million inhabitants. Each municipality is divided into a number of areas, denoted 'school districts'. However, a school district in Denmark is an administrative unit parallel to a 'catchment area' in other countries. The school district or catchment area is typically defined by the geographical distance to the school and it is not regularly regulated due to for instance demographic changes in the area. Thus, the number of children in a catchment area depends on fertility in the area, demographic composition of inhabitants, housing and job opportunities. It may of course be argued that settlement patterns to some extent depend on public school quality. However, private school tuition is heavily subsidized in Denmark reducing the incentive to settle based on school quality. We discuss this question further in Section 5.

The municipality is the political unit while the school districts in Denmark have no political or administrative resources, see Heinesen (2005). Therefore, we use the notion 'catchment area' instead of 'school district'. A catchment area takes up most of the school-age children in the district. In theory it is possible to enroll in a school in another catchment area than your residential catchment area, but it would depend on the regulations at the municipality level and the discretion of the school. Parents may choose to send their children to a private school. During the period 1986–2004, 10 to 13 percent of Danish children attended private schools, Statistics Denmark (1989, 2006). The share of children attending private schools has been increasing over the sample period and in recent years. More children attend private schools in higher grades than in lower grades.

The municipality has the full responsibility for the public schools within their area. This means that the municipality decides on the number of schools and the resources devoted to schools within the municipality. Given

[2] Grade 0 is now part of compulsory school, but for the time period we consider in this paper it was voluntary.

these resources, the board of the school and the principal decide on the number of classrooms at a given grade level. However, there are national rules (decided by the Danish parliament) on for instance the maximum class size (currently 28 students), minimum teaching hours in different courses, syllabus etc. Since the costs of teachers, i.e. salaries, working conditions, number of teaching hours, preparation etc. are based on collective agreements between the teachers' union and the organization of municipalities (KL), the municipality is not able to affect a number of the school cost drivers, except for school size. Thus, in practice, school size is the main political instrument for the local politicians when monitoring schools and adjusting school costs to their budgets and local tax incomes.[3] However, since the municipalities are obliged to pay about 80 percent of the costs of private schools in the municipality, it often happens that parents in a local area start up a new private school if the municipality decides to close the public school in the area. This may be one of the reasons why the politicians in many municipalities until the latest decade have been quite reluctant to close public schools and therefore public schools in Denmark tend to be fairly small. The average school size for public compulsory schools was 326 students in 2008[4], a figure which has been quite stable during the latest decade, see Feilberg (2010).

4 Data and Descriptive Statistics

We use register data from Statistics Denmark covering the entire population of Danish children who attended grade 9 in public schools in the years 1986–2004. These data include detailed information on parental background and the catchment area in which they reside, such as the number of children in a specific age group, the fraction of immigrants etc., since we have access to these data for the entire Danish population.[5] For these cohorts we are able to observe the school outcomes 6 years after they leave grade 9. For individuals in the earlier cohorts, we also use earnings measured at the age of 30 as an outcome variable.

Table 1: Description of Estimation Sample

Description	Number of individuals	Percentage of total
Grade 9 cohorts 1986-2004	1,160,612	100.0
With non-missing information on		
- Personal identifiers and birth records	1,158,401	99.8
- School district	1,110,335	95.2
- School size	974,629	84.0
Who		
- Attended public schools	848,441	73.1
- Did not live in the municipality of Viborg	840,669	72.4
- Attended schools that did not recieve students from feeder schools	605,125	52.1
Estimation sample	605,125	52.1

[3] Besides local taxes, each municipality receives a subsidy from the central government ('bloktilskud'), and further there is a 'redistribution tax' ('udligningsbidrag') from rich municipalities to poor municipalities. None of these sources depend on school costs.

[4] The average school size reported here is based on all schools and is therefore not directly comparable to the numbers reported from the estimation sample.

[5] It was only possible to link street addresses to school catchment areas back to 1993. For the grade 9 cohorts 1986–1992, we therefore use the 1993 street address-catchment area link. That is, for an individual attending grade 9 in 1986, we use the catchment area that the individual would have resided in 1993 given his 1986 street address. Our main results are robust to whether or not the cohorts prior to 1993 are included.

In order to get an understanding of the composition of our sample, Table 1 shows the sample selection process. At the outset, we consider all children who attended grade 9 in the period 1986–2004. If a child attends grade 9 more than once, we use the first occurrence. We drop a minor part of the sample due to missing values in the register data (on either their unique personal identifiers, birth information, or school district information). A larger part of the sample is dropped due to missing information on school size. Whether information on school size is available, depends on how good the schools were at reporting these key figures. In most years, validity and coverage of the school size data are considered to be high, but especially for private schools there are some problems. We therefore restrict attention to students attending public schools in grade 9. Additionally, there is a clear inconsistency between the number of schools and the number of catchment areas in the municipality of Viborg, and children who reside in this municipality are also dropped from the analysis. Finally, we exclude large schools that collect students from feeder schools that do not have lower secondary school grades. In some municipalities, some small schools may only have grades up to grade 6 or 7, i.e. these schools do not have lower secondary school grades and function as feeder schools to larger schools in the area. Thus, some grade 9 students have only attended their current school for a few years, and these students are excluded in order to get a more homogeneous sample.[6] This also means that school size is likely to be somewhat persistent over time and one can argue that the estimated effect of school size in grade 9 in fact will represent an aggregate effect of school size. There are 605,125 students in the baseline estimation sample.

4.1 School Size and Catchment Area Size

Information on school size stems from a data set containing actual school enrollment numbers each year that is obtained from The Danish IT Centre for Education and Research (UNI-C). This data set contains enrollment numbers by year-school-grade level. We aggregate at the school level to get the school size in each year. School size in a given year is defined as the number of students in grades 0-9 in the school. Similarly, catchment area size in a given year is defined as the number of children in the catchment area aged 7–16 years at the end of the year.

The average school size in our sample was about 450 in 1986, decreasing to about 400 students in the middle of the 1990s (mainly due to small birth cohorts born in the 1980s) and then increasing to 450 again in 2004, cf. Figure 1. Thus, our schools are larger than the average public school and the average catchment area because we condition on the school having grade 9. The increase in school size up to 2004 is both due to demographic changes and an increasing number of school closures.[7] Figures 2 and 3 show the sample distribution of school size and catchment area size in 2004, respectively. School size varies from around 200 to 800 students while the variation in size of catchment areas is considerably larger varying from very small catchment areas to a few very large catchments areas

[6] We do not have exact information on which schools collect students from feeder schools. Therefore, we use an approximate way to exclude these large schools: For each year, we calculate the country proportion between cohort sizes in grade 9 and grade 5. If a school in a given year has a school-specific proportion exceeding the country proportion by more than 10 percent, the school is excluded.

[7] In 2004, the average school size for all Danish public schools was 330 increasing to 374 in 2011. During this period 20 percent of all public schools in Denmark were closed, see Feilberg (2013).

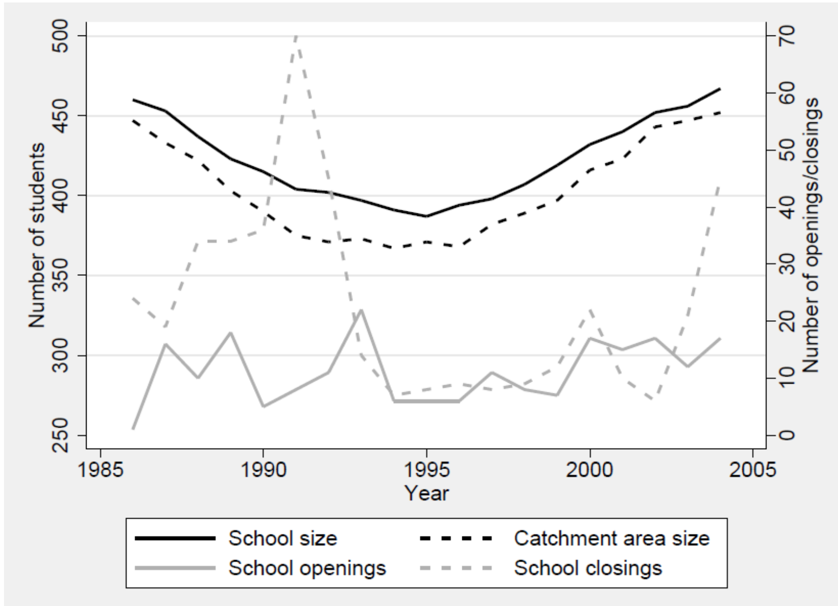

Figure 1: School size, catchment area size, school closings and school openings in the period 1986–2004.

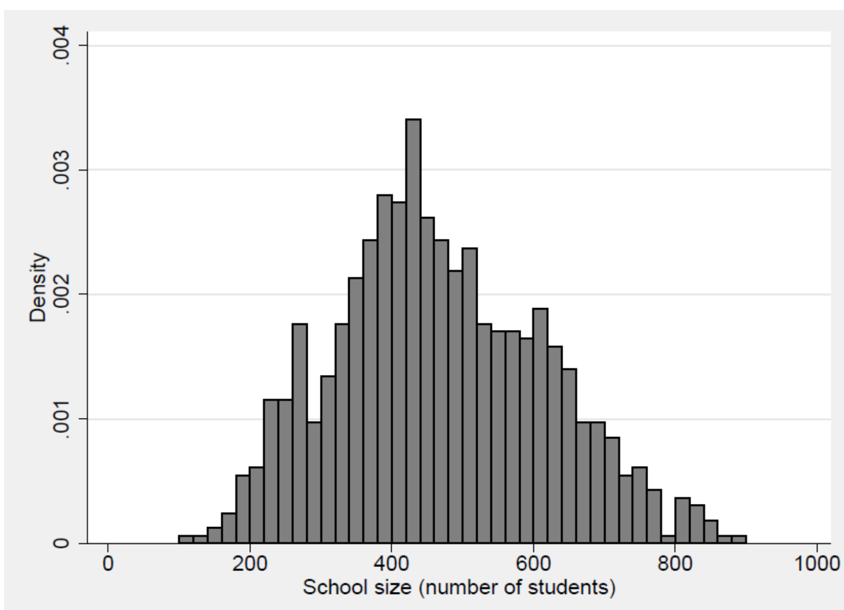

Figure 2: Distribution of school size in 2004.

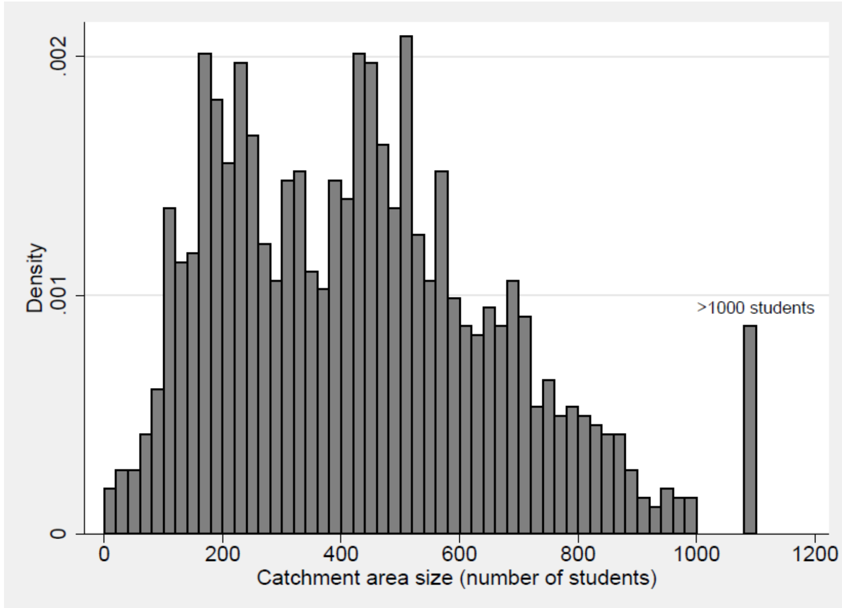

Figure 3: Distribution of catchment area size in 2004.

(mainly in the Copenhagen area) with more than 1,000 students. Figure 1 shows the average annual school size and size of catchment area in each of the years during the sample period 1986–2004. It is clear that average school size and average catchment area size change in the same direction over time. The figure also shows the number of school closures and school openings (including private schools) in each of the years 1986–2004. The maximum number of school closures during the period was 70 in 1991 while the largest number of school openings in a particular year was about 20. Thus, the sample period potentially contains a considerable number of 'shocks' to school size for the surviving schools in the sample. This is important for the estimation strategy discussed in Section 5. In order to use school openings and closings as exogenous variation in school size, we define two instrumental variables. The instrumental variable for school openings (closings) is defined as the change in school size from one year to the next *if* a school opens (closes) in the municipality of the school. If no school opens (closes) in the municipality, the instrumental variable is set equal to zero.

4.2 Student Outcome Measures

We consider a number of alternative long-term student outcomes. For all the cohorts observed during the period 1986–2004, we are able to measure the school and labor market outcomes 6 years after attending grade 9. For these cohorts, we observe (1) whether the student has completed high school or vocational education and training 6 years after grade 9 (i.e. at about age 22). However, it is quite common in Denmark to be delayed during this phase of the educational system, either because the student takes a sabbatical year, drops out of the education or enrolls in a new education etc. Therefore, we include an alternative and less restrictive measure of education: (2) enrollment in or completion of high school or a vocational education and training 6 years after grade 9. Finally, we define the outcome measure: (3) 'inactivity' 6 years after grade 9. We define 'inactivity' as a residual category in the sense that individuals are labelled inactive if they have neither completed nor are enrolled in an upper secondary education and their earnings are below DKK 40,000 (measured in year 2000 DKK). Since the effective minimum wage per hour in Denmark was about DKK 100 in 2000, this corresponds to working a maximum of 400 hours a year, corresponding to about 25 percent of the standard annual hours in a full-time job. For individuals in the earlier cohorts, we also consider the outcome: (4) annual (deflated) earnings at the age of 30. The latter outcome reflects a product of earnings potential and human capital (i.e. hourly wage rate) and employment (including both labor supply and unemployment effects) in the year. Further, for the most recent cohorts attending grade 9 in 2002–2004, we include one short-term student outcome measure: (5) average grades from the grade 9 exit exams in Math and Danish.[8] Except for the latter outcome, these outcomes have the benefit of not being affected by dropouts from school as is the case for many in-school outcomes, especially at non-compulsory levels. In other words, these are outcomes that we can observe for all individuals in the sample.

4.3 Descriptive Statistics

All children are linked to their parents through unique personal identifiers. We have information on the age, education, earnings and marital status of the parents. All parental background variables are defined at the time the child is 15 years old, i.e. one year before a typical child completes grade 9. Further, we control for immigrant status, number of siblings, birth order, age in grade 9, and the degree of urbanization. Finally, we include two variables that characterize the general socioeconomic environment of the catchment area: the share of non-Western immigrants and descendants in the population of 7-16-year-olds and log of average earnings.

Table 2 shows the means for the estimation sample. About 42 percent of the boys and 58 percent of the girls in the estimation sample have completed a high school education or vocational education six years after attending grade 9. Considering a broader measure of high school completion that does not condition on completion, about 78 percent of the boys and 81 percent of the girls in the sample have completed or are enrolled in an upper secondary education. These figures reflect that girls generally are more successful in the educational system, while boys have much larger dropout rates compared to their female peers. About 7-8 percent of individuals in the sample are characterized as inactive. For the remaining outcomes, the sample means are based on subsamples of individuals attending grade 9 in the earlier part of the sample period (log annual earnings) and in 2002–2004 (average grades in Math and Danish). Girls get higher average grades at the exit exam in grade 9, but when it comes to annual earnings, the gender gap is opposite. On average men have higher annual earnings at the age of 30, partly because of longer working hours and less time spent being on child-related leave etc. The typical child attending grade 9 in the sample is 16 years old, and about 5 percent of the children are non-Western immigrants.

[8] Specifically, we compute the average of the exam grades in oral Math, written Math, oral Danish, and written Danish. Exit exam grades were not centrally registered prior to 2002 implying that it is only possible for us to consider this outcome for the most recent cohorts.

Table 2: Sample Means

	Students attending grade 9 1986-2004			
	Boys		**Girls**	
Variable	Mean	Std. dev.	Mean	Std. dev.
Outcomes				
Completion of high school or vocational education	0.420		0.582	
Enrollment or completion of high school or vocational education	0.784		0.806	
Inactivity	0.060		0.078	
Log earnings at age 30[a]	11.064	3.763	10.565	3.954
Grade 9 exit exam grade[b]	7.326	2.572	7.707	2.358
SS and CAS				
School size (SS)	460.915	133.165	461.074	132.385
Catchment area size (CAS)	633.169	564.339	632.863	559.440
Change in school size - opening	0.577	18.157	0.540	17.594
Change in school size - closing	1.189	21.845	1.150	21.415
Controls				
Parental information at age 15				
Mother's education				
Missing	0.041		0.041	
Basic	0.383		0.392	
Vocational	0.339		0.338	
Higher	0.238		0.229	
Father's education				
Missing	0.072		0.075	
Basic	0.287		0.292	
Vocational	0.419		0.420	
Higher	0.222		0.213	
Mother is missing	0.004		0.002	
Father is missing	0.017		0.017	
Mother's log earnings	9.668	4.800	9.681	4.788
Mother's age	41.177	7.064	41.155	6.941
Mother's earnings or age is missing	0.016		0.015	
Father's log earnings	9.794	5.124	9.735	5.155
Father's age	42.287	11.242	42.141	11.446
Father's earnings or age is missing	0.052		0.055	
Mother is single	0.162		0.172	
Family type is missing	0.059		0.058	
Individual variables				
Western immigrant or descendant	0.005		0.005	
Non-Western immigrant or descendant	0.049		0.048	
Missing country of origin	0.046		0.038	
Number of siblings	1.511	1.047	1.520	1.072
Missing number of siblings	0.002		0.003	
Age when attending grade 9				
15	0.033		0.059	
16	0.863		0.879	
17	0.096		0.055	
Greater than 17	0.004		0.003	
Age is missing	0.008		0.007	
Birth order				
First	0.618		0.617	
Second	0.309		0.309	
Third or higher	0.070		0.071	
Multiples	0.018		0.019	
Municipality of residence				
Capital area	0.352		0.355	
Large cities	0.390		0.389	
Smaller cities	0.217		0.216	
Rural areas	0.041		0.040	
Share of non-Western immigrants and descendants in catchment area	0.045	0.078	0.046	0.079
Log average earnings in catchment area	11.997	0.195	11.997	0.193
Number of observations	305,429		299,696	

Notes:
a) Log earnings at age 30 are only observed for part of the sample.
b) Grade 9 exit exam grades are only observed for students attending grade 9 in the period 2002 to 2004.

5 Identification Strategy

The fundamental challenge to the identification of the causal effect of school size is the possible endogeneity of school size. To clarify, our interest is in the following model for individual i who belongs to family j, and attends grade 9 in school s at time t:

$$Y_{i,j,s,t} = \alpha + \beta SS_{s,t} + \mathbf{X}'_{i,j,s,t}\delta + \sum_{t=1986}^{2004-1} \gamma_t D_t + \mu_s + \eta_j + \varepsilon_{i,j,s,t} \tag{1}$$

where Y is the out-of-school outcome, e.g. high school completion. $SS_{s,t}$ is the school size (SS) of school s in year t. $\mathbf{X}_{i,s,t}$ is a vector of control variables, including both individual-level control variables such as gender and parental background, but also control variables at the school catchment area level, such as the share of immigrants in the catchment area. We will refer to an individual attending grade 9 at time t as being in cohort t. The D_t's are cohort dummies. $\mu_s + \eta_j + \varepsilon_{i,j,s,t}$ is a composite error term, where μ_s is constant within schools, η_j is constant within families and the last term is specific to individual i. β is the main parameter of interest and captures the causal effect of school size. The key assumption for identification of β is that school size is uncorrelated with the error term.

By using fixed effect estimators we can eliminate some of the concerns about endogeneity of school size in the outcome equation. Students are not randomly assigned to different schools of different sizes. It may be that students in small schools have different unobserved characteristics than students in large schools which would imply that estimates of the effect of school size that are based on a comparison of individuals in differently sized schools would be biased. Including a school fixed effect eliminates this problem and ensures that we only compare students who attended the same school. Another threat to identification is that parents may select the location of their residence and as such the school their child goes to. If particular types of parents have specific preferences for school characteristics, particularly school size, then simple OLS estimates of the above outcome equation would again be biased. By employing a sibling fixed effect estimator, we can ensure that we only compare students who have the same family background.

The fixed effect estimators can solve the type of static selection problems mentioned above. However, additional challenges can be caused by dynamic selection into schools and covarying trends in school size and student outcomes. For example, if there are shocks in some years that affect both enrollment and students' outcomes or simply a positive linear trend in both enrollment and outcomes, then the fixed effect estimators will be biased. Therefore we include school-specific linear trends in some of the estimations as, for example, Black et al. (2013).

If school size, $SS_{s,t}$, is correlated with the error term in Equation (1), even when we include the school or sibling fixed effects, we cannot rely on ordinary least squares to identify the parameter of interest, β. One way to circumvent this problem is to combine the school fixed effect estimator with an instrumental variables strategy. We present FE-IV estimates based on two alternative instruments for school size in order to evaluate the robustness of the fixed effect estimators.

The IV strategies are both based on variation that has been used in other applications in the empirical literature on education economics. Firstly, we use the natural population variation in the residential catchment area as an instrument for school size. Natural population variation has also been used to identify the effects of class size, Hoxby (2000), and peers, Black et al. (2013). The size of the catchment area is typically determined by fertility and settlement. In order for catchment area size (CAS) to be a valid instrument for school size, it cannot have an independent effect on students' outcomes other than through its effect on school size. In other words, it may not be correlated with the error term, $\varepsilon_{i,j,s,t}$. Since parents may in principle choose to locate in a specific catchment area based on for example the quality of the school in the area, we combine the CAS instrument with school fixed effects. Since we control for factors that are constant within schools over time, the main threat to identification is differential selection into catchment areas over time or covarying trends in catchment area size and student outcomes. Thus, we have to assume that potential trends over time in fertility or settlement are not related to school performance in terms of students' later outcomes. As suggested by Figure 1, the size of the catchment area is positively correlated with school size in the municipalities. In addition, we assume that catchment area size cannot affect other variables that determine student outcomes, e.g. peer composition. For example, if school mobility increases with catchment area size, then catchment area size may affect student outcomes through peer composition in the schools. How peer composition and student outcomes are affected would depend on the characteristics of the students who opt out of the local school.

Secondly, along the lines of Kuziemko (2006), we construct two instrumental variables based on school openings and closings (SOC). If school reorganization is mainly determined by political decisions, for instance general economic conditions in the municipality, the fact that a neighboring school will then increase in size can be considered an exogenous shock to school size. The validity of the SOC instruments is discussed extensively in Kuziemko (2006). It is obvious that school reorganizations over time will be related to the development of school size. However, the exact timing of the political decision of school reorganization is expected to be much less correlated with school size and school quality.

In order to get an idea of how the individual characteristics are associated with the instrumental variables, Table 3 presents results from school fixed effect estimations where two of the outcomes and the three instrumental variables are regressed on the entire set of control variables. Firstly, the results in columns (1) and (2) indicate that the key covariates are important predictors of student outcomes. We report results for completion of high school or vocational education and earnings, but the picture is similar for the remaining outcomes. In columns (3), (5), and (7) we regress the three instruments on the same set of control variables in order to see whether the instruments vary in a systematic way with some of the control variables which might be an (indirect) indication of potential correlation with unobserved factors affecting school outcomes. The joint F-test and the significance test indicate that we can reject the null hypothesis that the coefficients are jointly zero in the case of the CAS instrument. Since our regressions include a large number of observations (more than 600,000), even small correlations tend to show up in significant coefficients. For example, having a mother with a vocational education (relative to basic education) significantly increases the probability of completion by 7.4 percentage points. On the other hand, this same variable significantly increases catchment area size, but the order of magnitude of the coefficient is less than one hundredth of a standard deviation. Being a non-Western immigrant is associated with a large estimated coefficient on catchment area size, but even in this case the order of magnitude of the coefficient is only a little bit more than one twentieth of a standard deviation. Looking more closely at the signs and the size of the coefficients, it is difficult to find a systematic structure of the coefficients in column (3), except for the urbanization variables where the coefficients for municipalities in the large cities and the Copenhagen area have systematically larger school catchment areas. The fact that catchment area size is related to the degree of urbanization is hardly surprising, and this underlines the importance of controlling for these variables. Overall, it is clear—also from the F-statistics—that the control variables matter more for the student outcomes than for the instrumental variables. For the instruments based on openings and closings, we only see a few significantly estimated coefficients, see columns (5) and (7).

Table 3: Identification Tests

	(1) Completed high school or vocational education	(2) Log earnings at age 30	(3) CAS	(4)	(5) School closings	(6)	(7) School openings	(8)
	Coef./Std.err.	Coef./Std.err.	Coef./Std.err.	p-value	Coef./Std.err.	p-value	Coef./Std.err.	p-value
Mother's education								
Missing (Ref. Basic)	-0.039***	-0.219***	-0.050	0.106	-0.000	0.847	-0.001	0.609
	(0.004)	(0.064)	(0.031)		(0.002)		(0.002)	
Vocational	0.074***	0.264***	0.046***	0.000	-0.001	0.140	0.000	0.814
	(0.002)	(0.015)	(0.010)		(0.001)		(0.001)	
Higher	0.111***	0.118***	0.023	0.094	0.000	0.870	0.000	0.427
	(0.002)	(0.020)	(0.014)		(0.001)		(0.001)	
Father's education								
Missing (Ref. Basic)	-0.007	-0.057	0.039	0.280	-0.004**	0.011	-0.001	0.657
	(0.005)	(0.061)	(0.035)		(0.002)		(0.002)	
Vocational	0.058***	0.228***	0.027***	0.015	-0.001	0.264	-0.000	0.936
	(0.002)	(0.017)	(0.010)		(0.001)		(0.000)	
Higher	0.107***	0.167***	0.090***	0.000	-0.001	0.238	0.001	0.397
	(0.002)	(0.021)	(0.013)		(0.001)		(0.001)	
Family background								
Mother's log earnings	0.005***	0.039***	0.002*	0.066	-0.000	0.463	-0.000	0.092
	(0.000)	(0.002)	(0.001)		(0.000)		(0.000)	
Mother's age	0.007***	0.025***	0.004**	0.013	-0.000	0.316	0.000	0.917
	(0.000)	(0.002)	(0.002)		(0.000)		(0.000)	
Father's log earnings	0.003***	0.032***	0.005***	0.000	0.000	0.878	0.000	0.168
	(0.000)	(0.002)	(0.001)		(0.000)		(0.000)	
Father's age	0.000	-0.006***	-0.002	0.140	0.000	0.625	0.000	0.695
	(0.000)	(0.002)	(0.001)		(0.000)		(0.000)	
Family with a single mother	-0.098***	-0.501***	0.004	0.848	-0.000	0.977	0.000	0.476
	(0.002)	(0.021)	(0.019)		(0.001)		(0.001)	
Individual variables								
Female	0.028***	-0.528***	0.005	0.458	-0.000	0.960	-0.000	0.909
	(0.001)	(0.015)	(0.007)		(0.001)		(0.000)	
Non-Western immigrant or descendant	0.039***	-0.652***	0.356***	0.000	0.001	0.513	-0.002	0.235
	(0.005)	(0.062)	(0.093)		(0.002)		(0.001)	
Number of siblings	-0.031***	-0.139***	-0.006	0.197	-0.000	0.754	0.000	0.238
	(0.001)	(0.008)	(0.005)		(0.000)		(0.000)	
Age when attending grade 9								
16 (Ref. 15)	-0.026***	-0.079**	0.110***	0.000	-0.001	0.697	0.000	0.829
	(0.003)	(0.034)	(0.026)		(0.002)		(0.001)	
17	-0.177)***	-0.814***	0.038	0.177	0.000	0.908	0.001	0.310
	(0.004)	(0.242)	(0.028)		(0.002)		(0.001)	
Greater than 17	-0.081***	-0.985***	0.042	0.713	-0.001	0.925	0.004	0.614
	(0.016)	(0.242)	(0.110)		(0.009)		(0.008)	
Birth order								
Second (Ref. First)	-0.020***	0.019	-0.006	0.506	-0.000	0.935	-0.000	0.531
	(0.001)	(0.015)	(0.009)		(0.001)		(0.001)	
Third or higher	-0.032***	0.009	-0.031	0.136	0.001	0.647	0.000	0.944
	(0.003)	(0.038)	(0.022)		(0.001)		(0.001)	
Multiples	0.047***	0.242***	0.012	0.698	0.001	0.758	0.000	0.785
	(0.005)	(0.051)	(0.031)		(0.002)		(0.002)	
Municipality of residence								
Capital area (Ref. Rural areas)	0.109***	0.506	5.604***	0.000	0.025**	0.010	0.014*	0.071
	(0.030)	(0.375)	(0.444)		(0.010)		(0.008)	
Large cities	0.145***	1.026***	2.031***	0.000	0.003	0.601	0.001	0.654
	(0.020)	(0.240)	(0.158)		(0.005)		(0.003)	
Smaller cities	0.058***	0.510*	0.584***	0.000	0.005	0.322	0.003	0.202
	(0.021)	(0.262)	(0.159)		(0.005)		(0.003)	
Catchment area								
Log average earnings in catchment area	0.012	0.208***	-0.458	0.107	-0.004	0.674	-0.015*	0.087
	(0.008)	(0.071)	(0.288)		(0.009)		(0.008)	
Share of non-Western immigrants and descendants in catchment area	-0.120***	-0.109	6.954***	0.000	-0.013	0.476	-0.011	0.409
	(0.020)	(0.230)	(0.715)		(0.018)		(0.013)	
F-statistic (joint test)	562.08	137.17	46.13		2.28		2.86	
P-value	0.000	0.000	0.000		0.000		0.000	
Number of individuals	605,125	366,231	605,125		605,125		605,067	

Notes:
a) Columns (1) and (2) report results from OLS-FE regressions on the controls summarized in Table 2.
b) Columns (3), (5), and (7) report OLS-FE results of regressing the given instrument on the controls.
c) Columns (4), (6), and (8) report p-values from OLS-FE regressions of the given control variable on the instrument and the remaining controls. The p-value is for a t-test of the significance of the given instrument.
d) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
e) All standard errors are corrected for clustering at the school level.

6 Robustness of Estimators and Alternative Outcomes

In order to evaluate the sensitivity and robustness of the estimators, and in order to select the preferred estimator for the main results of this study, this section presents and discusses the results from a number of alternative estimators. Table 4 shows the estimated coefficients of the school size parameter on one of the outcome variables, the probability of completing high school or vocational education, based on seven alternative estimators.[9] The first column shows the OLS estimate. The correlation of school size and high school completion is positive. Since school size is measured in hundredths of students, the coefficient suggests that increasing school size by 100 students increases the probability of completing high school by 0.8 percentage points. Including school fixed effects (FE) to take account of unobserved time-invariant school-specific heterogeneity leads to a lower estimated but still significantly positive coefficient on school size. In column (3) school-specific trends are added to the school FE model. This eliminates the estimated positive school size effect which now turns insignificant. A regression of school size on school fixed effects and cohort indicators yields an R^2 of about 0.25 (0.71 when trends are included) suggesting that there is a substantial variation in school size within schools over time. In column (4) we estimate a sibling fixed effect model which controls for unobserved time-invariant family heterogeneity. This estimate is based on a subsample of students with at least one sibling in the same school. Again the school size coefficient is insignificant and close to zero.[10] A regression of school size on sibling fixed effects and cohort indicators yields an R^2 of about 0.32 suggesting that there is a substantial variation in school size within sibling pairs. In the last three columns of Table 4 alternative IV estimates are presented, cf. Table A.1 in the Appendix for more detailed results from the corresponding first-stage regressions. In column (5), the CAS instrument is combined with school fixed effects. The positive school size effect increases relative to the OLS estimate. According to this estimate, an increase in school size by 100 students increases the probability of completing high school by 2.6 percentage points. The predictive power of the instrument in the first stage is reasonable (with a t-statistic of 8.17). In column (6) the CAS instrument and the school fixed effect estimator are combined with school-specific trends. The estimated effect of school size increases dramatically. Looking at the partial R^2 of the instrument,[11] Table 4 indicates that the CAS instrument becomes fairly weak when we include school-specific fixed effects and trends. Therefore, we do not consider these estimates to be very reliable. Finally, in column (7), the SOC instrument is applied. For the first time, the estimated coefficient of school size turns negative, but it is numerically small and insignificant, even at a 10 percent level. In addition, the partial R^2 is only about 0.018, suggesting that this is a weak instrument.

[9] In alternative estimations not shown here, we have excluded the observations with the largest catchment area sizes (more than 1,000 students) and reestimated Table 4. The results are qualitatively similar when large catchment area observations are excluded. The results are available on request from the authors.

[10] When applying a simple OLS (same model as column (1) in Table 4) on the subsample of siblings, the estimated coefficient is 0.005 (significant at the 1 percent level), i.e. the coefficient for the sibling sample is slightly lower than for the full sample (0.008).

[11] A low partial R^2 is a sign of weak instrumental variables, see Cameron and Trivedi (2005).

Table 4: ROBUSTNESS OF ESTIMATORS

Outcome: Completion of high school or vocational education	(1) OLS	(2) OLS-FE	(3) OLS-FE with trends	(4) OLS-FE Sibling sample	(5) IV-FE CAS	(6) IV-FE CAS with trends	(7) IV-FE SOC
Variable	Coef./Std. err.	Coef./Std. err.	Coef./Std. err.	Coef./Std. err.	Coef./Std. err.	Coef./Std. err.	Coef./Std. err.
School size/100	0.008***	0.003**	0.002	0.001	0.026***	0.116**	-0.007
	(0.001)	(0.001)	(0.002)	(0.003)	(0.009)	(0.046)	(0.009)
School FE	No	Yes	Yes	No	Yes	Yes	Yes
Sibling FE	No	No	No	Yes	No	No	No
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	605,125	605,125	604,951[d]	276,146[e]	605,120	604,949[d]	605,062[f]
R-squared	0.112	0.095	0.095	0.029	0.086	0.080	0.086
First stage							
Instrument (CAS / SOC - openings)/100					0.035***	0.007***	0.042
					(0.004)	(0.001)	(0.075)
t-statistic					8.17	7.49	0.56
R-squared					0.275	0.707	0.266
Partial R-squared					0.030	0.003	0.018
Instrument (SOC - closings)/100							0.323***
							(0.048)
t-statistic							6.66

Notes:
a) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
b) All standard errors are corrected for clustering at the school level.
c) Specifications with trends include school-specific linear trends.
d) 11 schools are disregarded, corresponding to 174 observations. For IV estimation, 176 observations are disregarded.
e) Only individuals with a sibling attending the same school have been included in the sample.
f) 63 observations have been excluded due to missing instrument values.
g) All regressions include the controls summarized in Table 2.

In order to directly compare the different estimates in Table 4, it is necessary to assume that all individuals respond in the same way to changes in school size, in other words that responses to treatment (school size) are homogeneous. In the case where individuals respond differently to changes in school size, the IV estimates are no longer directly comparable to the OLS and FE estimates, since they no longer identify the same parameters. When treatment intensity is variable, IV estimates can be interpreted as a weighted average of the average causal response, Angrist and Imbens (1995). This relies on a monotonicity assumption. In the simplest case of a binary instrument, monotonicity entails that the instrument has to move everybody in the same direction. For example, it rules out that school size decreases for some individuals when they experience a school closing in their municipality, but would have increased if they had not experienced a school closing. Thus, based on the results from Table 4 we conclude that the effect of school size on completed vocational or high school education tends to be positive but small and the estimated impact of school size seems to be fairly robust to specifications. Our results do not suggest large negative effects of school size on long-term outcomes. Also, since the results suggest that the instrumental variables are relatively weak, one should not put too much weight on the IV estimates.

Table 5 shows the estimated coefficients of school size for a number of alternative long-term student outcomes. Despite the fact that the results presented in the previous table indicated that the estimated coefficients were somewhat robust to the choice of estimator, we choose to show the results from four preferred alternative estimators.

The general impression from Table 5 is that the estimated coefficients of school size on educational outcomes (panels A and B) are numerically small and most often positive — in the sense that large schools are 'good'. For inactivity, i.e. not being active in the educational system or labor market, the estimated coefficient is negative, i.e. students from larger schools appear to be more successful in the educational system or labor market when evaluated 6 years after the student attended grade 9. The sibling fixed effect estimates are generally insignificant as are the FE-IV estimates based on the SOC instruments—except for grade 9 exit exam grades (E).

In panel D, we report the estimates for earnings at the age of 30. All estimated coefficients are positive and in most cases insignificant. Thus, our results indicate that also in a long-run perspective, large schools do not in general harm the chances of completing a formal education and the earnings potential in the labor market. The size of the coefficients varies considerably for this outcome variable, ranging from very close to zero (insignificant) to an about 50 percent increase in annual earnings at the age of 30. The latter effect is probably strongly exaggerating the life cycle income effect of school size since it is measured at a very early stage in the labor market career. If we consider the school fixed effect estimate of school size which is 0.020 (insignificant), the interpretation is that an increase in school size of 100 students increases earnings by about 2 percent. This roughly corresponds to the order of magnitude of the estimate of the effect of a one student increase in class size on earnings from Fredriksson et al. (2013)—albeit with opposite sign.

Table 5: ESTIMATES OF THE EFFECTS OF SCHOOL SIZE ON ALTERNATIVE STUDENT OUTCOMES

	(1) OLS-FE	(2) OLS-FE Sibling sample	(3) IV-FE CAS	(4) IV-FE SOC
Outcome	Coef./Std. Err.	Coef./Std. Err.	Coef./Std. Err.	Coef./Std. Err.
A. Completion of high school or vocational education	0.003**	0.001	0.026***	-0.007
	(0.001)	(0.003)	(0.009)	(0.009)
B. Enrollment in or completion of high school or vocational education	0.002	0.002	0.028***	-0.003
	(0.001)	(0.002)	(0.009)	(0.008)
C. Inactivity	-0.004***	-0.002	-0.011*	-0.004
	(0.001)	(0.001)	(0.006)	(0.006)
D. Log earnings at age 30	0.020	0.049	0.542**	0.009
	(0.018)	(0.037)	(0.227)	(0.082)
E. Grade 9 exit exam grades	0.013	0.075	0.807	-0.179**
	(0.025)	(0.055)	(0.933)	(0.080)
School FE	Yes	No	Yes	Yes
Sibling FE	No	Yes	No	No
Observations (A, B, C)	605,125	276,146[c]	605,120	605,062[d]
Observations (D)	369,001	173,276[c]	368,994	368,936[d]
Observations (E)	79,240	27,244[c]	79,235	79,235[d]

Notes:
a) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
b) All standard errors are corrected for clustering at the school level.
c) Only individuals with a sibling attending the same school have been included in the sample.
d) Some observations have been excluded due to missing instrument values.
e) All regressions include the controls summarized in Table 2.

For a small subsample of students attending grade 9 in 2002–2004, we have information on short-term in-school student outcomes in the form of exit exam grades at the end of grade 9. Columns (1)-(3) indicate no significant effects of school size but the estimate obtained by using school openings and closings as instruments is negative (significant at the 5 percent level). This might be an indication that school size effects tend to be more negative for short-term test scores and exam results compared to long-term outcomes in the educational system and labor market. It could also reflect that school restructuring is associated with a short-run disruption effect. However, one should be cautious to draw too firm conclusions based on these estimates since the FE-IV estimates based on the SOC instruments are not very robust when we restrict the sample period to only three years. In alternative estimations not shown here, we have estimated the models included in Table 5 for all 3-year periods of the cohorts 1986–2004 for the outcome measures in panels A, B, and C. The SOC estimator is very unstable, probably because the instruments tend to be weak and the explanatory power low in the first-stage estimation when we restrict the sample to only three consecutive years. For the subsample of students attending grade 9 in 2002–2004, we have also estimated a number of models where we include grade 9 exit exam grades as a control variable.[12] The resulting estimates of the effect of school size on outcome measures A, B and C tend to be insignificant and numerically very small.

Thus, based on the results in Table 5 we cannot document that larger schools in Denmark are harmful to student long-term outcomes.[13] If anything, the tendency is the opposite, i.e. students in larger schools tend to have more success in the educational system 6 years after attending grade 9 and in the labor market at the age of 30. For the short-term outcome, the average grades in Math and Danish at the exit exam in grade 9, our results are mixed and inconclusive.

7 Heterogeneous Responses: Gender, Father's Educational Level, and Degree of Urbanization

As discussed in Section 2, earlier studies of the impact of school size have often found that the impact varies over subgroups of students. More vulnerable children, and particularly boys, often benefit from smaller classes and smaller schools in the empirical studies which tend to find negative effects of large classrooms or large schools. Since we do not find negative effects of larger schools in Denmark, it is interesting to study whether the non-negative effects of larger schools carry over to subgroups of students.

Table 6 shows selected results from estimating a number of models where we allow the school size coefficient to vary among subgroups: Gender, father's educational level, and degree of urbanization.[14] We do not find any differences in the impact of school size on long-term school outcomes based on immigration status and birth order.

[12] The results are not shown here but are available on request from the authors.

[13] This is robust to the inclusion of grade size in the specifications. The estimates of school size remain very small or positive in this case.

[14] The estimated models are the same as in the previous section, except for the interaction variable.

Therefore we do not show these results in Table 6. We apply the simple OLS estimator with school fixed effects in the results shown since the robustness tests in the previous sections indicated that the estimates are fairly robust, although the school FE tends to yield numerically more conservative estimates.

In column (1) school size is interacted with an indicator that equals one if the student is a boy and zero if the student is a girl. The coefficient of school size captures the effect of school size for girls while the coefficient on the interaction term captures the differential of the effect of school size for boys and girls. Thus, if the coefficient on the interaction term is statistically significant, we can reject that boys and girls respond similarly to changes in school size. The results in column (1) indicate that the estimated effect of school size tends to be larger for boys than for girls. However, a puzzling result appears when we consider the 'inactivity' outcome. Here large schools seem to favor girls more than boys.

The education level of one's parents is usually an important determinant of one's own educational outcomes. One explanation for this is that educational inputs potentially interact with parental education in the production function for human capital accumulation. Column (2) in Table 6 confirms that parental education level matters for the school size effect. Children of fathers who have basic or missing education (i.e. no vocational or higher education) seem to benefit significantly from large schools, cf. the coefficient on school size. The estimated impact of school size on long-term educational outcomes, the measure of inactivity, and the earnings at the age of 30 are all significantly positive (negative for inactivity). For children from families with a more highly educated father, the estimated school size effect tends to be smaller (closer to zero) than for children with uneducated fathers. We did a similar analysis for parents' earnings, but did not find evidence that the effect of school size varied substantially over quartiles. This result is in line with the findings in other studies on school outcomes in Denmark that social and educational mobility and the correlation of earnings across generations mainly are related to educational and social capital in the family and less related to family income resources. The main reason for this result is attributed to the Scandinavian welfare state model with very generous student grants, no tuition fees at all (public) educations and a fairly high level of income transfers for families with low earnings capacity.

Finally, column (3) reveals potentially interesting differences between schools in urban and rural areas. The positive estimated school size coefficients are mainly driven by positive effects for students attending schools in the Copenhagen area and the other big cities in Denmark while the estimated school size coefficient is zero or even negative for schools in smaller cities and rural areas. However, this result is not robust to a more flexible specification where the sample is split by degree of urbanization. Splitting the sample yields qualitatively similar results in the sense that school size does not adversely affect student outcomes using neither the urban sample (municipalities in the capital area and large cities) nor the rural sample (municipalities with small cities and rural areas).[15] Potential differences between urban and rural areas could be caused by a nonlinear relationship between school size and the outcome, since the average school size is only about 360 students in rural areas compared to 450 students in capital

[15]The results are available on request from the authors.

areas. Introducing the square of school size or splines in the model does not yield any evidence of nonlinearities, but it is important to note that this does not necessarily imply that school size is nonlinear. The distribution of school size in the sample may simply not allow us to identify nonlinearities since it is relatively compressed.

When comparing the results for urban and more rural areas, one should keep in mind that the dynamics of school choice are also very different. Copenhagen is a much more densely populated area than the remaining parts of Denmark and school mobility is very high in Copenhagen. In a study based on detailed data from Copenhagen on catchment areas and school choice, Rangvid (2010) reports that about 53 percent of the students in the sample do not attend the school in their catchment area. In addition, 28 percent of native Danish students attend private schools. As such, the composition of the sample is slightly different in urban areas.

Table 6: OLS Estimates of the Effects of School Size for Subgroups of Students

	(1) Gender		(2) Father's education			(3) Degree of urbanization		
	Main effect	Interaction	Main effect	Interaction	Interaction	Main effect	Interaction	Interaction
Outcome	Girl	Boy	Basic or missing	Vocational	Higher	Rural or Small	Large	Capital
A. Completion of	0.003**	0.000	0.005***	-0.003***	-0.002*	-0.003	0.009**	0.006*
high school or vocational education	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)
B. Enrollment in or completion of	0.001	0.002***	0.004**	-0.002**	-0.003***	-0.004	0.007**	0.006**
high school or vocational education	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)
C. Inactivity	-0.005***	0.003***	-0.005***	0.002**	0.002***	-0.002	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
D. Log earnings at age 30	0.018	0.003	0.035*	-0.025**	-0.022	0.004	-0.000	0.036
	(0.019)	(0.011)	(0.019)	(0.011)	(0.014)	(0.037)	(0.040)	(0.041)
School FE	Yes		Yes			Yes		
A, B, and C	605,125		605,125			605,125		
D	369,001		369,001			369,001		

Notes:
a) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
b) All standard errors are corrected for clustering at the school level.
c) All regressions include the controls summarized in Table 2.
d) Both 'Missing education' and 'Basic education' dummies have been left out as baseline in column (2).
e) Both 'Rural areas' and 'Smaller cities' dummies have been left out as baseline in column (3).

The estimation results in Table 6 reveal that the children who are often considered more vulnerable, like boys and children from families with a low educational level, are more positively affected by school size than other children. Given that we find positive effects of school size, this is in line with much of the existing research on educational resources. In the economics literature on school size, proponents of large schools typically emphasize economies of scale and efficiency, but large schools are also generally believed to have favorable effects due to higher teacher quality and their more heterogeneous student-teacher composition, see Leithwood and Jantzi (2009) and Kuziemko (2006). While our data contains very detailed information at the student level, we have no information on teachers. Thus, we can only speculate about the potential mechanisms that generate our results. One central aspect of large schools is that they typically have a higher number of students per grade. Since we consider the total policy effect of school size, grade size is considered part of the effect of school size. Having more classrooms per grade can be beneficial since the teachers in the different classrooms can interact, cooperate and develop teaching material which is very common in Danish schools. Large schools can exploit the potential positive diversity effects among students from different social groups, allow for flexibility with respect to matching groups of students and teachers and to attract higher quality teachers. Vulnerable children may be especially sensitive to e.g. the quality of the teacher. However, one should keep in mind that the numerical size of the effects in most cases is not very large. On the other hand, this study does not give support to the argument that large schools (in Denmark) are harming the educational outcomes of students.

8 Conclusion

Does school size have an impact on students' long-run education and labor market outcomes? This question has been an important policy question in many countries because school size is one of the parameters in school policy which is fairly easy for politicians to control. While class size may mainly affect the actual learning environment and thereby students' cognitive skills, school size may have other or supplementary effects on especially noncognitive skills that may be important for students' success in the labor market and the ability to complete an education. However, it is not an easy task to document the causal impact of school size on students' outcomes because school size is a variable which is potentially endogenous in a student outcome equation, and it is hard to identify valid instruments for school size. Our estimation strategy is to control for a really large number of background variables and to apply a number of alternative estimators and instruments, evaluate the robustness of our estimates, and refer the more conservative estimates of the relation between school size and school outcomes.

With these reservations in mind, we find that for students attending grade 9 in Danish public schools, school size tends to have no effect or even a positive effect on educational outcomes and earnings later in life, at the age of 30. This result is different from the results found in a number of studies from mainly the UK and the US. However, Danish schools are on average much smaller than schools in the US and the UK. The average school size in this study which covers almost all public schools with grade 9 in Denmark was about 460 students. Another interesting result is that the positive effects of school size tend to be larger for boys when we consider educational outcomes like the probability of completing high school or a vocational education and training program, and for children who have fathers with a low education level. Thus, students who are traditionally considered more vulnerable seem to benefit from larger schools.

If school consolidations imply cost savings due to economies of scale, this study indicates that there may be a case for school consolidations in countries such as Denmark where the average school size is small. The results found in this study do not support the arguments often put forth that increasing school size harms student outcomes. On the other hand, it is important to stress that local schools often have many functions and may be important for the social activities in a local community. These potential negative effects of school consolidations are not included in this study.

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A Tables

Table A.1: FIRST-STAGE ESTIMATION RESULTS

Dependent variable: School size/100	(1) IV-FE CAS Coef./Std. err.	(2) IV-FE CAS with trends Coef./Std. err.	(3) IV-FE SOC Coef./Std. err.
Instrument (CAS / SOC - openings)/100	0.035***	0.007***	0.042
	(0.004)	(0.001)	(0.075)
Instrument (SOC - closings)/100	n.a.	n.a.	0.323***
			(0.048)
Mother's education (Ref. Basic)			
Missing	0.002	0.008***	-0.004
	(0.005)	(0.003)	(0.002)
Vocational	-0.006**	0.002*	0.018***
	(0.002)	(0.001)	(0.003)
Higher	0.017***	0.004***	-0.025***
	(0.003)	(0.001)	(0.006)
Father's education (Ref. Basic)			
Missing	-0.028***	-0.003	-0.004**
	(0.006)	(0.003)	(0.002)
Vocational	-0.005***	0.000	0.011***
	(0.002)	(0.001)	(0.003)
Higher	0.008***	0.002	0.083***
	(0.003)	(0.001)	(0.019)
Family background			
Mother's log earnings	-0.001***	0.000***	0.001***
	(0.000)	(0.000)	(0.000)
Mother's age	0.001***	-0.001***	0.043***
	(0.000)	(0.000)	(0.016)
Father's log earnings	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Father's age	0.000	0.000	0.026**
	(0.000)	(0.000)	(0.012)
Family with a single mother	-0.005**	0.000	0.000
	(0.002)	(0.001)	(0.001)
Individual variables			
Female	0.000	0.000	0.001
	(0.002)	(0.001)	(0.005)
Non-Western immigrant	-0.042***	-0.013***	-0.030***
or descendant	(0.009)	(0.004)	(0.010)
Number of siblings	0.005***	0.001**	0.005***
	(0.001)	(0.000)	(0.001)
Age when attending grade 9 (Ref. 15)			
16	0.025***	-0.002	0.029***
	(0.005)	(0.002)	(0.005)
17	0.007	-0.006*	0.008
	(0.006)	(0.003)	(0.006)
Greater than 17	-0.009	0.015	-0.007
	(0.020)	(0.012)	(0.020)
Birth order (Ref. First)			
Second	0.050***	0.008***	0.051***
	(0.005)	(0.002)	(0.005)
Third or higher	0.033***	0.002	0.034***
	(0.004)	(0.002)	(0.004)
Multiples	0.018***	0.004	0.018***
	(0.006)	(0.004)	(0.006)
Municipality of residence (Ref. Rural areas)			
Capital area	-0.299***	-0.080***	-0.111***
	(0.047)	(0.025)	(0.041)
Large cities	-0.123***	-0.052**	-0.053
	(0.035)	(0.022)	(0.035)
Smaller cities	-0.080***	-0.034*	-0.062**
	(0.030)	(0.019)	(0.030)
Catchment area			
Log average earnings	0.352***	0.058***	0.338***
in catchment area	(0.071)	(0.022)	(0.073)
Share of non-Western	-0.020	0.091*	0.228*
immigrants and descendants	(0.125)	(0.052)	(0.128)
in catchment area			
Number of individuals	605,120	604,949	605,062
R-squared	0.275	0.707	0.266

Notes:
a) The reported results correspond to the first-stage regressions of columns (5), (6), and (7) in Table 4.
b) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
c) All standard errors are corrected for clustering at the school level.
d) All regressions include the controls summarized in Table 2.